

Materia medica, or, Pharmacology and therapeutics (Volume 1, Part 1).

Contributors

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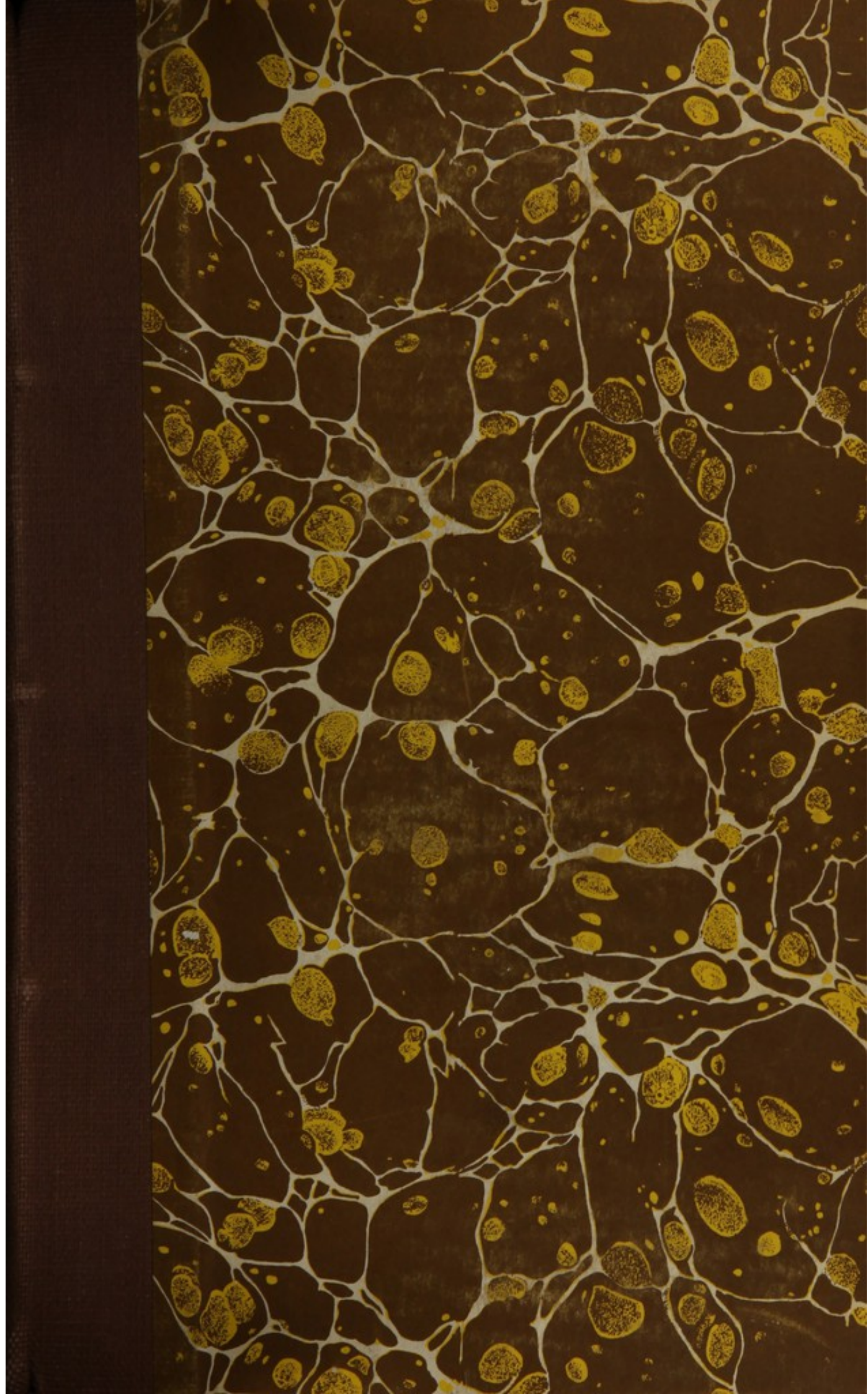
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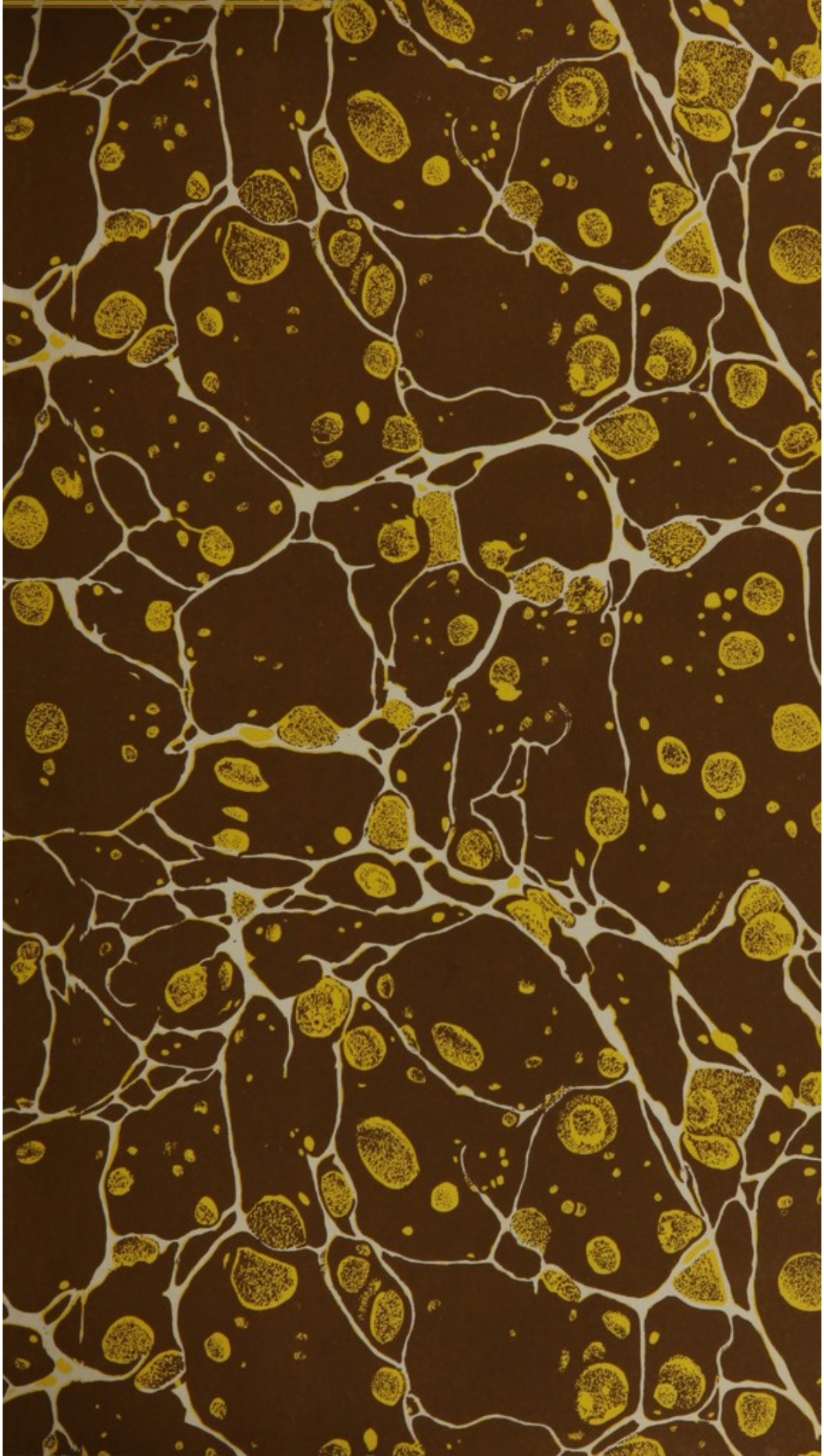
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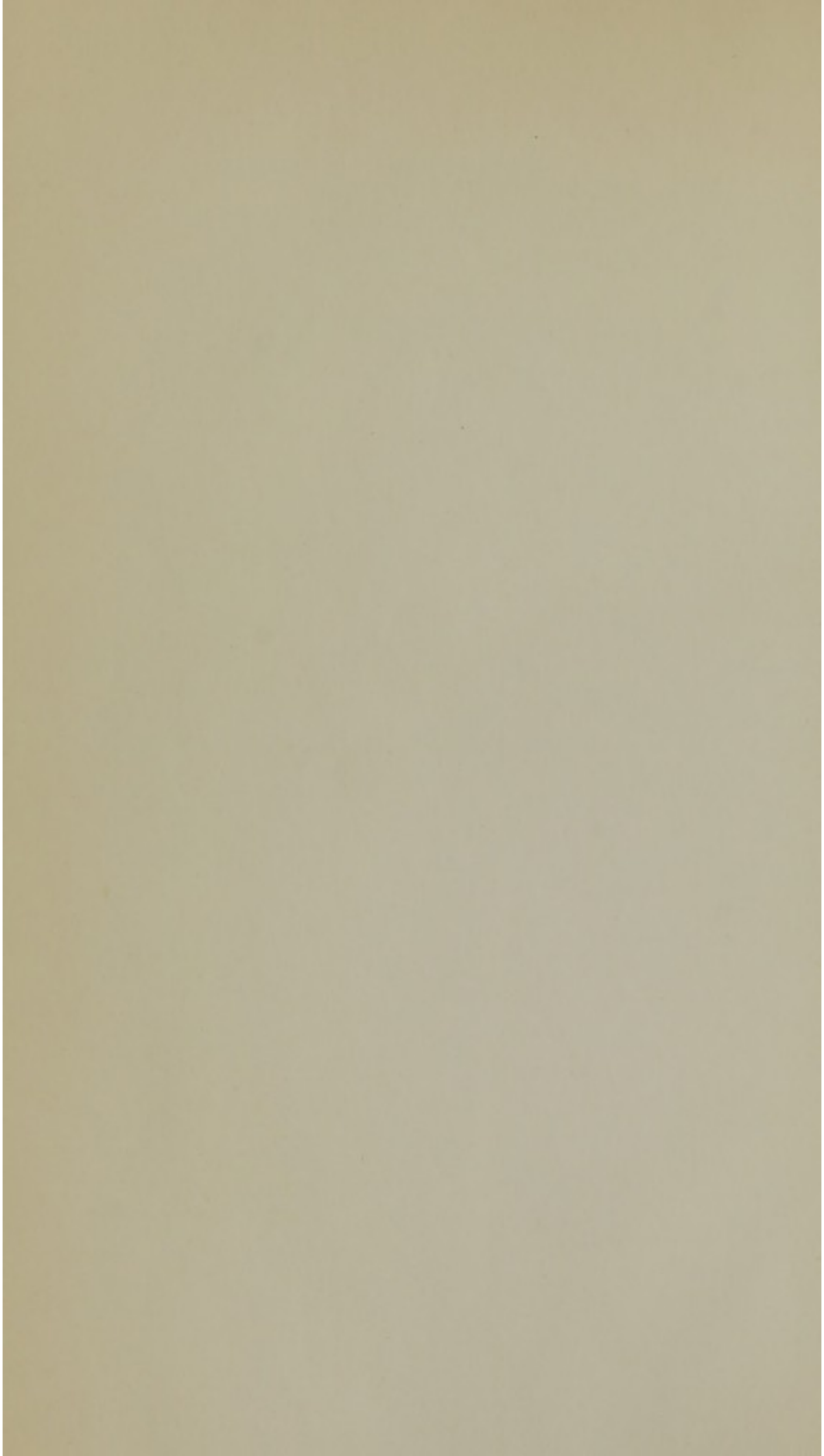
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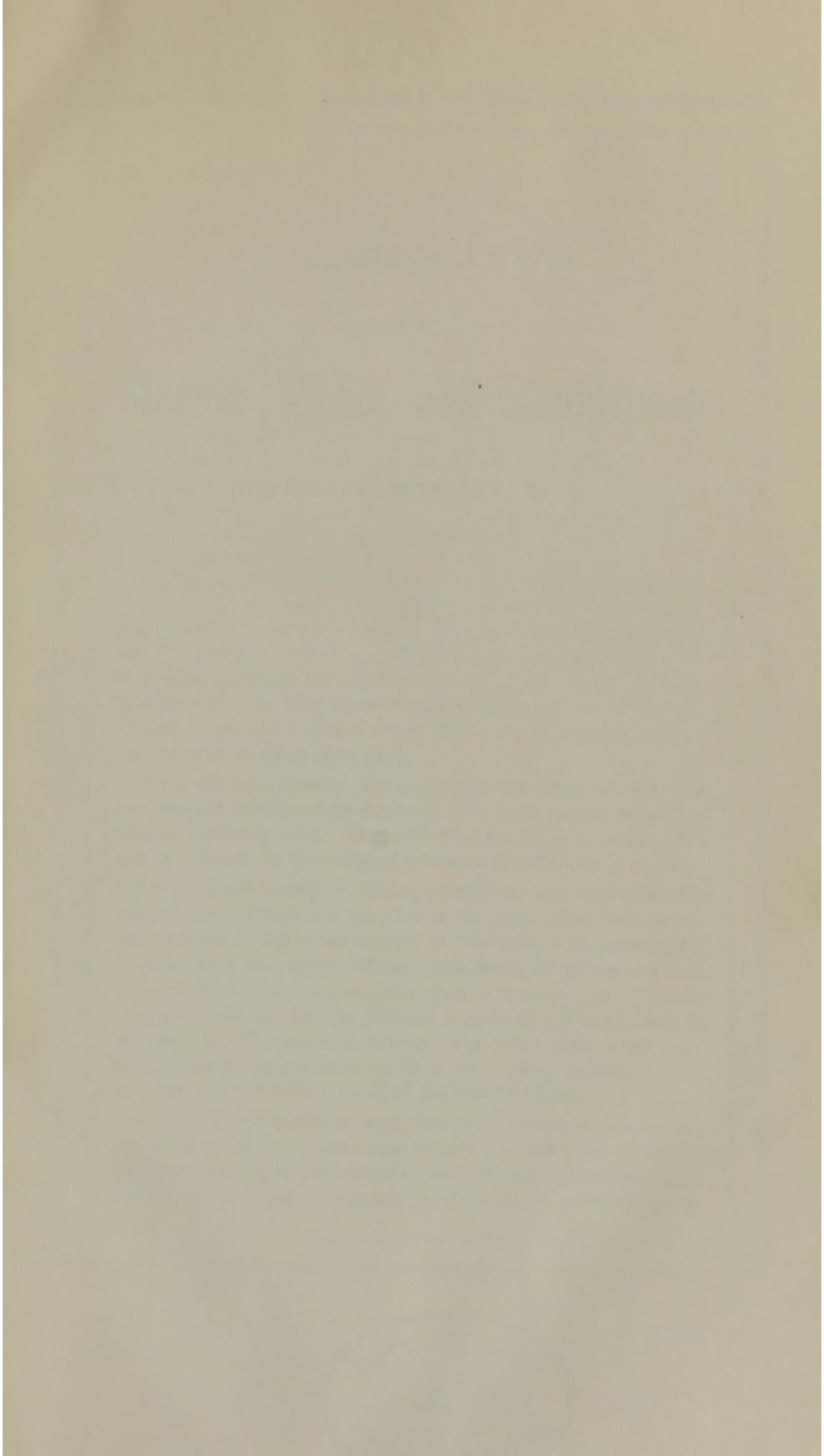
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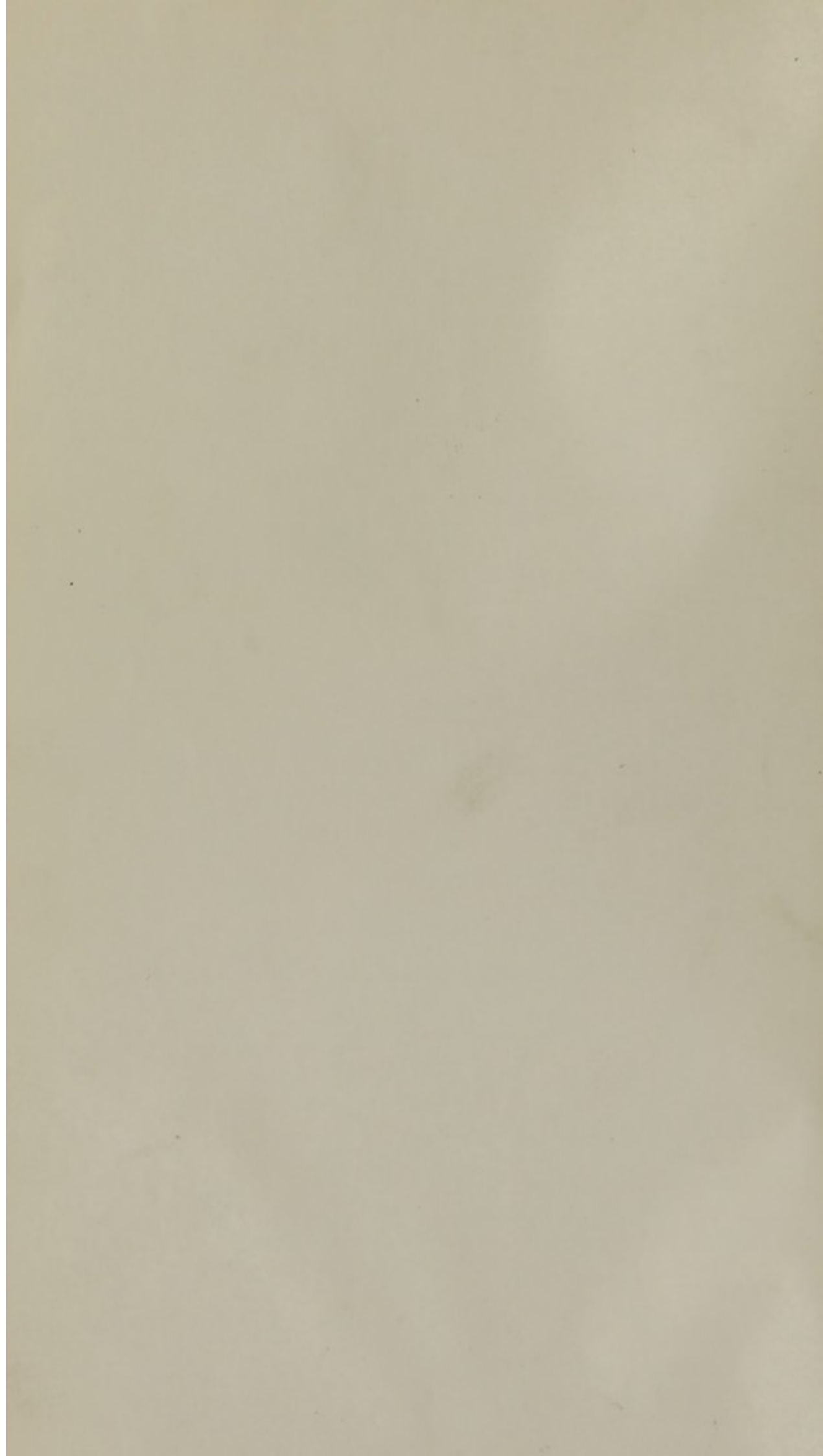
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PROSPECTUS

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Materia Medica and Therapeutics.

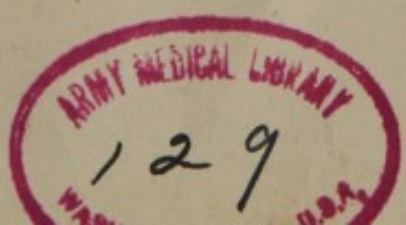
BY WILLIAM TULLY, M. D.

WE propose to publish this work of Dr. Tully's if a sufficient number of subscribers can be obtained to warrant us in so doing. The plan we propose is to issue it in numbers of sixty-four pages each, and if it should be sufficiently patronized to continue it till the whole of his manuscripts on the subject are published, or enough of them to make one of the most extensive and complete systems of Materia Medica extant, and in such a manner that it can be bound in separate volumes with an index at its close.

Those who have attended the author's instructions, or otherwise been enabled to judge of its character, have long desired to see his Materia Medica in print. They will subscribe for it unasked. We rest its claims to the patronage of others on the following grounds:

First. It will be original, having none of the characteristics of a compilation. The plan of the work is his own. The field he has explored, the objects he has sought; and the manner of pursuing his enquiries have been quite different from those of ordinary writers. His classification differs materially, and is founded upon different principles from any that has preceded it, and will commend itself to any one who will examine it critically, and with a view to its practical tendency. It will therefore be a new system essentially, and may justly be entitled a revision of the Materia Medica.

Second. It will contain a large amount of practical information not found in ordinary books on the subject. It will treat of all the important articles in common use, and of many potent ones, not commonly mentioned, or only briefly alluded to by authors; and in



such a way as not only to test the correctness of prevailing opinions and theories, but to add new facts and principles to the science, and thus to enlarge the resources which this department of medicine brings to the aid of the practitioner. His experience with many new articles, which are mostly indigenous to America, and also with those officinal ones, the powers and therapeutic applications of which, have not been clearly defined heretofore, will be found particularly interesting and valuable. The precise operations of some, in regard to which conflicting opinions exist, have been settled, probably beyond dispute, by his experiments and researches.

Third. The powers and operations of medicines will be described with minuteness and precision. Both their primary and secondary, or their immediate and remote effects will be clearly distinguished; and such modifications of them as result from their different modes of management, and from the different pathological states in which they are administered. The different and distinct powers that each individual article possesses, will be distinctly pointed out, as will also the particular parts of the system on which they principally operate. Even among those that act principally upon the nervous system, important distinctions are made, not only as respects the strength, quality and duration of their effects, but the particular subordinate parts of the nervous system in which they are more especially manifested, and to which, in regard to some, they seem almost wholly confined. Similar distinctions are made in treating of those that affect, principally the secernents and absorbents or the glandular system; and also of those belonging to other classes. As regards their therapeutic applications, distinctions of this kind are highly important, and have been so regarded as far as they have been noticed heretofore; but the extent to which they have been carried, the clearness with which they are recognized, together with the therapeutic principles deduced from them, by the author of this work, will constitute one of its distinguishing characteristics.

Fourth. The doctrines it inculcates, are not speculative or theoretical, but eminently practical, and are founded upon extensive observation and experience in the use of medicines.

Fifth. It will be written with ability and learning such as would do credit to the profession in any country. "Dr. Tully has been justly regarded by all who have known him, as one of the most erudite and philosophical scholars in the medical profession." His ardent devotion to the subject for more than forty years, his vast amount of manuscripts which have been accumulating during the



whole of this period, and the particular attention he has bestowed upon articles indigenous to this country, peculiarly fit him for the undertaking ; while his thorough knowledge of Botany, Chemistry, and all the departments of natural history that can be brought to his aid, his attainments in lexicology, his clearness and precision in the use of language will enable him to give it a degree of perfection and scientific accuracy, of which from the nature and diversity of the subjects embraced, it hardly seems susceptible.

Sixth. Considering the intimate connections of pathology and therapeutics in a practical point of view at least, it is somewhat remarkable that the former should have received so much attention of late, while the latter has been comparatively neglected, especially as the *Materia Medica*, so far as practical results and useful discoveries are concerned, presents by far the most inviting field of labor. The deficiency occasioned by this neglect will be made up, in a measure at least by this publication.

Such will be some of the characteristics of the work, which as it would seem must render it highly acceptable to physicians, and induce them to aid us in our attempt to publish it, by sending us their names as subscribers. Without undervaluing other works of the kind already published, and without claiming for our author infallibility in all his statements and inferences, or perfect soundness in all his reasonings, to the medical student we can promise a rich reward for his labor, in perusing any portion of it, and to the practitioner who shall possess it entire, a book of reference, the place of which cannot, at present, be supplied by any other, and on that branch of his profession, upon a thorough knowledge of which, more than upon any other, his success must depend.

In common with others upon whose judgment full reliance may be placed, we should deeply regret that so large an amount of valuable information as is contained in these manuscripts should remain unpublished and be finally lost to the medical world, a result that must be anticipated unless the publication should be speedily commenced, and it would be discreditable to the profession, to say the least, especially to the profession of our own country, if talents, industry and learning, such as have been employed in collecting and arranging it, should be passed unnoticed and unrewarded. Aside from our belief in the practical utility of the work, these considerations, more than any other, prompt us to engage in its publication, from the responsibility of which we would gladly relieve ourselves, if others could be found who were willing to assume it.

NEW HAVEN, July 13th, 1852.

TO J. CHURCH, M. D., SPRINGFIELD, MASS

DEAR SIR:—I am very glad to learn by your letter of the 28th of June of the proposed publication by Wm. Tully, M. D., of a series of essays on *Materia Medica* and kindred subjects. I can not doubt that an undertaking of this kind will be sustained by the profession. Dr. Tully has long been known as a most assiduous and successful cultivator of this branch of medical science. His acknowledged talents, unwearied industry, and great minuteness of research, will enable him to prepare a work of great value and usefulness to medical men, and one which will advance the interests of medical science. I wish you all success in this undertaking, both for the sake of the author and of the medical profession. Yours with Respect,

JONATHAN KNIGHT, M. D.

We most fully and cheerfully concur in the above,

CHARLES HOOKER, M. D.

HENRY BRONSON, M. D.

Other letters of similar import to the above have been received from men of high standing in the profession which we are at liberty to publish.

It will be printed with good type on good paper, and in the best style. The Nos. will be issued monthly commencing in Nov. next. If from the state of the author's health, he should be unable to revise his manuscripts with sufficient rapidity it may be necessary to lengthen the intervals of publication, of which due notice will be given.

Terms twenty-five cents each, or one dollar for every four numbers, payable in advance, on the receipt of the first. No subscription received for less than four, and to be discontinued at the option of the subscriber. It will be sent to subscribers by mail.

 The postage to any part of the United States will be one cent a number.

As to the size of the work we have no means of judging. Probably it will extend to twenty numbers.

Those who wish to become subscribers can do so by writing their names and post-office address on this sheet, and returning it to "J. Church, Springfield, Mass.," to whom all communications on the subject should be addressed, post paid.

J. CHURCH, M. D.

E. SEEGER, M. D.

SPRINGFIELD MASS., Oct. 1852.

RECOMMENDATIONS.

"Dr. Tully has been known as a popular teacher of medicine for many years, and as one of the ablest scholars in our profession. He has devoted himself with much assiduity to the study of his favorite pursuit, and has tried the effects of medicinal agents upon himself and upon others to an extent perhaps unequaled by any other physician in our country. * * * * * No one can read the portion of the work already published, without being impressed with the great extent as well as accuracy of the author's knowledge."—*New York Medical Times*, March 1853.

"The ninth number fully sustains the character of the work. We shall wait with no little desire for the completion of this work."—*New York Journal of Medicine, &c.*, March 1854.

"Professor Tully's great work on *Materia Medica* will prove one of the most learned and able productions ever published on that science."—*N. Y. Scalpel*, November 1853.

"With regard to the indigenous Materia Medica, we are persuaded Dr. Tully has much that is new and valuable to impart. * * * We hope its success will be ample enough to render it remunerative, and should look upon it as a national loss if the work should be stopped short of completion."—*New York Journal of Pharmacy*, April 1853.

"The fourth number has come to hand, and fully bears out the reputation of the previous three. This work will form an era in American Medical Authorship."—*Philadelphia Medical and Surgical Journal*, April 1st, 1853.

"He" (the author,) "is described by those who know him, as a man of great independence and originality, of great diligence and enthusiasm; and his work will bear evidence of all these characteristics. It is impossible to say to what extent his views of the mode of action and uses of our various remedies differ from those of ordinary writers; but the fact that they do differ should lead to a careful investigation of them, and to a calm decision upon their merits."—*New Hampshire Journal of Medicine*, Jan. 1853.

"That it will be original the first numbers give unmistakable evidence, but we very much fear that the profession will not generally indorse the propositions of the distinguished author."—*North Western Medical and Surgical Journal*, February 1853.

It needs but a cursory view of the numbers before us to show that the author has approached his work with a scholarly and scientific preparation extremely rare in its thoroughness among American authors. The next apparent fact is that the author is an independent thinker, who, versed in the learning of his predecessors and contemporaries, adopts none of their systems of thoughts or modes of intellectual procedure, but makes himself the center of analytical and sythetical action upon all the facts and theories and systems that are piled

around him. With these qualifications—this essential vitality of thought, insight and investigation—an author cannot fail to write interestingly and usefully, even if in many points he may be in error.—*Springfield Daily Republican, August 8th, 1853.*

“ We have received Nos. 13, 14 and 15 of this work, which brings it up to the Proem to the class Euphrænica. We hope this work, which is unquestionably the most learned which was ever written on this subject, will receive such support as will enable its venerable author to bring it to an early completion.—*New Hampshire Journal of Medicine, April 1856.*

“ We cannot too strongly recommend it to the patronage of the profession, as we are satisfied no one will regret of his investment.—*The Peninsular Journal of Medicine, Feb. 1856.*

“ By men of talents in the medical profession who have been accustomed to his lectures, correspondence and conversation, it will be confidently predicted that a comprehensive work upon *Materia Medica* from Professor Tully, will be the most original, philosophical and practically useful book for the physician which has appeared since the writings of Sydenham. Professor Tully is one of the few physicians who are imbued with the true spirit of the Baconian Philosophy. Not a hypothesis or mere theory has he ever advanced.

At his present age, and in consequence of his numerous places of residence, which have been in regions of differing climatic and endemic influences; of his good fortune in opportunities for observing fatal and peculiar epidemics; of his uncommonly social habits in collecting information from his professional brethren; of his widely extended and systematic correspondence; and of his great success in enlisting the talents and zeal of co-laborers in his investigations; few men, since the time when Hippocrates spent his time in traveling over Greece, have had an experience in diseases and their remedies, so extensive, minute and accurate as that of Professor Tully. Having resided in the mountainous parts of New England, in its hilly regions, and in various alluvial districts on its rivers—living by his

profession in rural parishes and in manufacturing and sea port towns—having practiced extensively in malarious regions of the State of New York—having lived by his profession for a year in the interior of South Carolina—and having enjoyed a wide and high reputation as Professor, Instructor, and Counseling physician in those central places of resort and travel, Albany and New Haven, his opportunities of residence, joined to his constant correspondence, have made him familiar, as well with the local influences, as the general diseases of the whole extent of the United States. During nearly half a century of professional life, his cool enthusiasm and ardent industry have scarcely relaxed for a single day. He has expended the amount of a modest pecuniary independence in accumulating a large professional library, and is well known as a man of learning and of truly scientific accuracy and caution.—*From a Physician who has been in habits of intercourse with Dr. Tully for twenty-three years.*

“I have read this work with much satisfaction. It evinces a mind in the author of high and varied culture and of great powers. It is original and suggestive to a high degree, and has a rare power of exciting thought and a spirit of investigation in the reader. Every physician should read it—yet I can hardly think that it would be a safe guide for the student or young practitioner.”

M. W. WILSON.

“Why does the Cyclopædia exclude all notice of Theodoric Romeyn Beck? His brothers John B. Beck and Lewis C. Beck, are also entitled to favorable notice as contributors to medical science.”

“So is Dr. Wm. Tully, of Springfield, Mass., formerly President and Professor of Theory, and Practice in the Vermont Academy of Medicine, and Professor of Materia Medica and Therapeutics in Yale College,—undoubtedly one of the most profoundly and extensively learned physicians of America, who has done more in the line of Pharmacological research, and in contributions to the knowledge of the indigenous Materia Medica of the country, than any other living man.” “His great systematic work on Materia Medica, now for a year or two in process of publication (serially) gives him a high place in American Medical Literature, as do also his previously published investigations in Medical and natural science.”—*Review of Cyclopædia of American Literature, New Englander, August 1856.*

MATERIA MEDICA
OR
PHARMACOLOGY
AND
THERAPEUTICS.

BY WILLIAM TULLY, M. D.

VOL. 1. PART 1.

"IGNORANCE is preferable to error; and he is less remote from the truth who believes nothing, than he who believes what is wrong.—*Jefferson's Notes on Virginia.*"



SPRINGFIELD:
PUBLISHED BY JEFFERSON CHURCH, M. D.
1857.

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1858

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SPRINGFIELD:

PUBLISHED BY JEFFERSON CHURCH, M. D.

1857

PREFACE.

SEVERAL different plans for the execution of the following work were contemplated and discussed; and the author finally selected that which the publisher seemed to prefer, viz: the plan of a systematic arrangement, i. e. a regular classification, under which all the operations and effects that pertain to a whole class are to be stated, explained and enforced in the form of a proëm to the class, instead of doing it in connexion with each individual article. Again, the several articles were to be distributed into groups subordinate to the class, and every thing peculiar to each group was to be said in connexion with the group, and not in connexion with each individual article; while only the peculiarities of each article, when it happened to have any, were to be mentioned under the individual article. Upon this plan all the individual articles were to have been fully treated of in their respective classes; and of course were to have been mentioned only in one single place, viz: immediately following the proëm to the class into which they were reduced. Under this method, nothing but the peculiarities of the article in comparison with other articles of the same group, would have remained to be said under the individual article itself; and in many instances this would have been scarcely any thing. What medicinal peculiarities, in comparison with each other, that can be specifically described, have Sulphate of Potassa, Sulphate of Soda, Sulphate of Magnesia, Tartrate of Potassa and Soda, etc? Again, what medicinal peculiarities have a large number of the simple bitter Tonics in comparison with each other? If this plan had been adhered to, it would have been the briefest of any that was contemplated.

As soon as it became known that this work was positively undertaken and actually begun, there were numerous private applications that it should contain many things not ordinarily comprised in the common works on materia medica, as 1. The chymical composition of all the articles of inorganic or chymical origin; and of all the medicinal principles that have been separated and analyzed from organic substances. 2. A specification

of the Natural History, orders and tribes, under which all organic substances properly fall. 3. The most correct Natural History and chymical names of all the articles, together with their whole synonymy ; or at least the whole ever used in the *materia medica*. The first and second were desired because few physicians and apothecaries possess scientific works sufficient to furnish the desired information in all cases ; and the third was wanted as a key to the nomenclatures both of old books and new ; those of continental Europe and Great Britain, as well as of our own country. 4. That all articles definitely known to possess valuable medicinal powers should be mentioned (briefly if in no other way) in their proper places in the work, since articles new to daily practice are constantly undergoing more thorough investigation, and coming into extensive use. 5. It was desired and requested that a brief exposition of the principles and laws of the nomenclature of the antiphlogistic system of chemistry (so far at least as this nomenclature was to be employed in the work, if not further) should be added to the introductory matter.

These are by no means all the requests that were made, since some, for example, desired an exposition of the several subordinate parts of the nervous system, with an account of the peculiar functions of each, because a general understanding of this subject seemed to be necessary to the *materia medica*. How far these several requests could be complied with, neither myself nor the publisher of the work ever decided ; but the statement that they were actually made will explain the occurrence of some things in the work which otherwise might not have appeared ; and the statement that the printing could not be afforded will explain the discontinuance of some of these, which were actually begun.

It may be necessary, for the explanation of certain circumstances in regard to the work, to state that its plan has undergone a very material change several times since its printing was begun. Till most of the introduction (i. e. the institutes of the *materia medica*) was printed, there was no thought of giving a synopsis of the classification. This we were finally induced to do from the frequent complaints that there were so many novel terms employed in the work in relation to this subject which were not readily understood. This complaint I then deemed, and still deem groundless, since, as I firmly believe, every such term has been directly or indirectly explained when it was first employed. However this synopsis was of no consequence except as occasioning so much superfluous printing since it was of necessity repeated in the proëms to the classes.

The original plan of the work, as I have just said, was that each article in the *materia medica* should be treated of in immediate connexion with its name under each group at the end of the proëm. But very little

interest seemed to be felt, under the prevailing character of our diseases, in the class *Antiphlogistica* ; and it was judged that being a less interesting class it would occasion a prejudice against the work ; and that therefore only the grouping of the articles should follow the proëm, the consideration of the individual articles being left for a different place.

It will be recollected that the groups of the articles were founded upon medicinal affinities and discrepancies, but that in a number of the early classes their Natural History affinities were likewise specified. After a while, it was determined, in order to save printing, that the expression of the Natural History affinities should be omitted, though the same reference was really had to them as before in making the grouping. At a still later stage of the work, and still in order to save printing, it was decided to dispense intirely with the grouping of the articles. As will readily be perceived, this must involve a totally different arrangement of the subsequent volume on individual articles from what was contemplated at first and must in fact make it an independent work in all respects except as respects the understanding of the terms employed.

In the early part of the work it will be recollected the chiminal composition was annexed to all the articles of inorganic origin. This was considered troublesome to print and the omission lessened the expense, and therefore it was discontinued.

Upon the original plan and under a strict arrangement founded upon medicinal affinities and discrepancies, many articles being treated of comparatively, might be treated of briefly, and yet clearly and satisfactorily. Now under quite a different arrangement (as yet I know not what) each article must stand by itself, and will require to be treated in a greater or less degree as a monograph. So far as respects my own judgment, I greatly regret that my own original plan could not have been carried out ; and if my time of life and public favor would allow me to anticipate a new edition, I should think it decidedly best to restore the work to that plan, which under the circumstances of a reprint, would not be liable to the same objections, and would be comparatively easy. Then, according to the original plan, what belongs to the whole class would be found in the proëm to the class, the peculiarities of each group would be found under the group, and the peculiarities of the individual under each article.

Objections are made to the extent of the catalogue of the *materia medica* ; but should not every article that has valuable medicinal powers be kept before the medical profession ? How otherwise is this department to be improved ? Certainly it was in a far better state in the time of Sydenham than in the time of Dioscorides ; and it is now in a far better state than in the time of Sydenham. But how has this improvement been

effected? Simply by a more thorough investigation of articles not in former use, and substituting them for articles of very inferior value that had long been employed. I cannot doubt in the least, that there is far more improvement still to be made in the *materia medica*, in comparison with that of the present period, than that of the present period in comparison with that of Dioscorides. But if every article not in constant and daily use by the medical profession is to be kept out of sight, how can we expect the advancement contemplated?

But we hear a great deal of declamation about a long and extensive jargon of medicines, when only a small number are required. But even supposing it true that only a small number are required, is that any reason why these should not be selected from a large list, so as to be the better adapted to the peculiar circumstances of the case? I never yet happened to meet with a physician who had practised his profession the average length of time who did not readily admit that his *materia medica* had undergone great change, and as he thought, great improvement, and who did not suppose that this would never have taken place, if he had not constantly had before him a much more extensive catalogue of articles than he ever employed at any given time. There is never any considerable number of physicians of any given period, that have such a knowledge either of chemistry or natural history as to allow them to travel to any extent out of works of *materia medica* for newer and better articles.

But I have often heard physicians of high popular reputation and character declaim against *materia medica* generally, and in fact absolutely; though they themselves did not admit that it was quite absolutely. These gentlemen seemed to entertain the opinion that physicians would finally be able to cure all diseases some how or other by a more perfect knowledge of anatomy, physiology and pathology, and by the aid of regimen only in addition. Some of them however have proposed to call in the aid of certain instruments. I have heard of physicians who believed that the microscope was likely to be of more value towards the treatment of disease than the whole *materia medica*. A medical gentleman once said to me that he thought so. I inquired if he thought it would do this by leading to more correct indications of treatment. His answer was in the negative. He thought that it would lead to knowledge that would supersede the use of medicines, but exactly how, I could not ascertain. In short he seemed to consider anatomy as the ultimate end of medicine. I have often been applied-to, to take charge-of, and treat patients on the condition that I should not prescribe or administer any medicines, and this in diseases in which mere regimen, as I judged, could not have been of the least avail. Such cases however I have always declined, so that I

have never had opportunity to ascertain exactly what would have been expected of me. To physicians with such views I can not expect that the extent of my catalogue of articles, or my views and principles respecting the treatment and cure of disease will be congenial.

For myself, I can not doubt that to the very end of time, what are called medicines will continue to be the chief and indispensable instruments in the treatment and cure of diseases, and that the larger our list of articles and the greater the number which we carefully and thoroughly investigate, the more rapid will be the improvement of the *materia medica*. No man living values a thorough and accurate knowledge of pathology more than I do, and no man would pursue it with more zeal than I would; but what is the most perfect pathology without *materia medica*, i. e. without the instruments for remedying it. I have not the least doubt that *materia medica* without pathology is far preferable to pathology without *materia medica*. Many a disease is curable of whose pathology we are ignorant.

But though I am greatly in favor of an extensive catalogue of articles in this department, yet the relinquishment of my original plan of grouping out my articles according to their medicinal affinities and discrepancies, and treating of them under such groups, has left me at full liberty, under a different arrangement, to treat of as many or as few as circumstances seem to require. This work was never intended to be a synopsis or compendium of the *materia medica*. It does not appear to me that such a work is at present wanted; and provided it is, I could not do justice to the principles contained in my first volume within such a compass. As appears to me, we need such a work for the present time as Baron John Andrew Murray's was for its time, except that it should contain a natural classification, as far as such a thing is practicable. It will be recollected that his work extended to six octavo volumes, and comprised about every article then definitely known to possess valuable medicinal powers.

It ought perhaps to be here stated, that the proëms of several of the later classes, and one even of the earlier, have been abridged altogether beyond what I should have chosen, and beyond what was desirable, in order to shorten the work. For example, under Oresthetics nothing of any amount is said of their external i. e. their Epispastic application, since as briefly as that subject could well be treated, it was likely to occupy more space than could be afforded. This was decided when it was expected that all the proëms could be bound in a single volume. It was intended to supply the deficient matter under individual articles.

I consider it as very much to be regretted that the intire manuscript

for the work could not have been wholly written out before the printing was begun. My manuscripts, from which I formerly gave instruction, were to a great extent mere briefs like those of an advocate in our courts of justice, and where any thing was actually written out in detail, it was not in a shape suited to a work on the *materia medica*, and could not possibly have been printed without being rewritten.

In regard to the time occupied in the preparation of the work, it must be remembered that I still have the cares of a family, that I am still dependent upon what little professional business I can do for my support, though few physicians have been longer in practice, and few have treated more cases of disease, even in the course of a long life. It is well known that the emoluments of the medical profession (unless possibly in the great cities) are by no means in proportion to the amount of business done (I have some times thought that they were in the inverse ratio of it) and as for the medical schools in New England (unless those of Boston are an exception) they diminish rather than increase the income of the instructors; at least, such has certainly been the fact as respects myself; though I have wasted my time sixteen years in one institution and fourteen in another. At the present time I have arrived at an advanced age; and though I have no specific bodily infirmities that I have not had from comparatively early life, yet I am less able to endure them, and am obliged to favor myself in various modes and degrees not formerly necessary. These statements will furnish the clue to all the delays in the progress of the work that have been and will be due to me. It is but proper to say here that there have been causes of delay however with which I have had no connexion.

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MATERIA MEDICA

OR

PHARMACOLOGY.

INTRODUCTION.

MATERIA MEDICA or PHARMACOLOGY, in contradistinction from every other branch of physical science, may be defined to be that part of medicine, which investigates the powers, and treats of the operations and effects produced by remedial agents and processes upon living and diseased animal bodies, together with the circumstances and conditions which indicate, regulate and control their use, as well as their doses, their proper periods of repetition, and every thing which relates to their most appropriate and best management. It must be observed that by the definition I have restricted the action of remedies not only to living, but also to diseased animals, since where there is no life the effect must be either purely mechanical, or purely chymical; and where there is no disease there can be no remedial effects.

Most writers and teachers incline to extend the limits, as well as to magnify the importance, of the particular department of knowledge of which they treat. It is seldom that the definition of the subject upon which an author writes does not encroach in the most palpable manner upon all the neighboring and allied subdivisions of human knowledge, so that, if his view of the matter were, admitted a considerable number of the most common, and the most proper divisions of science, must inevitably be rejected. Materia medica or pharmacology, is often made to include a con-

siderable portion of chemistry, mineralogy, botany, and zoölogy. However necessary these branches may be to a thorough understanding of the subject, they can not, in strict propriety, be considered as making any part of it; and though it may be necessary to make such use of them, as best suits the purpose of an author or a teacher, yet they are not by any means to be claimed, as belonging to this branch. But, if the definition is to be more limited in some particulars, it ought to be extended in others, beyond what is commonly conceded, viz. to those circumstances of the patient, and his disease, which require, regulate and control the administration of remedies.

Cullen defines *materia medica* or pharmacology to be "that branch of medical science which treats of medicines," and he defines medicines to be such articles as "are capable of variously changing the state of the body, and particularly of changing the state of disease into that of health." (*Cul. Mat. Med.*, N. Y., 1802, V. I., p. 134.) This definition of *materia medica* or pharmacology, in as much as it comprehends every thing that is known of an article, is obviously much too comprehensive, as well as much too vague. It does not exclude many things that are certainly no part of *materia medica* or pharmacology, such as the use of Nitrate of Potassa in the preparation of Gunpowder, the use of Crocus and Curcuma, in making colors, etc.; nor does it describe this department of medicine with sufficient precision to distinguish it from some branches of knowledge, which do not belong to medicine. It comprehends the chemistry, the natural history, etc. of the several articles which are not medicine in any respect. It likewise comprehends pharmacy, perhaps properly, though this is commonly reckoned a distinct branch of medicine, as is constantly and universally recognized by the set phrase "*materia medica* and pharmacy." *Materia medica* or pharmacology assuredly should not comprise the uses of its articles in diet, the arts, etc. These things however may be advantageously mentioned incidentally and briefly along with it, without impropriety, though it should be distinctly understood that they make no part of it. If we take Cullen's definition of a medicine into consideration along with his definition of *materia medica* or pharmacology, (as I think we may do, with propriety, as *materia medica* comprises nothing but medicines,) he would seem to imply by the terms of his defi-

dition, that every thing belonging to the *materia medica* should be capable of absolutely curing disease. According to the definition, medicines must be "capable of variously changing the state of the body, and particularly of changing the state of disease into that of health." Now nothing can be plainer than that an article which has the power of palliating disease, even when it does not cure, of protracting life in incurable cases, and of rendering the patient comfortable, or comparatively so, in hopeless maladies, is as much entitled to a place in the *materia medica* as those articles which possess positive curative powers. In fact an absolute curative power is merely a relative quality which may or may not be possessed by any agent according as it is employed in one or another disease, or is managed in one or another manner. Let it be observed that I am criticizing Cullen's definition of *materia medica* or pharmacology, not inquiring what his real views and opinions were, or whether they accorded with the terms of his definition or not.

"*Materia medica*," says John Murray of Edinburgh, "in the (most) 'exclusive signification which has been attached to the term, comprises the history both of aliments and of medicines.'" This as appears to me is an error. When the two are treated of together, the work is said to be upon *materia medica* and *materia alimentaria*, which terms in themselves expressly recognize a difference. Because two or more topics happen to be treated of in conjunction, they are not thereby rendered identical. Murray continues, "it is however more frequently, and more correctly used as opposed to the *materia alimentaria*, and in this limited sense," (it) "may be defined, that department of medicine which describes the properties and investigates the effects on the living system, of those substances which are employed as remedies against disease, —substances which are not necessary to the immediate support of the functions of life, to repair the waste of the body, or to furnish matter whence its secretions are derived; but which are more peculiarly adapted to excite the actions of the system, or produce changes with a view to the removal of morbid states." "It includes the history of these substances, independent of the preparation to which they are subjected to fit them for administration, this belonging to the department of pharmacy." (J. Murr. Mat. Med., N. Y., 1824, p. 49.)

I repeat that in my opinion the *materia medica* does not comprehend all the properties of the articles or agents of which it treats; nor are all the effects produced by such agents medicinal, though a knowledge of all their effects upon the living system may be necessary for their safe as well as most useful employment. This is generally admitted by the recognition of such a branch as toxicology; I do not pause here to discuss the propriety with which such a branch is recognized.

Medicinal powers are truly distinct from nutrient powers, but an agent may be both medicinal and nutrient at one and the same time. Most vegetable substances are capable of digestion; and when this is the fact, I doubt not that they furnish nutriment, (it may be a homeopathic quantity,) but still it is some times appreciable. I doubt not that in their ordinary doses, and in ordinary cases, Quinine, Cinchonine, Morphine, and even Strychine, etc., are digested, and furnish a very minute quantity of nutriment. The same is true (in my opinion) of genuine natural Wine (I mean the vinous principle independent of the water and other matters, with which it is merely mixed,) and of Alcohol. The kernel of the Cacao-nut is truly medicinal by virtue of the Theobromine which it contains, while at the same time, it is nutrient in a minute degree, by means of the digestion of this principle, and very far more so, by means of its other proximate principles. What is commonly called a demulcent operation is a truly medicinal operation, and yet all of the demulcents so called, are more or less nutrient by means of digestion in the stomach. It is to be observed that Murray here expressly distinguishes pharmacy from *materia medica* proper.

Again, Murray says, that "the subjects of inquiry in the study of the articles of the *materia medica* may be comprised under their natural history, their chymical history, and what may be more strictly denominated their medicinal history," (*J. Murr. Mat. Med. N. Y.*, 1824, p. 49.) To this Murray might have added, with equal propriety their pharmaceutic history, and their toxicological history so called. All this may be, and doubtless is necessary to the proper understanding and knowledge of the *materia medica*; but it is only what Murray seems to intend by their medicinal history, that constitutes the *materia medica* proper.

An other distinguished writer on this subject, says that the

materia medica treats of "those means which are employed, either in the prevention, or cure of disease." I doubt not that volumes might easily be made out, which should treat of "those means, which are employed in the prevention, or cure of disease," without containing a single sentence properly belonging to the materia medica. Similar strictures might be made upon nearly, if not quite all the definitions of materia medica, that I have seen. There may for aught I know, be many definitions, that I have never seen, that are more correct, I can speak only of such, as I have met with. Now if it is worth while to have any divisions and subdivisions in science or human knowledge, it is worth while to know what these are, and where are the metes and bounds between the several branches; and in many cases it is doubtless best to pay a strict regard to them; but this is not necessary in all cases. So far as the following essays, or the following treatise is concerned, though it may be mainly materia medica or pharmacology and therapeutics, yet it is my purpose to avail myself of every other branch of knowledge with which I am at all acquainted, elucidating and illustrating my principal subject, and I do not question but that there may be other branches not within the limits of my attainments that might be usefully employed in subservience to my general purpose. I can discover no good reason why I should be limited and restricted in any such way, though I am fully aware that there have been numerous cavils and objections to my past course in this respect.

MODUS OPERANDI MEDICAMINUM.

Health is at least indicated by, if it does not actually consist in, a natural easy regular and perfect discharge of all the functions of every part of the living animal system. *Disease* in its widest acceptation is any deviation from health, either in function or structure or both, in a part or the whole of a living system; or it is some vitiation of the actions or sensations or both in conjunction, of the living acting and sensitive solids. The distinctive and essential symptoms, in their whole aggregate, of any individual disease, in fact constitute that to which we apply the name. The particular unnatural uneasy irregular and imperfect discharge of the functions of one or more of the parts of a living animal system;—or in other words, the particular deviation from health either in function or structure, or both, in one or more of the parts

of a living animal system constitutes the pathological condition. Except from mechanical lesions, as appears to me, there is no such thing as a primary structural disease. It will at once be obvious, that without a mechanical lesion, a change of structure can not possibly be produced except by a change of function.

A medicine, or in other words a remedy for disease, is an agent or process, which by proper application or employment, changes, counteracts or overcomes disease, and either directly restores the functions to a healthy state, or produces such a condition merely as will readily become a healthy state on the discontinuance of the remedial agent or process. Healthy function then consists in right actions or motions and right sensations in the living solids. Disease consists in wrong, or vitiated actions or motions, or wrong or vitiated sensations in the living solids. Medicinal influence consists in the counteraction of wrong or vitiated actions or motions, and of wrong or vitiated sensations in the living solids. When no wrong or vitiated actions or motions and no wrong or vitiated sensations exist in the living solids, the greatest portion of medicinal agents, when given in medicinal doses or quantities, are capable of producing temporary deviations from right actions or motions, and right sensations in the living solids, but usually in too slight a degree, and of too transient a character to amount to disease.

The susceptibilities, and the actions and sensations of every part of an animal system, are regulated by peculiar laws in health; but these laws are varied and modified (to a greater or less extent) in a different and peculiar manner, in every specific disease which affects them. Under different circumstances, and at different seasons, and in different places, even the same individual specific disease, is well known to vary greatly as respects the quality, strength and activity or rapidity of its actions, and the quality of its sensations. This was perfectly understood, and explicitly recognized by the ancients. Thus Celsus says, "*differre quoque pro natura locorum genera medicinæ*" (kinds of practice?) *et aliud opus esse Romæ, aliud in Ægypto, aliud in Gallia.*" (Celsus Lib. 1. p. 7. l. 19., Edin. 1831.) This circumstance alone may be considered as proving incontrovertibly that the essence of disease consists intirely in the peculiar quality or condition of the actions and sensations.

An author justly remarks that, "if all diseases consisted simply of an increase or diminution of the healthy actions of the system, the science of medicine would be reduced to a few simple rules; and the only articles requisite for a *materia medica* would be the lancet and a bottle of Alcohol." "With these implements properly directed, the practitioner of medicine might go forth the triumphant vanquisher of disease;—but the fact is far otherwise" since "in addition to the increase or diminution of vascular action" either in strength or activity "every disease has superadded its own specific" and peculiar "morbid action, and herein consists the great art of the science of healing,—the ability to discover the" precise "indication of the disease, and the application of the most suitable means to answer that indication." (See Strobel on *Aral. spinos*, Charleston, S. C., 1826, p. 35.) A similar variation in the reducing and supporting or invigorating powers of many of the same individual remedies in relation to different cases of disease, in like manner evinces that their essential operation, is the production of an action peculiar in quality, and consequently that it is no more to be expected that the effects of any two distinct articles should be precisely the same, than that the quality of the actions and sensations of two distinct diseases should be perfectly identical. In this sense, and in no other, can remedies ever be considered as specifics. It is to be observed that though the elements of a living animal system are united by a species or variety of attraction falling under the definition of affinity, yet that the laws of this attraction, or affinity, as controlled by the vital power vary very essentially from chymical attraction in inanimate substances. These variations are to a certain limited extent, peculiar to every distinct species of animals, but they are more especially prominent in many different parts of the same animal. These various parts of the body, being variously organized, are endowed with various, and peculiar susceptibilities, as for example, the eye which is affected only by light, and the ear which is affected only by the vibrations or undulations of the air, etc.

Moderate remedies are in general inert upon a healthy subject, though active ones are usually capable of producing certain operative effects. There is however a very great difference between the effects even of these in health and in disease; as in two such distinct conditions, the system has been found to be governed by

very different laws. In the words of an author, (Caldwell on Malaria, American Journal of Medical Sciences, Aug. 1831, No. xvi, p. 339.) "Medicinal substances" and processes "are intended and suited, as their name imports, to restore health when lost, not to sustain it when possessed." "Let them be reserved therefore," (says our author) "for actual indisposition, and then administered without loss of time, and with the skill and vigor required." The action of medicinal agents upon dead animal matter, has even much less analogy to remedial effects than their action upon the healthy living subject.

Under the head *modus operandi medicaminum*, I do not intend to attempt any impracticable things, such as how antiphlogistics prove directly exhausting, how narcotics prove directly antirritant anodyne and soporific, how tonics directly increase vital energy and strength of action; how emetics vomit or how cathartics purge, etc. To none of these inquiries is an answer possible, or will it ever be possible; and I never had the least relish for the absurd and groundless hypotheses that so many authors have amused themselves with, so much more than they have profited their readers. All the topics that I shall attempt to discuss, under the head of *modus operandi medicaminum*, will be capable of determination by ascertainable facts, though the facts on which a decision depends may not yet be well ascertained. There are very many topics, that I shall not attempt to discuss on account of a deficiency of facts, though all the desired facts are perfectly capable of being well ascertained. For example, it would be highly desirable to ascertain in what manner all the powers of the whole *materia medica* destroy life, that are capable of doing this at all. This would greatly assist us in judging of the adaptation of particular medicines to particular cases; it would show us when to avoid particular medicines intirely: it would enable us to decide when it is proper to push a medicine to its fullest operative effects; and it would indicate the very first approach of danger from all medicines. All this might have been ascertained long ago, if there had been any thing like accurate and complete observations, (by persons thoroughly acquainted with the nervous system,) of the cases of what are called poisoning, that have occurred under the observation of physicians, for the last century, and perhaps for the last fifty years. Even now there are cases enough on record to

furnish the desired information, if they had been properly observed and described; but as they have been given to us, all the most important points are intirely untouched and omitted, at least as a general rule.

What sort of spasms or convulsions does each specific narcotic produce? I believe it may be considered certain that every narcotic, when pushed far enough, always produces some sort of spasms or convulsions, as, first clonic like Epilepsy, second subtonic like those of common convulsion, and of Hysteric convulsion; and third exquisitely tonic, like Tetanus, Rabies, etc. This knowledge would contribute much to a more correct therapeutic application of the narcotics, since the narcotics are among the most effectual, and the best remedies for spasm or convulsion, and since I have ascertained, as I think, that those which produce clonic spasms are by no means so well adapted to the treatment of diseases which consist essentially in clonic spasms; that those which produce subtonic spasms, are not so well adapted to the treatment of diseases which consist essentially in subtonic spasms; and that those which produce exquisitely tonic spasms are not so well adapted to the treatment of diseases which consist essentially in exquisitely tonic spasms. In fact I doubt whether a narcotic producing a given sort of spasm or convulsion, should ever be given in diseases, essentially consisting in the same sort of spasm or convulsion. I have long been well satisfied that these different sorts of spasm or convulsion are specific in comparison with each other, that they never change into each other, and that they are peculiar in their beginning, in their middle and in their end, or in other words, in their forming stage, their course and progress and their event. I can not doubt that their essential and characterizing causes,—their *causæ sine qua non*—are equally specific, and therefore different and distinct. At all events, such a narcotic in such a case, (it must at once be obvious,) can not be pushed till it either relieves the spasm or convulsion, or till it produces some operative effect, which forbids its further use, at least without danger of its coinciding with the disease and aggravating and keeping-up the very symptoms which it is given to relieve. Indeed I cannot well see how the effect of the medicine can be distinguished from the disease under such circumstances. I can not doubt that all the previous effects of the narcotics are as different, distinct and spe-

cific as the several sorts of spasm or convulsion, so that their proximate effects must have just about the same adaptation to given cases, as their ultimate effects. In addition to this, I doubt not that the different sorts of spasm or convulsion are incompatible with each other, so that if the narcotic produces a different sort of spasm from that of the disease, it may often be useful to push it to the point at which it might produce its own peculiar convulsion or spasm, if it were not given where a different sort of convulsion or spasm already exists. Where the narcotic produces the same sort of convulsion or spasm as the disease, even provided its proximate effects might possibly be useful, its ultimate ones could not be, but must coincide with the existing disease.

Which narcotics produce spasms or convulsions as a primary part of their operation when given in full doses ; and which produce them as a secondary part merely ? Which narcotics produce spasms or convulsions first through nerves of involuntary and instinctive motion ; and which produce them first through nerves of voluntary motion ? These last two questions have very important practical bearings, which considered here, would be greatly out of place. *Which subordinate part of the nervous system has its function first suspended by fatal doses of each individual narcotic ?* Although out of place I have here to make a few explanations upon this subject, since without these, I could hardly illustrate what I desire to show. I think it may be considered as absolutely certain that when narcotics are taken to such an amount as to destroy life, this effect is always produced either by an interruption or suspension of the functions of the nerves of involuntary and instinctive expressory motion ; or of the nerve of involuntary and instinctive chemical action nutrition and reproduction. When a narcotic destroys life by suspending the functions of the nerves of expression, the functions of the nerve of chemical action and nutrition go-on, for a considerable time, longer or shorter in different cases, with only moderate disturbance, and will continue to go-on, provided factitious respiration is kept-up till the effects of the narcotic have time to pass-off wholly and intirely, so that, in reality death need never be occasioned in this way. When a narcotic destroys life by suspending the functions of the nerve of chemical action nutrition etc., the functions of the nerves of expression go-on, for a considerable time, longer or shorter in

different cases, with only moderate disturbance, and would doubtless continue to go on if there were any mechanical or medicinal way of keeping up the actions of the sanguiferous system, and of the secernents and absorbents or the glandular system, etc. till the effects of the narcotic have time to pass off wholly and intirely. Beside many other important practical bearings, it will be seen at once, that these facts show which of the two vital parts of the nervous system, every narcotic acts much more especially upon, and therefore affords an indication for the selection of a narcotic for a given case of disease, provided that is investigated in the same manner as the narcotic, that is, as to the subordinate part of the nervous system in which it has its sole or principal seat.

There is still an other class of remedies, of which Ignatia amara, Strychnos Nux-vomica, etc. may be taken as types, that, beside destroying life in the two ways in which the narcotics do it, exhibit an other mode of producing this effect, viz. by a peculiar action and influence on the nerves of voluntary motion, thereby fixing them in paroxysms of intense tonic spasms, which gradually become more frequent and more protracted, till at last they continue so long that the muscles of respiration can never again be brought into action. That this is accomplished through the voluntary system of nerves is evident from the fact that the whole of the voluntary muscles is involved, and no part receiving nerves of expression merely, or nerves of nutrition merely, is at all affected with spasm. These articles then must act more in proportion upon the nerves of voluntary motion than upon any other. All this is undoubtedly of very considerable practical importance; and yet, as I have already said, physicians seem rarely to observe cases with sufficient minuteness and accuracy to furnish us with the desired facts, or at all events they omit to give them to the public.

Thus it is evident that there are topics enough which fall under the *modus operandi medicaminum*, that need investigation research and inquiry, and which are capable of furnishing sufficiently certain results; but it is not exactly such topics as these that I intend to discuss in what immediately follows, though the topics that I have selected, I trust, are capable of sufficiently certain results.

POSITIVE AND RELATIVE — PROXIMATE AND REMOTE — PALLIATIVE
AND CURATIVE EFFECTS.

MEDICINES in general produce either

1. A positive or absolute effect.

2*

A medicinal effect is positive or absolute, when it is capable of taking place as well in the healthy subject, as in the sick, or diseased one; and in all the various morbid or pathological conditions, that are ever liable to occur. Thus, emesis and catharsis may be mentioned as examples of the positive or absolute effects of medicines.

2. A relative or comparative effect.

A medicinal effect is relative or comparative, when it is incapable of taking place, except in circumstances, which do not exist in all cases, both in health and disease; or which do not exist in all cases of disease. For example, the antiphlogistics, though so highly efficacious for the relief of the morbid irritability and irritation, the morbid sensibility and sensation, the morbid mobility restlessness and jactitation, and the morbid thirst heat and dryness of skin, pain, etc. of the phlogistic or entonic diathesis, are intirely incapable of obviating the symptoms, which are called by the same names, in the atonic or asthenic diathesis. On the contrary, the antiphlogistics aggravate the morbid irritability and irritation, the morbid sensibility and sensation, the morbid mobility restlessness and jactitation, and the morbid thirst, heat, dryness of skin, and pain, of the atonic or asthenic diathesis.

Again, Papaver, though so very effectual as an antirritant, anodyne, and soporific, in atonic or asthenic diathesis, and even in conditions of the system, which are neither atonic nor entonic, will not only intirely fail of producing any of these effects, in phlogistic or entonic diathesis, but on the other hand, will increase irritation, pain and wakefulness.

In certain forms of Tetanus, such an amount of Papaver, as would inevitably destroy life in health, and in all ordinary diseases, may occasionally be taken, without producing any perceptible or appreciable effect of any sort. Some times, however, an enormous quantity of Papaver will wholly control the tonic spasms of this disease without producing any other operative effect, by which, the acutest phyiscian, not having a knowledge of the fact, could find ground for even a suspicion that the patient had taken a particle of this agent. I have witnessed cases of other and very widely different diseases, in which the operation of Papaver was precisely the same. When only just sufficient Papaver for the obviation of any very highly intense or exquisitely urgent symp-

tom, is administered in a case, to which it is truly appropriate, it is not infrequently the fact that none of the customary operative effects of Papaver will appear in any degree, except barely the perfect obviation of the symptom, for which it is employed, and this in subjects not at all accustomed to this agent, and that have perhaps never taken any of it before.

In epidemic malignant Cholera, provided the case was sufficiently severe and intense, agents and measures could occasionally be employed without the least apparent effect, either for good or for evil, that would have speedily destroyed the life of any person in health, or laboring under any ordinary disease. In such cases, we may say the system is too powerfully occupied by the disease, to feel the influence of any remedial agent, or process.

Antisbestics and tonics can not be expected to obviate atony anorexia, and debility, when neither of these conditions exists. On the other hand, they will certainly augment phlogistic or entonic diathesis. At all events, it must be obvious that antisbestics and tonics can not produce their proper operative effects, in all conditions of the system,—can not produce them, except in atonic or exhausted states,—and must necessarily aggravate a phlogistic or entonic condition.

The operations and effects of styptics or astringents, of adenagics, of diuretics or uragogues, and of diaphoretics or hidrotagogues, are almost equally relative or comparative, as the operation and effects of antiphlogistics, narcotics, antisbestics, and tonics.

In cases of diseased stomach, medicines taken into the stomach affect the system very far less, (at least as a general rule,) than when the stomach is in a healthy state. As far as I have observed, Skirrhus, or organic affections of the stomach prevent the operations of medicines upon the system at large, much more than functional diseases. The degree in which a structural or organic disease prevents the operation of medicines upon the system at large, depends not only upon the specific nature and character of the disease, but also upon its degree or intensity. In some cases of Skirrhus Pylori, the subject of the disease is usually able to take exceedingly large quantities of certain medicines, with very little operation upon the system at large. For example, in Skirrhus Pylori, even highly susceptible subjects are capable of taking very large quantities of Papaver, and this in even a liquid state, with scarcely any appreciable effect upon the system at large.

These very cases however, will at the same time be exceedingly intolerant of various other medicines, that are intirely destitute of any positive irritant power.

It is quite certain then that many of the operations of medicines are altogether relative or comparative; or in other words, the effects, which they produce, depend nearly as much upon the condition of the system when they are administered, as upon the peculiar powers of the article employed.

3. A primary, proximate, immediate or speedy effect.

A primary proximate immediate or speedy effect is that which takes place as soon as the medicine, or medicinal process begins to operate at all. For example, the primary proximate immediate or speedy effect of depletion of blood is almost invariably a sudden but at the same time a transient and fugacious mitigation of most, if not absolutely the whole of the morbid symptoms existing for the time being, in nearly every disease, whatever may be its nature and character. Primary proximate immediate or speedy effects, which seem to be temporarily beneficial, do not by any means furnish evidence that the secondary ultimate remote or distant effects will be so likewise. The primary proximate immediate or speedy effect of a Mercurial taken in uniform and non-cathartic doses, at regular and comparatively short intervals, may be only an extremely disagreeable Mercurial taste, or a very troublesome ptyalism or salivation; and yet, its secondary ultimate remote or distant effect may be a cure of a violent constitutional Syphilis. The primary, proximate immediate or speedy effect of such a quantity of Cinchona, as may be necessary for the cure of an Intermittent may be diminution of appetite, or even positive offense of the stomach, indicated by more or less nausea, or even rejection of the article from the stomach, owing to its bulk and disagreeable taste; and yet, its secondary ultimate remote or distant effect may be a perfect cure of the disease. The primary proximate immediate or speedy effect of Cinchona may be a very unpleasant sensation in the ears like the distant hum of numerous Bees, or in short, one of the varieties of tinnitus aurium; and yet, its secondary ultimate remote or distant effect may be a perfect cure of the disease. The primary proximate immediate or speedy effect of an epispastic of that grade called rubefacient, and also of that grade called vesicant, is seldom primarily mitigant, however much so, it may be secondarily.

4. A secondary ultimate remote or distant effect.

A secondary ultimate remote or distant effect is that, which is produced upon the course or progress, the duration or continuance, and the final event or result of a disease, by agents or processes employed at the outset, or at all events, in the early stages of a disease. The secondary, ultimate, remote or distant effects of depletion of blood may be to aggravate and to protract a disease, and to render the result or event fatal, when the disease without this process, would have been comparatively mild and transient, and the patient would certainly have recovered from it. The secondary ultimate remote or distant effects of depletion of blood may be all this, when its primary proximate immediate or speedy effects may be palliative of most, or all of the unpleasant symptoms for the time being.

There is then certainly both a primary proximate immediate or speedy effect; and also a secondary ultimate remote or distant effect of most, if not all active medicines, viz: their immediate and transient mitigation of existing symptoms; and their remote operation in producing a permanent relief, occasioning a disease to come to its final crisis a week, or three days sooner (according as its natural duration is a fortnight or a sevensnight,) than it otherwise would do; and also causing the crisis to be favorable, rather than unfavorable or fatal. The primary operation of a medicine or medicinal process (as I have already said) may certainly be a present mitigation of symptoms, when the secondary operation is an aggravation of the disease, a protraction of its duration, and the production of a fatal crisis; or, the operation may be directly the reverse.

The primary mitigation of a disease, at the expense of a secondary augmentation of it, is far more likely to be the effect of exhausting antiphlogistic and actively evacuant agents and processes, or in short, of every thing which directly diminishes the vital energies of all the subordinate parts of the system, which are dependent for their active or motive power upon what is commonly called the great sympathetic nerve, but which is in fact the nerve of chymical action nutrition and reproduction. In fact, I can not say that I am acquainted with any other primarily mitigating remedies, that are secondarily aggravating agents.

Invigorating agents, whether antisbestics or tonics, in phlogis-

tic or entonic diseases, are as much primarily as secondarily aggravating; and the pure narcotics neither primarily mitigate, nor secondarily aggravate such diseases.

It is rare that I form a new acquaintance among physicians, and become acquainted with the medical practice of such new acquaintance, without finding the very common, and almost everlasting fallacy of judging of, and deciding upon the benefit rendered by depletion of blood and catharsis in atonic, Typhoid, and Typhous Febrile diseases, by the immediate, and indeed instant relief of all the uneasy sensations under which the patient previously labored, which invariably takes place as soon as either of these processes is completed, provided they have been pushed to an efficient degree in relation to the violence and severity of the disease, and the extent to which it occupies the system, though the relief so produced, is exceedingly transient and fugitive, some times not continuing more than half an hour, an hour, or at most, two hours.

The remote effects of these processes, their effect in producing a favorable or unfavorable crisis, when the time of the crisis arrives; their effect in anticipating or postponing the crisis; their effect in fixing the disease upon the patient, beyond the resolvent power of any agent or process, or any succession of such agents or processes; and their power of increasing, sooner or later, all the distressing symptoms of the malady, are intirely overlooked, and utterly unknown. Beside all these, the processes in question very greatly diminish the suscepibility of the patient to the best remedial effects, of the truly appropriate agents and processes.

In my opinion, there is no case in medicine, where it is more important to consider the remote rather than the immediate effects of remedial agents and processes, than in the employment of depletion of blood, and much more especially catharsis in atonic Typhoid and Typhous Febrile diseases;—I say much more especially the latter, because I consider that the former is never truly indicated, in such cases, but, as it is ordinarily employed, is much less injurious; while the latter is not infrequently more or less indicated, but is at the same time, much more liable to do mischief.

5. A palliative effect merely.

A palliative effect merely is one, which only diminishes the

present urgency of the symptoms, and thereby renders the subject of the disease more comfortable for the time being, and perhaps contributes to prolong life more or less, without any prospect of producing permanent benefit, or of affecting an ultimate cure. Some medicines are never capable of accomplishing any thing more than mere palliation; while others become mere palliatives from the manner in which they are managed, being capable under other and better management, of becoming curative remedies. Many whole classes of medicinal agents are incapable of accomplishing any thing beyond mere palliation, being intirely inadequate to the cure of any single disease. That class of agents commonly called by the barbarous Greci-Latin term *antacida*, are never more than mere palliatives of that disease of the stomach, one of whose symptoms is the production of an inordinate quantity of some acid in that viscus, they being wholly incapable of accomplishing any thing more. Indeed they not infrequently aggravate the disease for which they are given. That class of agents, which I am in the habit of calling *leantics*, but which make two classes, with most authors, viz. *demulcents* and *emollients*, according as they are taken internally, or applied externally, are mere palliatives, being utterly incapable of curing any disease whatever. That class of agents called in the *materia medica* *diaphoretics* or *hidrotagogues*, I believe, are always mere palliative medicines. That class of agents called *diuretics* or *uragogues*, is often palliative in several diseases; but it does not seem to me to be curative in any disease whatever. That class of agents commonly called *cathartics*, does not appear to me to be curative of a single individual disease; but it is palliative in very many diseases. The several species of the genus *Smilax*, as at present prepared and employed in medicine, under the general name *Sarsaparilla*, are, at the most, mere palliatives (and hardly that,) of a few diseases, being wholly incapable, so far as I could ever find satisfactory evidence, of curing a single malady, by any management. When I say this, I would not be understood as expressing any doubt that *Smilacic Acid*, the active principle of the *Smilaces* when they are active, if obtained in a separate state and properly administered and managed, might prove a valuable curative remedy, in a considerable number of important cases.

6. A curative effect.

An effect is curative, when it wholly counteracts and overcomes, and completely alleviates a disease, and that permanently, unless the subject is again exposed to its efficient causes, so that it is produced anew. Cinchona properly managed is an absolute curative remedy for pure and uncomplicated Intermittent, and also for many complicated cases; but it may easily be administered under such management, and with such accompaniments, that it will do no more than palliate, and perhaps not even that. Papaver properly managed, is a curative remedy of pure and uncomplicated Diarrhœa, and also of many complicated cases; but, it may be so administered, and under such management, and with such accompaniments, that it will do no more than palliate, and perhaps not even that. Mercurials with proper management, are capable of curing constitutional Syphilis; but with injudicious management, and with bad accompaniments, they may be only palliative, and perhaps not even that; and as sometimes happens, they may be even aggravating.

PARTS OF THE SYSTEM ACTED ON BY MEDICINES.

In my opinion, the very first effects of remedies are always produced upon the solid parts, to which they are applied, the fluids being affected only through the medium of the solids. The parts to which remedies can be applied, are

1. The internal parietes of the stomach and the upper part of the intestinal canal, which, possessing the highest degree of suceptibility, and having the most extensive nervous connexion with other parts, are usually the most eligible. Does the alimentary canal from the stomach downwards, receive any nerve of common sensation, except those, which accompany the nerves of chymical action nutrition and reproduction? I have been in the habit of considering that they do not, but perhaps incorrectly.

I doubt not that the inner parietes, or in other words, the lining mucous membrane of the stomach, and upper or smaller intestines, is altogether and intirely the most eligible part for the reception of the very first impressions of most medicines, and for the propagation of their impressions to the other subordinate parts of the system. As would seem, no other part is endowed with a suceptibility so exquisite, and so great, and none is so well adapted in quality for this purpose. As would seem, no other part has

as numerous, as various, and as high a degree of its several sympathies, and sympathies too, with every other subordinate part of the system. As would seem, this part must be more extensively endowed with nerves than any other part of the system. As the stomach and upper and smaller intestines form a tube with a considerable cavity, they are better fitted to receive and retain medicines, till they shall have exerted all the operations, and produced all the effects, which they are capable of producing, or intended to produce. No better part for the reception of the primary impressions of most medicines can possibly be needed, or ought to be desired, except when the stomach itself is diseased in such a mode or manner, as to render it intolerant of the impressions of remedies, or to destroy its capability of transmitting such impressions to other parts of the system. I have heretofore made a few remarks as to the manner in which disease of the stomach modifies the effects of medicines taken into it. It is not necessary to repeat them in this place.

2. The inner parietes of the rectum and colon, by means of articles administered by way of enema or suppository.

My attention has been much turned to the subject of enemata and suppositories, as a mode of administering both medicine and food in disease, and the result of my observations, and of the testimony which I have obtained, is a conviction of their utter worthlessness, for both purposes, with the exceptions only of their employment for obtaining the effects of Opium, and a few other narcotics, and of certain cathartics. I do not consider enemata in cases of local disease of the rectum, as in some cases vaguely called Proctica for example, as constituting an exception to the foregoing remarks, since, in such cases, if properly managed, and not too frequently repeated, they operate as mere topical applications. As to enemata nutrientia, I am well satisfied that they are not only altogether worthless, but even positively injurious, and that the idea of nourishing the system, in this manner, is a perfect chimera. As far as I am informed, there are no lacteals in the rectum and colon; and if there were, there is no reason to conclude that the lacteals are ever capable of absorbing any thing that has not been digested, or that has not at least passed through the organs of primary digestion. The lower intestines certainly do not possess any power of digestion, but are merely the outer

extremity of an excretory duct, the reservoir of the refuse of the food, and, as such, have scarcely any sympathies with other parts of the system. They are therefore utterly incapable of becoming an avenue of support to the system, as much so, as the urinary bladder. But, like the outer extremity of other excretory ducts, the lower intestines are capable of being greatly irritated by being used for a purpose, for which they were never intended; and when thus irritated, much disturbance of the system at large will often be the consequence.

3. The skin or external surface of the body, which though much less susceptible, (at least of motion or action properly so called,) and having much less extensive connexion with other parts, is never the less so easily accessible, as to be the next in eligibility, to the stomach and upper intestines.

As a general rule, an ulcerated surface, a newly blistered surface without any ulceration, and diseased surface generally, are much less susceptible of medicinal impression and influence, and of propagating medicinal impression and influence to other parts, than sound or healthy surface. The only exception to this law, now within my knowledge or recollection is that of actually irritating impressions and influences, such for example, as the impression and influence of *Cantharis* and other analagous articles, through every possible grade and degree of actual irritant power. The susceptibility of the surface to the impression and influence of different medicinal powers, varies very greatly with the nature and character of such power. The surface is often quite insusceptible to impressions and influences, to which the stomach is highly susceptible; and vice versa. This would seem to indicate that, in general, the susceptibilities of the surface, and of the stomach, are specifically different. The urinary bladder seems to have a peculiar sympathy with the skin under the topical irritation of *Cantharis vesicatoria*, and doubtless other species of *Cantharis*. In some cases of urgent painful vesical irritation, from an epispastic of *Cantharis*, such irritation is relieved the most speedily, and the most perfectly, by applications to the blistered surface; and the vesical irritation can not be relieved till the irritation of the blistered surface is relieved, in whatever way this may be accomplished. Such cases of vesical irritation I have been in the habit of considering as merely sympathetic, and in all probability,

merely nervous. I doubt whether the bladder would exhibit any marks of disease to the eye, could it be seen. But of this, we have no positive knowledge. Those cases of urgent painful vesical irritation, from an epispastic of Cantharis, in which there is some blood voided with the urine, would undoubtedly exhibit visible disease, could the bladder be seen. In all cases, upon which I have had opportunity of making observations, painful vesical irritation, from an epispastic of Cantharis, is always in pretty exact proportion to the painful irritation in the part blistered; and it has always disappeared in the same proportion. From this remark, cases in which Cantharis is likewise taken into the stomach, ought obviously to be admitted as an exception.

4. The mucous membrane of the nostrils, and of the lungs, which, though more susceptible than the skin, (at least of motion or action properly so called,) yet from its situation, and more especially the nature of its functions, admits only of the application of vapors and gasses, and in a very few instances powders, but only to a very limited extent.

5. The inner parietes of the blood-vessels, by the injection of liquid medicines into the veins;—a method of administration however, which may be truly said to be neither convenient nor safe, and therefore never likely to come into use,—nor even to be employed at all, at least where any regard whatever is paid to the maxim “saltem non nocere.”

The lining membrane of the blood-vessels is indeed a mucous membrane, (as I am satisfied) as well as that of the alimentary canal; and the blood-vessels, like the alimentary canal, receive the great sympathetic nerve—the nerve of chymical action nutrition and reproduction—accompanied with a few filaments from every pair of nerves of common sensation (except the third pair (Willis) in the system; and yet, judging from the ordinary susceptibility of the blood-vessels, I should think that they were much less copiously furnished with nerves, than the alimentary canal, and on this account therefore, not so well fitted to receive and propagate the first impression of remedial agents. At all events, this would seem to be true of the lining membrane of the blood-vessels, in comparison with the lining membrane of the alimentary canal, whatever may be the fact with the other textures that contribute to make-up both those subordinate parts of the system.

6. The inner parietes of the urinary bladder, by the injection of liquid medicines through the urethra.

This has been gravely recommended, (not by a Frenchman, but actually by an Englishman,) as an eligible part for the application of medicines intended to operate upon the system generally, and not as mere topicals, in diseases of this viscus! This last part, if we may by any means form a correct judgment from certain well attested statements, cannot be very susceptible to the impression of remedies, since we are assured that complete intoxication so called, from *Amanita Muscaria* will intirely pass-off, while the urinary bladder contains sufficient of the intoxicating agent, to affect five other persons powerfully in the same manner, if it should be taken into the stomach.

I make no mention of the blood as a part of the system upon which medicines can ever produce their primary effects, because I consider it capable of the highest proof, that disease never has its primary and essential seat in the blood, but always in the living acting moving and sensitive solids; and that the blood is altogether incapable of any positive action or sensation, it being only susceptible of passive motion, and therefore can only be acted upon by the living, moving and sensitive solids.

It appears to me to be abundantly susceptible of proof that disease, (as I have already said,) always consists in some vitiation of the actions or sensations or both of the living acting and sensitive solids; and likewise that these actions and sensations, are essentially dependent upon motor and sensor nerves; and that where there are no nerves of motion there can never be any positive action, but only passive motion; and that where there are no nerves of sensation there can never be any sensation at all. If disease consists in some vitiation of the actions and sensations of the living, acting, and sensitive solids, it follows of course, that remedies must act upon these same parts, and must have the power of changing actions and sensations in such solids, and therefore can act only on parts endowed with motor and sensor nerves. But if it is the fact, I certainly have it yet to learn, that the blood possesses either motor or sensor nerves, or that it is capable either of positive action or sensation.

I have made no mention of the preposterous and absurd hypothesis that medicines are first received into the blood unchanged,

and then brought into actual contact with, i. e. extravasated and absolutely deposited in the substance of those subordinate parts of the system, in which the manifestations of their operations are perceived. That medicines operate either by actual contact with the organ or organs, in which are perceived the first manifestations of their effects, and this by being taken into the mass of the circulating fluids, and, (what must follow, of course, though I believe it is not commonly mentioned as a part of the creed,) by being extravasated, or some how deposited, either in, or upon the organ or organs affected ; or else that they operate upon the blood, I believe, is the prevailing faith among the physicians of the whole civilized world.

I judge that one of these two opinions, is the prevailing belief, not because authors and practitioners maintain either, in all its details, but because they constantly write and talk of the operation of medicines by being taken into the circulation, and as constantly combat every other view of the subject. I judge also that this is the prevailing opinion, because Dr. Christison (the Professor of *Materia medica*, in the University of Edinburg,) mentions what he calls "the startling doctrine lately advanced by Messrs. Morgan and Addison, in regard to the actions of poisons generally," viz. "that they act by entering the blood, and producing on the inner membrane of the blood-vessels, a peculiar nervous impression, which is instantly conveyed by sympathy, along the nerves, to the organ remotely and ultimately affected." Dr. Christison does not mention this doctrine, in opposition to the doctrine of their action upon the inner membrane of the alimentary canal, etc. but, in opposition to the doctrine of some other mode of operation, by means of being taken into the blood. Now when a medicine is taken into the blood before its medicinal operation, I know of no mode in which it can then operate, except by impression upon the lining membrane of the blood-vessels, or by extravasation in, or upon the organ or organs, in which we first perceive the manifestations of its effects. I take it for granted that no one at the present period, can possibly suppose that medicines operate upon the blood merely.

As respects the notion of the operation of remedies by being taken into the mass of the circulating fluids, and being actually extravasated, so as to come into contact with the proper sub-

stance of the part, in which the manifestations of its operation are perceived, nothing, as appears to me, can be more improbable, and more absurd, nothing can be more destitute of all proof.

It appears to be the fact that the cerebrum for example, is neither sensitive to, nor motive from the direct and immediate influence of remedial agents applied to its substance; and the same, in all probability, is equally true of every other organ in the system. The actual contact of a narcotic, for instance, with the proper substance of the brain, when a portion of the skull has been removed, will not produce any grade or degree of narcosis. I consider it as certain that medicinal agents will not produce their operative or remedial effects by application to the denuded cerebrum, cerebellum, spinal-cord or nerves merely. Medicinal effects can be produced only by an operation upon the organs, to which nerves are sent, and not by operation upon denuded nerves. As well might we expect to see, by the impression of light upon the denuded optic nerve without the intervention of that complicated organ the eye; or to hear by the impression of the undulations of the air upon the denuded acoustic nerve, without the intervention of that complicated organ the ear.

It would seem that nerves, whether motor or sensor, are incapable of receiving medicinal impressions except at one of their extremities. Motor nerves receive their impressions only at what is sometimes termed their origin; while sensor nerves receive impressions only at what is sometimes termed their termination. Medicinal impressions are certainly never received at any point of the trunk of the nerve, between their origin, and their termination.

It is equally certain that medicinal impressions are never received by any part of the surface either of the hemispheres of the cerebrum, the hemispheres of the cerebellum, the crura of either of these, the medulla oblongata, or the spinal-cord, whether the voluntary motor the involuntary and instinctive expressory or the common sensory tracts.

If medicines then could be conveyed to, and extravasated upon every point of the system, there is no degree of probability that they would be capable of exerting their medicinal powers, and of producing their medicinal effects, except where there are the extremities of nerves. It is often said that there is no point in

the whole system, where there are not extremities of nerves ; but this does not appear to me to be exactly the fact. What extremities of nerves are there in the substance of the cerebrum, in the substance of the cerebellum, of the medulla oblongata, of the spinal-cord, of the trunks or bodies of the nerves, of the crassamentum of the blood, &c. ?

SYMPATHY.

It is my present belief, founded on a conviction resulting from considerable research and investigation, and from long attention to the subject, that by the contact and impression of the agent employed, a peculiar condition is produced, or a peculiar sensation or action is occasioned, in the part to which the agent is first applied, which is propagated or extended, by the following methods exclusively ; viz.

1. By mere continuity of similarly organized, and similarly susceptible parts, — parts which exercise similar functions. This is commonly called *CONTINUOUS SYMPATHY*.

2. By means of natural adhesion to parts dissimilarly organized, and performing dissimilar functions, as from one texture to an other of all those organs, which consist of a diversity of textures. This is commonly called *CONTIGUOUS SYMPATHY*.

3. By nervous connexion, to contiguously situated, but dissimilarly organized parts, as happens between the stomach, lungs and heart, in consequence of their being severally supplied by filaments of the same nerves. This is commonly called *NERVOUS SYMPATHY*.

4. By nervous connexion, to distant, but similarly organized and similarly susceptible parts, — parts which perform similar functions, — while intervening parts of dissimilar organization, dissimilar susceptibility, and dissimilar functions, are not at all affected. This may be called *TEXTURAL SYMPATHY*.

5. By nervous connexion to distant and dissimilar parts, — parts which perform different functions, — and which possess exclusive susceptibility to certain peculiar operations of individual remedies. This may be called *SENSORIAL SYMPATHY*.

By the first two modes of sympathy so called, the progress of disease, and the operation of remedies are comparatively gradual ; but, by the other three, they are often nearly instantaneous.

By the foregoing statement, it is not to be understood that the

real and true first operation of remedies is ever in fact exerted upon any other subordinate part of the system than the nerves, through the medium of the part or organ to which they are sent, where ever the primary manifestations may happen to be ; for I consider it as demonstrable, that no medicinal effect is ever capable of being produced, except through the nerves. It will at once be plain and obvious that all active motion, (in contradistinction from passive motion,) and all common sensation in the living fiber or solid, must necessarily depend upon influence conveyed through motor and sensor nerves ; for, where all motor, and all sensor nerves, leading to any portion of the living fiber or solid, are cut-off, such living fiber or solid, is always absolutely and completely acinetic and anesthetic. As a necessary consequence of this law, it is evident that in order to produce motion or action and sensation in the living fiber, the influence intended to produce them must be primarily exerted upon the motor and sensor nerves leading to such living fiber or solid. All medicinal agents therefore, which are intended to produce motion or action and sensation, in the living fiber or solid, must exert their remedial influence primarily and directly upon the motor and sensor nerves of the living fiber or solid, rather than upon the fiber or solid independent of its nerves. It appears to me then, to be not only clear, but incontrovertible, that all medicinal agents must always act primarily and directly upon some part of the nervous system.

SEATS OF PRIMARY MANIFESTATIONS OF OPERATION OF REMEDIES.

The primary manifestations of the operation of all medicines are in either

1. The nervous system generally, or some subordinate part of it ;
2. The primary digestive system.

The manifestations in the primary digestive system, relate mainly to appetite, chymification, chylickation, and peristaltic action. Many, in fact most of the functions, and pathological conditions, that are commonly referred to the primary digestive system, (as by John Mason Good, for example,) most assuredly belong elsewhere.

3. The secernents and absorbents, or the glandular system ;
4. The sanguiferous system ;
5. The reproductive system.

The manifestations in the reproductive system, it is obvious, can relate only to puberty, deficiency or excess of venereal appetite, deficient power of erection of the penis, Ithyphallus persistens, Agenesis, sexual intercourse, Aphoria, conception abnormal and nothous, pregnancy; and parturition. Many, in fact most of the functions and pathological conditions, that are commonly referred to the reproductive system, (as by John Mason Good, for example,) most certainly belong elsewhere. As respects the manifestations of the operations of medicines in the other subordinate parts of the animal economy beside the primary digestive system, and the reproductive system, it is not necessary here to specify to what functions they relate.

There are no primary manifestations of the operation of medicines in any subordinate system, that can with any shadow of propriety be called a respiratory system. The decarbonization of the blood is a process that belongs to the secernents and absorbents or the glandular system, it being a mere and pure piece of excretion. As to the motions of the walls of the thorax, the diaphragm, and the lungs proper, they all belong to the function of expression, (being only accidentally, not necessarily, subservient to the decarbonization of the blood,) and all the manifestations in the organs of expression, fall under the nervous system generally, or some subordinate part of it.

The primary digestive system, the secernents and absorbents or glandular system, the sanguiferous system, and the reproductive system, each and all depend upon the involuntary and instinctive motor nerve of chymical action, nutrition, and reproduction, commonly called the great sympathetic nerve, for all their motions or actions; and upon the few filaments from every pair of nerves of common sensation, (except the third pair (Willis) which seem to accompany the great sympathetic, wherever it is sent, for all their sensations. In some cases however, additional nerves of common sensation are sent to particular parts of two at least of these subordinate systems. All of the manifestations then of the operations of medicines in all of these subordinate systems, except the nervous system and its subordinate parts, have their essential and ultimate seat, in nerves of common sensation, and in the great sympathetic nerve commonly so called.

NON-ABSORPTION OF REMEDIES INTO THE SYSTEM.

As appears to me, there is sufficient evidence that those remedial agents, which are taken into the alimentary canal, make all their medicinal impression upon the nerves of its inner parietes, whence it is propagated by some one of the modes of sympathy already explained, to the part or parts, in which we perceive the first manifestations of their operation; — that no part or portion of one medicine in fifty, or a hundred, ever leaves the alimentary canal, except to pass-off with the fæces; and that, when some part or portion of a medicine happens to be capable of being taken into the mass of the circulating fluids, it produces all its medicinal effects before it is so taken-up; and no medicinal effect, (at least as a very general rule,) after it leaves the alimentary canal. As appears to me, medicines operate according to the same laws when they are applied to the skin, to the Schneiderian and bronchial membranes, etc.

I would not be understood to deny that the inner parietes of the blood-vessels are more or less susceptible to remedial impressions, provided medicines could be safely brought into contact with them. But there is abundant evidence that this can not be done, since it is well known that in all ordinary cases, it is extremely hazardous to inject even a little water, a little milk, or a little mucilage, into them; and it would certainly be abundantly preposterous to think of doing this, as often as it is commonly necessary to repeat the doses of most medicines. I do not consider it as at all contravening the truth of this general statement, that, in a very few extraordinary cases, in which the system is so completely occupied by some very powerful disease, that its susceptibility to almost every thing is destroyed, large injections of water holding salts in solution, have been thrown into the blood-vessels with impunity, especially after they have been extensively drained by depletion. We can no more infer, from such cases, that the injection of crude medicines into the blood-vessels, would be safe practice in ordinary diseases, then we can infer that it would be safe to give two hundred grains of Opium in twelve hours, because it has been done, not only with impunity, but even with advantage, in a few rare cases of Tetanus. As to getting any material number of medicines into the blood-vessels, through

the alimentary canal, I am confident that a thorough investigation of the subject, will satisfy almost any man, that this is not practicable, except in a very few exceedingly rare cases of agents of no great power; and even in these, in such minute quantities, that no remedial effects are to be expected from them.

The real truth appears to me to be that almost every thing is so modified, by its passage through the organs of primary digestion, that as a general rule it ceases to possess any medicinal powers, at least in any material degree, after such passage; and articles not capable of such modification, never get into the blood, in sufficient quantity to produce any appreciable effect, after they are received into the blood-vessels.

If remedies, when topically applied to the surface of the body, and when taken into the alimentary canal, operate in both cases only from being received into the mass of circulating fluids, as the very prevalent notion is, the operation, in each case, ought to be exactly the same in kind or quality, and likewise the same in universality. But under these different modes of employment and application, the effects of most medicines differ to a greater or less extent, and in many cases very widely. When the effects of medicines differ in kind or quality, under these two modes of employment, I think that such a fact affords a conclusive argument against the opinion of their operating by means and in consequence of absorption into the mass of circulating fluids, and deposition upon particular parts or organs; and in fact proves clearly that they must operate through some other medium. If a given agent when externally applied, operates only by being taken into the mass of the circulating fluids, it appears to me that it ought to affect either the whole, or a majority of the subordinate systems of the animal economy, just as it would affect them, if taken into the alimentary canal, and thence received into the mass of the circulating fluids. But if from external topical application, a given article operates only upon a particular and very limited, but still remote part, without at all affecting the system at large, as appears to me, it must be evident that it operates in some other manner, than through the medium of the mass of the circulating fluid; and if, under these particular circumstances, it operates in some other manner, it must be clear that under different circumstances, (as when taken into the stomach,) it may operate in

some other manner; and, I think it behoves us to inquire what this other manner may be.

It is a plain question of fact, and not all a matter of theory or hypothesis, viz. Do medicines that are taken into the alimentary canal produce their medicinal effects by an impression and influence made first upon the inner parietes of the stomach and upper and smaller intestines, and thence propagated by the nerves to the parts or organs, in which we perceive the primary manifestations of operation;—or, is the medicine in its intire state, or its active proximate principle unchanged, received into the mass of the circulating fluid, by which it is carried-about, till it is brought into actual contact with the parts, in which we perceive the primary manifestations of its operation? Does the very small number of medicines, which are capable of being injected into the blood-vessels with impunity, produce their medicinal effects by an impression and influence made first upon the inner parietes of the blood-vessels, and thence propagated by the nerves to the parts or organs, in which we perceive the primary manifestations of operation;—or, is the medicine in its intire state, or its active proximate principle unchanged, carried-about by the blood, till it is brought into actual contact with the parts, in which we perceive the primary manifestations of its operation? Do those medicines, which are applied to the skin, and the bronchial membrane, produce their medicinal effects by an impression and influence made first upon these parts, and thence propagated by the nerves to the parts or organs in which we perceive the primary manifestations of their operation;—or, is the medicine in its intire state, or its active proximate principle unchanged, actually absorbed into the blood by the skin, and bronchial membrane, (two non-absorbing textures,) and carried-about, till it is brought into actual contact with the parts, in which we perceive the primary manifestations of its operation?

Some medicines appear to be absorbed in quite small quantity, into the blood-vessels, and this in their intire state. It will be obvious that elements can be thus absorbed only in their intire state; but several salts of inorganic and chiminal origin are absorbed in the same manner. Vegetable and animal organic compounds are very rarely, if ever, absorbed intire into the blood-vessels. It is only some one of their proximate principles, and

that often in a much modified state, that is ever found in the blood, or emunctories, except the lower and larger intestines. Coloring and odorous principles are almost exclusively the principles of vegetable and animal organic compounds that are ever found to be received into the blood vessels, from the alimentary canal. These are rarely the medicinally active principles of vegetable and animal organic medicines. When the odorous principles of vegetable and animal organic medicines are absorbed, from the alimentary canal into the blood-vessels, they are commonly perceived subsequently either in the urine, the sweat, or the halitus from the lungs. When the coloring principles of vegetable and animal organic medicines are absorbed from the alimentary canal into the blood-vessels, they are commonly perceived subsequently in the urine only, of all the excrementitious secretions.

The quantity of any medicine received into the blood, or elsewhere within the system, never bears any regular relation to the quantity taken into the alimentary canal. There is as much manifested within the system, when only a small quantity has been taken into the alimentary canal, as when a large quantity has been swallowed. The quantity of an intire medicine, that is ever found to be absorbed into the blood, is never, to my knowledge, sufficient to produce its medicinal effects; and the primary medicinal operative effects may generally, if not always be perceived before there are any manifestations of the article out of the alimentary canal, and any where else within the system. In addition to this, the operative effects of medicines are usually, if not always, pretty accurately proportioned to the quantity taken into the alimentary canal. Many medicines, when injected into the lower and larger intestines, will produce their effects topically; and many I think are capable of producing their effects upon the whole system;—and yet the lower and larger intestines never absorb any thing. I do not forget that the opinion is entertained by many, that absorption does take place from the lower and larger intestines; but as appears to me, such opinion is a very great error. The lower and larger intestines are a mere reservoir for the refuse of the food, from which every thing, that is capable of being useful in the system, has already been separated, in the upper and smaller intestines. There can be no more need that the lower and larger intestines should be absorbing organs, than that the urinary bladder should be an absorbing organ.

Many medicines, when applied to the healthy skin, produce topical effects at least; and some medicines when thus applied affect the constitution, or system at large, and that even powerfully; though I consider it abundantly ascertained that the healthy skin is never an absorbing surface. A moderate number of medicines are capable of affecting the system at large, through the medium of the lungs, and by means of inhalation; and yet, contrary to a very common, and a very prevalent opinion, I do not consider the bronchial membrane as any more an absorbing texture than the skin. When any odorous principle is perceived in the urine, after it has been inhaled for some time continuously, I doubt not that it always gets into the system by means of admixture with the saliva in the fauces, whence it passes into the stomach, and thence follows the course of substances taken into the alimentary canal.

In order to prove that any particular or individual medicine operates by its impression upon the blood, or upon any other of the fluids of the body, or upon the inner parietes of the blood-vessels, or by contact with, or extravasation upon the organ or organs diseased, it is necessary to show that such medicine is capable of being taken, and is actually taken into the mass of the circulating fluids; but showing and proving that such medicine is capable of being taken, and is actually taken into the mass of the circulating fluids, does not in the slightest degree contribute to prove that the medicine either operates by its impression upon the blood, or any other fluid of the body, or upon the inner parietes of the blood vessels, or by contact with, or extravasation upon the diseased organ or organs. It is clear, that even when an article is actually taken into the mass of the circulating fluid, it may exert all its medicinal operations upon the inner parietes of the alimentary canal, before it is received into the blood-vessels.

That a few intire articles, and certain proximate principles of others, are actually taken into the mass of the circulating fluid, in comparatively minute quantities, I suppose can not be questioned; but then the number of the articles of the whole materia medica thus admitted into the blood-vessels, in comparison with those, of whose admission into the blood there is no evidence, is comparatively small, so that such admission can not be considered as essential to the operation of medicines generally.

But I will consider a few of the cases, in which medicines are actually taken into the blood-vessels. Sulphurum elementarium, is frequently claimed as an article, which, as a medicine, must necessarily be admitted into the blood-vessels, and which operates as a medicine in consequence and by means of such admission. Indeed I believe it is commonly supposed to pervade the system much more extensively than this, and even to be excreted by the skin, the regular emunctory for the effete heat of the body, and by the lungs, the regular emunctory of the effete Carbon of the body. Now I believe that Sulphurum is a regular element of the human body, but I believe that the quantity entering into its composition is comparatively small. Such being the fact, we should naturally expect that a small quantity of it might leave the alimentary canal, be absorbed into the blood-vessels, and even pass beyond these. That it does this is accordingly the prevalent belief. If Sulphurum is received at all into the system, it must be in its intire state, since as our knowledge now stands, it is reckoned an element. Sulphurum is cathartic, and also what I have been in the habit of calling adenagic and neuragic. When Sulphurum is administered as a cathartic, it produces this effect before there are any manifestations of it in the blood-vessels, or any other parts of the system, or in the excretions; and the amount of its cathartic effect is proportioned to the amount of this agent taken into the alimentary canal. For its cathartic operation, a far larger quantity must be taken into the alimentary canal, than is ever capable of being received into the blood-vessels, or the system beyond these. Such facts seem to me to prove that it always operates as a cathartic, by means of its impression upon the inner parietes of the stomach and upper and smaller intestines.

When Sulphurum is given in frequently repeated doses, but still in as large quantities in the twenty-four hours, as can be tolerated without catharsis, it produces its fullest adenagic effects before there is any indication of it in the blood, or any excretion; and these adenagic effects are proportioned to the quantity taken into the alimentary canal, and not at all to the quantity capable of passing further into the system. As an adenagic then, this article seems to operate upon the inner parietes of the stomach and upper and smaller intestines. As neuragic effects are

slow of production by Sulphurum, there is always ample time for the reception of as much of it into the system, as is capable of being received; but as the neuragy seems to be proportioned to the quantity taken in the whole, rather than to the quantity capable of being received into the blood; and as it seems to prove adenagic, by means of its operation upon the inner parietes of the stomach and upper and smaller intestines, we may fairly presume that it produces its neuragic effects in the same manner.

Cullen, in his *materia medica*, article *demulcents*, says that "the Vitriolic" (Sulphuric) "Acid passes copiously by the skin, in its acid state, when" (by internal use) "it cures Itch; and the Muriatic" (Chlorohydric) "acid is found ready to irritate Ulcers and open Issues." This may perhaps be considered a very plain and fair case of the operation of a medicine by means and in consequence of being taken into the system, as common language is. All I have to say in reference to this case is, that I never could get either Sulphuric, Chlorohydric, or Nitric Acids to pass the skin in this manner. I have tried very perseveringly to cure Itch by the internal use of Sulphuric Acid, and also to get Sulphuric Acid to pass-off by the skin, but all my attempts have invariably proved failures. I have given it with great freedom, and persevered long in its use. I have likewise given Chlorohydric and Nitric Acids in the same manner applying tests to the excretion from the skin, but all to no purpose, they would not pass off by that emunctory. I have often known them to irritate the alimentary canal severely; and in a few instances, I have known Ulcers irritated, though without any indications of an Acid, in the excretion from such Ulcers. When taken very freely into the alimentary canal, I have known these acids to produce a considerable amount of constitutional irritation, but always without any appearance of Acid in the excretion from the skin. When taken internally in comparatively moderate quantities, I have often known them to vitiate the milk of a wet-nurse to such a degree that the child could not take it, without distress and pain; and yet tests exhibited no trace of an Acid in the milk.

The Nitrate of Potassa is often, if not commonly mentioned, as proving triumphantly, that medicinal agents are received into the blood-vessels, pass-off by the excretories, and, (as seems to be considered a necessary consequence,) produce their remedial effects,

in consequence of being so received into the mass of the circulating fluids, because, it is said that when ever repeated doses of it are taken, it will be found in the urine, as can be evinced, under such circumstances, by dipping some paper into this fluid, and subsequently drying and burning it, when it will be found to deflagrate. But, according to my observations, much more has been inferred from this fact, than (as I think) it can be admitted to prove. When the Nitrate of Potassa is taken for a considerable length of time, particularly if the intestines are rather torpid and slow as respects peristaltic action, during its use I have always found on proper investigation moderate manifestations of it in the urine, but still the quantity that seemed to pass-off by this emunctory, has invariably been very short of the whole, that the patient has taken, and it has never borne the least regularity of proportion, to the operative effects of the remedy upon the system. On the contrary, in instances where I have known it taken in quite large quantities, its use persevered-in for a very considerable time, and with the production of its most decided effects upon the system, provided the alimentary canal was kept in a moderately lax state, no trace of it could be found in the urine.

The correctness of these observations is recognized by writers on materia medica. Thus Dr. Paris, when he informs us, that we must take care that the bowels i. e. the intestines are not disturbed, i. e. made lax by this medicine, in order to find manifestations of it in the urine. Now this bears immediately upon the point at issue. If it is not found at all, or only very sparingly in the urine, under circumstances when it has produced its most decided medicinal operations, and if, when it is actually found there, the quantity is small in proportion to what has been taken, small in proportion to its operative effects, and if its operative effects are in as exact proportion as can be appreciated, to the quantity in fact taken into the stomach, instead of to that, which is found in the urine, then I think it may be considered as satisfactorily certain, that it does not produce its remedial effects, by virtue of being taken into the blood-vessels, and consequently into the mass of the circulating fluids. I am aware that in those instances, in which this salt is taken in large quantities, while there is rather a lax state of the intestines, and in which it does not pass-off in the urine, but affects the whole system very pow-

erfully, it has been alleged by some, that it is permanently retained some where in the system, and that this is the reason why it produces so much greater operative effects, than when it passes-off by the urine. But does it, under such circumstances, continue permanently in the system, remaining as a component part of the blood, or being assimilated to the solids ;—or is it eliminated by the lungs, or skin,—or is it, by some selecting and inverted action of the left subclavian vein, the thoracic duct, and the lacteals, returned to the intestines, in order to pass-off by the rectum, a process that I once heard detailed as a method, by which certain things were imagined to be got-rid-of, by the animal economy. As it does not pass-off by the kidneys, it requires to be disposed-of in some way or other ;—and as appears to me, we shall have by far the least difficulty in regard to its disposition, if we suffer it to remain, from its ingestion to its final elimination by the rectum, in some part of the alimentary canal.

The truth is, there does not seem to me to be a particle of proof that, in the cases referred to, it is ever taken at all, into the mass of the circulating fluids, or why should a lax state of the intestines be so favorable,—indeed so necessary, to this supposed absorption ; or why should a costive state prevent it ? It seems to me also, that the assumption of its absorption, in the cases referred to, is absolutely destitute of all proof,—is in the highest degree improbable, and for the reasons stated, may be pronounced to be altogether unfounded ;—but this is a specimen of much of the reasoning of those who have maintained the doctrine of the medicinal operation of remedial agents, by means of absorption into the circulation.

Of the true Chlorite of Potassa, formerly called Hyperoxymuriate of Potassa ($\text{K} + \text{Cl}$.) Dr. Duncan says “it is singular that, in some cases, in which it produces little or no effect, it passes-off undecomposed in the urine.” He adds “in these cases, Mr. Cruickshank proposes to remedy the defect by giving after each dose ten or fifteen drops of Muriatic” (Chlorohydric) “Acid.” But, how will Chlorohydric (or Hydrochloric) Acid prevent the Chlorite of Potassa from passing-off undecomposed in the urine ? In twelve experiments, upon three different persons, made by me in 1832, with the Chlorite of Potassa, the only case in which it speedily proved a cathartic, exhibited by far the greatest effect

upon the system. Now, we cannot reasonably suppose that in this case, more of this medicine was taken into the circulation, than when it did not pass-off, under twenty-four, thirty-six, or even forty-eight hours from the time it was taken.

Dr. Anthony Todd Thompson says that "Lunar Caustic" (produces its ordinary medicinal effects) "through the medium of the nerves of the stomach upon which its primary action is exerted." I very much doubt whether this opinion prevails to any material extent, among the members of the medical profession. Among my acquaintance, it seems to be generally believed that the Protonitrate of Silver passes unchanged into the blood-vessels, and then, some how or other, produces its medicinal effects, without need of any other explanation. It must be got-out of the alimentary canal, and into the blood-vessels, and then it seems to be considered competent to take care of itself, so that the question—what next? must be a very impertinent question. Even Dr. Anthony Todd Thompson, though he allows this article to operate first upon the nerves of the stomach, adds how ever that after this "it is absorbed and taken into the circulation,—a fact demonstrated by the Leaden hue of the skins of those who take it." For my part, I should rely much more upon a mathematical demonstration, than upon such an one as this. While I was a professional pupil, and for the first four or five years of my practice, the internal use of the Protonitrate of Silver was intirely unknown in that section of the country in which I resided; and yet I saw several well marked, and even intense cases of Leaden discoloration of the skin, where it was absolutely certain that a particle of Nitrate of Silver had never been taken. From the period of my first prescribing Nitrate of Silver internally, I continued to meet with cases of Leaden discoloration of the skin; but to the present time (viz. August 1852) with only a single exception, (to be more particularly mentioned here after,) all these cases have been subjects that never took Nitrate of Silver at all. In altogether the most intense cases that I ever saw, it was perfectly ascertainable that they had never taken a particle of this agent. The single individual and only case, ever falling under my observation, in which a patient had this Leaden discoloration, after taking the Protonitrate of Silver, now deserves a passing notice.

The disease of this patient was a compound one, viz. Struma or

Strumous softening of the spinal-cord, of course attended with a sub-acinesia of the lower extremities, impaired power of the upper extremities, and considerable disturbance of the function of common sensation, much more particularly in the lower part of the trunk, and in the lower limbs. Conjoined with this existed Chronic Diarrhœa. This patient constantly used water brought through Leaden pipe, and had had paroxysms of Gastrodynia or Enterodynia, I now forget which. She was also harrassed with a great deal of epigastric sinking. The medicine was begun in doses of an eighth of a grain, repeated four times in the twenty-four hours. On two days' trial, this quantity was found to produce Tormina and Diarrhœa. Just half the quantity was given, three or four days longer, but as it still continued to produce Tormina and Diarrhœa, its use was abandoned. I am sure the patient did not take half a dozen grains in the whole, and, I think, not four grains, but the exact quantity can not now be ascertained. I suppose that the strong tendency to Diarrhœa in this case, and perhaps an inadequate amount of the medicine used for its restraint, will be admitted to explain the great susceptibility of this patient to the irritant effect of the Protonitrate of Silver. But even in addition to all this, the Leaden discoloration had begun, and in fact existed in a very decided degree, before even this small quantity of the Protonitrate of Silver was taken, and therefore the case would not have been worthy of mention in this connexion, had not a highly popular physician of one of our large cities decided ex cathedra, that the Leaden discoloration was undoubtedly the effect of the Protonitrate of Silver. The late Theodore Woodward, M. D., (of the Vermont Academy of medicine,) informed me that he had met with a number of cases of Leaden discoloration of the skin, but never in a subject that had taken any Protonitrate of Silver. He said that much the worst example of this Leaden discoloration, with which he ever met, was in a lady, that he was sure had never taken a particle of this salt in her life. On the other hand, since I first became acquainted with the internal use of the Protonitrate of Silver, I have used it very often, and many times very freely. In several instances, I have given fifteen grains a day for three and four months, in uninterrupted succession, and never yet saw a case of the Leaden discoloration of the skin, for which I have prescribed. Dr. Theodore Woodward, heretofore quoted, informed me that he had

used the Protonitrate of Silver, in all probability as often as I had, and in many cases, in as large a quantity, and for as long a time, and he had never seen the Leaden discoloration, even in a single case, in which this salt had been thus used. I have met with very many physicians, who have been long in the habit of using the Protonitrate of Silver, with great freedom, in their general practice, and yet have never seen the Leaden discoloration of the skin, in any of their patients. Again, I have often been told by physicians, that they had met with cases of this Leaden discoloration, where no Protonitrate of Silver had been previously taken. I have certainly known of several cases of this Leaden discoloration of the skin, which have been hastily pronounced to have been produced by Protonitrate of Silver, when thorough investigation of the facts of the cases resulted in the unquestionable conclusion, that this salt had had no sort of instrumentality in the matter, any more than in the case which I have heretofore specified, as occurring under my own observation. In short, I do not believe that the Leaden discoloration of the skin, now under consideration, is ever produced, in any degree, by the Protonitrate of Silver. If it may take place in so many instances, as I have known, or had authentic information of, without the instrumentality of Protonitrate of Silver, it undoubtedly takes place in all cases without it. Since this hypothesis was first promulgated, I have known many observations for the purpose either of proving or disproving it; but all have tended rather to disproof than to proof.

But whether this hypothesis is true or not, I doubt whether any practical physician will ever pretend that this Leaden discoloration is necessary to the medicinal effects of Protonitrate of Silver. If this Leaden discoloration is the effect of the absorption and deposition of Protonitrate of Silver, I suppose it will be admitted that where there is none of this Leaden discoloration, there can have been no absorption of this salt, and consequently that it produces its medicinal effects without such absorption. On the other hand, if this Leaden discoloration is not the effect of the absorption and deposition of Protonitrate of Silver, (as I have long been well convinced,) what evidence is there that this agent is ever absorbed? Even on the supposition that the Leaden discoloration of the skin, which is attributed to the Protonitrate of Silver, is truly due to this article, there must be a deposition of the salt in the

whole rete muscosum so called, which is not a part in which there are any manifestations of its medicinal operation, and therefore this deposition can not be taken as an illustration of the manner in which medicines affect one texture part or organ, in preference to, or more than an other. Besides, the Protonitrate of Silver, is supposed to remain permanently in the system, never being excreted as effete matter, which assuredly will not be maintained in regard to medicines generally, if it is in regard to any other agent what ever.

It is commonly supposed that when Chlorohydrargyrous Acid, Dichlorid of Mercury, or Calomelas, is taken into the stomach, it passes into the mass of the circulating fluids; and even further, is deposited among the solids, how long to remain there, the hypothesis does not specify. At all events, this has been the opinion of by far the greatest number of physicians, with whom I have had opportunity of social intercourse; and it would seem to be the opinion of very many authors, if not most of them. Some, I believe, suppose that absorbed and assimilated Mercury, whether as elementary Mercury or as its Dichlorid, — I am not aware that the hypothesis decides which, — is finally excreted by the skin, — an emunctory whose regular function is to excrete the effete heat of the system latent in the vapor of water. Assuredly both elementary Mercury, and its Dichlorid, are sufficiently diverse from the legitimate subject of excretion from the skin.

For myself, I was never able to find the evidence of such absorption into the system, as seems to be contemplated, in this case. It appears to me that the Dichlorid of Mercury is never normally taken into the mass of the circulating fluids, whatever may be the fact under what may be termed the caprice of disease. If it is ever thus absorbed, where are the proofs, or even the indications of it. I have repeatedly made investigations in relation to this subject, and it has always appeared to me, that the whole of the Dichlorid of Mercury, that is ever taken into the stomach, in any given disease, ultimately passes-off with the intestinal discharges, in the form of a black powder. If the intestinal discharges, after a large dose of Dichlorid of Mercury, happen to contain but a small proportion of fecal matter, and a large proportion of watery matter; and especially if such discharges are largely diluted with an additional quantity of water, with other appro-

priate management, the black powder will be sufficiently abundant to require all the Dichlorid of Mercury, that has been taken, to produce it. There can be no reasonable probability that the greater part of a dose, or of several doses, that are taken into the stomach, passes through the alimentary canal, and is voided in the form of a black powder, without exerting any medicinal operation, while much the smaller part, — a part so small as to be almost, if not quite inappreciable, — is absorbed into the mass of the circulating fluid, and thereby produces all its effects. If this were the manner of its operation, the amount of the effects would not be proportioned to the quantity taken, as they seem actually to be, but rather to the quantity absorbed into the mass of circulating fluid. The black powder, into which the Dichlorid of Mercury is converted, in the alimentary canal, I have always supposed to be the Disoxyd of Mercury, once called *Æthiops per se* produced by the Oxyd of Sodium contained in the saliva, gastric liquor pancreatic liquor, etc. It is possible, but not probable, that this black powder is the Disulphid of Mercury once called *Æthiops mineralis* produced by the Sulphihydrous Acid once called Sulphuret of Hydrogen or rather Protosulphuret of Hydrogen. I have long supposed that, when Dichlorid of Mercury is taken into the stomach, it is immediately converted into this black powder, and before it produces any of its ordinary medicinal operative effects. This however can happen when the Dichlorid of Mercury is taken only in ordinary quantities, since there is not Oxyd of Sodium, or possibly Sulphihydrous Acid, enough in the alimentary canal, to decompose extraordinary quantities of the Dichlorid of Mercury. In a few cases of Colic, treated almost exclusively with enormously large quantities of Dichlorid of Mercury, I have been credibly informed that the greatest part, if not the whole of this agent usually passes off unchanged, and as Dichlorid of Mercury. I have long supposed that there is good reason for believing that when we administer Dichlorid of Mercury, we always obtain the effects of Disoxyd of Mercury, in consequence of the double decomposition, of which I have just spoken. A friend once objected to this view on the ground that the operation of Disoxyd of Mercury, given as such, varied materially from that of Dichlorid of Mercury; whereupon I immediately prepared more than a pound of the Disoxyd, by the double decomposition

of the Dichlorid, and the Hydrite of Potassa. The preparation was a remarkably pure and good one. This I divided into ounces, and distributed them amongst my professional friends, (such as Dr. M. F. Cogswell, and Dr. E. Todd, of Hartford Ct.; Dr. E. F. Reed, South Windsor Ct.; Dr. T. Miner of Middletown Ct.; Dr. S. B. Woodward of Wethersfield Ct.; Dr. H. Fish of Salisbury Ct. and several others,) requesting them to employ the article, just as they employed the Dichlorid, and inform me of the difference in their effect. The report of the whole was the same, viz. that they could perceive no sort of difference in the operation and effect of the two articles in the same doses and quantities. I had previously been in the habit of using the Disoxyd of Mercury, so prepared, and I have continued to employ it ever since, but without ever being able to perceive any difference in the effects of this agent, and the Dichlorid. I am apprized that there is some difference in their atomic weights, the Disoxyd being 208, while the Dichlorid is 235.48. As the doses are so small, I do not think that this difference in atomic weight can make any difference in the doses of each, at all necessary to be regarded in practice.

Although I have often administered the Dichlorid, and the Disoxyd of Mercury, to wet-nurses, yet I never knew the infant purged by it, when administered in this way. I believe that it is the prevalent opinion among physicians, that nursing infants may be purged in this way; though, on how satisfactory evidence the opinion rests, I am unable to say. Admitting the truth of the opinion in question, I can not perceive that it is necessary to the effect upon the infant, that it should actually get into any of the substance of the wet-nurse's body or milk. It is well known that mere strong emotions and passions often change prominently the qualities of the milk of a wet-nurse, so as to cause it to produce morbid effects upon the nursing child. One of the most common of these morbid effects is sudden Tormina and Diarrhœa. This I have seen very often. Now, if strong emotions and passions will so change the qualities of the milk as to affect the nursing child in this manner, where there is nothing taken into the stomach to pass the organs of primary digestion unchanged, to pass the lacteals, the mesenteric glands, the receptaculum chyli and thoracic duct, the whole mass of the circulating fluid, and finally the mammary gland unchanged, it seems to prove

that the milk may be very greatly altered without receiving any thing into it, and in fact, that it may be rendered cathartic, without receiving any thing cathartic into it. If a wet nurse employs improper diet, it frequently produces *Tormina* and *Diarrhœa* in the nursing infant, though the food used did not possess a particle of cathartic power, and besides, was perfectly digested by the stomach of the nurse. Under such circumstances, nothing cathartic can possibly have been absorbed into the system, escaped from the mammary gland of the wet-nurse, and reached the infant. A very much fatigued exhausted and greatly heated wet-nurse who drinks very cold water freely, while actually suckling the child, will be much less affected by the water than the child; and what is worthy of remark, the child will often be strongly affected, very soon indeed after the water comes into contact with the wet-nurse's stomach, the child will cry-out with pain apparently within the abdomen, and this will usually be followed sooner or later by a *Diarrhoic* discharge from the intestines. At all events I have witnessed this brief train of phenomena several times, and too often to suppose it occurred as a mere accident.

It is worthy of remark in this place, that whatever vitiates the quality of the milk of a wet-nurse, almost invariably, if not always, renders it capable of producing *Tormina* and *Diarrhœa*, whether the substance doing the mischief, happens to be cathartic, or intirely destitute of any such power. Now, if strong emotions and passions, and if improper food not at all cathartic, and perfectly digested, will so change the qualities of the milk of a wet-nurse as to give it the power of producing *Tormina* and *Diarrhœa*, why may not certain medicinal agents do the same, without being absorbed, and passing into the milk. It has long appeared to me to be the fact, that, when any thing taken into the stomach of a wet-nurse, produces *Tormina* and *Diarrhœa* in a nursing infant, it is accomplished by changing the quality of the secretory action of the mammary gland, and thereby the quality also of the milk, by which it becomes capable of producing the effects in question. It appears to me therefore that we may very properly conclude that this agent is never absorbed into the circulating fluids, never deposited among the solids, nor excreted by any other emunctory than the lower intestines; nor is the *Disoxyd* of *Mercury*, into which I suppose it is converted, by the *Oxyd* of *Sodium*

of the saliva, of the gastric liquor, of the pancreatic liquor, etc. I think that it produces all its medicinal effects, by action upon the inner parietes of the alimentary canal; and that it never leaves this canal, till it finally passes-off, by the rectum.

I have frequently been informed by patients that were taking Creasote (so called) continuously, and for a considerable time, that there was some times a strong smell of it in the urine. But the medicinal effects of Creasote took place before this smell was perceived, and even when it never occurred, and were not at all proportioned to the amount of this smell, but rather to the amount taken into the alimentary canal. The presumption is therefore that it operates upon the inner parietes of the stomach and upper and smaller intestines.

While a man is very cold, and while he is to continue some time longer in the cold, if he takes Cider, and I suppose any other liquor, strongly impregnated with Capsicum, his urine will very soon have the odor of Capsicum, and there will often, but not always, be a sensation of scalding in the urethra, when the urine is evacuated; but under these circumstances there will usually be no apparent effect of the Capsicum upon the system, till these symptoms have passed-by. Some times, when these symptoms do not occur at all, there will be far more effect upon the system at large. On the contrary, if this agent is taken when a man is not unnaturally cold, and is not subsequently to remain in the cold, there will be no smell of the Capsicum in the urine, so far as I could ever find, and no sensation of scalding when it is discharged; but there will immediately be the ordinary effects of the Capsicum upon the system. This (as appears to me,) is analogous to the manner, in which the Nitrate, and the Chlorite of Potassa operate; and it contributes to show that all these articles produce much less effect upon the system, when they are actually taken into the circulation, than when they are not.

Such facts as these, would seem to show that if we desire the greatest effect from a given quantity of any medicine, that happens to be capable of being at all absorbed into the circulation unchanged, or but little changed, we must hinder such absorption as much and as long as possible. On the contrary, if there could be any possible motive for desiring a patient to take a large quantity of medicine, with the least possible effect, it would seem

that we ought to accelerate and promote such absorption, by all means in our power.

Cases are alleged, by various authorities, to have happened, in which the topical application of Cyanid of Hydrogen* to the sound skin has been supposed to destroy life. I have always considered it as well ascertained that this substance, in its most concentrated form, may be applied to an ulcer, without affecting the constitution at all. If such were the fact, it would be intirely in conformity with what appear to me to be ascertained laws of the animal economy. The external topical application of Protocyanid of Hydrogen of a given strength, and in a given quantity, and to an equal extent of surface (as is said) will produce a far greater effect, when it is made to the sound skin, than when it is made to an Ulcer. Indeed it is said, though I doubt the authority, that that quantity which will actually destroy life, when applied to the sound skin, will produce, very little or no effect upon the system, when applied to an ulcerated surface;—and yet the sound skin is not an absorbing surface, while an Ulcer is alleged to be an absorbing surface. This, if correct, (which I doubt) at least evinces that Cyanid of Hydrogen, does not produce its medicinal operation, by virtue of being taken into the circulation. The truth is, the susceptibilities of the sound skin are in a natural state,—a state of integrity and perfection, while the susceptibilities of an ulcerated surface, are in an imperfect and morbid state. With the sound skin all the healthy sympathies exist, while with an ulcerated surface the sympathies are morbid and interrupted.

The statement, whether true or false, is in conformity with the laws of physiology. Disease of the stomach and upper or smaller intestines, is said, in like manner, and, as is supposed, upon the same principle, to prevent the regular operation of medicines. But I do not find reason to believe that Cyanid of Hydrogen is an agent of such intense and powerful activity, as this would require it to be. As far as I can find evidence upon the sub-

* I call this agent Cyanid of Hydrogen in preference to Cyanohydric Acid because it certainly performs the functions of a salifiable base, as in the Dicyanoferrite of the Cyanid of Hydrogen, ($2 \text{ H. Cy.} + \text{Fe. Cy.}$) but, I have no knowledge that it ever performs the functions of a salifying compound or acid, in any single instance.

ject, it would seem to be much less active than is commonly supposed; and I consider it doubtful whether it is capable of at all affecting the system at large, by topical application to the skin, whether sound or diseased. If a sufficient quantity of its vapor were to be inhaled, it might affect the system at large and quite probably destroy life. I suspect that some of the opinions just referred to, originated from the following tale, without Mr. Granville's comments.

Mr. Granville says that "In the year 1815 a report went abroad that a Mr. Scharinger of Vienna, had fallen a victim to the poisonous action of" (Cyanid of Hydrogen) "Prussic Acid" (erroneously so-called) "accidentally applied to his naked arm, while engaged in some experiments concerning its preparation." "This" (supposed) "fact was repeated in the" (London) "Medical Repository for that year, when" (says Mr. Granville) "owing to my being then engaged on the subject of the Acid in question, I ventured to address the editor of that respectable journal, with an expression of my doubts as to the truth of the" (supposed) "fact itself, and supported my doubts, by what I deemed unanswerable evidence. The result of this address was an application from the editor to a correspondent in Germany, who, in consequence of a communication from Baron Jacquin, the well known Professor of Botany at Vienna, was enabled to contradict the report, as far as it related to the" (Cyanid of Hydrogen or) "Prussic Acid's having destroyed Mr. Scharinger; for it appeared that this chimist had died of apoplexy." "Yet notwithstanding this contradiction from authority, the" (supposed) "fact has again been repeated, in some recent publications, with great disregard to truth, and apparently for the purpose of throwing a good share of odium on the physicians who endeavor to establish the claim of" (Cyanid of Hydrogen or) "Prussic Acid, to the confidence of every practitioner as a medicine." (*A. B. Granville on Hydrocyanic Acid, London 1820, 2nd Edition P. 40, 41.*) After I became a public instructor and had repeated Mr. Granville's statement in my instructions time and again, the tale, without Mr. Granville's correction, was adopted into a text-book of one of my colleagues, and thus retailed in one of the lecture rooms of the Institution where I gave my lectures.

Although Cyanid of Hydrogen is a very simple pure and

intense narcotic, and as such, affects the whole of the nervous system, yet it undoubtedly affects the nerves of involuntary and instinctive expressory motion more in proportion than any other subordinate part of the nervous system, since it destroys life by suspending the functions of the nerves of expression, while the functions of the involuntary and instinctive motor nerve of chymical action nutrition and reproduction remain unimpaired, so that death from it might doubtless be always prevented by keeping-up factitious respiration, till its narcotic effects have had time to pass-off intirely. Thus far, I can discover no reason for concluding that Cyanid of Hydrogen is ever taken (without decomposition and recomposition into a new form, that is, in other words, digestion,) into the mass of the circulating fluids, and much less that it ever exerts any additional operation after it disappears from the alimentary canal. The effect it produces before its digestion, it is true, continues some time after digestion. If it did not, its operative effects would be transient indeed, since it is one of the most easily decomposed compounds that is used in medicine. Its rapidity and facility of decomposition is very remarkable for an article not of vegetable or animal organic origin; for I am not aware that it is ever found in any vegetable or any living animal. The operation of Cyanid of Hydrogen is undoubtedly by far too rapid, to be maintained to take place, any other way than through the nervous system. I do not think that an argument to the contrary can possibly be founded on the facts of the case.

In violent vesical irritation, from an epispastic of Cantharis, but without discharge of blood, I have often witnessed speedy and effectual relief from the application of pledgets of lint wet with a strong infusion of the seeds of *Datura Tatula* to the blistered surface or part. Such an operation can not be uncommon, since I have often received testimony to the same effect from numerous professional friends and acquaintances. Now the fact that an epispastic of Cantharis produces such vesical irritation, and the additional fact that it is capable of relief by the topical application of *Datura* to the blistered part, I have often heard quoted as irrefragable proof of the absorption into the system, and deposition in or upon the urinary bladder, both of Cantharis and of *Datura*. These facts do not seem to me even to contribute any

thing at all toward the proof of what is claimed from them. The vesical irritation in such a case, under my observation, has always been very exactly proportioned to the irritation in the part blistered, and I have never known the vesical irritation relieved till there was relief in the part blistered. In addition to this, the system is never at all affected by the Datura, when it relieves the vesical irritation by topical application to the blistered part. In fact I never knew the topical application of Datura, however extensive it might be, to affect the system at large, and much less any individual organ remote from the place of its application. In a case of vesical irritation from an epispastic of Cantharis, the most extensive application of Datura to other than the blistered part will not render the least benefit. As the urinary bladder is much more liable to become irritated by an epispastic upon the inside of the thighs, I have in some cases of vesical irritation from Cantharis, applied Datura to the whole of the inside of the thighs, but never with the least benefit, as relates to the vesical irritation. As appears to me, such facts are absolutely incompatible with the notion of the operation either of Cantharis or Datura, by absorption into the system, and deposition upon the urinary bladder, as the common theory is. If the urinary bladder is irritated by the absorption and deposition upon it of the Cantharis, how comes it that it is so exactly proportioned to the irritation of the blistered part, and is always relieved in the same proportion with that, and never without it? If the vesical irritation, produced by an epispastic of Cantharis, is occasioned only by an absorption and deposition of Cantharis upon the urinary bladder, (as the common language is,) and if it is relieved only by an absorption and deposition of Datura upon the urinary bladder, how comes it that an absorption for effectual relief can take place only from the blistered part, which immediately occasioned the vesical irritation. Where I have had occasion to apply a speedy succession of epispastics of Cantharis, I have some times, dressed the whole with Datura, in order to prevent vesical irritation. If I continued to repeat the application of the epispastics, the vesical irritation would take place at last, but I never could relieve it by topical applications except to the very blistered part, which immediately produced it, though I have often tried to do so. By this I do not mean to say that I could not relieve it by internal remedies. I leave these out

of the question in the present discussion, as I think they do not at all pertain to it. I have good reason to conclude that facts of the character just mentioned may be adduced with regard to *Atropa lethalis*, *Papaver somniferum*, *Camphora officinarum*, and various other narcotics. *Papaver* taken internally for vesical irritation has never, under my observation, relieved such irritation, until it has relieved the irritation of the blistered part; a fact which likewise tends strongly to the conclusion that the vesical irritation is merely sympathetic of the irritation of the blistered part. As appears to me, such facts tend strongly to the conclusion that the vesical irritation sometimes resulting from an epispastic of *Cantharis* is merely sympathetic of the irritation of the blistered part.

But perhaps it may be asked how vesical irritation is occasioned by *Cantharis* taken into the alimentary canal? I have only to say that I treat of what I think I know, or of what seems in the highest degree probable from other ascertainable facts. I am not bound to account for or explain every thing, but only as much as is within my power, and certainly not more than I undertake. I am certainly ignorant of any topical irritation in the alimentary canal from the *Cantharis*, analagous to that of a blistered part, of which the vesical irritation can be considered as sympathetic, nor do I discover any necessity for such an one. I can not discover why the vesical irritation may not be sympathetic of the first impression made by the *Cantharis* upon the inner parietes of the organs of primary digestion, without any necessity that such first impression should occasion as much irritation upon the parts on which it is made as there is in a blistered surface. It must be recollected that I do not attempt to explain such points as how *Cephaelis Ipecacuanha* vomits—how *Exogonium Purga* purges—how *Cantharis* irritates, etc. No one knows this, nor will it ever be known. Such inquiries are palpably hopeless. It is not so with the question whether medicines operate by nervous communication from the parts on which their first impression is made, to the parts in which their operation is manifested, or are taken into the mass of the circulating fluids and carried to the parts in which their effects first appear. This, as appears to me, is a hopeful, and therefore legitimate subject of inquiry, and one which I think can be determined by facts and without the aid of hypothesis.

As a general rule, *Nicotiana Tabacum* may be applied to a diseased part or organ, much more copiously, without affecting the system at large, than to a healthy part. In acute Strumous Phlogosis of the synovial membranes of the knee joint, such a cataplasm of *Nicotiana Tabacum* as would affect the system powerfully, if applied to the healthy joint, will fail intirely of producing anything but a mere topical effect; at least this is sometimes, if not always the fact, as I have witnessed. The same is also sometimes true in chronic Strumous Phlogosis of the cancellar texture of the bones, as I have also witnessed. It is also equally true in some, if not in all cases, in which the disease is upon the surface. If the system were affected by means of the absorption of *Nicotiana Tabacum* into the mass of the circulating fluids, I should not think that such deep-seated affections could possibly prevent absorption from the skin. I can easily understand how an interruption of the healthy susceptibilities of the surface, even from a deep-seated disease, may prevent the regular propagation of the impression of the medicine to the system at large.

Dr. Christison, who supposes that the alcaloid Coniine produces its effects by absorption into the mass of the circulating fluids, so far as I understand him, because he has concluded that when topically applied, its activity has seemed to be proportioned to the supposed activity of absorption in the texture, with which it was brought into contact, expressly says that even where so much has been taken or applied, as to destroy life, not the slightest odor, or other manifestation of it could be detected in the blood. As appears to me, a hypothesis that the effects of a medicine are exactly proportioned to the supposed activity of absorption in the parts to which the medicine is applied, (I believe by inoculation) constitutes but slender evidence of the absorption of such medicine into the blood-vessels, in opposition to the fact that not the slightest odor, or other manifestation of the medicine, can be detected in the blood. If such a substitute for evidence is received, any thing, no matter what, can be proved. To quote Christison's words, "it would seem to me however, to be very nearly made-out, by a fact already mentioned, that although absorption into the blood may be a part of the chain of sequences, which attends the action of Coniine, yet this is not all; and that this poison does not act by being carried

substantially with the blood." "I allude to its tremendous rapidity when injected into a vein." "That it acts more swiftly in this way, than in any other" (says he) "is perhaps evidence enough that it enters the blood, before it operates." "But further, its effect, when thus introduced, is too swift, for its action to depend intirely on the blood's being poisoned; for it is impossible that in three seconds, which was certainly the limit of interval, when all voluntary movement, and" (all) "respiration had ceased, the poison could have passed, with the blood from the femoral vein" (the place where it was injected) "to the heart, from the heart to the extreme ramifications of the pulmonary artery, back again to the heart by the pulmonary veins, and lastly by the general arterial system to the" (upper part of the) "spine" (spinal cord rather) and to the medulla oblongata, the place of the origin of the involuntary and instinctive motor nerves of expression, by producing an absolute acinesia by which, it destroys life. Christison adds — "If a correct view of the facts is here taken, there scarcely seems any refuge for the physiologist, except in the novel, and at first view, startling doctrine, (lately advanced by Messrs. Morgan and Addison, in regard to the action of poisons generally,) that Coniine acts by entering the blood, and producing on the inner membrane of the blood-vessels, a peculiar nervous impression, which is instantly conveyed by sympathy along the nerves, to the organ remotely and ultimately affected." (*Christison. Transac. Roy. Soc., Edinb., 1836, Vol. xiii, p. 396.*) Christison, it will be observed, pronounced the subject of the experiment with Coniine dead in three seconds, because all voluntary, and all respiratory motion had ceased in that time. Now as appears to me, it was a great omission with the experimenters not to ascertain, or not to report, whether there was any cardiac and arterial action after this time. If there was such action, the subject was not really and truly dead, and might doubtless have been saved by factitious respiration kept up long enough for the effects of the narcotic to pass-off. I believe that it is not only a general, but a regular and uniform fact, that when life is destroyed by an agent, which suspends the functions of the involuntary and instinctive motor nerves of expression, cardiac and arterial action continues for a considerable time after respiration, and voluntary motion, have ceased. If such is the fact, it is

an important one to be known, in reference to saving the life of the subject of the operation and effects of the narcotic. After all, the final operation of Coniine, if I understand Christison, is admitted to be by the nerves, even when it is injected into a vein; and it must be the nerves of the parietes of the blood-vessels, which receive its first impression and influence. Now, when Coniine is taken into the stomach, I can not discover why the inner parietes of the alimentary canal may not just as well receive its first impression and influence. Even when Coniine is employed by inoculation, I can see no reason why the nerves of the part, into which it is inoculated, may not receive its first impression and influence. With those who suppose that medicines operate by being taken into the blood, I have always found great obscurity of views beyond this fact barely. They seem to be satisfied with merely getting the medicines into the blood. But, when the medicine is in the blood, does it operate upon the blood merely? Although the crassamentum of the blood doubtless possesses a low degree of vitality, yet, it is intirely destitute of all nerves, whether of sensation or motion; and how, without such nerves, it can either receive or propagate medicinal impressions or influences, I can not possibly conceive. I can not discover how, after it has received an absorbed medicine, it can differ materially from any other tepid and rather thick and viscous medicated liquid as respects the propagation of medicinal influence; nor can I discover how it can affect the system at large, except by operating upon the inner parietes of the blood-vessels, after the manner of an embrocation. The inner parietes of the blood-vessels are a living organized texture, receiving both common sensor nerves, and the involuntary and instinctive motor nerve of chimical action, nutrition, etc., and are of course capable of receiving and propagating medicinal impressions, and influences, though I should doubt whether as perfectly and as as completely, as the inner parietes of the stomach and upper and smaller intestines. Does it operate upon the inner parietes of the blood-vessels? For myself, I can discover nothing so very "startling" as Christison calls it, in the doctrine of Messrs. Morgan and Addison, that a medicine once got into the mass of the circulating fluids, should act upon the inner parietes of the blood-vessels. Indeed, I can not discover what other part or texture, it can act upon, at least

with any probability that its impression or influence will thereby be propagated to other parts of the system. I will here observe by the bye, that the notion that a medicine, when injected into the blood-vessels, must act upon their inner parietes did not originate solely and exclusively with Messrs. Morgan and Addison, since I recollect hearing this point frequently talked of, and as if it was even then an old notion, between the spring of 1807 and 1812, the period of my professional pupillage. It was talked of however only in reference to medicines injected into the blood. Is it extravasated or deposited upon the part or organ, in which we perceive the first manifestations of its effect? It always appeared to me that the notion that medicines, in their integrity, are taken into the mass of the circulating fluids, and by the blood-vessels carried and deposited, or extravasated in, or upon the part or organ, in which we perceive the first manifestations of their operation and effects, is by far too extravagant to merit serious consideration and attempt at refutation. I never met with any thing like evidence of such a hypothesis, and I hardly know how to set about disproving it. The last remark, which I have made in regard to the supposed absorption of Protonitrate of Silver,—even if such absorption is admitted,—sufficiently shows that such absorption can not be used as an analogy in favor of the operation of other remedies by absorption, since, according to the hypothesis, that salt is deposited in a part in which there are never any manifestations of its effects, and since, according to the hypothesis, it remains permanently in the system, never being excreted as effete matter.

From the speed of the operation of *Ignatia amara*, *Strychnos Nux-vomica*, and other species analagous in their medicinal powers, when given in sufficiently large doses; and from the fact that all the manifestations of the operation of that power of these articles, which is commonly called narcotic, (but which I have been in the habit of distinguishing from a true narcotic effect, by the denomination of erethistic,) is in the nervous system, I should think there would be as much difficulty, in explaining or accounting for the effects, of these agents, upon the hypothesis of absorption into the mass of the circulating fluids, and actual contact thereby with all parts of the nervous system, but especially with the voluntary motor nerves,

as there is in explaining the operation of Coniine in the same manner.

James Blundell, M. D., made "experiments" (which contribute) "to prove that in poisoning by" (Strychnos) "Nux-vomica, and other poisons probably of a similar" (i. e. of a narcotic? erethistic rather) "operation, the poisonous dose which destroys life, is not contained in the circulating blood of the animal." (*Pettigrew's Medical Portrait Gallery, London. Prt. iv, p. 14. Life of James Blundell M. D.*) "Those experiments were completed before the year 1828." (*Ibidem*) "The poison was inserted by an arrow passed into the muscles of the neck." (*Ibidem.*) "In three experiments, it was found"(by Dr. Blundell,)

1st. That if a dog be poisoned with the minimum dose of Strychnine sufficient for the purpose, its life cannot be saved by drawing off its blood when the poison is getting into full action, and replenishing it with blood from an other and healthy dog." "The vessels in some experiments were three times washed out, as it were, by drawing away the" (supposed) "poisoned blood from the same animal when under the influence of the poison, and as frequently replenishing it, yet without preserving life."

2nd. That if a dog be drained of its blood, and replenished from an other dog under the influence of the poison, the dog which receives it is not poisoned by the blood thrown-in, nor does it in general manifest any obvious signs of the presence of the poison." In one experiment however," (slight) "characteristic spasms were observed in the dog receiving blood from an other dog under the full influence of the Strychnine, though in this case the animal speedily recovered." (*Ibidem.*) Were not these "characteristic spasms" produced by the irritation and disturbance of the act of the transfusion of the blood? If I do not greatly err in my recollection, I have before met with mention, of slight subtonic spasms in connection with the transfusion of blood, when the subject had swallowed nothing.

"From the whole of the experiments it was found" (concluded merely, I suspect) "that when an animal is poisoned by Strychnine, *some* of the poison is contained in the circulating blood, but the quantity therein contained is not sufficient to destroy life." (*Ibidem.*)

From all the facts within my knowledge, my own firm convic-

tion is that these agents do not produce their medicinal effect by means of being taken into the mass of the circulating fluids, and much less by being brought into actual contact with the parts or organs, in which we perceive the primary manifestations of their operation. As appears to me, all important facts in relation to the subject, concur in leading to the conclusion that the influence of these agents is propagated by the nervous system, from the part upon which their first impression is made, whether such part is the cellular substance, arrived at by inoculation; the inner parietes of the blood-vessels, when these articles are injected into the veins; or the inner parietes of the organs of primary digestion, when they are received into the stomach.

Nothing is better known at the present day, than that the effects of the extracts of the barks of *Strychnos toxifera* (Schomburgk); *Rouhamon Curare*? (De Candolle); *Rouhamon Gujanense* (Aublet), operate quite differently when employed by way of inoculation, and when received into the stomach. When introduced into a puncture or incision, almost in the smallest quantity it occasions death in a few minutes; whereas, it may be taken into the stomach in far larger quantities, not only with impunity, but with valuable medicinal effects. [*See. Th. Thom. Chim. Org., Bod. Vegt.* 1838. *Pg.* 286, *Article Alcaloid, Curarina or Curarine.*] The active principle of these articles is well ascertained to be a true and genuine alcaloid called *Curarina* or *Curarine* with all the essential characters of this class of substances, and with individual characters to distinguish it as an unequivocally distinct species, in comparison with every other individual alcaloid. Now according to received opinions an article, whether inserted into a wound or taken into the stomach, operates by being absorbed into the mass of circulating fluids, without any material change in its properties, and certainly without any thing like decomposition. If this is the true *modus operandi medicaminum*, and of this, as well as of others, how is there so much difference in its effects, when employed in the two modes that I have just mentioned? As appears to me, such facts furnish very strong arguments against the notions and opinions that I am combating.

But the operation of the Venom of the Rattle Snake, which I shall soon mention, when received into a punctured wound, and

when taken into the stomach differs more widely, than that of the alkaloid Curarine.

Much has been said about the liability of the sucking child to be affected by narcotics taken by the mother or wet-nurse. Now, it has so happened that I never witnessed a case of this sort. I have given all sorts of narcotics, that I ever employ, with all the freedom that any disease not absolutely incompatible with the regular secretion of the milk has ever seemed to require, without witnessing any effect whatever upon the child. This I have done so often, with all the narcotics, that I am in the habit of using, that I have ceased long ago to watch for, or have any expectation that they would exert any operation upon a sucking child. As to Papaver, I have always given that also to wet-nurses, when ever it has been indicated; and while its force has been expended in meeting and counteracting disease, I have never seen the child affected by it. When the disease was suspended, I have always abstracted this medicine. I have often given Papaver freely to a wet nurse, for the suspension of a paroxysm of Diarrhœa; and so long as no more was administered, than was necessary for the purpose for which it was employed, I have never seen the child affected by it. Perhaps the existence of Diarrhœa in the mother might have occasioned Diarrhœa in the child, independent of the Papaver. If the Diarrhœa of the mother has been obstinate, and has not yielded speedily, the secretion of milk has commonly been suspended. I do not know that I have ever known a mother to retain her milk under any protracted disease requiring the use of any considerable amount of Papaver. I am sure that I never witnessed a case of the use of Papaver in a wet-nurse, the facts of which ever furnished the least ground for suspecting the passage of intire Papaver, or any of its active principles, through the digestive and excretory apparatus of the mother, so as to reach the child. But were a sucking child to be powerfully affected by Papaver taken by its wet-nurse, it would afford no sort of argument that it did not affect the wet-nurse, by means of its impression upon the inner parietes of the stomach and upper and smaller intestines,—extending its influence thence to the whole system through the medium of nerves. Under such circumstances, it would doubtless be found that the wet-nurse was affected by the Papaver before there were

any manifestations of it in the blood, if there should be any manifestations at all of it in this fluid. Again, should a sucking child be affected by *Papaver* taken by its wet-nurse, I doubt not from the analogy of cases that have been investigated, that it would be affected while the milk of the wet-nurse is in the alimentary canal, and before it has had time to be digested, and to pass into the mass of the circulating fluids.

The supposed fact that the sucking infant is some times affected by cathartics taken by the mother or wet-nurse, (even supposing that there is no mistake upon the subject) fails altogether of proving that the mother or wet-nurse, is purged by means or in consequence of the reception of the cathartic into the blood, or even that the infant is purged in the same manner. I have however no recollection of ever witnessing such a case, though I do not feel absolutely certain that I have not. I have not infrequently had occasion to prescribe a cathartic to a wet-nurse, under the operation of which, the sucking child has not been purged; and this has been so much the general rule where I have had opportunity of observation, that I can not recollect a case to the contrary.

It is commonly supposed that the first milk secreted after parturition is always more or less cathartic. I never saw any decided evidence of this, though I have always supposed that it is most probably the fact. If this is the fact, it evinces that the mammary gland is capable of producing a cathartic secretion, and that with readiness and facility. The milk of a wet-nurse, as I have witnessed, some times becomes cathartic when such wet-nurse has taken no cathartic. I recollect one mother whose milk always operated as a cathartic upon her children, so that she could never nurse them. Her health was always bad. I believe that while affected by, or laboring under certain chronic diseases, the milk of a wet-nurse generally occasions *Diarrhœa* in the child; or in other words is cathartic. If I were to witness the purgation of a child in consequence of the wet-nurse's taking a cathartic, I should not be satisfied that any of the cathartic had actually passed into the milk, and reached the child, unless such cathartic could actually be detected in the milk. I think that a saline cathartic would be more likely to pass into the blood of a wet-nurse, and to be excreted by the mammary gland, than a vegetable

cathartic, which must be so much more digestible. Perhaps the reason why I have never seen a sucking infant purged by a cathartic taken by a wet-nurse, may be the fact that I have never (within my own recollection,) prescribed a saline cathartic for such a subject. I have prescribed the Dichlorid, and the Disoxyd of Mercury for nurses, without purging the child; but these articles differ widely from the cathartic salts. I can not discover why the milk of a wet-nurse may not be changed by certain medicines, as well as by certain diseases, so as to produce Diarrhœa, or in other words, to purge, without containing any of the medicine.

I have heard much said about the operation of medicines taken by a wet-nurse, upon the sucking child, but I never had opportunity to witness much, if in fact any thing of this sort. When a wet-nurse and her sucking child have had Intermittent at the same time, I never knew the cure of the wet-nurse to cure the child. After the cure of the wet-nurse I have known her to continue the use of the medicine for a considerable time longer, in the hope of curing the child, but I never witnessed success, even in a single instance. I have known a wet-nurse and a sucking infant to be attacked simultaneously with simple Diarrhœa, and the wet-nurse promptly treated and cured by Papaver alone, while the disease in the infant, instead of being cured, was not even mitigated; and yet when Papaver was administered to the child, the Diarrhœa yielded at once and was as readily cured radically, as in the wet-nurse. As far as I now remember, Intermittent and Diarrhœa are the only diseases of a sucking child, that I ever knew the wet-nurse treated for; but always without success, under my observation. I do not think that the least true evidence in favor of the operation of medicines by means of absorption into the blood, or by means of extravasation and deposition either in or upon the parts or organs in which we perceive the primary manifestations of their effects, can be drawn or deduced from all the well ascertained facts on the records of medicine in regard to the effects of remedial agents taken by a wet-nurse upon a sucking child. Nothing is more common than such a vitiation of the milk of a wet-nurse, as to make a sucking child quite ill, by mere food, which the wet-nurse takes with perfect impunity, so far as she herself is concerned. The illness produced in the child under these circumstances is commonly Tormina or Diarrhœa, or both;

some times how ever it is anorexia or fever. I do not think that there is the least reason to conclude that these ill effects upon the child, produced by improper food of the wet-nurse, are ever occasioned by the passage of food in its intire state through the organs of primary digestion and excretion of the nurse, and also through the organs of primary digestion of the child, and its absorption into the blood, and how much farther I know not. If food does not produce its ill effect in this manner, I know of no reason to conclude that medicine operates in this manner.

Diluted and aromatized Sulphuric Acid taken to any operative extent by a wet-nurse, produces Tormina and Diarrhœa in the sucking child ; but chimical tests afford no evidence of its existence in the milk, and I do not think that it would be detectible in the blood of the wet-nurse, though I never knew any investigations in this line. When ever the milk of a wet-nurse is so much vitiated as to make the sucking child sick, it does not necessarily (if ever) contain the vitiating agent, as it was taken by the nurse. But even if the vitiated milk of a wet-nurse always contained the substance taken by the nurse to vitiate it, such fact would not constitute the least evidence, that the agent in question did not operate upon the nurse by means of an impression or influence first made upon the inner parietes of the organs of primary digestion, thence propagated to the mammary gland through the medium of nerves ; nor if the sucking child actually receives the disagreeing agent in the milk of the nurse, does it prove that the milk itself is not vitiated, otherwise than by the presence of the article, nor that it does not affect the child by means of its impression upon the inner parietes of the alimentary canal, rather than by its passage into the blood-vessels, or still farther into the system, if such a thing is possible.

Facts in relation to the operation of the Venom of the *Caudisona horrida* (*Crotalus horridus*) or Rattle Snake, contribute to throw light upon this subject. When taken into the stomach, the Venom of the *Caudisona horrida* (Rattle Snake) operates as a pure and powerful euphrenic, of a peculiar character indeed ; but, when injected into a punctured wound it first produces an instantaneous and powerful exhausting effect upon the whole nervous system, in all respects very widely different from, and even opposite to the effects of a euphrenic, and second it produces a very

peculiar Erythematic Phlogosis spreading with comparative slowness in every direction, from the wound made by the bite. Finally life is soon destroyed, (as I believe) by suspension of the functions of the involuntary and instinctive motor nerve of chymical action, nutrition, and reproduction.

Now it is the common hypothesis that all these effects, whether produced by taking the Venom into the stomach, or by injecting it into a punctured wound, are caused by means of its absorption into the mass of the circulating fluids, and some subsequent action, or series of actions not specified. If this is so, why are not the effects the same, from both modes of applying or employing the Venom? The real truth appears to be that this agent produces its euphrenic effects only when it acts upon the inner parietes of the alimentary canal; and the other two sets of effects only when it is injected into a punctured wound, where in all probability, its primary impression is made upon the parts with which it is brought into contact, and is thence propagated to the rest of the economy, through the medium of nerves, perhaps of the nerves of the blood-vessels wholly, and perhaps by these only in part. Whether any of the Venom, when it is injected into a punctured wound, is ordinarily absorbed into the mass of the circulating fluid, I know not, though, as appears to me, probability is against it. I do not question however that it may some times be so absorbed; but even if it is, it may be considered as very uncertain how far its effects are modified thereby. The instantaneous exhausting effect upon the whole nervous system, but especially of the nerve of chymical action, nutrition, etc. when it is injected into a punctured wound, is much too speedy to be occasioned by the absorption of the Venom unchanged into the blood-vessels; and the very peculiar Erythema takes place much too slowly, and spreads too equally on all sides of the wound, to be produced in the same manner. Besides, if the Erythema were in fact occasioned by the absorption of the Venom into the blood-vessels, I can not discover why, after a short time, it ought not to take place simultaneously through out the whole system, instead of spreading gradually and in all directions from the bite. If the influence of the Venom of the *Caudisona horrida* or Rattle Snake were in fact propagated only by the blood, its effect, (as seems to me) ought to take place at first only in the direction in which the blood flows,

but very soon simultaneously in all parts, to which any blood is sent. This however is notoriously not the fact.

Copland says "that the very instant" (aneous) "and intense effects produced upon the blood" (rather the system at large) "of the human subject, from the bites of" (venomous) "serpents, can not arise from the diffusion of the poison in the blood, must be evident not only from the rapidity with which they occur, but from the morbid impression made by them upon the vital or ganglial nerves, (the nerves of common sensation are "ganglial nerves," but they are not vital nerves,) and instantly propagated, through out the frame; the effects of this impression first appearing as a manifest lesion in the part where the injury was inflicted, and in the blood, which, as a part of the vascular system, is coördinately affected with the class of nerves supplying both it and the vessels which contain it, with vital influence." (*Cop. Dic. Prac. Med. Bost.* 1834, *Prt. I.*, p. 194-5, *Sec. 142, Blood. Op. of Poisons.*) Observe that Copland says that the effects of this Venom on the blood can not arise from its diffusion in the blood, (in reality there can hardly be said to be enough of it to be diffused) but must be produced in some other way, though exactly how he supposed it is accomplished, is not clear to me, from his language. He says, that the blood, as a part of the vascular system, is coördinately affected with the class of nerves supplying both it, and the vessels which contain it, with vital influence. Now, what is here meant by vital influence? If it means life itself, I do not think it can be maintained that nerves, in any case, supply any part with life. Life is something distinct from nervous influence. Nerves endow parts with the power of sensation, and the power of motion, that is, it is through the instrumentality of nerves, that sensation and motion are produced. All the nerves that are sent to a particular part or organ, may be divided, without producing the death of such part or organ; though by such division of all the nerves, the part or organ may be deprived of all power of sensation and motion.

But though I can not pretend to understand the whole meaning of Copland, in what I have here quoted, yet I think it is clear that he intends to give it as his opinion that in this case, what ever change the blood undergoes is produced by what he calls the "vital or ganglial nerves," under the influence of the poison, and not

by a direct action of the poison upon the blood, in consequence of admixture with it. As I have already said, nerves of common sensation are ganglial nerves, but they are not vital nerves, and therefore I conclude that by the phrase "vital or ganglial nerves," Copland means one single nerve, viz. what is commonly called the "great sympathetic nerve," which Bichat called the ganglionic system of nerves, though why this name is more appropriate to this particular nerve, than to all the nerves of common sensation, he omits to inform us. As appears to me, Copland is correct, in ascribing the effects which are perceived in the blood, under this poison, to the influence of the nerves of the blood-vessels, rather than to any direct operation of the poison on the blood, in consequence of its admixture with it. This is in fact all that I am attempting to show. But, I believe that the influence of nerves of common sensation, in conjunction with what is called the great sympathetic nerve, is necessary to the effect in question. It must be recollected that a few filaments from every pair of nerves of common sensation, except the third pair of nerves (Willis), accompany the great sympathetic, so called, to every part or organ, to which it is sent. My argument here is that the effects of the Venom of the *Caudisona horrida* or Rattle Snake, operates on the living animal system through the medium of the nerves, and in no way through the medium of the blood, whether it is received into a punctured wound, or taken into the alimentary canal; and, it appears to me that unless this is fact, no reasonable explanation can be given of the difference in its effects, when it is applied or employed in these two different ways.

It is well known that the Venom of the *Vipera Berus* or common European Viper produces no ill effects, when taken into the stomach, in any thing like reasonable doses; but when injected into a punctured wound, its operation is very analogous to that of the *Caudisona horrida* or Rattle Snake. This was well understood by the ancients, since Celsus says of the *Venena* of poisonous serpents,

"Non gustu," (sed) "vulnere nocent;"

and Luncan says

"Morsu virus habent, et fatum minantur,
Pocula morte carent."

The following statement was made to show that the healthy

skin is not an absorbing texture, which is doubtless true, but it may be used for an other purpose. Dr. John Warren, of Boston, in his work on Mercurial Practice, (p. 21-22) states the supposed fact that the vapors of Musk, Camphor, Turpentine and Garlic, being respired, the urine becomes impregnated with their odor; but immersing the arm in them, or anointing the body with them, without admitting the vapors into the lungs, is not followed by any such effect. I doubt whether the odorous principle of Musk is its medicinally active principle, and whether the intire system of the human subject could be appreciably, or decidedly and unequivocally affected by any amount of the odorous principle of Musk, to which such a subject can be exposed. Not but that such an amount of the odorous principle of Musk may be inhaled as to become oppressive to many subjects, but this is quite a different thing from its regular euphrenic effects. I can not perceive therefore, how the fact that the odorous principle of Musk is received into the system through the pulmonary apparatus, (if such is really and truly the fact,) can be made to throw any light upon the question whether medicines produce their medicinal effects, by means, and in consequence of being taken into the blood-vessels, or even by being carried beyond them.

I think however that such is not the fact with Camphor. I doubt not that a person may be exposed to a sufficient amount of the vapor of Camphor to cause his system to be powerfully affected by this agent. By exposure to enough vapor of Camphor, I think there is no room to question that even its characteristic clonic spasms might be produced. Camphor has no odorous principle independent of its own proper and peculiar substance. The odorous principle of Camphor is Camphor itself in a state of vapor. Camphor is the Protoxyd of a compound radical consisting of H^8 . C^{10} . in strict chymical combination, and constituting a liquid of definite bulk, whose name in chimistery is Camphogen, but, in older and more common language, its name is Oil of Turpentine, a substance well known to almost every body. The very same compound radical occupying a very different bulk, is a liquid of a very different character, whose name in chimistery is Limonogen, but, in older and more common language, its name is Oil of Lemon. Now the Camphogen has an intirely different odor from Camphor, as has Oxygen, Carbon, and Hydrogen; so that Cam-

phor is its own odorous principle. When a person is exposed to so much of the vapor of Camphor as to affect his system powerfully, and even to produce the clonic spasms, there are no greater manifestations of it in the urine, than when he was exposed to such a moderate quantity of the vapor, as not to produce any appreciable operative effect upon the system. This fact shows conclusively that the operative effects of the Camphor do not depend upon the small quantity actually received into the system, but rather upon the much larger quantity that has made its first impression upon the bronchial membrane, and had its influence propagated from thence by the nerves, to the whole animal economy.

As to Turpentine, that is, any terebinthinate Oleiresin, it is only an odorous principle, that is perceivable in the urine, after exposure to its vapor, and in small quantity. Even this odorous principle appears to be considerably modified when in the urine, in comparison with the odorous principle, before it is received into the system. But, as in most other cases there is only a certain moderate amount of this odorous principle in the urine, when a very large quantity of the Oleiresin has been taken, not more than when only a small quantity has been taken. The odor in the urine is never proportioned to the quantity taken, nor are the medicinal operative effects at all proportioned to the amount of the odor in the urine, but always to the amount taken. If this is true, when an Oleiresin is received into the stomach, it must be so, when its vapor is inhaled.

As to Garlic, this can never be tasted, touched or handled, any more than it can be smelled, without leaving its scent about the person. I know nothing how long its odorous principle must be inhaled, in order to be manifested in the urine; nor do I know that its odorous principle is at all medicinal. I can not discover therefore how any useful inferences can be deduced from the facts in relation to this article. In fact, I entertain doubts whether this article is really and truly medicinal in any material degree. Perhaps the juice of the recent bulb may possibly possess the power, which I am in the habit of calling oresthetic.

Thus far, I have not questioned the supposed fact that the odorous principles of Musk, Camphor, Turpentine and Garlic, gain admittance to the urine, in consequence of being absorbed by the

bronchial membrane either unchanged or more or less changed. It remains to be added that I have long been satisfied that the bronchial membrane is no more an absorbing texture than the skin. The lungs perform two intirely different and distinct functions, having no sort of necessary, but only an accidental connexion with each other.

1st. The bronchial membrane is one of the emunctories of the system, viz. that for the effete Carbon, which can be excreted only in the form of a gas, and therefore must have an apparatus fitted for this especial purpose. The actions necessary for this excretion are dependent upon the pulmonary branch of the great sympathetic nerve, commonly so called, which is the involuntary and instinctive motor nerve of chiminal action, nutrition and reproduction. Now as an excretory for a part of the effete matter of the system, we should not expect that at the same time, it could be an organ of supply or nutriment; nor is it in fact such, since what is supposed by some to be capable of absorption by the bronchial membrane, is too trifling in amount and quality, to be of any value either for nutriment or medicine, or any other purpose.

2nd. The lungs are an organ of expression, or more definitely, they are the bellows of that exquisite wind instrument the larynx. As an organ of expression, all the motions of the lungs proper depend upon the pulmonary par vagum the second division of the eighth pair of nerves (Willis) and all the auxiliary actions of the intercostal muscles, diaphragm, etc. depend upon other nerves belonging to the same system, viz. the involuntary and instinctive motor nerves of expression. As an organ of expression, that is, a bellows to the wind instrument called the larynx, the lungs are certainly not an organ of supply, or nutriment to the system, nor does any process of nutrition ever depend upon nerves of expression.

These two functions of the lungs have no necessary connexion with each other. Their conjunction is an example of that economy of organs which is not infrequently found in living organized bodies. There are many of the inferior animals in which the decarbonization or arterialization of the blood is performed by organs intirely distinct from any organs of expression. I doubt not that the odors of Musk, Camphor, Turpentine and Garlic,

when they are perceived in the urine, in consequence of being inhaled, always find their way into the system by passing from the fauces with the saliva into the stomach, and subsequently taking the regular and natural course. When articles, whose odorous principles get into the system in this peculiar manner, produce any medicinal operative effects, it is doubtless by means mainly of a first impression upon the bronchial membrane, which is propagated to other parts of the system through the nerves.

Good says "there is an other process, which has been lately adopted in France. It consists in a revival of the impregnated aqueous injections of Stephen Hales, with a view of determining how far such impregnating materials may reach the lungs, and be thrown-off, by the bronchial exhalents." (*Good's Stud. Med. Bost.* 1826, 4th Amer. Edit., Vol. 1, p. 602, *Article Asthma, Treatment.*) Into what part are these injections thrown? Good immediately says, "Messrs. Magendie and Nysten have ascertained that"(common)"Æther, Camphor, Alcohol, together with the gasses are conveyed to the lungs, and transpire from the surface of their air-cells." (*Ibidem.*) Admitting that a very little Protoxyd of Etherogen or common Æther,—a very little Protoxyd of Camphogen or Common Camphor,—a very little of the Unihydrite of Protoxyd of Etherogen or Alcohol,—is exhaled from the lungs, after each of these articles has been taken into the stomach, what then? Do not these articles produce their medicinal operative effects, (of course when only a single dose is employed) before they appear in the halitus of the lungs? Is there any more of them in the halitus of the lungs, when a large dose has been swallowed, than when only a moderate one has been taken? Is the medicinal operative effect at all proportioned to the quantity in the halitus of the lungs; but is it not rather proportioned to the dose taken? I consider it certain that not one of these agents produces its medicinal operative effects by means, and in consequence of being received into the blood. Their medicinal operative effects are always proportioned cæteris paribus to the quantity of them taken into the alimentary canal; whereas the quantity exhaled from the lungs has no regular relation to the quantity swallowed.

An odorous and sapid principle is found in the milk of Mammals that eat Meum Athamanticum, some times called Spignel,

and some times Cicely. If Cows eat this plant, not only the milk, but also the butter and cheese made from it, have a strong flavor, like that of the Swiss Schabzieger-käse, which seems to be the flavor of the principle improperly called Coumarine. In the Schabzieger-käse, this flavor has been determined to result from Coumarine, which is derived from *Melilotus cœrulea*. This principle is contained also in *Melilotus officinalis*,—and *Dipteryx odorata*,—and probably in *Trigonella Fœnum-Græcum*,—and *Meum Athamanticum*. Now Coumarine and *Meum Athamanticum*, produce euphrenic effects; but these effects take place before there are any manifestations of Coumarine in the system beyond the alimentary canal. Of course they are produced by an impression or influence upon the inner parietes of the stomach and upper and smaller intestines.

A coloring principle from the root of *Rheum (officinale)*, whether exactly as it exists in the root, or modified more or less, is some times found in the urine; but this is not the active principle of *Rheum*; and if it were, this article produces its cathartic effects before there is any manifestation of this coloring matter within the system. A coloring principle from the root of *Rubia tinctorum* or Madder is absorbed into the system, and if the article is taken long enough, is ultimately deposited in the bones. Whether this coloring principle, as it is found in the bones, is identical with any coloring principle that previously existed in the root of the *Rubia*, has never been ascertained. It may be some thing wholly formed within the living system of the animal taking the *Rubia*, or it may be a modification of something that previously existed in the root. But for the argument's sake, we will admit that this coloring principle did previously exist in the root, just as it is found in the bones. Is there any reason how ever to suppose that this coloring principle is the active principle of the *Rubia*? Even if it is, are not all the medicinal effects produced before there is any manifestation of color in the bones? How long this coloring principle exists in the blood, before it appears in the bones, I know not, nor is it at all material if it is not the active principle. If it is the active principle, it remains to be shown that all the medicinal effects of *Rubia* are not produced before there is any appearance of this coloring matter, except in the alimentary canal.

The Oleïresin of *Copaïfera*, if taken continuously, produces what is called a Violet smell in the urine. This smell, as appears to me, is unlike the smell of the Oleïresin, which affords a presumption that it is due to something formed in the system while under the influence of the Oleïresin. But waiving this point intirely, the smell is never at all proportioned to the amount of the Oleïresin taken, whereas the medicinal effects are apparently always so proportioned. The Oleïresin of *Copaïfera* when taken continuously, gives a peculiar odor to the breath; but this odor is not proportioned to the amount taken, whereas the medicinal operative effects seem to be so. These facts afford a strong presumption that this Oleïresin acts by means of its impression upon the inner parietes of the alimentary canal.

There is no sort of connexion between that odor of the urine which *Asparagus officinalis* occasions, and its diuretic operation. Where I have known the greatest amount of the odor of this article in the urine, there has been no diuresis at all; and where there has been the greatest degree of diuresis, that I have ever known it produce, there has been no more odor in the urine than when it produced no diuresis at all. I do not think therefore, that this article produces any of its medicinal operative effects by means of any principle, or principles received into the blood-vessels, and much less any that appear in any of the excretions.

Doct. Eli Todd, the founder and physician of the Connecticut Retreat for the Insane, informed me that during the prevalence of Typhus syncopalis, in the County of Hartford, Ct. in 1809, when ever epispastics of *Sinapis* were employed (as they were very frequently in that epidemic) the odor of the *Sinapis* was perceived very strongly in the breath of the patient, much more strongly than is customary under such a use of this agent. I have myself witnessed the same phenomenon, in certain epidemics of Typhus nervosus, in some much more than in others, even though the *Sinapis* was used in the same manner and to the same extent, as near as possible. Now the odorous principle of *Sinapis* is not its active principle; and if it were, all the effects of this agent were produced long before there were any manifestations of the odorous principle in the halitus from the lungs.

Two individual articles, each with a very prominent peculiarity in comparison with every other article of the *materia medica*

within my knowledge, may perhaps be adduced as opposing very strongly the conclusion to which I have myself arrived. My argument is that only a small proportion of the articles of the *materia medica*, or of their active proximate principles, are ever absorbed intire, and without digestion and decomposition, into the mass of the circulating fluids; and that those, which are so absorbed, produce all their medicinal operative effects before such absorption has taken place, and by their impression on the inner parietes of the alimentary canal; and further, that when an article can be injected into the blood-vessels with impunity, it makes its first impression upon the inner parietes of the blood-vessels, and produces its operative medicinal effects upon other parts of the system, through the intervention of nerves, and not by being carried and extravasated or deposited in any manner upon the parts or organs, in which we perceive the manifestations of its operation. But I consider the inner parietes of the blood-vessels as by no means so well fitted and adapted to receive and propagate the first impressions of medicines, as the inner parietes of the stomach and upper and smaller intestines. These conclusions, (as I claim) are founded upon observation and experience, in regard to a very considerable number of articles, and are inferred from analogy, to be true of many others in regard to which there is a deficiency of observation and experience.

But *Amanita Muscaria* furnishes a case without a parallel in all the rest of the *materia medica*, viz. of an article, under the fullest influence of which, the most complex and the most crude excretion from the system, seems to possess the same powers, and to be capable of producing the same operative effects, as the agent under which the excretion was made, with the remarkable circumstance (if there is no mistake upon the subject) that the power of the original medicine is greatly multiplied or augmented and increased in the excretion. The facts in relation to the operation of *Amanita Muscaria* a Fungus which possesses what I have been in the habit of calling a euphrenic power in an intense degree, I have often heard mentioned as proving triumphantly the doctrine of the operation of medicines by means and in consequence of absorption into the mass of the circulating fluids. *Amanita Muscaria* is commonly reckoned a mere narcotic, and nothing else; and as a narcotic, it is reckoned very similar to Al-

cohol. Its fullest effects are commonly called intoxication, one of the most vague terms in the language estimating it by its customary applications. According to my judgment, its effects are far more like the Protoxyd of Nitrogen; and on the whole, I do not view it as narcotic at all. Neither does it possess either of the powers that I am in the habit of calling oresthetic and antisbesitic, both of which are possessed in a prominent degree by Alcohol, the latter indeed to a greater extent by Alcohol, than by any other agent in the whole materia medica.

Langsdorf, as translated by Greville, says—"the most singular effect of Amanita"(Muscaria)"is the influence it possesses over the urine." "It is said that from time immemorial it has been known that this Fungus imparts an intoxicating quality to this secretion which continues for a considerable time after taking it." "For instance, a man moderately intoxicated to-day, will by" (tomorrow) "morning have slept himself sober; but as is the custom, by taking a tea cup of his" (own) "urine, he will be more powerfully intoxicated than he was the preceding day." "It is therefore not uncommon for confirmed drunkards" (on this article) "to preserve their urine, as a precious liquor against a scarcity of the Fungus." "This intoxicating property of the urine is capable of being propagated, for every one, who partakes of it, has his urine similarly affected." "Thus, with a very few Amanitæ, a party may keep up their debauch for a week." "Dr. Langsdorf mentions that, by means of the second person's taking the urine of the first, the third that of the second, and so on, the intoxication may be propagated through five individuals."—(*Grev. Transl. Langsd. Sec. Lindl. Introd. Nat. Syst. Bot. N. Y. 1831, p. 232 and 233. Ord. Nat. Fung.*) (*Lindl. Veget. Kingd., Lond. 1846, p. 38. Alliance Fungales, with a little alteration.*) It is said that the urine of a person who has taken but just enough for his own intoxication, will intoxicate several persons to an equal extent; and that the urine of each of these persons thus secondarily intoxicated, will intoxicate several others, and that it will continue to operate in this manner, through about five sets of persons. I take all this, of course on testimony; but it seems to rest on good and satisfactory evidence. It is a very curious subject, and well worthy of investigation, that here there would seem to be a multiplication of power, for which it is impossible to ac-

count upon the hypothesis that only the active principle of the *Amanita* taken, passes into the urine. Is not this multiplication of the active principle, or the power of this article, more or less analogous to the multiplication of the virus of *Variola*, in a case of that disease? The particle inserted under the cuticle in inoculation, must be multiplied more than a million fold, in many cases. I do not pretend that these cases are by any means exactly parallel; but may not the multiplication of the virus of *Variola* throw some light upon the multiplication of the active principle, or the power of *Amanita Muscaria*? *Amanita Muscaria* has never been satisfactorily analyzed, and therefore its active principle has never been detected; but whatever it may be, I believe it may be considered as being more or less soluble in water, since this agent proves active in the pharmaceutic form of infusion. None of the *Fungi*, so far as my knowledge extends, have been ascertained to contain any principle soluble in water, that is not easy of decomposition, or that is known to be difficult of digestion, or indeed any principle, that is such, even though it may not be soluble in water. It is then difficult to understand how any thing contained in an infusion of *Amanita Muscaria* can escape digestion, and consequently decomposition, in its passage through the stomach, the upper and smaller intestines, the lacteals and mesenteric glands, the receptaculum chyli and thoracic duct, the blood-vessels, and finally the renal glands. Is it not possible that the intoxicating power (so called) of the urine, may be due to some thing formed by the specific secretory action of the renal glands, while the system is under the powerful influence of this agent, rather than to any thing, which was originally contained in this *Fungus*, which has resisted primary digestion, has been carried into and mingled with the whole mass of circulating fluid, and finally been separated in its intire state by the renal glands? It would be exceedingly interesting to receive some light upon this subject of the multiplication of the power of *Amanita Muscaria*, if there is no mistake about the matter. For myself, I have never yet met even with an attempt at an explanation. But this is to wander from my proper subject. It is particularly worthy of remark in this place, that a person who takes *Amanita Muscaria* is intoxicated (to use common language) by it, before the urine acquires any intoxicating powers; and that the intoxication pass-

es-off intirely, while the urine possesses the highest degree of intoxicating power. If this fact does not prove that *Amanita Muscaria* produces all its operative effects while it remains in the alimentary canal, and before any portion of it is received into the mass of the circulating fluid (if such is ever the fact) it proves at least that the urinary bladder may contain that which is intensely active, without any operation upon the system at large; and of course that the urinary bladder is an ineligible part or organ for the reception of the first impression of medicines, and the propagation of their influence to the rest of the economy. But as appears to me, it contributes some what to the probability that *Amanita Muscaria* produces all its operative effects while it exists in the alimentary canal, and by action upon its inner parietes. If its active principle were ascertained, and we possessed some good test for it, there would seem to be no difficulty in determining whether there were any indications of it in the blood before it produces any of its operative effects; but in the present state of our knowledge, I know not how to arrive at any certainty. I should not be at all surprised if nothing of the *Amanita* should ever be detected in the blood either before or after the ordinary operative effects of it. I can not however divest myself of the opinion that, bye and bye, when its active principle is well known, and when we possess good tests of its presence, it will be found that the urine secreted under its influence, does not really contain it, and that such urine will be found to contain some thing actually formed in and by the kidneys (under the influence indeed of *Amanita*) which is different and distinct chimically, but which produces analogous, or even similar effects,—all of which, I am inclined to expect, for reasons resulting from the laws of primary and ultimate digestion, absorption, etc.—reasons which I have specified more particularly under *Spermœdia Clavus*, the next article, whose operation I am to consider.

I was once called in consultation to prescribe for a case, in which so large a quantity of *Spermœdia Clavus* had been administered during parturition, as to produce general convulsions of the sub-tonic sort; and after this, so much *Papaver*, as to produce a degree of narcosis quite alarming to the bystanders. For thirty-six, and I believe nearly forty-eight hours afterwards, the breath of this patient had the odor of *Spermœdia* in a very powerful degree,

though all its operative effects were produced before this odor was perceptible. But is it not certain that the odorous principle of *Spermœdia* is not the medicinally active principle of this agent? It may perhaps be said that if the odorous principle of *Spermœdia* is received into the blood-vessels and exhaled from the lungs, it may be presumed that principles destitute of odor are also received into the blood-vessels, and among these, the medicinally active principle, what ever this may be. Before any safe conclusions can be deduced, it ought to be proved in some way or other, or at least rendered in some degree probable that such is the fact. A mere presumption is not a safe foundation for an argument. The following will perhaps be considered as sufficient evidence in relation to this point.

An unequivocal and decided narcotic effect is often produced, and that always very speedily, upon the fœtus, by the mother's taking a sufficient quantity of the *Spermœdia Clavus*, during parturition. As far as my knowledge extends, there is not an other known fact analogous to this, in relation to any other article of the *materia medica*. Now, it is commonly said that, as there is no nervous communication between the mother and the fœtus (which, for the present, need not be controverted) this effect must necessarily be produced through the medium of the blood. It is absolutely certain however, that the blood of the mother, and of the fœtus in utero are not common stock, and do not circulate continuously through the blood-vessels of both, since the vessels of the mother can be drained, without draining those of the fœtus, and vice versa, those of the fœtus can be drained without draining those of the mother, as is abundantly proved, in cases of fatal hemorrhage from the mother, and in cases of mutilations of the fœtus which are some times resorted to, in difficult parturition. I do not forget that it was once supposed that there were large blood-vessels passing from the uterus to the placenta, or rather communicating from one to the other; but I believe that this opinion was long since disproved, and is now no longer entertained, by any body. I believe it is sufficiently certain that the uterus and the placenta communicate only by numerous small vessels.

It is however considered by some to be exceedingly doubtful whether the fœtus ever derives its nutriment from the blood of the mother, which is sent to the placenta, and whether the placenta

performs any other function for the fœtus, than that which is performed by lungs, viz. the decarbonization or arterialization of the blood. I dare say it is well known to most physicians of the present day, that it has been maintained, and that, with more than plausibility merely, that the fœtus (like the chick in the egg) is nourished by the contents of the ovum. But I would not be understood as at all insisting upon this, or the contrary, since, as appears to me, we are destitute of positive proofs upon this subject, and therefore I will reason from the more commonly received opinions, in relation to this matter.

If the fœtus then, does in fact derive its nutriment from the blood of the mother, it may be considered certain that the blood of the mother, in its intire state, is not received by the fœtus ; and I think it may also be considered certain, that not even the proximate principles of the blood of the mother are received, in their intire state, by the fœtus, but only the elements, or very simple compound principles, out of which, blood suitable for the fœtus, may be elaborated. This elaboration, as we should suppose, must be performed in part by the uterine vessels of the mother, by a process analogous to secretion, and in part by the placenta, by a process analogous to the elective absorption of the mesenteric lacteals. At all events, I believe that this is the most commonly received doctrine, with those who believe that the fœtus is nourished by means of the blood of the mother ; and at all events, I believe it is certain that the vessels of the uterus possess true and proper secreting power, as is sufficiently manifested by the catamenial discharge ; and I am not apprized that those, who suppose that the fœtus derives its nutriment from the blood of the mother, can possibly find a peg, on which to hang a doubt, in reference to the absorbing powers of the placenta, such powers actually as belong to other absorbents.

Now is it at all probable that the *Spermœdia Clavus*, or any of its principles, can pass through the whole apparatus of primary digestion in the mother, through the secretory apparatus of the uterus, and through the absorbing apparatus of the placenta, without undergoing so much change as to destroy its medicinal powers ? Is it probable that any proximate principle of the *Spermœdia* can do so, without undergoing decomposition ? The active principle of the *Spermœdia* is certainly liable to spontaneous de-

composition ; and the apparatus through which it is supposed to pass in this case, certainly possesses a very considerable degree of decomposing power, so great indeed that no other agent is known to escape intire. But to account for the narcotic operation of *Spermœdia* upon the foetus when it is taken by the mother during parturition, by its passage through the blood, it would be necessary that its active principle should be conveyed unchanged through all this vital and acting apparatus, both of the mother and of the foetus. Assuredly the foetus is much too speedily affected by *Spermœdia* taken by the mother, during parturition, to allow of its being accomplished in this manner. Assuredly the foetus is affected under circumstances in which we can not suppose it to be actually receiving nutriment from the mother. If so much must be conceded to a mere hypothesis, assuredly any thing may be established in such a manner.

But it may be said that if the explanation of the operation through the medium of the blood should not be admitted none can be given. Granting that this is the fact, it affords no evidence of the truth of the explanation. An argumentum ad ignorantiam must be admitted to prove nothing. As appears to me, the operation of the *Spermœdia* under consideration, instead of proving its absorption, in fact contributes, as far as it goes, to prove that medicines do not produce their remedial effects by means, and as a consequence of their being taken into the mass of the circulating fluids, but really makes weight in the opposite scale, so utterly inadequate is this hypothesis to furnish any thing like a satisfactory explanation of the phenomenon under consideration. But what ever explanation may yet be given of the manner in which *Spermœdia* affects the foetus in utero, I can not discover how it can possibly vary our reply to the question whether this agent exerts its operation upon the mother, before it is received into the mass of the circulating fluids (if it is ever so received) and consequently by means of its impression upon the inner parietes of the stomach and upper and smaller intestines.

From the speed with which it affects the mother, I should be inclined to consider it as very nearly certain, that it must exert its operation upon her, before it can possibly have had time to get into the mass of the circulating fluids ; but this, like the case of *Amanita Muscaria*, is without a parallel in the *materia medica*,

and therefore can not be as easily settled as an ordinary and simple case. The fact that, in the case of *Spermœdia*, the foetus in utero is ultimately narcotized by it; and that, in the case of *Amanita Muscaria*, the urine becomes endowed ultimately with the power of the Fungus taken (only many times multiplied in amount) in my opinion, considerably complicates these cases. For the determination of the point in question, I suppose, as in the case of *Amanita Muscaria*, that nothing short of an analysis, the determination of the active proximate principle, the discovery of some good test for it, and the application of such test to the blood of a person that has taken *Spermœdia*, and that just before the beginning of the operative effects of the article, can finally decide the question under consideration. The mere fact of the supposed absorption, or the actually ascertained absorption of an article, or of one or more of its proximate principles, by no means indicates that it does not produce all its medicinal operative effects before such absorption, and by means of its impression upon the inner parietes of the stomach and upper and smaller intestines, as I have so often inculcated. At present, I can not but be persuaded that the effects of *Spermœdia* upon the foetus in utero, will yet be ascertained to be produced in some other manner, than by the passage of its active principle unchanged through the stomach and upper intestines, the lacteals and mesenteric glands, the receptaculum chyli and thoracic duct, the mass of the circulating fluids, the uterine vessels and maternal placenta, the fetal placenta, and the mass of the fetal circulating fluid, either to make its impression upon the inner parietes of the blood-vessels, or to be brought into contact with the whole nervous system of the foetus, in which we perceive the manifestations of its operation.

Such a passage of a mere vegetable proximate principle through so many organs of such great power for the decomposition and recomposition into a new form of all ordinary vegetable matters; organs whose regular function it is to digest and prepare for ultimate assimilation every thing which is brought within their influence; or if they are unable to accomplish this, whose function it is to reject and prevent reception into the mass of the circulating fluid. Is there any true continuity of vital parts between the mother and the foetus? Can the growth of the foetus be explained upon the ground of the nutriment contained in the ovum, or

of mere contact of the vital parts of the mother and fœtus, without any true continuity? I must confess, I am inclined to think that there is an actual continuity of vital parts, between the mother and the fœtus, for the time being; but I am not absolutely sure. I consider it certain that the common dogma that there are no nerves of any sort in any part of the umbilical cord and placenta, is not true. Wherever there is both vitality and active motion (not mere passive motion) there must be nerves, and I think there is no possibility of doubting that the placenta and umbilical cord have both active motion, and vitality. It is well known that the involuntary and instinctive motor nerve of chymical action, nutrition, etc. commonly called the great sympathetic nerve, is distributed to all the blood-vessels, and it must be distributed to the blood-vessels of the umbilical cord, and the placenta, as well as to all other blood-vessels. As the umbilical cord and the placenta are organs of the fœtus, they must derive their nerves from the fœtus. I have often heard it said that there is only contact of the vital parts of the mother and of the fœtus; but such contact could not be maintained without actual adhesion; and if there is actual adhesion of the vital parts of the mother and of the fœtus, they must be situated to a greater or less extent, in regard to each other, as in cases of two different textures, that are in adhesion, and must be subject in a greater or less degree, to the laws of what I call contiguous sympathy. By contiguous sympathy, it is true that impressions, actions and sensations are conveyed only to a very limited extent, and in a very imperfect degree; and in this case the extent and degree must be still more limited, and still more imperfect, than in any other; but still, as appears to me, this explanation of the facts under consideration is attended with rather less difficulty (and not much neither) than the explanation by means of an operation through the medium of the blood; which latter, so far as I can discover, amounts to an absolute impossibility, while the former, as I think, falls within the bounds of possibility, though perhaps not within the bounds of probability.

To those not familiar with the laws of the propagation of motion or action and sensation through the nervous system, the question may occur, can the cerebrum, for example, be affected in the manner in which it is by narcotics, through the medium of the nerves of the blood-vessels? For myself, I have at present no

sort of doubt that this is the manner in which it is always affected, in all cases whatever, by narcotics, whether they are taken into the stomach or applied elsewhere. The manifestations of the operation of the narcotics upon the hemispheres of the cerebrum, undoubtedly (as I think) have their ultimate seat in the nerves of the blood-vessels of this organ; and as these manifestations consist both in sensation and motions or actions, they must have their seat both in the nerves of common sensation and the nerve of sympathetic motion of the nutritive system, these being the only nerves with which the blood-vessels are furnished, and this being the only nervous connexion between the alimentary canal and the cerebrum. As the cerebrum itself is insusceptible either of common sensation, or of motion, from the immediate and direct contact of a narcotic with its proper substance or parenchyma, it would be useless to suppose actual contact of the narcotic, from its being conveyed in substance in the blood-vessels, and deposited by extravasation or otherwise, within this organ. It is well known that the involuntary and instinctive motor nerve of chymical action, nutrition, etc. is distributed much more profusely to the blood-vessels of the encephalus, than to the blood-vessels of any other part, this nerve (with its accompanying nerves of common sensation) actually forming a net-work around the primary carotid from its origin to its bifurcation and then continuing to do the same along the internal carotid, until after it enters the cranium. It is also equally well known that this same nerve, commonly called the great sympathetic nerve (as I have hinted above) has in association with it a few filaments of every pair of common sensor nerves in the system, except those making a part of the third pair (*Willis*).

As appears to me, it has been pretty nearly if not quite shown, that the *Spermædia* can not operate upon the fœtus through the medium of the blood, when it is taken by the mother during parturition. Whether it can possibly operate through the medium of the nerves of the blood-vessels, in the manner just referred to, I can not possibly determine. All I can say is, that it is a point intirely unexplained, unless this should be admitted as an explanation; but that this does not contribute at all to prove the correctness of this proposed explanation, and I desire it to be expressly understood that I attach no importance to this proposed explana-

tion. The subject therefore must be considered as perfectly open to inquiry.

In this place, I beg leave to be allowed to repeat that the narcotic effects produced by the *Spermœdia* upon the fœtus in utero when taken by the mother during parturition, constitutes a single, solitary and insulated case, a case to which there is no known parallel in the whole *materia medica*, since no other narcotic agent whatever, is known to be capable of affecting the fœtus in utero, when taken by a gravid or parturient woman; and therefore it can not be safely assumed as proof of any supposed general principle, which is not supported by other adequate evidence. I have repeatedly had occasion to give very large quantities of *Papaver* to gravid and to parturient women, and in the former case, to continue such quantities, even for weeks, and yet I never knew the fœtus affected in any way by it. I can say the same of *Datura*, *Conium* and one or two other articles. None of the articles which are supposed to operate upon infants at the breast, through the medium of the milk, are known ever to be capable of affecting the fœtus in utero. The importance of that provision of nature, which exempts the fœtus in utero from the operation of remedies taken by the mother, will be sufficiently obvious, when it is considered how exceedingly inconvenient it would be for the fœtus in utero to be purged, and vomited, as often as many practitioners of medicine deem it necessary to purge and vomit gravid women; and if the fœtus in utero were to be depleted when ever a practitioner of medicine should deem it necessary to abstract blood from the mother, and should be depleted at the same rate, I fear that the human race would terminate in "hidden untimely births," and in "infants which never saw the light" and in "the untimely births of women, that may not see the sun."

There are a few plants not known to be medicinal, which are sometimes eaten by kine, that often, perhaps always give a peculiar flavor to the milk, cream, butter and cheese, of these animals, but these cases afford no argument either for or against the views that I am considering because they are not medicinal. *Erigeron* (*Cænотus*) *Canadensis*, (Lin. and Nutt.) an *Asteraceous* and *Erigeronteous* plant, with a rank odor and taste, is eaten by kine, when it is young and tender, and often imparts its flavor to milk, cream, butter and cheese. This flavor seems to re-

side in an essential oil, which I have often had in its separate state. It is evidently more or less modified, as it exists in and gives flavor to milk, etc. Although this essential oil is reputed to be medicinal, yet its operative effects are so obscure, and perhaps even doubtful, in the very small doses and quantities in which I have seen it exhibited, that it must be exceedingly difficult to determine whether it produces its medicinal operative effects before there are any manifestations of it in the system, and out of the alimentary canal, or not. *Spartina glabra* (Muhlenberg,) a coarse Gramineous plant, commonly growing in muddy brackish or salt marshes, and described as having a strong and rancid smell and taste, is often eaten by kine, when it is young and tender, and very frequently occasions the same taste and smell in the milk, cream, butter and cheese. This plant however is not medicinal.

If I were to consider every article known, any portion of which, in its intire state, or any proximate principle of which, unmodified or modified, is ever found any where within the system, and not in the alimentary canal, we should have no facts, that have not already been fairly exhibited by the articles which I have considered. I believe that there are no cases that exhibit greater difficulties in the way of, or greater objections to my view of the state of facts in relation to this subject, than those that I have adduced. I do not think that any additional cases can possibly be brought forward, which are better calculated to support a different view than the cases that I have exhibited.

The number of instances in which there is good reason to suppose, i. e. in which there is something like proof, that medicines are taken into the circulation, is extremely small, in comparison with those where there is no proof of such reception, altogether too small to afford sufficient reason to believe that this is a mode in which medicines produce their remedial operations. It appears to me to be a well proved fact, that as a general rule, in the few instances in which medicines are actually taken into the circulation, their medicinal effects do in reality take place previous to any manifestations of the article in the circulation, and these are not enhanced, and sometimes not even continued, after their absorption takes place. In a few instances in which medicines are taken into the circulation, the quantity so received is very limited and

exceedingly small, even though ever so much is swallowed. Now the operative effects produced on the system are always proportioned to the quantity taken into the alimentary canal, and not to the quantity taken into the circulation. It is very often the fact that there is less of a given medicine taken into the circulation, when a large quantity is taken into the stomach, and vice versa. As appears to me, the real question in fact is, whether those medicines which are taken into the stomach, affect the system by virtue merely of the impression which they make upon the lining membrane of the alimentary canal, or whether they are received into the mass of the blood, and affect the system by virtue of the impression which they make upon the lining membrane or inner parietes of the blood vessels, or whether they are actually excreted from the blood vessels upon, and thus brought into contact with the very parts on which they ultimately operate.

The hypothesis of the necessity of the absorption and reception of unchanged medicinal substances into the blood-vessels, and of their mixture with the blood, was first made, when diseases were supposed to be exclusively seated in the blood, and to consist intirely in some change in the qualities of the fluid. When it began to be understood that many diseases at least, were not seated at all in the blood, but in the living and acting solids, the old hypothesis was modified accordingly, and it was maintained that the medicinal substance must, in such a case, some how come into contact with, or be deposited (I suppose, particle by particle) upon those living and acting solid parts, in which the disease is seated. But of the last opinion at least, there is a total deficiency of all proof; and in regard to the former there is an equal deficiency of proof that they ever produce any medicinal effects in this way. Assuredly we are not more bound to receive any thing in medicine without proof, than in philosophy generally, and as appears to me, it is extremely unphilosophical to do so. So far as respects myself I feel quite satisfied that there is not a single article of the materia medica now known, which produces its remedial effects by actual contact with diseased parts, conveyed through the medium of the circulation, or in other words, by being taken into the mass of the circulating fluids, and carried to the diseased parts, or the parts in which its operations are manifested. Where medicinal effects are produced by the injection of remedial agents

into the blood, it appears to me, that the direct and immediate impression of the agent is always confined exclusively to the inner parietes of the blood-vessels.

I must here remark, by way of further explanation, that where remote ill effects take place from the use of Antimony, Mercury, Lead, Arsenic, Iodine, etc. when continued for a considerable length of time at regular intervals, in doses so small as not to produce any instant or immediate disquiet, either in the stomach, or any other part of the system, I know no reason to believe that the substance of these articles is actually accumulated in any part of the system. On the contrary, it is far more probable (indeed I think there is actual proof of it) that from a succession of slight impressions, they produce a succession of slight changes, which in appropriate cases, after a longer or shorter time, overcome those diseases for which they are remedial. When continued beyond the point where they cease to be sanative, or when long used in health, the changes above mentioned are ultimately extended so far as to constitute disease. Those articles which never produce any of these remote ill effects (as Iron and Cinchona for example) are capable of producing this succession of slight changes only to a certain point, and then they cease to produce any effect at all, being incapable of producing so much change, as is necessary to constitute disease. Most commonly the system has so strong a power of preserving the due proportion and quality of the principles, both proximate and ultimate, that enter into its composition, as to interfere with the absorption of medicines, even to a very slight extent; yet in certain diseased states of the digestive organs this balance is sometimes very materially disturbed. This happens occasionally in dram-drinkers as they were once called, or tipplers as they are now called, in whose bodies the proportion of combustible matter is some times so greatly augmented that ordinary chymical affinity some times supersedes vital organic affinity, and spontaneous combustion some times takes place; or combustion is enkindled, by otherwise inadequate means. In these instances, Alcohol has for a considerable time, answered to a great extent the purpose of nutriment. An opposite state more rarely exists, in which the system seems capable of appropriating, and disposing of concentrated chymical acids, and even of Arsenous Acid and Protochlorid of Mercury or Corrosive Sublimate, in

very large doses. It is maintained then, that no medicines taken into the stomach produce their remedial operation by passing into the mass of the circulating fluids, and of course, that such a reception into the blood is not necessary to their sanative effects. As appears to me, the discussion on this subject in some of our common works on materia medica only serves to show how a writer may wander on all sides of a topic, without ever touching the only important part of it, the real essential point at issue.

The doctrine of the existence of morbid matter in the mass of the circulating fluid, and the necessity of the reception of medicinal substances into the blood-vessels in order to obtain their remedial operation, and indeed all the doctrines of the humoral pathology, appear to me to be intirely in opposition to the principles of the Baconian philosophy, as they rest on no proof, and shackle medicine even to this day. It is still very nearly a universal opinion that this supposed morbid matter in the mass of the circulating fluids, and even these vitiated fluids themselves must be let-out, and the system must be purged, either by depletion of blood, by emesis or catharsis, or by diuresis or diaphoresis, or the quantity of this morbid matter must be diminished by depletion and evacuation of some sort. Such speculations or hypotheses, for they do not mount to the dignity of theories, encumber medicine far more than phlogiston encumbered and embarrassed chimistery; and till the non-medical multitude, as well as physicians, are thoroughly rid of them, they will continue to be like a millstone about the neck.

The opinion that all remedies, or even remedies in general, are taken into the mass of the circulating fluids, and produce their effects either on the blood itself, or by their impression upon the inner parietes of the blood-vessels, or by contact with the proper substance of the parts or organs in which the manifestations of their operation are perceived, appears to me to be destitute of all good foundation. The opinion that they operate upon the blood on the one hand, or by contact with the proper substance of the parts or organs in which the manifestations of their operation are perceived on the other hand, appears to me to be an exceedingly rude and mechanical notion. Very numerous pathological facts of considerable diversity of character might be cited to show that the actual contact of the medicine with the proper substance of

the part or organ, in which the manifestations of its operation are perceived, can not possibly be necessary for the production of the medicinal effects of any internal remedy whatever, not even of emetics and cathartics.

Professor Knight (of Yale College) once informed me of a case that came under his personal observation, in which by the passage of a loaded cart across the abdomen, the *intestinum jejunum* was completely divided between the wheel and the spine. The physician who was first called immediately administered a plentiful quantity of a solution of one of the antiphlogistic and hydragogue cathartic salts, in some vegetable infusion. In four or five hours, this dose purged freely, producing the peculiar watery evacuations, which these salts ordinarily occasion. In a little more than twelve hours, the patient died, and on post-obit examination, the lesion above mentioned was discovered, and apparently the whole of the cathartic solution, which had been taken, was found in the cavity of the abdomen. In fact by far the greatest number of the remedial articles now in use, as far as is known, are utterly incapable of being at all taken-up unchanged from the alimentary canal into the mass of circulating fluid; and in the few instances in which this seems to happen, the medicinal effect, as I am well satisfied, is demonstrably produced before this takes place. Indeed the proportion of remedies which is capable of being so taken-up, is so small, when compared with those which are not, as to evince conclusively, to my mind at least, that they are mere exceptions to the general rule. In the few instances likewise, in which there is supposed by some to be evidence that substances are absorbed by the skin, or from the external surface, it does not appear to me that any remedial effects are produced subsequent to and in consequence of such supposed absorption; nor in my opinion, can this part be considered as any more an avenue for aliment, than for medicine. In the present state of our knowledge, I am very decidedly of the opinion that the few cases of the absorption of remedies unchanged from the alimentary canal into the mass of the circulating fluid, which have been satisfactorily proved to occur, should, upon every sound principle of philosophy, be considered as unexplained and apparently useless anomalies.

The cases in which secreted and morbid matters belonging to

one part of the system have been, as is supposed, suddenly transferred to, and consequently found in an other, as I think, have in all probability contributed to keep-up the doctrine of the necessity of the absorption of remedies into the mass of the circulating fluids, a doctrine which originated exclusively in the humoral pathology; but to me, they only prove the doctrine of vicarious action. In all probability, this is the true case in every supposed metastasis of pus, except when it passes through the cellular tissue, which does not constitute a true metastasis. Thus as seems to me, this remnant of the humoral and mechanical pathology vanishes before investigation, the cases that I have stated, being some of the strongest in its favor, with which I am acquainted.

Two intirely different plants may grow side by side, and even their roots may be interwoven, so that they must derive their nourishment from the same soil, and yet each will be made up of its own proper proximate and ultimate principles, evincing clearly that the vegetable absorbents do not take-up unchanged every thing that presents. Different individuals of the same animal species fed upon different food, retain all their specific peculiarities of composition, and also produce secretions with all their specific peculiarities of composition, while individuals of different and widely diverse species, when fed upon the same food, do the same. One animal is capable of subsisting upon articles that are poisonous to another. When an animal is either quickly or slowly destroyed by poisonous food, its flesh is not thereby rendered poisonous. The flesh of inferior animals, that have eaten substances poisonous to us, and not to themselves, or whose lives have been destroyed by substances poisonous both to us and to themselves, is not thereby rendered poisonous, the prevailing opinion to the contrary notwithstanding. The flesh of those animals which are killed by arrows, armed with the inspissated juice of *Rouhamon Curare* and *Strychnos toxifera*, or *Woorara*, and *Antiaris toxicaria* or *Upas-Antiar* and other analogous articles, is not poisonous, but is constantly eaten by the people who use such arrows. This is conclusive evidence that such poisons are not taken into the blood-vessels unchanged. If they were so received such flesh would inevitably be poisonous. This fact shows that such agents always act upon the nervous system, in consequence of their mere impression on the alimentary canal, or upon other parts to

which they may be applied, and upon which they are capable of acting.

Mr. George Borrow says "the accusation" (of the Gypsies of England) "of producing disease and death among the cattle was" (once) "far from groundless." "Indeed, however strange and incredible it may sound at the present day, to those who are unacquainted with this caste, and the peculiar habits of the Rommanees, the practice is still occasionally pursued, in England, and many other countries, where they are found." "From this practice, when they are not detected, they derive considerable advantage." "Poisoning cattle is exercised by them in two ways: by one, they merely cause disease in the animals, with the view of receiving money for curing them, upon offering their services; the poison is generally administered by powders cast at night into the mangers in which" (and upon the food on which) "the animals feed; this way is entirely confined to the larger cattle, such as horses and cows." "By the other, which they practice chiefly on swine, speedy death is almost invariably produced, the drug administered being of a highly intoxicating" (narcotic instead of intoxicating) "nature, and affecting the brain." They then apply at the house or farm where the disaster has occurred, for the carcass of the animal, which is generally given to them without suspicion, and then they feast on the flesh, which is not injured by the poison, which only affects the head" (rather the nervous system). (*George Borrow's Zincali, or Gypsies in Spain, New York, J. Winchester, 1843.* In another place Borrow says, "by means of Drao" (a poison employed by them) "the Gitanos" (or Gypsies) "procure themselves food by poisoning swine as their brethren in England do, and then feasting on the flesh, which is abandoned as worthless" by its owner. One of their songs runs thus:

"By Gypsy Drow the Porker died,
I saw him stiff at evening tide,
But I saw him not when morning shone,
For the Gypsies ate him flesh and bone."

(*Borrow's Zincali, Chap. vi., Part. II.*)

It has been supposed that the flesh of the Tetrao Umbellus or Partridge of New England, is some times rendered poisonous by the birds eating the leaves of *Kalmia latifolia*. I have been informed on the best authority, that this bird is rarely taken in

the autumnal season in Connecticut, without having more or less of the leaves of *Kalmia latifolia* in its crop, and sometimes it is full of it; and yet these birds are constantly eaten in great numbers, and by very many persons, and always without inconvenience. I never could learn that any body in Connecticut has ever been incommoded in the least, by eating their flesh. The *Kalmia* seems to be one of the natural articles of food for this bird. It has been supposed that the flesh of the same bird is sometimes rendered poisonous by the bird's eating the drupes of *Rhus venenata*. I have also been informed on the best authority, that this bird is rarely taken in the autumnal season in Connecticut, without having more or less of the drupes or fruit of the *Rhus venenata* in its crop; and yet, notwithstanding this, these birds are constantly eaten in great numbers, and by very many persons, and always, as far as I have been able to ascertain, without inconvenience, at least in Connecticut. Indeed this fruit seems to be a natural article of food for this bird, being always eaten greedily by it.

I have long been well satisfied that an article which is the natural and ordinary food of an animal can never be noxious or poisonous to such animal, unless from mere excess in quantity. I have long been well satisfied that when an animal, from extreme hunger has been induced to eat an article which is noxious or poisonous, and even deadly to itself, its flesh is never poisonous to any other animal in consequence. If a truly poisonous article is not digested, it does not pass-out of the alimentary canal, and therefore it can not be assimilated to the solids of the animal eating it. If a truly poisonous article is digested, it is thereby decomposed and converted into some thing not poisonous; and therefore, if it is assimilated to the flesh of the animal eating it, it can not possibly render it poisonous.

But it is said to be certain that the flesh of certain animals, that are generally wholesome, is occasionally noxious or poisonous. This is true; but there is abundant evidence that such an operation is not immediately owing to any thing that the animal has recently eaten, because the flesh of animals not infrequently operates in this manner, when it is certain that they have eaten nothing except their most common and ordinary food. When the flesh of animals ordinarily wholesome proves noxious or poisonous to

man, it is undoubtedly from some disease of the animal. Such disease may indeed be remotely and indirectly occasioned by unwholesome food, or it may not be; but this is some thing intirely different from what I am combating. If the lacteals were not governed by fixed and uniform laws, the blood would soon be a mass too heterogeneous in its composition and qualities, to nourish the system; and if the ultimate assimilating secretories deposited and animalized every thing unchanged that might be contained in such a heterogeneous mass of blood, all peculiarities of species among animals would soon be inevitably lost. Under such circumstances, who can say what, after a short time, a few years perhaps, would be the composition of the Hypochondriac, who is constantly swallowing drugs.

On a little consideration it will be evident that there must be some power that presides over and regulates what shall, and what shall not enter into the composition of organized animal bodies; and if there is such care in our organization and endowments that nothing in the way of nutriment, at least to any amount, should enter the system through the medium of the blood, that is not fitted for its proper support and growth, is it probable that medicines (most of which are foreign enough) when taken into the stomach should not be subject to the same laws and should not be compelled to undergo the same ordeal, when they are presented to the several sentries which guard the portals of access to the mass of the circulating fluids? As I have already said elsewhere, those who maintain the opinion that medicines act by being taken into the circulation, do not attempt to show whether they produce their effects by their action upon the blood itself, by their action upon the inner parietes of the blood-vessels, or by extravasation upon and contact with the diseased parts. In the discussions of this subject by authors it would seem as if it had been more an object to oppose each other successfully, than to arrive at a knowledge of the truth.

To those who imagine that medicines exert their action upon the blood, the inquiry may be put—what is the power of the blood independent of the vital and nervous solids that are instrumental in its production and formation, in ultimate assimilation, in secretion and in excretion? Though it may actually have a low degree of vitality, yet what can the blood accomplish towards carrying

on any one of the functions of the animal economy independent of the influence of the nervous system, and the other living, moving and acting solids? It appears to me to be merely a passive instrument of these powers, and liable to be modified for better or for worse, according as these living, moving and acting solids are in a healthful or morbid condition. If the blood were the primary seat of disease, or the primary seat of the effects of medicines, it must of necessity be so, as an intire mass, so that upon this hypothesis there could never be such a thing as a topical disease, or a topical effect of remedies. In short, upon the subject of the *modus operandi medicaminum*, I concur mainly with Dr. Dewees of Philadelphia, who says "experiments carefully made and often repeated, prove to absolute demonstration, the intire conversion" (i. e. decomposition and recomposition into a new form) "so far as regards its activity" (active principles) "which every substance undergoes, preparatory to its admission into the circulation." "In making this averment" (says this gentleman) "we are not ignorant that some of the properties" (rather principles) "of certain matters, may be traced in the secretions and excretions, such as the odor," (rather odorous principle) "of Garlic, the coloring matter of Madder, etc." He continues, "but it is manifest that the" (medicinally) "active properties of these articles are previously destroyed or neutralized, by the process of" (digestion and) "animalization, and nothing remains, saving the nutritive, or at least inoperative parts." "In this conclusion" (says Dr. Dewees) "we are warranted by a variety of considerations, and especially by the incontrovertible fact that however bland the fluid may be, whether milk or mucilage, or oil, or pus, it can not pass, even in the smallest quantity, directly into the blood without producing even the most fatal consequences."

It is here to be observed that I do not consider the fact that, in a few instances, patients affected by such attacks as malignant epidemic Cholera, or some other equally intense disease, under which recovery is hopeless, have tolerated large injections into the blood-vessels, of water holding salts and various other substances in solution, as constituting an exception to this position, since it is perfectly well known that the system, when very powerfully occupied by certain violent and severe diseases, will tolerate measures and agents, that under any other circumstances, would destroy

life speedily. "That the absorbents" (continues this writer) "are endowed with the power of digesting" (as it were, at least to a certain extent) "and animalizing whatever they take-up, is too strikingly evinced to be denied." "This is one of the provisions of nature to prevent noxious substances from penetrating into the circulation unchanged." "Of that antiquated and truly absurd system" (the humoral pathology, continues Dr. Dewees) "we still retain much, even at the present day, insinuating itself into our reasonings and opinions, and clouding many of our medical views." "By the humoral pathology," (says he) "it was held, that all substances, by whatever avenue brought into the body, are conducted through the circulation, to some part on which they make an impression." "Every article, whether noxious or remedial, was supposed to produce its effects in this way." "In short, no other idea was entertained of the operation of agents on the living frame, except by direct touch on some portion of the solids, or mixture with the fluids." "The hypothesis under consideration" (says our author) "is purely gratuitous." "It is built on a postulate, of which there is not the slightest evidence, and it results from the most wretched species of philosophizing." "Give me, cried Archimedes, with the enthusiasm of genius, a spot on which to fix my machinery, and I will move the globe." "Equally may the theorist exclaim, in the ardour of confidence, grant me my premises, and there are none of the arcana or mysteries of nature, which I will not reveal or explain." "Nothing is more easy." "To answer such purposes hypotheses may be made to start up like exhalations." "Be it remembered however, that these intellectual phantasies have ever proved the bane of truth, and the curse of medicine."

Dr. Dewees proceeds, "now perhaps we may render our theory" (if it is a theory rather than a plain matter of fact, I would abandon it) "acceptable even to the humoral pathologist, by showing that a sympathetic action is precisely the same," (so far as regards results) "as that produced in either of the above modes." "As preliminary to the main consideration, we assume it as indisputable, that whenever an agent, remedial or poisonous, is applied to a susceptible portion of the body, externally or internally, an action" (sensation or condition is occasioned) "which is extended more or less according to the diffusibility" (of the operation or

effect) "of the article or the degree of sympathetic connexion which the part may have with the body generally." "Thus" (a condition is produced, or) "a set of" (sensations or) "actions is" (occasioned) "which is precisely similar, provided they are confined to the same" (subordinate) "system, by which is to be understood parts of an identity of structure, or having the same official" (or functional) "capacities." "But where the chain runs into other" (subordinate) "systems it loses its homogeneous character, the actions being" (more or less) "modified, according to the peculiar organization of the parts in which they may take place." "To illustrate our meaning more distinctly" (says Dr. Dewees) "we will state a very familiar case." "By inserting a particle of variculous matter under the skin," (a peculiar and specific) "local irritation is created," (and) "in a few days, this action becomes diffused, and a fever ensues, which after a short continuance, produces an eruption." "It is in this way" (says our author) "that morbid action" (likewise) "distributes itself." "When diseases arise from a point" (as probably all diseases do, though more strikingly those occasioned by inoculation) "the matter introduced is not infinitely divided and spread over the body, but" (in reality only) "the action, which that matter had originally" (produced). "These," (says Dr. Dewees) "are the general principles which apply to the system in every condition, and which explain the *modus operandi* of medicines, as well as the" (*modus operandi* of the) "causes of disease." "Whatever, in short, operates upon the living frame, is obedient to the same laws." "The spot thus acted upon, is the focus, from which is irradiated the more diffused impressions." (*Amer. Jour. Med. Sci. New Series, No. 3, 1825, pages 150, 157.*)

Dr. Nathaniel Chapman says that "the hypothesis of the absorption of medicines into the blood-vessels, is evidently a relic of the humoral pathology." "By the disciples of that doctrine" (says he) "it was held that disease chiefly consists in a depravation of the blood from too great tenuity or visciduity;" (from) "an excess of acid or alkaline acrimony," (or from) "morbific matter entering from without, or generated within, by a process analogous to fermentation or putrefaction." "As a necessary consequence of this view of disease, medicines were suffered to enter the circulation, and by a sort of chymical action, to correct the vitiated

condition of the fluids and hence the origin of the terms inspisants, attenuants, antacids, antalkali"(nes) "antiseptics, diluents, demulcents, etc." "To demonstrate the fallacy of these speculations, by any detail of facts, or reasonings" (says Dr. Chapman) "can not now be required, as they are pretty generally exploded." Dr. Chapman continues, "my opinion is, that all changes in the condition of the fluids are wrought by impressions made through the intervention of the solids." "Not the slightest proof exists, so far as I know, of their undergoing any mutations, either by spontaneous action or from the introduction of foreign matters, much less that such is the cause of disease, or the mode in which our remedies operate." Dr. Chapman says in order "to penetrate into the circulation, medicines must pass either by the lacteals, or the lymphatics." "Now" (says he) "it seems" (much) "more than probable that in either case, their powers would be so changed by the preparatory processes of" (digestion and) "animalization" (or of absorption) "as to be deprived of all activity." "Can it indeed be credited that any substance, after a subjection to the digestive and assimilative powers, retains, in the slightest degree, its original properties?" "On the contrary, experiments show that chyle, however diversified the materials may be out of which it is formed, whether they are animal or vegetable, has invariably an identity of nature, and instead of being crude, as is commonly imagined, is in reality a highly elaborated fluid, having many, and perhaps most of the properties of blood, except merely its red color." "It contains at least three of the constituent parts of blood" (viz.) "1st. There is one portion of the chyle which preserves its fluidity during life, but coagulates after death by exposure to the air." "This" (in all probability) "is essentially" (coagulable lymph, or) "fibrine." "2nd. There is another portion which resembles serum in continuing fluid when exposed to the atmosphere, and in coagulating at the same degree of temperature as serum." "3rd. There is another portion still, consisting of globules similar to those of the blood, with this difference only, that they are much more minute." "The fact of the perfect and uniform constitution of chyle, seems to me" (says Dr. Chapman) "to put down at once, the hypothesis which I am combating." "But perhaps it may be said that it proves nothing in the case of medicines administered in other ways than by the stom-

ach, as when applied to the surface of the body, or introduced into the lower intestines." "To this objection the answer is obvious, and I think very satisfactory." Dr. Chapman as well as Dr. Dewees says "no one who has carefully attended to the phenomena of the absorbent system, can help admitting that every section of it is endowed with the power of digestion" (as it were, at least to a certain extent) "and assimilation, and the lymphatics quite as conspicuously as the lacteals." "This capacity is given as a provision of nature, to exclude noxious matters from the circulation." "The absorbents in most" (if not all) "instances, are fully adequate to this end." "That some of the properties" (principles rather) "of certain articles, as the odor"(ous principle) "of Garlic, and the coloring matter of Madder, are displayed in the secretions and excretions," Dr. Chapman says "I am not disposed to deny." "But it does not hence follow that these substances entered the circulation, in their primitive shape." "Directly the reverse seems indeed to be proved as neither the one nor the other can be detected in the serum of the blood. (*Vide Hodges's Inaugural Thesis published in 1801.*)"

Dr. Chapman says "to me it is clear that the process of assimilation, as performed either by the chylopoietic viscera, or by any part of the absorbent apparatus, completely decomposes all," (compound) "substances, and however discrepant in their properties, reduces them to one homogeneous fluid fitted for the purpose of nutrition." "But when thrown into the secretions or excretions, being removed beyond the sphere of the vital energies, the chymical affinities are again some times brought into play, by which these substances are in part or wholly regenerated." Dr. Chapman says "whether this explanation shall be received or not, it must at least be acknowledged, that no substance in its active state ever reaches the circulation, since experiments have shown that even a few drops of the mildest fluid, as milk or mucilage, oil or pus, can not be injected into the blood-vessels without occasioning the most fatal consequences." Dr. Chapman says "conceding how ever to the humoral pathologists, all which their doctrine demands, still insuperable difficulties remain in the way of its adoption to account for the operation of medicines." "Not to dwell tediously on this subject, I shall content myself, at present, with merely mentioning, that we are not at all informed by

it, why our remedies, after being mixed with the blood, should be directed to one organ in preference to another, as Mercury to the salivary glands, etc." "As regards the Mercurial preparations" (says our author) "the example particularly selected by Murray, to illustrate the hypothesis of absorption, there is the most conclusive proof, that in what ever manner employed, they are not carried into the circulation, and no doubt such is the case with all the articles of the *materia medica*." (*Vide the experiments of Drs. Physic and Seybert published in vol. V. of the Medical Repository.*) Dr. Chapman says "it results therefore from what I have said, that we are to reject the fluids altogether, in our inquiries relative to the operation of medicines because, in addition to the reason already stated for doing so, we have in that law of the animal economy called sympathy, or consent of parts, a solution of the problem, infinitely more consistent with the existing state of our knowledge." On this point Dr. Chapman agrees with Dr. Dewees, for he says that "conformably to this theory, whenever a medicinal substance is applied to a susceptible portion of the body externally or internally, an action is" (occasioned) "which is extended more or less, according to the diffusibility of the" (operation) "of the substance, or the degree of sympathetic connexion, which the part may maintain with the body generally." "Thus a set of actions is" (produced) "every one of which is precisely similar, provided they are confined to the same" (subordinate) "system of parts, by which is to be understood, parts of an identity of structure." "If however the chain runs into other" (subordinate) "systems, it loses its homogeneous character, the actions being modified by the peculiar organization of parts in which they may take place." "These are principles of universal application." "In every case, whether it respects the operation of remedies, or the production of disease, the spot primarily acted upon, is a point from which is diffused the radiated impressions." (*Vide Chapman's Edition of Murray's Materia Medica, New York, 1821. Vol. 1, pages 98-100.*)

DIGESTIBILITY OF MEDICINES.

The operation of medicines is often very greatly affected by their digestibility or non-digestibility. I do not think there is any room for doubt that most vegetable organic medicines at least, undergo a more or less perfect digestion during their passage

through the organs of primary digestion, some articles being unquestionably more completely and more perfectly digested than others. This is evident from various facts and circumstances. In the first place, the proportion of vegetable organic medicines, of which any trace can be found, in the mass of the circulating fluid, is (as I have said) exceedingly small, in comparison with the whole *materia medica*. No greater traces of these same medicines can be found either in the fecal discharges from the lower and larger intestines, or in any other of the excretions. It follows of course, that in their passage through the stomach and upper and smaller intestines, they must have been subjected to the process of digestion, which necessarily involves a decomposition generally, and a recomposition into a new form. That primary digestion involves this, will be evident from the fact that the proximate composition both of chyme as it is formed in the stomach, and of chyle as it is formed in the upper and smaller intestines, is different from the proximate composition of any individual article of food before digestion, and is always uniform in all healthy subjects, whatever may have been the material digested. I say that the proximate composition of chyme and chyle is always uniform in a healthy subject, what ever the material digested, by which I mean that the several proximate principles are always the same, as respects number, composition, nature and character, though perhaps they may vary some what, as respects relative proportion. In diseased subjects, and in the case of subjects confined to a very limited or narrow range of food, a range too small to be capable of furnishing all of the several elements (at least in any thing like the due proportion) necessary to support the growth, and repair the waste of the system, it may be other wise ; but then under such circumstances, disease is the inevitable consequence, at least sooner or later, and if the range of food is not increased, or the whole diet changed, death is the result after a time. Among those who are exceedingly indigent, too much so, to be able to obtain the variety of food necessary to health ; and, in State prisons, where the variety of food is either limited by laws made by popular legislatures, or by the arbitrary will of the prison-keeper both equally ignorant of the whole subject of dietetics, disease of a certain sort almost always follows. Struma, Porphyra, general exhaustion etc., are among the most common

diseases, that result from such a diet. In fact when vegetable organic medicines are not digested to a greater or less extent, their operation is generally disagreeable and unkind; and they seldom produce the beneficial effects expected from them. Almost every physician, who has been in practice for any length of time, must undoubtedly be familiar with the fact that when the cathartic Oil of the *Ricinus communis* for example passes through the intestines undigested and unchanged, it is always productive of tormina and other disturbance; it seldom purges in a regular manner, and it as seldom renders the service expected from it.

But if medicines are to a greater or less extent digested (or decomposed and recomposed into a new form) during their passage through the organs of primary digestion, it may be asked when, or at what time, they exert their medicinal powers, and produce their medicinal effect? I answer that in general, they do this before their absolute digestion, and total decomposition and recomposition into a new form; and they do it by their mere impression or action upon the inner parietes, or in other words, upon the mucous membrane of the stomach and upper or smaller intestines, for which there is abundant time, since vegetable substances are not so soon digested as animal substances, and medicinal substances not so soon as food. In the digestion of crude vegetable medicines however, there are doubtless several stages. For example, when crude bark of *Cinchona* in fine powder is swallowed, the first stage of its digestion seems to be such an extrication of its several proximate principles from their combination with each other, that they are enabled to exert their medicinal powers and produce their medicinal effects, which they would not otherwise do. I have very often observed that where the energies of the organs of primary digestion are by any means so much impaired, that this degree of digestion does not take place, the medicine produces no other than mere mechanical effects, which are commonly irritant in some mode or degree. It is doubtless owing to a deficiency of this grade or degree of digestion, that the powder of crude bark of *Cinchona* so often fails of producing any useful or salutary effects in many cases of *Typhus putridus* etc. that are attended with very great atony, or in other words, extreme exhaustion of the vital energies of the heart and blood-vessels, and at the same time, of all the parts dependent upon the great sympathetic nerve.

Many of the disagreeable effects of medicines in the stomach, when they are strongly indicated by the condition of the system at large, are undoubtedly the result of the inability of the stomach to perform this first grade of digestion. When the Alcaloid Quinine (perhaps also the Disulphate of Oxyd of Quinum) is taken into the stomach, this first grade of digestion ceases to be necessary, because the medicinally active principle only is taken in a separate state, and the stomach no longer requires a separation of it from the medicinally inert principles, with which it is combined in the crude bark. It has long appeared to me that the medicinal operation of the pure Alcaloid Quinine is more pleasant, agreeable and kind than that of the Disulphate of Oxyd of Quinum. If this is the fact I think it is mainly explainable by its greater digestibility. I have long believed that the vegetable Alcaloids generally, are less likely to disagree with the stomach, less likely to operate disagreeably and unkindly when they are taken in an uncombined state, than when taken in combination at least with the strong chymical acids. If this is true, and if the difference results from the greater digestibility of the uncombined Alcaloid, it will probably be found that the Salts of the Alcaloids with the vegetable organic Acids, will be more kind in their operation than their Salts with the strong inorganic or chymical Acids. When the powder of crude bark of Cinchona is taken, it is the product of the first stage of its digestion, that produces the medicinal effects that we desire; but when the Alcaloids Cinchonine Quinine etc. are taken, their effects must be produced before any grade or degree of digestion is effected.

The crude inspissated descending sap of *Papaver somniferum* commonly called Opium, requires a certain grade or degree of digestion to separate its active proximate principles from those which are inert. Of the latter Caoutchouc seems to be the principal, and this is extremely difficult of digestion. In some cases when this grade of digestion is not at all performed, Opium some times fails almost intirely of producing any of its regular effects. In violent paroxysms of Enterodynia I have more than once known a large amount of crude Opium in the shape of pill (made into this form while the article was soft, and subsequently suffered to become dry) administered with very little effect.

Finally resort was had to the Tincture of Opium from which relief was soon obtained. When the intestines came to be thoroughly evacuated, the pills of Opium were found together, softened indeed, but not dissolved. The invariable difficulty in the digestion of Caoutchouc, and the impairment of the digestive power by the disease, often undoubtedly prevents the regular effects of the Opium. The digestibility of the Alcaloid Morphine must be far greater than that of the inspissated descending sap of Papaver, since it contains no Caoutchouc, nor any thing less digestible than itself. I have often had occasion to observe the shortness of the duration of the effects of the Alcaloid Morphine, in comparison with the continuance of the effects of Opium, though I doubt not that the effects of both continue after the perfect and complete digestion of the doses. The speed with which the Alcaloid Morphine produces its effects is greater than that of Opium, but this is due to the circumstance that the Morphine (contrary to what is the fact with Opium) does not require any degree of digestion for its fullest operation.

Even when some medicines of vegetable organic origin, in minute quantities, are thrown into the mass of the circulating fluids, by injection into a vein, it has been found that they undergo a very speedy decomposition. This has been found to be the fact with some of the Salts of Oxyd of Morphinum, since no trace of them can be discovered in the blood, even by the most delicate tests, a very short time after the injection. Perhaps this ready digestibility is the reason why Morphine produces so little disturbance, in comparison with some other agents, when thus administered. But in medicines of inorganic and pure chiminal origin, there is frequently no true and proper digestion; but such articles not infrequently undergo a decomposition by some agent contained in the secretions of the stomach, and upper or smaller intestines. But whether such decomposition renders the operation of the article more or less kind, depends intirely upon the difference in the properties of the original article, and those of the products of the decomposition. However even medicines of inorganic origin very often require to be more or less modified and changed (if not properly digested) in their passage through the organs of primary digestion, in order to operate in the most favorable manner and to produce their best remedial effects. I have known

many instances, in which Chlorohydrargyrous Acid Dichlorid of Mercury or Calomel has passed through the alimentary canal unchanged, being found in the fecal discharges intirely unaltered; and in every such instance, it has invariably operated unkindly, producing tormina, not purging regularly, and rendering no service. It is true that this article, even when it is properly decomposed, and converted into the Disoxyd of Mercury in the organs of primary digestion, does not always operate kindly; but then I believe that it never operates kindly when it is not so decomposed or modified in some way or other. In the case of the Chlorohydrargyrous Acid Dichlorid of Mercury or Calomel already mentioned, it is my belief that it is the product of its decomposition in the stomach and upper and smaller intestines, by the Soda of the fluids of these organs, which produces all its medicinal effects. But at all events (as I believe) those articles of medicine that are digestible always operate more kindly than those which are not; and those which are the most easily digestible are the most kind in their operation, as a general rule at least. So far as this goes, I think it furnishes a just ground for preferring vegetable medicines to those which are of mere chiminal and inorganic origin, or those which have a metal for their base, or those which are of exclusive mineral inorganic origin.

I have long been satisfied, as I have just said, that many of the disagreeable and unkind operations of medicines result intirely from their difficulty of digestion, or their absolute indigestibility. Certainly many of the disagreeable and unkind operations of medicines are not the effects of any of those powers, by which they prove medicinal, or in other words, remedial of disease. This is a subject that is well worthy of consideration and investigation, and one that should be studied in immediate connexion with every individual article of the *materia medica*. If these views and opinions are correct, the disagreeable and unkind operations of medicines may undoubtedly be obviated in many instances, by administering them generally in such forms, as will render their digestion the most easy. If these views and opinions are correct, it will doubtless prove to be the fact that what is so often maintained and inculcated by the Quacks, has more or less just foundation, viz. that medicines of organic origin are more agreeable and kind in their operations, than medicines of

inorganic origin, because they are more digestible. It will not prove however that the Quacks arrived at this conclusion, either as a result of observation and experience, or as a rational and logical deduction from well established scientific premises; but it will only afford another example of that sort of accident, by which ignorance and rashness may some times arrive at a true and valuable result, and this without any precise accurate and definite knowledge of such result, or of its practical utility and importance. Linnæus has well observed — “Sane quæ non invenit ratio, sæpe invenit temeritas.”

In strict propriety, it is necessary to a thorough knowledge of every article of the *materia medica*, that we should know exactly how it is modified in the organs of primary digestion — whether, like the crude bark of *Cinchona*, it is only so far decomposed as to develop its active principles; or whether, like the Alcaloid Morphine, it is not only completely but speedily digested, so as (in all probability) to abridge and even diminish what would otherwise be its effects. But the subject of the digestion of medicines can hardly be said to have been at all observed or investigated by the profession at large, since if it had been, it must inevitably have greatly modified the views of physicians generally in reference to many points of the *modus operandi medicaminum*. What little I have to communicate upon this topic, I shall give in connexion with the individual articles of the *materia medica*, with little expectation of being any further useful, than by turning the attention of medical gentlemen having ample opportunity of observation, to what I deem an important subject, and one affording an ample field for discovery.

TRANSCENDENCY.

In reference to the *modus operandi medicaminum*, it remains to explain, in this place, what I have always been in the habit of calling the transcendency of one medicinal operation of given agents over other operations of the same agents. When so large a dose of any medicinal agent possessing two or more powers, is given at once, as to extinguish the susceptibility of the stomach and upper and smaller intestines to the ordinary operation of a much smaller dose, or to produce a new effect incompatible with it, so that an intirely different operation takes place, it

is said that by such a dose, the ordinary effect of a much smaller dose is transcended. Transcendency in almost if not in all cases, is believed to be owing to the exertion of another power, and the production of another effect, an effect not producible by smaller quantities of the agent, but an effect, which when produced, is incompatible with the regular operation of the smaller doses and quantities. For example, a nauseating and an emetic dose of Arsenous Acid transcends its tonic, and perhaps even its adenagic operation. Its smallest decided and positive orethetic dose transcends its emetic and cathartic operation. An extremely large quantity often transcends the production of the irritating, distressing and painful effects of an ordinary orethetic dose, and produces its neuragic effects only, in an intense degree. In other words, such a dose of Arsenous Acid may be swallowed as to extinguish life without any topical oræsthesia or Erythematic Phlogosis of the mucous membrane of the stomach and upper and smaller intestines, without any nausea, emesis or catharsis, and of course, without any adenagic or tonic effects. Under these circumstances, the neuragic effect of this agent transcends and supersedes all other operations. Again, when Arsenous Acid is administered in such doses as to nauseate or vomit, or to prove cathartic, it fails intirely of proving tonic, the first mentioned operations transcending and superseding the last mentioned.

The emetic operation of the Tartrate of Antimonia and Potassa, may even be said to transcend partially and to a very limited extent, the antiphlogistic or direct exhausting operation. Indeed it does actually transcend its true antiphlogistic operation, though it does not transcend a less degree of an exhausting operation; for it is a fact that the Tartrate of Antimonia and Potassa can never be taken in any operative quantity, without more or less direct exhaustion, since none of its other operations are absolutely incompatible with direct exhaustion. An immediately neuragic dose of the Tartrate of Antimonia and Potassa will often transcend and supersede both its orethetic and its emetic operation. I have known a very large dose of this salt taken by mistake for neutral Tartrate of Potassa commonly called soluble Tartar, without vomiting, or even nauseating, and also without producing any grade or degree of Erythematic Phlogosis of the mucous membrane of the stomach and upper and smaller intestines. It how-

ever materially diminished the tone of the organs of primary digestion, and of the system at large. I think it required between three and four months for the perfect restoration of the subject. An emetic dose of the Tartrate of Antimonia and Potassa is decidedly less exhausting than would be the same amount of this agent taken in merely nauseating doses, or in doses short of the nauseating point. This is owing, at least in part, to the fact that vomiting if not too often repeated, transcends and supersedes the exhausting effects of this agent.

An actively cathartic dose of Chlorohydrargyrous Acid or Dichlorid of Mercury usually transcends its adenagic operation; and so large a dose may be given as to transcend its cathartic operation. In this last case, its neuragic effects supersede its cathartic effects. That no more cathartic operation is often occasioned by a large than a small dose of Dichlorid of Mercury is a fact well known to every physician, who uses this article extensively. It is believed to be a prevailing opinion among physicians at the present period, that what I call a mere and pure antisbestic power, when applied in a relatively excessive degree, may transcend the invigorating operation, and produce a contrary i. e. an exhausting effect; but on careful and thorough examination I think that this opinion will be found to be incorrect. The *Cantharis vesicatoria* is antisbestic, i. e. invigorating in a peculiar way, in any operative dose and quantity, which falls short of the production of ultimate oræsthesis; but, as soon as this last operation begins to take place, all antisbestis i. e. invigoration of a peculiar sort immediately ceases, because ultimate oræsthesis and antisbestis are incompatible effects, and because ultimate oræsthesis is a more powerful operation than antisbestis. *Phosphorum elementarium*, *Rhus venenata*, *Rhus vernicifera*, *Rhus perniciosa*, *Rhus pumila*, *Rhus Toxicodendron*, *Rhus radicans*, and all other similar Rhoës, as well as *Hippomane Mancinella*, etc. are all antisbestic in moderate doses and quantities, but when given in doses and quantities beyond a certain point, they transcend an antisbestic operation, because they produce ultimate oræsthesis, which is incompatible with antisbestis, and being a more powerful operation, counteracts, overcomes or supersedes all antisbestis. Opium in certain doses and quantities is well known to be a moderate antisbestic; but in doses and quantities, larger to a certain extent, it intirely fails of exerting

any antisbestic power, and only proves ultimate-narcotic, an aggregate of effects not producible by antisbestic doses and quantities. Now an ultimate-narcotic operation is incompatible with antisbestis, and being a more powerful operation, it intirely counteracts and absolutely prevents any degree of it, though the first three grades of a narcotic operation are not at all incompatible with antisbestis.

Alcohol is oresthetic, euphrenic, antisbestic, subdiuretic and subdiaphoretic, in relatively moderate doses, at regular and short intervals. In a large and deadly dose, it produces a sudden, powerful and very transient oresthetic, euphrenic and antisbestic operation, soon transcended and perfectly superseded by ultimate-narcosis. As an antisbestic it produces a quickly diffused and transient increase of vital energy and strength of action of a peculiar sort, in the circulating system ; but as a narcotic (in a deadly degree of this operation) it extinguishes life in the circulating system, the motions of the expressory and respiratory apparatus continuing for a considerable time after the cessation of all arterial and cardiac pulsation. There are certain articles that possess a combination of narcotic (or erethistic ?) and oresthetic powers in such proportions, that certain doses and quantities will produce antirritant effects, while certain other doses and quantities, which are considerably larger than those which prove antirritant, will transcend this operation, and will only prove oresthetic because an oresthetic effect is incompatible with an antirritant effect, and being a more powerful operation of certain quantities of certain articles, will therefore take the precedence. I have probably said enough upon this subject, to explain and render intelligible my views and notions in regard to it. What I have to say further upon it, I shall reserve to be said in connexion with the individual articles of the *materia medica*, to which it more appropriately belongs.

INCOMPATIBILITY, CHIMICAL AND MEDICINAL.

At the present day, much is said about the incompatibility of medicines ; but as far as my observations and experience will allow me to decide, there is more error than truth, in what is taught in books upon this topic. But at all events, there is a very wide difference between the principles taught by the mere chimists, and the actual practice of the most scientific physicians, in

relation to the incompatibility of remedial agents; and though it might not be difficult to point out errors with the latter, yet it is believed that they are much more numerous with the former. It appears to me to have been from a deficiency of knowledge in the *materia medica*, that the chimists have so often fallen into error on the one hand; and from a deficiency of knowledge in chemistry, that the physicians have fallen into error on the other. Under these circumstances, it is certainly right and proper, indeed it becomes the duty of every practitioner of medicine, to study this subject carefully for himself, and to endeavor to arrive at more correct results, which is certainly within the power of every one who is well informed both in the principles of chemistry and of *materia medica*. To illustrate the importance of a knowledge of this subject, I will state that I once actually knew a case, where a complaint against, or an impeachment of a highly respectable medical practitioner of one of the large towns of a neighboring state, was made with all legal formalities, and he was put upon formal legal trial for alleged ignorance in his profession, and for consequent mal-practice of such a character and degree, as was supposed ought to subject him to the penalty of forfeiture of his diploma of license, and perhaps to a considerable fine, on three charges, the first two of which were merely the prescribing of supposed chimically incompatible medicines, though the last was that of having administered Opium and Brandy in conjunction, in an acute febrile disease.

In my opinion, there are some general principles upon this subject, which are not to be found in the common books, either of chemistry or *materia medica*, a knowledge of which may certainly be useful, or even highly important. There are in fact two sorts of incompatibility, viz.

1st. Chymical incompatibility; and

2nd. Physiological, pathological, or as it is far better named, medicinal incompatibility.

I consider the terms physiological and pathological so inappropriate in relation to this sort of incompatibility, that I should not even mention them, were they not employed by some highly respectable and distinguished authors. These two sorts of incompatibility are distinctly recognized by authors, but I fear too little regarded by practitioners.

Chimical incompatibility results from the destruction or change of remedial powers, by decompositions or new combinations, or both, between the medicinally active proximate or ultimate principles of two or more agents, that are administered in conjunction or connexion in some way or other. In all the common works on chemistry, I believe it is laid down as a law of affinity, that, when two elements, or two compound principles enter into strict chimical combination, i. e. combination in definite and uniform proportions, the product of this union will lose all the properties of its component parts, and acquire an entire new set, peculiar to itself. This may be, and in all probability is truly a law in regard to external and sensible properties merely, but it certainly is not a law in regard to occult medicinal powers generally, though it may possibly be in a few instances.

It is commonly supposed also by the chemists, (and the late writers on the materia medica have followed them implicitly and blindly) that, when ever a decomposition and a new combination, or simply a new combination without any decomposition, takes place, in consequence of the conjunction of two distinct medicinal agents or preparations, such agents or preparations must necessarily be chimically incompatible. This however is certainly not the fact. It should be observed that crude vegetable substances, as well as infusions, decoctions and tinctures, almost always contain several distinct proximate principles, though their medicinal effects usually depend upon one, or at most, two of these, the rest being completely inert, or at least of no importance to the medicine. When therefore chimical actions are confined to the medicinally inert principles of the several articles in a pharmaceutic preparation, or of two such preparations that are to be administered in connexion, it will be obvious that the remedial powers must remain unchanged, notwithstanding the chimical actions, and therefore, that they cannot be justly considered as chimically incompatible.

Certain elements which are medicinal per se do in fact retain their medicinal powers in every state of combination provided a sufficient quantity of the compound is administered to give the requisite dose of the active element. In this case, the medicinal powers can not be destroyed by combination, though they may be moderately exalted in some cases, and diminished some what

in others. In these circumstances then, the occurrence of chemical actions would not indicate chemical incompatibility in such a sense as to interfere materially, or even appreciably, with the operations and effects of the agents conjoined, and thus entering into combination. The following appear to be principles, or laws, in relation to this subject.

1st. Some elements are medicinal per se. When an element is medicinal per se, it retains its medicinal powers in combination, some times though rarely more or less exalted, but oftener slightly diminished. The more complex the compound, the greater is this diminution of its powers likely to be. Nothing therefore can be properly said to be chemically incompatible with such compounds as these (with only one class of half-way exceptions) viz. the conversion of a compound in which the medicinal powers are some what exalted, into one in which they are some what diminished, which, in consequence of the variation of the dose of the new compound, in comparison with the old one, may occasionally hinder the production of the desired effects. Iodine is medicinally active per se, and its compounds possess the same powers. Iodine therefore will afford a sufficiently good illustration of this law in relation to an element. Bromine is also medicinally active per se, and its compounds also will afford another sufficiently good illustration. Gold is considered as active per se, and its compounds possess the same powers, and therefore this element will afford another good illustration. Phosphorum is active per se, but its compounds have some peculiarities as respects their medicinal powers when compared with the preceding articles. The Oxygen Acids of Phosphorum can not be used for the production of the effects of Phosphorum, since their oresthetic powers resulting from the intensity of their sourness and consequent acrimony, prevent their being pushed to such an extent as to produce the effects of the Phosphorum merely; but some of the salts of these Acids, in which their intense sourness is perfectly neutralized, seem to be capable of producing to a certain extent the effects of the Phosphorum. This, I think, is the fact with the Diphosphate of Protoxyd of Iron, and also with the neutral Phosphates of Oxyd of Quininum, Oxyd of Cinchoninum, etc. At first view, the Oxygen Acids of Phosphorum might seem to be an exception to principles elsewhere laid-down,

in as much as they can not be used for the production of the effects of Phosphorum. This however is not owing to their not possessing such powers, but to their possessing an other power, which transcends and supersedes the ordinary effects of Phosphorum, all which is evinced by the fact that certain salts containing these Acids, with their sourness neutralized, produce the effects which were transcended and superseded by the oresthetic power of the uncombined Acids. Similar facts are equally true of several other elements that are medicinal per se. It would therefore be undesirable to conjoin in one pharmaceutic preparation, or to swallow in conjunction such compounds as would act upon each other in such a manner as to extricate in the stomach any considerable quantity of one of the Oxygen Acids of Phosphorum. I make this statement for the sake of explaining and illustrating what I am inculcating; but I must add that I know of no way in which such an effect could be produced, and certainly no convenient way.

2nd. Some elements are not at all medicinal per se. but always produce certain definite and uniform medicinal powers by combination, in some instances slightly greater, in others slightly less in degree. Those compounds of elements not at all medicinal per se, but which by combination produce certain definite and uniform medicinal powers, always retain these powers when they enter into more complex combinations, though some times in a somewhat diminished degree. Nothing can be considered as chemically incompatible with these compounds, except in a very slight degree, and in the same manner as mentioned in relation to the preceding group. Even those elements which are not medicinal per se, but which impart medicinal powers to their compounds, very seldom have their remedial effects varied, except moderately in degree, by entering into new and different combinations, unless indeed the element or compound-radical, with which they combine, imparts new powers.

For obvious reasons Kalium or Potassium can not be used per se, and therefore the question whether it is medicinal per se, can not be entertained. All the compounds of Potassium, I believe without exception, invariably possess exhausting powers in a greater or less degree, and almost always, antiphlogistic powers. This is obvious even in the Cyanid of Potassium, small as is its

dose. It is far more obvious in the Protiodid of Potassium. Notwithstanding it is so common, at the present time, to administer this compound as a preparation of Iodine and for the effects of that agent, yet I have never seen it taken to the extent to produce the effects in question without the production of very considerable exhaustion, not only in the organs of primary digestion, but also in the system at large. The exhausting effect of the compound is in proportion to the quantity of Kalium or Potassium, which the compound contains, without regard to the other ingredient, unless that happens to possess opposite powers. I know many practitioners of medicine who are in the habit of employing the Tartrate of Potassa and Protoxyd of Iron, and more especially the Tartrate of Potassa and Sesquoxid of Iron as tonics. These gentlemen almost, if not quite always, conjoin with these salts both vegetable bitters and acrids. I have employed these salts alone, i. e. intirely by themselves, and the result of my observations upon them, when thus used is that they are exhausting, rather than invigorating agents. This we might expect a priori from their composition, since they are made-up of two equivalents of Tartaric Acid and one equivalent of Potassa, both decidedly exhausting agents, and one equivalent of one or the other of the Oxyds of Iron, both commonly reckoned as tonics, but so feeble, that it is doubted by some whether they have any medicinal powers at all.

To this I doubt not that the Iodoquininate of Iodid of Potassium, and all other analogous compounds, are exceptions, the single equivalent of Quininum, in all probability preventing such a salt from being antiphlogistic, and possibly from being in any degree exhausting, though this latter I do not certainly know. I do not think there is the least probability that such compounds can possess any degree of tonic power.

The same remarks may be made of Natrium or Sodium and its compounds, that have just been made of Kalium or Potassium, with the exception that Sodium is a feebler exhausting agent than Potassium. It is quite probable that the same may be true also of some other of the metals near the electro-positive extremity of the scale of elements, and of their compounds, as Magnesium, Calcium etc. Indeed I very well know that it is true to a greater or less extent of the last mentioned two. With respect to

Stibium or Antimony, it is yet a matter of some controversy whether it is active *per se.* or not. The best authorities are upon the negative side of this question. But at all events, all its compounds possess certain uniform and definite powers undeniably resulting from the Stibium or Antimony. Suppose that Stibium or Antimony should be exhibited in the form of some salt of the Sesquoxyd, and suppose that this Sesquoxyd should be detached from its salifying principle or Acid, by something given in conjunction with it, of what consequence to its medicinal effects would this detachment be? On the contrary, suppose we should attempt to administer the Sesquoxyd in an uncombined state, but should conjoin so much of some salifying compound with it, as to convert it into a salt, of what consequence would this be to the effect of the Sesquoxyd of Stibium or Antimony? The dose and the effect would be the same in each case. As is altogether the most probable, Hydrargyrum or Mercury is not medicinal *per se.* but it always imparts two powers at least to all its compounds, viz. what I am in the habit of calling adenagic and neuragic powers. Perhaps it may be considered as likewise imparting more or less cathartic, nauseant and emetic powers, for I have never employed any Mercurial, which did not seem to exhibit these powers, provided it was taken in sufficient quantity. Still there are many Mercurials which can not be advantageously used for these operations. Some of the compounds of Hydrargyrum or Mercury some times have certain accidental powers intirely dependent upon certain external sensible properties, as the orethetic power of the Protochlorid of Mercury and the Protocyanid of Mercury. Now of what consequence can it be, at least as a general rule, if there is a change of composition among the compounds of those elements which are medicinal *per se.* and which impart their powers to all their pharmaceutic preparations; or among the compounds of those elements, which are not medicinal *per se.* but which impart certain powers to all their pharmaceutic preparations? It is true that it would not be desirable to have a large dose of Dichlorid of Mercury converted in a pharmaceutic preparation, or in the stomach, into Protochlorid of Mercury; but the cases in which such a change as this is possible, are exceedingly few indeed; and yet where it is possible for such a one to occur, the physician should assuredly be perfectly apprized of it, and take care to avoid it.

3d. Some elements are neither medicinal per se, nor capable of producing any definite and uniform medicinal powers by combination. But certain compounds of elements neither medicinal per se, nor capable of imparting any uniform medicinal powers by combination, are never the less actively medicinal as compounds, e. g. Cyanogen, Arsenous Acid, Arsenic Acid, Alkaloids generally. The medicinal compounds of elements neither active per se, nor capable of imparting any definite and uniform medicinal powers when they enter into more complex combination, follow the same laws as respects the preservation of their powers in their compounds, as those elements which are medicinal per se. It is commonly supposed and believed (but not yet by any means ascertained) that medicinal compounds lose their medicinal powers, when they unite in more complex combination with Gallic and Tannic Acids; and that their medicinal powers are diminished, but not intirely destroyed, when they unite in more complex combination with Carbonic Acid. I shall show hereafter, that there are many cases of which this is not true; and that there is a great probability that it is not true in any case, with perhaps a single set of exceptions only. Any substance that decomposes or destroys the medicinal compounds of elements neither medicinal per se, nor capable of imparting any uniform medicinal powers by combination, may be considered as positively and certainly chimically incompatible with them. The only true exceptions to these rules are that medicinal powers which are dependent upon external sensible properties, are not retained in combination when such external sensible properties are lost; and vice versa, when the new compound possesses those external sensible properties that occasion medicinal powers, such powers will be possessed, whether or not their proximate or ultimate principles possessed them in their uncombined state. For example, the power that I call antiphlogistic depends in the case of the vegetable organic acids (and I believe in the chimical inorganic acids also) upon the external sensible property of sourness to the taste, so that whenever this is neutralized and destroyed by combination, their antiphlogistic power is destroyed. The power that I call oresthetic, generally depends upon the external sensible property of pungency or acrimony to the taste, so that when this is neutralized and destroyed by combination, the oresthetic pow-

er is destroyed. It is believed however that there are a few articles at least, that are decidedly oresthetic, without possessing the external sensible property of pungency or acrimony to the taste.

There is ground for suspicion that the tonic power of the vegetable bitters depends upon the external sensible property of bitterness to the taste, so that when this is neutralized and destroyed by combination, their tonic power is destroyed. However, this is not by any means certainly ascertained. A styptic or astringent power in crude vegetables depends upon the external sensible property of stypticity or astringency to the taste, so that when this is neutralized and destroyed by combination, their styptic or astringent power is destroyed. So far as I now recollect facts the same is equally true of the chymical inorganic styptics or astringents. There are cases, in which, on mingling infusions, decoctions and tinctures of crude vegetable bitter-tonics, with infusions, decoctions and tinctures of crude vegetable styptics, both the bitterness and the stypticity to the taste disappear. Now as the medicinal power of stypticity certainly depends upon the external sensible property of stypticity to the taste, the power is lost when the taste is lost; but how is it with the tonic power of the vegetable bitter? If that is lost with the external sensible property of bitterness to the taste, it will show that the tonic power of the vegetable bitter tonics depends upon the external sensible property of bitterness to the taste. The bitterness to the taste of infusions, decoctions and tinctures of *Menispermum Canadense*, *Cocculus palmatus*, *Cinchona lanceolata*, etc. is neutralized or rather destroyed by admixture with infusions, decoctions and tinctures of crude vegetable styptics, as is the stypticity to the taste of the latter. This destruction of the external sensible properties of both bitterness and stypticity to the taste, is doubtless accomplished by a combination of Tannic Acid, the styptic principle of vegetables — with the Alcaloids Calumbine (better Cocculine) of the first two articles, and Cinchonine and Quinine of the last. Is there always a combination of a salifiable base with a salifying principle, when both bitterness and stypticity to the taste are neutralized and destroyed, as above specified? Is the tonic power of these articles destroyed along with their bitterness? I strongly suspect that it is, and yet I have never determined the

matter with certainty, as desirable as it is, that it should be done.

The elements of the vegetable Alcaloids are neither medicinal per se, nor do they by combination impart any power or powers to their compounds, and yet some of the most active articles in nature are found in this class of compounds. It is not a little remarkable that the vegetable Alcaloids, in all their compounds, retain not only their medicinal powers, but also their external sensible properties, with the single exception of those which constitute their chymical alkalinity; and it is still more remarkable that some of them, though combined in equal equivalents, do not in any degree neutralize the sourness of Acids. For example, it is said that the neutral Sulphates of Narcotine and Indigotine, are just as sour to the taste as the quantities of Acid which they contain would be if uncombined. However most of them neutralize the sourness of the Acids with which they combine. Of what consequence is it whether such bodies are administered as Alcaloids, or whether they are combined with an additional equivalent of Hydrogen, and this compound Oxydized, Chloridized, Iodidized, Sulphidized etc. and then combined with an Acid, so as to constitute a salt? I do not think there is the least difference between the operation of the Alcaloid Morphine and the Chlorid of Morphinum, except what may result from difference of solubility. I do not think there is the least difference between the operation of the Alcaloid Morphine and the Iodid of Morphinum, except what may result from a difference of solubility.

So far as relates to effects as a preparation of Cinchona, I do not think there is the least difference between the operation of Quinine and Chlorid of Quininum or Iodid of Quininum; but as the dose of Quinine is so much larger than that of Morphine, we shall, in all probability, get some of the effects of Chlorine, from the Chlorid of Quininum, and we shall very certainly get some of the effects of Iodine from the Iodid of Quininum, provided this article is used with sufficient efficiency. I do not think that there is the least difference between the operation and effects of the Alcaloid Quinine and the Disulphate of Oxyd of Quininum—the least difference between the operation and effects of the Alcaloid Morphine and the neutral Sulphate of Oxyd of Morphinum—the least difference between the operation and effects of the Alcaloid Strychnine and the neutral Sulphate of Oxyd of

Strychninum, unless it may be in speed or rapidity, owing to a difference in solubility. It does not seem to be of the least consequence whether we administer the uncombined Alcaloids, or what are commonly reckoned their salts; and if we employ their salts, so considered, it is perfectly immaterial whether it may be the Sulphate, the Nitrate, the Acetate, the Tartrate, the Citrate, etc. provided only that the sourness of the Acid is neutralized. The Phosphate of Oxyd of Quininum does seem to exert a certain degree of the effects of Phosphorum, which in all probability would not be appreciable, if the doses were no larger than those of Phosphate of Oxyd of Morphinum, Phosphate of Oxyd of Strychninum etc. I have often heard it said that if the Chlorid of Morphinum, the Sulphate of Oxyd of Morphinum, or the Acetate of Oxyd of Morphinum, should be decomposed in any pharmaceutic compound, or in the stomach, and should be converted into the Tannate of Oxyd of Morphinum, it would thereby be rendered inert, or its powers very greatly impaired. For the purpose of testing the truth of this opinion, I have repeatedly prepared the Tannate of Oxyd of Morphinum, and have often employed it in medicine, and I have never been able to perceive that it is any less active, as a preparation of Papaver, than any other salt containing an equal amount of this base. As it has rather less taste than any other salt of Morphinum, it is, on this account, rather more eligible for administration to children.

None of the medicinal powers operations or effects either of Morphine or Oxyd of Morphinum, seem to depend at all upon their external sensible properties, and therefore those salts which have the least of these, are just as active as if their sensible properties were preserved in the fullest degree, and in the greatest perfection. Of what consequence then is it whether the Sulphate of Oxyd of Morphinum is decomposed in a pharmaceutic preparation, or in the stomach, and the Alcaloid extricated, or in some other way combined, or whether it remains a Sulphate? If we administer the Alcaloid, or conjoin some thing with the Sulphate of Oxyd of Morphinum that will decompose it, and extricate the Alcaloid, it may perhaps be digested a little more speedily, and possibly operate a little more kindly; but the difference is slight, and in most cases unimportant.

I suspect that this is not the fact with those Alcaloids which possess only tonic powers. I have long suspected that the power of the tonic Alcaloids had an inseparable connexion with the external sensible property of bitterness to the taste, and therefore that those salts of these Alcaloids, which lose their external sensible property of bitterness to that taste, by their salification, lose also their tonic power. If in fact the tonic power of the vegetable bitters depends upon the external sensible property of bitterness to the taste, then every chemical action of any other medicinal agent, which destroys this bitterness to the taste, must be considered as chemically incompatible with those bitters. But I know of no chemical action except the combination of Tannic Acid, and perhaps Gallic Acid, with the bitter principles of those vegetable bitter-tonics, when such principles happen to be Alcaloids, that destroys their external sensible property of bitterness to the taste. It is a curious and very remarkable fact that bark of *Cinchona lanceolata*, (and other species of *Cinchona*) should abound with Tannic Acid, without any impairment, and much more, without any destruction of its external sensible property of bitterness to the taste, or of its peculiar tonic power, and yet that a mixture of its infusion, decoction or tincture with an infusion, decoction or tincture of any crude vegetable styptic, should immediately destroy all bitterness and all stypticity to the taste of the mixture, and as would seem, all tonic, and all styptic power. This subject is worthy of further investigation.

The conversion of certain other salts into Tannates however, might destroy their power, provided that power was oresthetic, and depended upon the external sensible property of acrimony. For example, all the power of Protonitrate of Silver with which I am acquainted, is that of an oresthetic, which depends upon the external sensible property of acrimony. Now as the dose of Protonitrate of Silver is small, if it were to be made into pill with the Gall of *Quercus infectoria*, it would probably be converted into a Tannate or Gallate or both, would thereby lose its acrimony, and consequently its oresthetic power, so that this Gall may probably be said to be chemically incompatible with the Protonitrate of Silver. But very different statements are some times made in relation to this subject.

We are informed, on what is considered adequate and even high

authority, that the vegetable organic substances commonly called Alcaloids when presented in their free or uncombined state, to Chlorine, Bromine, Iodine, etc. in solution in Alcohol, enter into combination with these elements, and thereby lose all the powers not only of the Alcaloids, but of the basifying and acidifying elements in combination with them, thus becoming absolutely inert, or so nearly inert, that the compounds in question are incapable of doing injury in almost any quantities. That the Alcaloids are not decomposed and recomposed into some new form, in these compounds, is argued from the fact that they may be easily separated unchanged and in a perfect state, from the combinations in question. In the North American Medical and Surgical Journal (*No. xvii. Pgs. 231, 232, Jan. 1830*) we are told that "some very interesting results in relation to the action of Chlorine, Bromine and Iodine, upon the vegetable Alcalies" (Alcaloids rather) "have been obtained by Doune, and communicated to the Royal Academy of Sciences, among the proceedings of which they are published in the *Révue Médicale* for Sept." (1829). "Doune has found that, by the addition of Alcohol impregnated with Chlorine," (i. e. Tincture of Chlorine) "Tincture of Bromine, Tincture of Iodine," etc. "seperately to the vegetable Alcalies" (Alcaloids rather) "compounds are produced upon the precise nature of which he can not yet pronounce decidedly, but which appear to him to be Chlorids, Bromids and Iodids." "The Alcalies" (Alcaloids rather) "do not seem to be changed in the combination, for they may be separated by an Acid with which they unite, to form salts having all their usual properties, while the Chlorine, Bromine and Iodine are liberated." "But the fact which more especially concerns the medical practitioner is that these compounds, how ever powerful their bases may have been, exert no action" (influence rather) "even in large doses, upon the animal economy." "Thus the Chlorids, Bromids and Iodids of Strychnine and Brucine" (Vomicine, not Brucine) "were administered to dogs, in the dose of two grains and a half, without any dangerous effects, while half a grain of" (free and uncombined) "Strychnine is sufficient to produce Tetanic paroxysms and even death; and the Sulphate and Chlorohydrate of this salifiable base" (really Sulphate of Oxyd of Strychninum, and Chlorid of Strychninum) "are still more active, in consequence of their solubility."

“An obvious inference from these facts is that” (Chlorine, Bromine and) “Iodine may be employed as antidotes to the powerful vegetable Alkalies” (Alcaloids rather) “when they have been taken in quantities sufficient to endanger life, and the correctness of the inference appears to be established by the result of actual experiment.” “Having ascertained that Strychnine in combination with Chlorine, Bromine and Iodine, produced no dangerous symptoms when given in the dose of two grains to a healthy dog, Doune tried the effect of the uncombined Alkali” (Alcaloid rather) “followed very speedily by doses of the substances, which he conjectured, might act as antidotes.” “We select the most interesting of his experiments.” “A grain of Strychnine was given to a dog and was immediately followed by Tincture of Iodine.” “No apparent effect was produced.” “After an equal quantity of Strychnine, Tincture of Chlorine was given with the same result.” “The Tincture of Bromine was not found equally efficacious, for though swallowed immediately after a grain of Strychnine had been administered, it did not prevent the animal from dieing with Tetanic convulsions; yet in a previous experiment, the Bromid of Strychnine” (probably an erroneous name) “had proved harmless.” “Two grains of Brucine” (really Vomicine) “followed immediately by Tincture of Iodine, were innoxious, and the same quantity of Veratrine similarly treated, produced no other effect than a great deal of foaming at the mouth, which is always observed in” (brute) “animals that have taken Alcohol.” “Doune found that it was necessary to exhibit the antidote very speedily after the poison, in order to obtain its favorable operation.” “A dog which took two grains of Strychnine and eight or ten minutes afterwards, some Tincture of Iodine, died with convulsions, at the end of half an hour.”

It is remarked by the editor of the North American Medical and Surgical Journal that “it is unfortunate that only the uncombined Alkalies” (Alcaloids rather) “are thus capable of being affected by the Tinctures of Chlorine, Bromine, and Iodine, for we are more in the habit of prescribing the salts, than their bases in a separate state, and can not therefore avail ourselves of the antidote where it is most likely to be required. But though the mineral” (inorganic chemical) “Acids decompose the Chlorids, Bromids, and Iodids, is it not possible that the affinity of some

of the vegetable" (organic) "Acids is less strong, and that the saline combinations, in which the Alcalies" (Alcaloids) "are found in nature, might be decomposed, and rendered harmless, by the bodies, which prove so advantageous in neutralizing the Alcalies" (Alcaloids) "themselves?" "Should it be found that Morphine may be separated from the Meconic Acid by means of Iodine, and form with this substance an inert compound, we should have in our possession an antidote as much more valuable than all other antidotes, as Opium is more frequently taken in fatal doses, than any other poison."

I do not think that there is the least probability that these compounds are what Doune seems to suppose them. As appears to me, probabilities are very strongly against Doune's view. It is however well known that any Alcaloid may be converted into a new compound-radical by combining with a single additional equivalent of Hydrogen, which new compound-radical may be rendered basic by combination either with Oxygen, Chlorine, Bromine, Iodine, Sulphurum, Selenium, Tellurium, Arsenic, Phosphorum, and Nitrogen, which, with Fluorine, constitute the eleven basifying and acidifying elements, without the presence of one of which, there can be no salifiable base, and no salifying compound. The only exceptions to this are the bases and acids that are basified and acidified by a few basifying and acidifying compound-radicals, of which Cyanogen and Ammidogen are the most prominent. The new compound-radicals, that are formed by the combination of an additional equivalent of Hydrogen with an Alcaloid, take the name of the Alcaloid, from which they are formed, with a change of its termination from *ina* to *inum*; and its basic compounds are called respectively Oxyds, Chlorids, Sulphids, Selenids, Tellurids, etc. of the compound-radical.

The Alcaloids are in fact not basic, being utterly incapable of combining with any Acid, and all their supposed salts are really compounds of Acids, with Oxyds of those compound-radicals, which consist of an Alcaloid united with an additional equivalent of Hydrogen. Thus the common Sulphate of Morphine so called, is in reality a Sulphate of Oxyd of Morphinum, the common Disulphate of Quinine so called, is in reality a Disulphate of Oxyd of Quininum, etc. I very strongly suspect that Doune's supposed Chlorids, Bromids, Iodids, etc. of Strychnine, Veratrine,

Morphine, etc. are really and truly Chlorids, Bromids, Iodids etc. of Strychninum, Veratrinum, Morphinum, etc. commonly but erroneously supposed to be Chlorohydrate, Bromohydrate, Iodohydrate, etc. of Strychnine, Veratrine, Morphine, etc. though if such is the fact, whence the Alcaloids derived the additional equivalent of Hydrogen necessary for their conversion into Strychninum, Veratrinum, Vomycinum, Morphinum, etc. I am at a loss to say. I should think doubtless from the decomposition of water, if I could tell how the Oxygen that must be liberated, could be disposed of. But if my suspicion in regard to the constitution of the compounds that Doune obtained, is well founded, what becomes of his inferences, or conclusions? It is not true that the Chlorids the Bromids, the Iodids, etc. of Strychninum, Vomycinum, Veratrinum, Morphinum, etc. are inert. On the contrary they are active precisely in proportion to the quantity of Strychninum, Vomycinum, Veratrinum, Morphinum, etc. which they contain. The Chlorid of Morphinum is in daily use; and I have prepared enough of the Bromid and Iodid of Morphinum for fair trial, and have ascertained that their effects are in perfect analogy with the effects of Chlorid of Morphinum. If we admit Doune's hypothesis of the constitution of his compounds, it will not extricate us from difficulty in regard to his conclusions. If an Alcaloid has no change produced in its medicinal powers, operations and effects, by combination with an equivalent of hydrogen, and an equivalent of Chlorine, Bromine, Iodine, etc. it ought to have none produced by combination with an equivalent of Chlorine, Bromine, Iodine, etc. without any additional Hydrogen.

Doune's cases appear to me to prove nothing. How large were the Dogs experimented upon? Were they of the size of a Rat, or of a Wolf; for the different varieties of the species of *Canis familiaris* comprehend individuals of all sizes within this range; and surely the size of the brute animal, to which the medicine was administered, must vary the amount of effect. But I very well know that different varieties of the species *Canis familiaris* have very different degrees of susceptibility. I have known several middle-sized Dogs of a particular variety killed by a single grain of Strychnine, though this is considerably less than is ordinarily necessary for this purpose, in a Dog of such a size. On the other hand, I have repeatedly been asked by gentlemen (owning

Dogs, to which they were very much attached, but which had acquired vicious habits, that made it necessary that they should be destroyed) how much Strychnine might be necessary to destroy a middle-sized Dog with absolute certainty, and with the greatest possible speed; and after naming some times ten, some times fifteen and some times even twenty grains, I have had report that it failed of killing the brute. I now recollect one case in which a scruple was given, and the Dog shut-up over night in an otherwise empty apartment. The next morning the brute was found perfectly well to all appearance. He had not rejected the medicine. This parcel of Strychnine was so pure, that I never prescribed it in a larger dose than a sixteenth of a grain, to an adult of ordinary susceptibility, without disagreeable stupor of the lower extremities; and a twentieth of a grain was as much as most subjects could well take. Doune's maximum dose of two grains and a half of the compounds that he experimented with, is not therefore sufficient to prove that these compounds were inert, or at all events incapable of proving noxious.

I have been in the habit of dissolving the Alcaloid Morphine in Aloholic Tincture of Iodine, and thus of administering the two articles in conjunction, in cases where both were indicated; and I never was conscious of thereby failing to get all the effects of the Morphine. So far as my own observations and experience will justify a conclusion, it appears to me that the whole of Doune's statement is palpable error; and of course that his practical applications are no better. As appears to me, the whole is at variance with what long and careful investigations have convinced me are fixed principles in relation to the retention of occult medicinal powers by elements and compound principles that have entered into strict chymical combination, when such medicinal powers do not depend upon, or result from external sensible properties. And yet, the American editor, whose summary of Doune's researches and results or conclusions I have quoted, seems to give his sanction to, and to adopt the whole. He even adds some suggestions to Doune's of exactly the same character.

For myself, I have never been able to make much, if indeed any thing, of French researches in medicine, French cases, and French deductions and conclusions, or in a word, French experience. Nearly all of them are commonly at variance with my

own researches, my own cases, my own deductions and conclusions, and my own experience. The fault may be in myself; but if it is, I have hitherto been unable to get rid of it. When any thing is received and adopted from the French, I must be allowed therefore to make such objections to it, as seem to me to be obvious and prominent. A friend once said to me, in a letter, that "a Frenchman's account of a disease, and its treatment, is a record of his own imaginations, instead of the real facts of the case." Is not this strictly and literally true of what I have just quoted and commented upon? This would seem to be more generally true of the French than of any other nation, of whose medical writings I have any knowledge; but it is also true, in some instances of the English, and of U. S. A. physicians.

It is commonly alleged and admitted that Gallic Acid is chemically incompatible with the Alcaloids, by which is meant that these Alcaloids become inert medicinally, or very nearly so, by combination with this acid. But notwithstanding this allegation and admission, it is commonly said that *Colchicum autumnale* depends upon a Supergallate of Veratrine for its activity. I do not know that there is any reason to doubt that the active principle of *Colchicum autumnale* is a Supergallate of some Alcaloid or other; but whether this Supergallate is a Sesquigallate, a Bigallate, or some other Supergallate I have never seen specified. But if this is true, it affords proof that combination with Gallic Acid does not neutralize or destroy the medicinal powers of all the Alcaloids. The truth seems to be that the principal, the most important medicinal powers of *Colchicum autumnale* do not depend upon external sensible properties. It is not styptic, it is not tonic, nor is it orethetic, though perhaps it might be the latter, if it were not for the Gallic Acid. I do not believe however that the Alcaloid known in its separate state as Veratrine is the substance upon which *Colchicum* depends for its powers, because the operation and effects of *Colchicum* are so widely different from the operation and effects of *Asagræa officinalis*, from which the Veratrine of the shops is certainly obtained. It seems to me to be the most probable that the Alcaloid upon which the powers of *Colchicum* depend, is one *sui generis*, being perfectly specific in comparison with every other Alcaloid, and Colchicine would doubtless be an appropriate name, though this term has been used as a synonym of Veratrine.

It is commonly said also that the active principle of *Veratrum album* is Supergallate of Veratrine. I do not know that there is any ground for question that the acid contained in *Veratrum album*, is the Gallic Acid; and that it exists in such proportions as to form a super-salt, but whether a sesqui or a bi-salt, is not specified by any author within the compass of my knowledge. What is admitted however, is all that is necessary for my present purpose, which is to exhibit the fact that combination with Gallic Acid does not destroy the powers of the Alcaloid upon which the activity of *Veratrum album* depends. It may be proper to observe here that I never met with any express statement from any chimist to the effect that he has ever obtained what is called Veratrine from *Veratrum album*; and so far as I know, it has been concluded that Veratrine is the active principle of *Veratrum album*, because it is obtainable in abundance from an other plant, erroneously supposed to be a species of *Veratrum* by the chimist who first obtained this Alcaloid. It appears to me to be quite certain that the active principles of *Veratrum album* and *Colchicum autumnale* can not possibly be the same, because there is so much difference in the operation and effects of the two articles. Again I judge that the active principles of *Veratrum album* and *Veratrum viride* can not possibly be the same, because there is so much difference in their operation and effects. *Veratrum album* is a drastic hydragogue cathartic, while *Veratrum viride* is not cathartic at all. And yet these two species of *Veratrum* are so nearly allied botanically, that they were long considered to be the same. It will be understood from this fact that there is no safety in attempting to decide what may be the active principles of one article, from its botanical analogy with an other article deemed to be very much like it. It is said likewise that Supergallate of Veratrine is the active principle of *Veratrum Sabadilla*. I know no reason to call this statement in question, or to doubt the activity of this species. If it is active, we shall be compelled to admit that Gallic Acid does not destroy the powers of the Alcaloid, on which its operation and effects depend. It is not necessary to my present argument to determine what the Alcaloid may be, with which the Gallic Acid is combined in this agent; and yet I should be glad to know whether it has ever been determined to be Veratrine by the analysis of any chimist, or whether it is inferred to be such

because the plant is a species of *Veratrum*? Supergallate of Veratrine is alleged to be the active principle of *Asagræa officinalis*. This again is a preëminently active article notwithstanding the Alcaloid which it contains is so combined with Gallic Acid as to make a super-salt, but whether a sesqui or a bi-salt I have never seen specified. That the Alcaloid, which is contained in *Asagræa officinalis*, is that which is called Veratrine, I suppose is certain. I believe it was first obtained from this plant, and that all of it, which is now found in the shops, is obtained from it. The chimist who first obtained it, supposed the plant was a *Veratrum*, and named the Alcaloid accordingly. Has this Alcaloid ever been obtained from any true species of *Veratrum*? From these specifications I think we may conclude with absolute certainty that Gallic Acid is not chimically incompatible with the Alcaloid Veratrine commonly so called, i. e. that its medicinal powers are not destroyed by combination with this acid. We may also conclude that Gallic Acid is not chimically incompatible with the Alcaloid contained in *Colchicum autumnale*, whatever this may be, since the combination of this acid with it does not destroy its medicinal powers. We are told that "by the administration of powdered Galls," (*Nidus Cynipis scriptoriæ*) "Manna and Milk, Guibourt cured a Dog which had been poisoned by" (*Strychnos*) "Nux-vomica, and was severely affected with Tetanic convulsions; and Virey states that the same substances have been recommended by Orfila, in cases of poisoning by Opium, and the salts of Morphine," rather the salts of Oxyd of Morphinum. (*Nouvelle Bibliotheque, Aout 1829. N. A. Med. and Surg. Journ., Numb. xvii. Jan. 1830. Pg. 232.*) In powdered Galls of *Quercus infectoria*, we have both Gallic and Tannic Acids, the former, I suspect, in the greatest abundance.

In complicated cases of disease, in which *Strychnos Nux-vomica* has been indicated, perhaps for Rheumatalgia, and Papaver, and styptics for *Diarrhœa chronica* I have not infrequently employed Tincture of the Gall of *Quercus infectoria*, or in other words, the Gall of *Cynips scriptoria*, with the Alcaloid Morphine dissolved in it without apparent impairment of the powers of either of the articles. I have also some times employed mixtures of Tincture of Galls, Tincture of *Strychnos Nux-vomica* and Tincture of Opium, without any perceptible loss of power in either of the Tinc-

tures. Such preparations, or rather such mixtures, have always been made by an Apothecary according to a written formula, so that I have always neglected to ascertain how much the external sensible properties of the several Tinctures have been impaired by their mixture. So far as I know, the only power of the Gall is that of stypticity, a power which depends upon an external sensible property. Of the several powers of *Strychnos Nux-vomica*, only one, viz. its bitter tonic power, never has any connexion with an external sensible property, and I am not certain that there is any such connexion between the tonic power of *Strychnos Nux-vomica* and any of its external sensible properties. Of the several powers of *Papaver somniferum*, I am not apprized that any one of them has any connexion with external sensible properties. For myself therefore, I should not expect any impairment of any power of any ingredient of this mixture, except the stypticity of the Tincture of Galls.

On looking-back to my own practice, I can not avoid the conclusion from it that the Galls of *Cynips scriptoria* or *Quercus infectoria*, does not impair any power, or destroy any effect of *Strychnos Nux-Vomica*, unless it may possibly be its tonic power and its tonic effect. I have often administered Opium, and likewise the salts of Oxyd of Morphinum, in conjunction with the Gall of *Quercus infectoria*, (which is the nest of *Cynips scriptoria*) and without any impairment of the powers of the Opium, or the Oxyd of Morphinum. I have made Opium and the Alcaloid Morphine into pills with the powder of this Gall, and I have added the Alcaloid Morphine, and also Tincture of Opium to Alcoholic Tincture of Galls without any diminution of the powers either of the Alcaloid or the Tincture, that I could perceive. As far as I now recollect, their external sensible properties were considerably impaired, but not their medicinal powers. At the time that I employed these preparations, noticing the impairment of their external sensible properties, I was suspicious of an impairment of their medicinal powers also, but did not perceive it. How much of the effect in question was due to the Gallic Acid, and how much to the Tannic Acid I know not. I never made a regular Gallate of Oxyd of Morphinum as I have made a regular Tannate, and employed it in medicine.

It is stated that Gallic Acid by combination with various med-

icinal salifiable bases beside the Alcaloids, diminishes or destroys their medicinal powers, and therefore that Gallic Acid is chimically iccompatible with all such salifiable bases. I believe that it is the fact that Gallic Acid by combination with various medicinal salifiable bases does actually diminish their external sensible properties, and perhaps in some cases, wholly destroy them, though I believe that the latter is rather rare. When external sensible properties upon which medicinal powers depend are thus diminished or impaired, the medicinal powers thus dependent will doubtless be diminished or impaired also; but I know much less about the operation of this Acid than I do about the operation of Tannic Acid. The external sensible properties on which medicinal powers depend, that are supposed to be destroyed by Gallic Acid, are pungency or acrimony which gives rise to oresthetic powers, bitterness which gives rise to tonic powers, and stypticity to the taste, which gives rise to styptic powers. Now so far as these external sensible properties are impaired or diminished by the combination of Gallic Acid, so far this Acid is chimically incompatible with acrid oresthetics, bitter tonics and styptics, and no farther.

It is also alleged that the combination of Carbonic Acid with medicinal salifiable bases impairs or destroys their medicinal powers. I do not know that this is not the fact, because I am not acquainted with a single compound of Carbonic Acid with an Alcaloid. I believe that, as a general rule, the Carbonates of the Oxyds of the medicinal and non-alcaline metals are their weakest preparations. I do not know however that it follows from this fact that the Carbonates of the Oxyds of the compound-radicals of Hydrogen, Carbon, Nitrogen and Oxygen, are the weakest salts medicinally of these bases. In fact I do not know that there are any Carbonates of the Oxyds of these compound-radicals. If there are how ever any such Carbonates, and if these Carbonates are less acrid, less bitter and less styptic to the taste, I doubt not that they will be found to be less oresthetic, less tonic, and less styptic than other salts, in which these external sensible properties are not impaired or diminished. So far and no farther, (as appears most probable to me) can Carbonic Acid be chimically incompatible with any of the medicinal agents which I have mentioned. When the Tartrate of Sesquoxyd of Antimony and

Potassa is decomposed by Tannic Acid administered with it, and the Tannate of Sesquoxyd of Antimony is produced, this new salt (as is commonly supposed) is either inert or at least altogether in-operative, in such quantities as can be produced in this manner. Here then (if the above mentioned supposition is true) is chymical incompatibility, as some believe. But it is worthy of inquiry whether Tannate of Sesquoxyd of Antimony is inert, as is here represented. I have already stated that if Stibium or Antimony is not medicinal per se, its compounds always possess certain powers in common, which are dependent upon the Antimony that they contain. It is true that I never investigated the powers, operations, and effects of Tannate of Sesquoxyd of Antimony, but I consider it as scarcely possible that this salt should be an exception to a law so general as that to which I refer above. I doubt not therefore that Tannate of Sesquoxyd of Antimony would be found on proper investigation to be not only antiphlogistic and neuragic, but also emetic and cathartic. Till there has been such investigation, it will be safer to presume this of it, than to affirm its inertness. I believe that the Tannate of Sesquoxyd of Antimony has but little, if any solubility, so that if water of Tartrate of Sesquoxyd of Antimony and Potassa were to be added to Infusion of *Cinchona lanceolata*, the Tannate of Sesquoxyd of Antimony, which would be formed, would doubtless be precipitated, and would be lost with the dregs, unless there should be very especial care to prevent it, more care than any one unacquainted with the chymical actions of the mixture, and the habitudes and properties of the resulting compounds would be likely to exercise.

As I shall here after state more in detail, in a more appropriate place, it is said, by those who have investigated this point, that one fluid ounce of the officinal infusion or decoction of the crude bark of *Cinchona lanceolata* is capable of decomposing, and (as the additional allegation is) of rendering inert, one scruple of the Tartrate of Sesquoxyd of Antimony and Potassa, and consequently (if this is exact) a pint of such infusion or decoction must be capable of decomposing five drachms and two grains of this salt. In view of these facts, I think it is inexpedient to add Tartrate of Sesquoxyd of Antimony and Potassa to vegetable infusions containing abundance of Tannic Acid, not because Sesquoxyd of

Antimony is rendered inert by combination with this Acid, but because it is thereby rendered insoluble, and liable to be lost with such dregs as an infusion of vegetable substances almost inevitably has. It is said that *Cinchona lanceolata* is chimically incompatible with Tartrate of Antimonia and Potassa, because it decomposes it and converts the Sesquoxyd of Antimony into Tannate of Antimonia. It must be observed however that all the compounds of Antimony possess all the powers that this element ever imparts, except such as depend upon some external sensible property. A given quantity of Sesquoxyd of Antimony will therefore produce the same effects, both in quality and degree, when combined with Tannic Acid as when combined with Tartaric Acid. If this statement is true, *Cinchona lanceolata* can not be chimically incompatible with Tartrate of Antimonia and Potassa.

It is commonly alleged and admitted that Tannic Acid is chimically incompatible with the Alcaloids, by which is meant that these Alcaloids become inert medicinally, or very nearly so, by combination with this acid. This is not true of any medicinal powers ever possessed by any of the Alcaloids, except such as depend upon external sensible properties. Now the only power of any Alcaloid that ever depends upon an external sensible property, I believe, is a tonic power. It is true that some Alcaloids are extremely acrid and are therefore oresthetic, which power is destroyed along with the acrimony, by combination with Tannic Acid and quite likely Gallic Acid also; but this is never of any consequence, because no Alcaloid is ever used for its oresthetic effects, since every oresthetic Alcaloid always has other powers, which transcend its oresthetic ones, and prevent its being used as an oresthetic. Although it has been supposed that *Cinchona lanceolata* is chimically incompatible with Tartrate of Antimonia and Potassa, because the Tannic Acid of *Cinchona lanceolata* decomposes Tartrate of Antimonia and Potassa and combines with the Sesquoxyd of Antimony forming Tannate of Antimonia, yet it has never been supposed that Tartrate of Antimonia and Potassa is chimically incompatible with *Cinchona lanceolata*, since the Tannic Acid of this bark is of no consequence to its medicinal powers, operations and effects, and since the Kinnate of Oxyd of Quininum, its active principle, is not in the least altered or changed by the presence of the Tartrate of Antimonia and Potassa.

There is a medicinal incompatibility how ever between Cinchona lanceolata and Tartrate of Antimonia and Potassa, resulting from the opposing powers of these articles, the former being directly invigorating, while the latter is directly exhausting, as I state much more particularly and fully elsewhere. This is quite as important an incompatibility as a chimical one, but it is one which there is much reason for thinking is often overlooked. Indeed I have often known this to be the fact.

Both Oxygen and Arsenic are inert per se. Oxygen communicates no powers to its compounds; but the compounds of Arsenic, in certain doses, are tonic and adenagic; in certain other doses, are nauseant, emetic and sub-cathartic; in certain other doses, or under certain management, they are neuragic. Now so perfectly are all these powers possessed by all these compounds, at least all with which I am acquainted, that, by calculating the quantity of elementary Arsenic in each, its proper dose may be ascertained with sufficient accuracy for all ordinary practical purposes. I have made no mention of the orethetic power of some of the compounds of Arsenic, since this depends upon an external sensible property viz. acrimony, which is not possessed by all these compounds. According to this, both Arsenous and Arsenic Acids must be medicinally active, according to the quantity of elementary Arsenic which each contains; and in a given weight, Arsenous Acid ought to be more active than Arsenic Acid, since the former contains two equivalents of elementary Arsenic to three equivalents of Oxygen, while the latter contains two equivalents of elementary Arsenic to five equivalents of Oxygen. It is sufficient to state that in the employment of these two Acids, the practical facts are in conformity to the requirements of the law which I have stated. It is well ascertained then, that the compounds of these two Acids are medicinally active in proportion to the quantity of elementary Arsenic which they contain in combination. Suppose we are administering Arsenate of Potassa at the same time that we are employing the Disulphate of Oxyd of Quininum, and suppose that a double decomposition and recombination takes place, combining the Potassa with the Sulphuric Acid and the Oxyd of Quininum, or a portion of it, with the Arsenic Acid, of what consequence can it possibly be to the medicinal effects of the active parts of each? If the statements

which I have heretofore made, are true, it will be obvious that no decomposition of any of the ordinary medicinal compounds of Arsenic, which does not separate it from all combination, and reduce it to its elementary state, can materially affect its remedial operation. Arsenous Acid is very little soluble in water. It is for this reason that it is best to administer it in the form of Disarsenite of Potassa, this salt being sufficiently soluble. Now if we were to mingle this salt before it is taken, with some pharmaceutic preparation that should separate the Potassa from it, the Arsenous Acid would doubtless be precipitated, and might thus escape being swallowed. Such precipitation is therefore to be guarded against. It is worthy of inquiry whether the two equivalents of Potassa to one equivalent of Arsenous Acid, in the Disarsenite of Potassa, do not interfere with the tonic effects of the Arsenous Acid. This is as much to be expected a priori, as that the quantity of Potassium in Protocyanid of Potassium should impair the tone of the stomach by a protracted use of that agent. If there is no disadvantage to the effects of the Arsenous Acid from the two equivalents of Potassa, a formula for the water of Disarsenite of Potassa ought to supersede the present formula of the pharmacopœiæ, which is neither wholly the neutral Arsenite nor wholly the Disarsenite, but part the one and part the other. As the neutral Arsenite is but little more soluble than the uncombined Acid, I suspect that it is usually lost to the preparation by remaining at the bottom as a precipitate. I have often known it stated that the Water of Arsenite of Potassa is chimically incompatible with the infusion, the decoction and the tincture of *Cinchona lanceolata*. I do not see how this is made-out. Upon the supposition that the Kinic Acid is the stronger Acid, and the Potassa is the stronger base, and that a double decomposition and recomposition take place, producing Arsenite of Oxyd of Quininum, and Kinate of Potassa, neither the Arsenous Acid nor the Oxyd of Quininum will have their powers at all impaired, and much less destroyed; and as to the Kinic Acid and the Potassa, their powers will be no more affected than those of the other Acid and base. Indeed, if the powers of the Kinic Acid and the Potassa could be annihilated, it would improve the medicine. I insist therefore that there is no chimical incompatibility here. The notion that there is chimical incompatibility doubtless originated

from the observation of the fact that there is chymical action, which is all that is necessary, in the opinion of some, to constitute chymical incompatibility. It is certainly a law that the compounds of Arsenic retain their powers in all modes of combination; and it is equally a law that the vegetable compound-radicals of H. C. N. O. retain their powers in what ever state of combination they exist, provided their integrity as compound-radicals of H. C. N. O. is retained, and provided none of their powers depend upon mere external sensible properties. At any rate, all the practical facts in regard to the compounds of Arsenic and the vegetable compound-radicals of H. C. N. O. justify my conclusion. Certain binary, ternary or even quaternary compounds,* whose elements are not medicinal per se, but which by particular combination acquire medicinal powers, retain these powers, in what ever state of more complex combination they may found, provided their integrity as binary, ternary or quaternary compounds is preserved in the more complex compound. For example, Nitrogen is inert per se, and it imparts no power to its compounds, and the same is equally true of Carbon, and yet Cyanogen, a compound-radical consisting of two equivalents of Carbon, and one equivalent of Nitrogen, is among the most active substances of the *materia medica*; and there is the best reason to think that all the compounds of Cyanogen, in which its integrity as Cyanogen is preserved, possess the same powers; provided a sufficient quantity is taken to obtain a suitable dose of the Cyanogen, and provided the powers of the other ingredients of the compounds do not interfere with the effects of the Cyanogen. When in combination however, it is supposed that its powers are some times, though not often, more or less diminished, and most so in the most complex compounds. I have been in the habit of using a Spirit or Tincture of Cyanid of Hydrogen, each fluidrachm of which contained one grain of this compound. I have also used Protocyanid of Potassium, Sesquicyanid of Aluminum, Protocyanid of Lead, Protocyanid of Zinc, Diterrite or Disoxyferrite of Sesquicyanid of Iron, all except the Protocyanid of Potassium prepared

*These terms relate to the number of elements in the compound; thus Quinine is a quaternary compound, as it consists of Hydrogen, Carbon, Nitrogen and Oxygen; and Iodid of Quininum is a quinary compound as it consists of Hydrogen, Carbon, Nitrogen, Oxygen and Iodine.

by myself, and I have uniformly obtained just about the same narcotic effects from doses of all these articles, which contained the same quantity of Cyanogen.

I have elsewhere mentioned that when I have continued the use of the Protocyanid of Potassium for a considerable time, I have found it to diminish the tone of the stomach, and even of the system at large; and that I have received testimony to the same effect from other medical gentlemen. I have never seen any thing but narcotic effects from Sesquicyanid of Aluminum, nor have I any reason to suspect it of being capable of producing any other. I have never seen any but narcotic effects from Protocyanid of Lead, but if its use were to be continued long enough, I doubt not that it would produce the ultimate-neuragic effects of Lead. The emetic power of Protocyanid of Zinc, contrary to all expectation, has not infrequently been manifested from narcotic doses of this compound. It must be observed that one grain of Cyanogen is contained in two grains and twenty-two hundredths of this preparation. We should hardly expect therefore that a narcotic dose would ever vomit; but it certainly does this occasionally, though probably not often. Whether by long continuance this compound is capable of producing ultimate-neuragic effects, is more than I know. Certainly I never witnessed any such effects from it. I never witnessed any thing like tonic effects from the Diferrite or Disoxyferrite of Sesquicyanid of Iron, notwithstanding it contains six equivalents of Iron to six of Cyanogen, and three of Oxygen. I suppose that its narcotic dose is too small, since two grains and one hundred and seventy-nine thousandths contain one grain of Cyanogen. Although I have repeatedly prepared the Protocyanid of Silver and the Protocyanid of Copper, for the express purpose of ascertaining their narcotic dose, yet accidental circumstances have hitherto prevented my doing it. The Dicyanoferrite of Cyanid of Potassium has been denied to be capable of operating as a narcotic, but I doubt whether correctly. Two grains and six hundred and seventy-three thousandths of this salt(I believe)contain one grain of Cyanogen. I have never myself investigated the question of its narcotic power, though young men have repeatedly reported to me that they have found it active but without mention of the dose. A priori we should as soon expect this salt to be active as the next two, which I shall mention.

I have often employed the Cyanid of Quininum, and the Dicyanoferrite of Cyanid of Quininum, and have found both of them operative as narcotics, in just about the proper doses. I have repeatedly prepared the Cyanid of Morphinum, and also the Dicyanoferrite of Cyanid of Morphinum, but both are too active as preparations of Papaver, to be capable of useful employment as preparations of Cyanogen. It is however strictly true that when ever a decomposition or a new combination, or both takes place on mixture, between the active principles of two pharmaceutical preparations, and the new products have different powers, such preparations may be said truly to be chimically incompatible, since the changes in question necessarily destroy the original powers for which the preparations are employed. But such cases are exceedingly rare in actual practice.

I have known Water of Protoxyd of Calcium and Aromatic Sulphuric Acid, prescribed to be taken alternately at intervals of three hours. Both of these articles are oresthetic, and I think that both are moderately exhausting, the Sulphuric Acid being more active in both respects than the Protoxyd of Calcium. As the oresthetic power of both depends intirely on the external sensible property of acrimony, this power would certainly be destroyed by their entering into combination and forming Sulphate of Calcia, and I think also their moderate exhausting power. If water of Protoxyd of Calcium and Aromatic Sulphuric Acid were to be mingled in one dose, or were to be taken in immediate succession, they would certainly enter into combination, and as I think, become inert; but taken alternately and at intervals of three hours, I think they must produce all the effects, of which they are capable, before they get together, and enter into combination. This however is conjecture with me, as I have no certain knowledge of the snbject. I have known Protacetate of Lead and Aromatic Sulphuric Acid prescribed for Hemorrhage, some times to be taken alternately at intervals of three hours, and some times to be taken in conjunction. When taken in the former manner, I think it more than probable that both articles produced their medicinal effects, but when taken in the latter way, Protosulphate of Lead must have been formed, which, as I believe, is wholly destitute of all stypticity, the power for which the Protacetate of Lead is given, in such cases. Stypticity to the taste, it

must be recollected, is a mere external sensible property, which is often, if not generally, lost by change of composition. For example, stypticity to the taste is a prominent sensible property of Protacetate of Lead, but this property is wholly lost by such a change of composition as converts it into Protocarbonate, and as I believe, Protosulphate. Lead is commonly believed not to be medicinal per se, and yet it is an element that always imparts power to its compounds. Stypticity however is not one of the powers, which it uniformly imparts, but only an accidental power of some of its compounds, a power, as I have already said, depending upon an external sensible property, possessed only by a very limited number of the compounds of Lead. I think that a neuragic power is the only power that Lead invariably imparts to all its compounds. I do not think that a neuragic effect would be produced by the small amount of Protosulphate of Lead that would be produced, in the circumstances that I am considering, and in the comparatively short time that the Lead would be used.

When the Protonitrate of Silver (a comparatively active agent) and the Chlorid of Sodium (a comparatively feeble agent) mutually act upon and decompose each other, (which they do with the greatest facility) producing Chlorid of Silver and Nitrate of Soda, the products are probably inert, at least in such quantities as can be produced in this manner. This is a case of prominent chemical incompatibility. The only power which is possessed by Protonitrate of Silver is more likely to be lost or acquired by change of composition, than any other in the materia medica, as it is very often inseparably connected with and dependent upon a particular external and sensible property. It will be obvious from these premises that when Protonitrate of Silver is administered internally, the patient should be restrained as far as possible from the use of Chlorid of Sodium, with his food, if we would have the effects of the former salt. Is there not always more or less Chlorid of Sodium in the saliva, in the Pancreatic secretion, in the Gastric secretion, etc? If there is, it must be sufficient to decompose and convert into Chlorid of Silver, minute doses at least, of Protonitrate of Silver. But the doses of this agent may be so large that there may not be Chlorid of Sodium enough in the secretions into the alimentary canal, if none is taken with the food, to convert it into Chlorid of Silver, and then the undecom-

posed Protonitrate of Silver will of course produce its proper medicinal effects. When Protonitrate of Silver is given internally, should not this consideration always be had in view in the regulation of its doses? I think it should. But regulating doses according to their immediate operative effects, will accomplish the desired purpose, even in this case, at least as appears to me. I doubt not that the decomposition of the Protonitrate of Silver by the Soda of the secretions into the alimentary canal, might always be prevented, by conjoining a little free or uncombined Nitric Acid with it, when ever it is taken internally; but this would be contraïndicated in most cases, in which Protonitrate of Silver is indicated, on account of the exhausting power of free or uncombined Nitric Acid.

I have heard it maintained that the reduction of Protonitrate of Silver to Protoxyd of Silver by other and accompanying medicines, is no disadvantage to its medicinal power, operation and effects; and the Protoxyd of Silver has been introduced into the shops and prescribed instead of Protonitrate of Silver. As appears to me, this view is founded upon erroneous notions as to the true powers, operations and effects of Protonitrate of Silver. As far as I have knowledge of this matter, Protonitrate of Silver possesses but one single power, viz. that which I am in the habit of denominating oresthetic. In the case of Protonitrate of Silver, the oresthetic power is inseparably connected with, and dependent upon the external sensible property of acrimony. Now so far as I am acquainted with Protoxyd of Silver, it is destitute of all acrimony and of all oresthetic power, and so far as I have ever been able to discover, of any powers at all. However, I never prescribed it but a few times, so I may be mistaken. At all events, when I have prescribed it, I have never perceived any effect from the doses and quantities in the twenty-four hours, that I directed. It appears to me to be far more probable that the black Oxyd of Silver, which I have always supposed to be a Disoxyd, is active. Indeed I feel quite confident that this is the fact; but I shall say more on this in its proper place.

But in some cases, two pharmaceutic preparations are occasionally mixed, for the sake of the new product or products, which are formed by decomposition and recomposition into new forms, and then the chymical incompatibility is no objection to the mix-

ture. The Sulphate of Zinc and the Acetate of Lead each in solution, are often mingled for the sake of their decomposition and recomposition into a new form viz. Acetate of Zinc. For myself however, I have great doubts whether Acetate of Zinc differs materially or essentially in its medicinal powers from Sulphate of Zinc; I believe that the former is commonly supposed to be less irritant and milder in its operation than the latter, but I have never been able certainly to perceive this. However the mingling of the solutions of Sulphate of Zinc and Acetate of Lead is a sufficiently good way of getting Acetate of Zinc, when none is otherwise at hand, provided one must have this latter salt. The Sulphate of Lead is so utterly insoluble that it does not adulterate the Acetate of Zinc, even in the slightest degree.

The *Mistura Ferri composita* of the *Pharmacopœiæ*, is commonly cited as an other example of this sort, though I think very improperly. This was a nostrum or quack medicine, adopted into the *Pharmacopœiæ*, by educated physicians, though I think it might better have been left to the exclusive use of the quacks. A fluidounce is the common dose employed. To this quantity (according to Pereira's formula) there are six grains and thirty-one hundredths of powdered Myrrh—one grain and nine hundredths of Protocarbonate of Iron—one grain and sixty-five hundredths of Sulphate of Potassa—and one grain and eighty-five hundredths of Carbonate of Potassa. The Myrrh and Protocarbonate of Iron are absolutely insoluble in the liquid part of the preparation, and are usually at the bottom of the phial, after the patient has taken the liquid, while the Sulphate and the Carbonate of Potassa, are always dissolved, and therefore always taken. Between three and four grains of two antiphlogistic salts, must constitute a glorious tonic, the purpose for which this preparation is given; nor is it much better, even where a due share of the Myrrh and Protocarbonate of Iron are taken with each dose. I think that it is just about as well to leave these two ingredients at the bottom till all the liquid has been swallowed, and at last, to take the whole at a dose. There is very little probability that the empiric or whoever else contrived this formula, understood the chymical actions that take place. If these had been at all understood, there would, in all probability, have been only enough Carbonate of Potassa added to decompose the Sulphate of Iron, and the liquid (I can

not say menstruum) would have been better calculated to suspend a heavy insoluble powder like Carbonate of Iron. In fact, if the contriver of this formula had understood the chymical actions which take place, and the products which result, he would doubtless have made an immediate mixture of Carbonate of Iron and powder of Myrrh in some liquid sufficiently viscid to suspend them, at least while the doses could be poured-out and swallowed.

I do not happen to be acquainted with a single case, in which a quack-medicine adopted by educated practitioners, has proved to be of any material value. Bishopric's "Compound Vitriolic Tincture" so called, will illustrate this as well as the preparation that I have just treated of. This was introduced to the medical profession as a valuable preparation of Protosulphate of Copper and Dragon's-Blood. Now though a large quantity of the former article enters into the formula, yet the menstruum is such that not a particle of it is contained in the preparation when made. Here the menstruum is incompatible with one of the articles to be dissolved. But what is Dragon's-Blood? Some where between twelve and twenty quite different substances—substances produced by plants of different species, often of different genera, and not infrequently of different natural orders, are called Dragon's-Blood, and the formula makes no specification of which is to be used. I never yet saw two specimens of the preparation made from different parcels of Dragon's-Blood so called, that were much alike. But all the substances called Dragon's-Blood have been rejected from all our late Pharmacopœiæ, on the ground that they are either inert, or have too little activity to be worthy of being retained; and yet the quack-medicine, of which I am treating, is extensively used in some parts of our country. I have frequently employed it, at the prescription of counsellors, and in other cases, when the patient had a strong desire to take it, because some non-medical person had recommended it strongly, but I never obtained any appreciable benefit from it. If nostrums or quack-medicines were not often inert, or very nearly so, they could not be used with impunity, as they are intended to be used, viz. with no other diagnosis of the disease, except that which the patient is able to make, with no sort of knowledge of the operative effects of the article employed, and with no sort of adaptation to the peculiar susceptibility of the patient, and no accommodation to

the intensity of the disease to be relieved. Assuredly such formulæ are not worthy of adoption by educated physicians.

Cases of true and proper chymical incompatibility are then far less frequent than we should be led to suppose from Paris's Pharmacology and the writings of authors since this work of Dr. Paris. These remarks will sufficiently explain the views which I (at least) entertain of chymical incompatibility, and I think they show how it happens that every case of chymical action between different medicinal articles, is not attended with a destruction or total change of remedial power, as some, at the present period, seem to suppose. I think there can be no doubt that the vital powers of the stomach often prevent decompositions and other chymical changes, among substances which are mixed in its cavity, that would undeniably take place, were the preparations mingled before they are swallowed; but the general laws of vitality, in relation to this point, have never been very satisfactorily investigated.

As respects those decompositions, that are produced by the living stomach, contrary to the ordinary laws of chemistry, there is the best reason for concluding that nearly all, if not absolutely the whole of the remedial effects, that ever take place, are produced before this change, so that the laws which regulate these changes, are of no further consequence than as they tend to throw light upon the function of digestion. These views place chymical incompatibility upon quite a different ground from that on which it is commonly supposed to rest, at least so far as I have knowledge, but still I think they place it upon its true and proper ground.

As I have already said, medicinal incompatibility is altogether a distinct thing from chymical incompatibility. Physiological pathological, or as it is far better named, medicinal incompatibility, is not connected with any chymical changes among the ingredients of a composition, or of articles administered in conjunction or speedy succession, but it results exclusively from the opposing remedial powers of any two medicinal articles. The employment in conjunction, or as a simultaneous part of an individual course of medication, of direct exhausting and direct invigorating agents, may be mentioned as one of the best specimens of medicinal incompatibility, that can be selected. As specific examples, may be mentioned the employment of Depletion of Blood—Nitrate of Potassa—Chlorite of Potassa—Chlorate of Potassa—Carbonate of

Potassa—Sesquicarbonate of Potassa—Bicarbonate of Potassa—Carbonate of Soda—Sesquicarbonate of Soda—Bicarbonate of Soda—Bitartrate of Potassa—Tartrate of Antimonia and Potassa, or any other antiphlogistic salt, whether non-evacuant or evacuant, either in conjunction or connexion with the simple Bitter Tonics, as *Picraena excelsa*—*Gentiana lutea*—*Cocculus palmatus*—etc. or the Styptic Bitter Tonics, as the *Cinchonæ* and all analogous articles, or the Acrid Bitter Tonics, as *Liriodendron Tulipifera*—the *Magnoliæ*—the *Illicia*—the *Xanthoxyla*—*Croton Eleutheria*—*Vinum Vitis vinifera*—*Alcohol dilutum*—*Papaver somniferum*, etc.

All those Acids, which are sour to the taste, that have hitherto been employed in medicine, are more or less exhausting, and always so much so, as to prove even antiphlogistic; and this exhausting and antiphlogistic power seems to depend as much upon the external sensible property of sourness, as an orethetic power usually depends upon acrimony, or a styptic power upon stypticity to the taste. Such being the fact, it will follow of course, that such combination of these Acids as neutralizes their sourness, destroys their exhausting or antiphlogistic power. I have seldom continued the use of Sulphuric Acid, for any considerable length of time, without producing more or less of that derangement of the stomach, which results from the use of the antiphlogistics, when administered without a "*nodus vindice dignus*." Now even when it has been employed in excess, for the purpose of converting the Disulphate of Oxyd of Quininum into the neutral, or the Bisulphate, I have known the long continued use of such an acidulous solution to produce the same effects, where the stomach happened to be very weak, the Oxyd of Quininum to the contrary notwithstanding. Indeed, it is quite common with physicians to charge the exhausting effects in question upon the Oxyd of Quininum. I consider Sulphuric Acid as decidedly antiphlogistic; and in a free and non-neutralized state therefore, as medicinally incompatible with Oxyd of Quininum and all other true tonics. The same is equally true of Nitric Acid. I am inclined to think that it is true likewise of free or uncombined Phosphoric Acid. I think I have seen the tone of the stomach materially impaired by the continuous use of this Acid in an uncombined state. In this state therefore, I can not but reckon it as medicinally incompatible with invigorating agents, whether anti-

bestics or tonics, although I am acquainted with physicians who are in the habit of prescribing it as a stimulant, as their language is. When Phosphoric Acid is so combined as to neutralize all its external sensible properties, and especially its sourness to the taste, I think it produces all the effects of Phosphorum, which do not depend on the external sensible properties of that element, provided the compound which contains it is given with the proper freedom or efficiency. As appears to me, the Diprotophosphate of Iron (when not greatly adulterated) is much more effectual for warming cold extremities, and for strengthening the pulse, than any other preparation of Iron within my knowledge; and I have had testimony to the same effect from physicians to whom no reason for such a fact had ever occurred.

I have made comparative trials of Diphosphate of Oxyd of Quininum, and Disulphate of Oxyd of Quininum, and I think that the former is decidedly more invigorating and more calefacient. I have also employed the neutral Phosphate and the Biphosphate of Oxyd of Quininum. The latter speedily disagreed with the stomach in exactly the same way, that all the pharmaceutical preparations of uncombined Phosphorum (that I ever used) are so liable to do; and the former, contrary to what I expected, had some tendency to do the same, so that, on the whole, I was led to prefer the Diphosphate to the neutral Phosphate, and much more especially to the Biphosphate of Oxyd of Quininum. It is my conclusion then that though combined and neutralized Phosphoric Acid enhances the invigorating powers of compounds into which it enters, provided they can be taken in sufficient quantity; yet free or uncombined Phosphoric Acid is exhausting and medicinally incompatible with invigorants. The orethetic power of the free Acid prevents its use in sufficient quantity to get any effect from the Phosphorum.

All the Oxygen Acids of compound-radicals of Hydrogen and Carbon, that are sour to the taste, are certainly antiphlogistics of greater or less power, and therefore are medicinally incompatible with all invigorating agents, whether antisbestics or tonics. Even some of this class of Acids, that are not sour to the taste, are more or less exhausting agents, though how many, I am unable to say. Taken in a free and uncombined state, they are therefore medicinally incompatible with invigorants. But combined to the

neutralization of all their external sensible properties, I do not think that they are at all exhausting. I suppose it would be perfectly immaterial as to the medicinal powers of the salt, whether we should employ the Disulphate, the Dinitrate, the Ditartrate, the Dicitrate, or the Disacetate of Oxyd of Quininum.

Carbonic Acid held in solution by water, is a weak but pure antiphlogistic, and hence it is medicinally incompatible, as far as it goes, with invigorating agents. When combined to neutralization, I do not know that it imparts any degree of what antiphlogistic power it possesses; but so combined as to constitute a supersalt, it certainly enhances the exhausting power of an antiphlogistic salt. The Sesquicarbonate of Potassa is more exhausting than the neutral Carbonate, and the Bicarbonate is decidedly more exhausting than the Sesquicarbonate. The difference that I think I have observed in the degree of the exhausting powers of the several Carbonates of Potassa, is in the reverse order of what I should expect a priori, since Potassa is more exhausting than Carbonic Acid. On this account, I should have expected a priori that the neutral Carbonate would be more exhausting than the Sesquicarbonate, and this more exhausting than the Bicarbonate, and possibly this may be the fact in doses of the same amount. As far as I have known these salts used, the Bicarbonate has always been given in the largest doses, on account of its perfect mildness to the taste, the Sesquicarbonate in smaller doses, and the neutral Carbonate in still smaller ones, because it is quite acrid and offensive to the taste. I can not say that I have any reason to think that the single equivalent of Carbonic Acid in the Carbonate of Iron, affects its power in any degree whatever; and yet this compound of Iron has always appeared to me to be the feeblest tonic of all the preparations of Iron. In fact, when given alone, or intirely by itself, I never could perceive that it produced any tonic effect at all. I have often heard surprise expressed at the fact that Hypocarbonic Acid (Oxalic Acid) should be so much more active as an exhausting agent than Carbonic Acid; but I think that the fact is explained by the circumstance that Hypocarbonic Acid is concrete or solid, while Carbonic Acid is gaseous. If concrete or solid Carbonic Acid did not possess such intense oresthetic power as to be, in all probability, a powerful caustic, it would doubtless be more efficiently exhausting than Hypocarbonic Acid.

I have already considered the question whether Tartrate of Antimony and Potassa and Cinchona lanceolata are chimically incompatible or not; and though the affirmative of this question is commonly received, I have ventured to differ from what seems to be the universal belief, and to adopt the negative. I trust that the reasons that I have assigned for my own opinion will be deemed satisfactory. I view it as a much more important question whether these agents are medicinally incompatible or not. As the remedial effects of Tartrate of Antimony and Potassa and of Cinchona are diametrically opposed to each other, and as the proper effects of each are properly indicated only in diametrically opposite conditions of the system, I consider it certain that these two articles are always medicinally incompatible.

Among medicinal incompatibles may be reckoned the employment of cathartics along with Papaver in Diarrhœa. There are very many cases of Diarrhœa so inconsiderable, nay even so trifling, that they will be speedily suspended spontaneously, if left wholly to themselves. No inconsiderable number of these will recover under the use of cathartics, though I consider them as always contraïndicated in this disease. These cases have so little intensity, that any strong impression of any character, whether appropriate or inappropriate, will arrest them; but from the treatment of such cases as these merely, no physician can possibly derive any reliable or useful experience in regard to the treatment of those cases, that will not disappear spontaneously, and that will not yield to any mere strong impression. It is some times said that where medicines of opposite and incompatible powers are given together, if they are in equivalent doses and quantities, they will just neutralize each other; but if one predominates, only a certain degree of the effects of that article will be produced. For my part, I am intirely incredulous of this dogma. In an exquisite case of true Cauma or Synocha for example, I very much doubt whether it would be possible to give Nitrate of Potassa, Tartrate of Antimony and Potassa, or Bitartrate of Potassa, in sufficient quantity to counteract the ill effects of an efficient use of Opium, Wine, Alcohol or Disulphate of Oxyd of Quinum; and on the other hand, I very much doubt whether it is possible, by any employment of invigorating agents, i. e. antisbestics and tonics, to counteract the ill effects of an efficient use of Tartrate of

Antimonia and Potassa, Nitrate of Potassa, Bitartrate of Potassa, and the antiphlogistic purging salts, in a disease of considerable atony, or in other words, exhaustion of the sanguiferous system, and probably of all other parts dependent upon the nerve of chemical action, nutrition and reproduction. Under such circumstances, in both sets of cases, the inappropriate remedy will do more or less injury, in defiance of the free use of the appropriate one, though in all probability, not as much injury as it would do, if not accompanied with appropriate remedies. As I am most firmly persuaded, an inappropriate and improper medicine will always do far more injury than the appropriate and proper remedies can do service in conjunction. No degree of antiphlogistication that is safe, can possibly counteract the injurious effects of invigorating agents in a truly phlogistic disease; and on the contrary, no proper quantity of invigorating medicines can possibly counteract the ill effects of antiphlogistic medicines in an atonic disease, provided they are given in conjunction. The first, the great body of physicians can easily see and understand, but the second, they can not and will not either take cognizance of, or believe.

MODUS TRACTANDI MEDICAMINUM.

In this place, it may be proper to make a few remarks respecting the rules and principles, which ought to regulate the doses and the periods of repetition, of medicines not materially, or not actively evacuant. It is to a great extent, the custom among physicians, to have what is called a medium dose of most medicines, in which they almost always employ them, without the least regard to immediate operative effects. Except in the case of cathartics and emetics, this medium dose is usually an inadequate dose for all persons of ordinary susceptibility, in ordinary cases, and much more for persons of extraordinary insusceptibility and in extraordinary cases. For persons of extraordinary susceptibility this medium dose may perhaps be excessive, and the chance is, that it may not be the proper dose for one person in fifty, or even a hundred. For now and then an individual of a grade of susceptibility intermediate between extraordinary and common, it may perhaps be just the proper dose, and therefore it may produce just the proper degree of effect.

Again, it is to a great extent the custom among physicians to have a certain set period for the repetition of the doses of

almost all the medicines which they employ, whatever may be the case; a period having no exact, but only an accidental reference to the duration of the effects of the medicines, and the nature and character of the cases, in which they are employed. The consequence of this method of practice is, that the proportion of cases of chronic disease, in which any benefit is derived from medication, is so small, that the practitioner soon becomes a perfect skeptic in regard to the medicinal efficacy of most of those remedial agents, which are ordinarily employed in chronic diseases; and he therefore expunges most of them from his *materia medica*, or employs them as placebos merely, and he is likewise liable to become skeptical in regard to the efficacy, and indeed utility, of all medical treatment, in any chronic case. That this is the situation and condition of very many practitioners, more especially in small places, but also to a very considerable extent, in the larger towns, I very well know, from ample opportunities of observation, and from a great deal of testimony. Very many such practitioners greatly dislike to undertake the treatment of chronic diseases; and when they are called-upon to do so, they merely pass through a routine of prescriptions, that they have tried a hundred times, perhaps to no purpose; and when the patient after a certain time, finding that he is receiving no benefit, gets weary of the course, and will pursue it no longer, the practitioner advises him to employ no further treatment of any sort, tells him that medication can render him no service in such diseases, and advises him to trust intirely to nature, as if what he meant by nature were an intelligent agent of great skill in the practice of medicine. How many dyspeptics are there, who have gone through what is called a course of Blue Pill and starvation in this consolatory way? How many Phthisical or Consumptive persons, having gone through what is called a course of small Bleedings, perhaps Emetics and Expectorants (i. e. Squill, Senega, Tartrate of Antimonia and Potassa) and often Mercurial Salivation, all either in succession or conjunction, with starvation in addition, have been dismissed in this manner? These routine courses are usually short, because it requires but little time to ascertain their injurious effects, too short to have benefited the patient very materially, even had they been judicious and appropriate.

In acute diseases, the medication of such practitioners (unfor-

tunately for their employers) does not so soon fall into disrepute, either with themselves or with others. Acute diseases always terminate in some way or other, within a certain limited and more or less definite period of no very long duration; and this just as certainly, whether the treatment is good bad or indifferent, and (except in the case of severe malignant epidemics) by far the greatest portion of them terminate in what is called recovery. These circumstances give both the patients and the practitioner confidence in the method of practice, however little it may merit it. Few facts in medicine are capable of higher proof than that the greatest proportion of all the cases of disease, not excepting fevers (provided they are only non-malignant) which actually occur, would recover sooner or later without any medicine. The plain fact that a large majority of cases does ultimately terminate in health, under any and every mode of management, and often even in defiance of the most injudicious treatment, is of itself sufficient to verify this broad assertion. It is obvious therefore, that there must be much greater fallacy, with respect to experience in medicine, than in any other practical branch of human knowledge. If a case recovers under no treatment, or even under bad treatment, few non-medical persons can be made to understand how much less severe, and how much more brief the case might have been, under truly judicious treatment, or how much less the constitution of the patient might have suffered. There is still greater fallacy with respect to the popular and even the ordinary superficial professional decisions in regard to the success (as it is termed) of physicians. Some epidemic diseases, that are attended with what would seem to be urgent symptoms to superficial observers, to persons not skilled in medicine, and also to physicians not familiar with such cases, are nevertheless productive of very little danger; whilst others, which seem to have very little immediate urgency, and in which the ordinary bystanders and attendants think there is no danger, may in reality be intrinsically deadly. A physician may practise for a long life amongst the former, without acquiring any true experience; for if he treats every case by bleeding to the amount of thirty, sixty, or ninety ounces, and purges, vomits, sweats and salivates even drastically, with the antiphlogistic salts, Antimonials and Mercurials, almost all his patients will recover after a more or less

protracted and severe disease ; and the physician may even congratulate himself upon his signal success, without ever inquiring whether the very same cases might not have been absolutely broken-up at their outset, or rendered mild in their progress or short in their duration, by an intirely different and even opposite course ; or whether they might not have recovered more universally, with much less suffering and hazard, and with a far less shock to the constitution, if the disease had been suffered to run its course unmolested.

It is a fact capable of proof, that, to the popular eye, and even to that of superficial physicians, an unskilful and consequently an injudicious practitioner, may seem to be the most successful ; and in truth, out of the whole number of acute febrile diseases that go through a regular course under his charge, he may actually lose a considerably less proportion than the well educated, intelligent and judicious practitioner. For instance, a judicious physician may arrest, or completely break-up in the early stages and with the appearance of only a slight ephemeral illness, ninety out of every hundred cases of simple, regular and non-malignant acute fever that comes under his charge. The remaining ten ; either from the original severity of the disease, or from some untoward circumstances intirely beyond his control, may of necessity go through a regular course. Out of this ten, two cases may from their essential character, be necessarily mortal, and the remainder may in their progress, prove unavoidably severe and dangerous ; but from peculiar care and attention, and great exercise of skill, they may at last recover. On the other hand, the unskilful and the injudicious practitioner may fail of arresting or breaking-up a single case in its early stages, and by bad practice, he may render forty out of every hundred severe (though they may ultimately recover after much hazard and alarm) in addition to the ten that would inevitably have been so ; and at last, and in the event, five may die. Under such circumstances, the public (as well professional as unprofessional) would undoubtedly adjudge the palm of success to the latter, and he would acquire the reputation of superior skill in his profession, since to all appearance, he rendered one half of his cases mild, by his good management, and ultimately lost only one-twentieth of their number ; while the former apparently rendered the whole of his cases se-

vere by bad treatment as will be supposed, and finally lost one fifth. The cases that were arrested or broke-up, and ended with a mere ephemeral illness, are never regarded by the public; nor can the real skill exercised, nor the real value of the service rendered, be expected to be either understood or appreciated by the patient. I am sure that I have repeatedly known just exactly this difference between two practitioners resident in the same place, and between practitioners resident in different places, and with the same unjust decision with regard to their respective merit; and if there were no such thing as right and wrong in this world, and if there were no world to come, in which we shall be judged according to the deeds done in the body, I should say the worse a man treated his non-malignant patients, the better would it be for his reputation. It is certain however that no man can say hereafter that he has done all he could do, unless he can say also he has known all he could know. But even in acute diseases, such practitioners are very apt to fall into the custom of doing little beside bleed, vomit, purge, salivate, blister and starve, because these are the only measures from which, under their mode of management, they ever perceive any obvious effects.

The instant operation of a free bleeding, in nearly all cases, is almost always to mitigate every symptom for a very short time, even though it ultimately aggravates the disease very greatly, or perhaps decides it fatally; and when the process of vomiting or of purging or of salivation is just completed, the patient commonly feels so much better than while it was going-on, that he persuades himself it has benefited him greatly, and the physician is imposed-on in the same way. I have very often met with patients, who had been in the habit of employing such practitioners, and who could not be persuaded that any thing was doing, except when they were treated by a succession of one or more of these measures. Such practitioners seldom consider whether there are any real and true indications for what they do or not; nor do they ever trouble themselves to consider what would constitute real and true indications. Their whole plan or scheme of practice, is to counteract one disease by producing another disease so much more powerful for the time being, that the original malady hides its diminished head. But as I am fully satisfied, it may be considered certain that such a method of practice is much

worse than nothing; that it is a method which will sometimes succede, can not be denied; but very often, even when it succedes, it leaves sequels worse on the whole, than the original disease. Very often when the patient recovers under it, it is the fact that he would have recovered sooner and more perfectly, without any medication. Very often even when recovery ultimately takes place under such a course, it is the fact that it has greatly aggravated the disease, and protracted its duration.

It should be distinctly understood by every student and every practitioner of medicine, that there are always true and proper indications of treatment, and true and proper rules or principles for the administration of every remedy, both of which it behoves every physician to be master of, and without a knowledge of which, he is intirely unfit to be trusted with the charge of the sick and with the lives of his fellow men. Indications of treatment should always be founded on a full and exact knowledge of the pathological conditions, i. e. a knowledge of the general state of the system, the laws of the access, progress and termination of the disease with which a patient is affected, and its character in all other respects as well as its stage and the existing symptoms for the time being, or as many of these circumstances, as can be arrived at, together with an equally full and exact knowledge of all the powers and all the operations of the remedies to be employed, or as many of them as possible. In investigating the pathology of any and every disease not organic or structural, and not produced by a mechanical lesion, the function first affected and disturbed must be carefully ascertained. This will certainly constitute the first link in the morbid chain. The several successive disturbances of other functions must next be carefully ascertained, and the order in which they took place accurately determined. If this is correctly done, the exact and true pathological conditions of the disease will be known. The pathologist must be careful not to mistake secondary disturbances of function for primary ones; accidental ones for essential ones; and he must also be particular not to overlook any individual disturbances of any function, whether essential or accidental. This is the most important knowledge a practitioner of medicine can possibly have in regard to any and every disease—the knowledge on which all rational and all scientific practice must inevitably be

founded. The conditions and circumstances, from which the indications of treatment must be deduced, are then 1st, The general condition of those parts dependent on the great sympathetic nerve, i. e. the nerve of chymical action, nutrition and reproduction, more especially as respects entony, atony or destitution of either, etc. 2d, The specific disease, its essential pathological conditions, both primary and secondary, the laws of its access, progress and termination, together with its analogies and affinities, and particularly its nosological relations. 3d, Accidental and unessential symptoms, circumstances and conditions, and particularly the symptoms for the time being.

When I say that all rational and all scientific treatment of disease must be founded upon a knowledge of the number and succession of disturbances of function, whether essential or accidental, that constitute the disease, I intend as thorough a knowledge as can possibly be obtained, not only of the degree and quality, but also of the nature and character, and above all of the relative importance of all the several disturbances of function that may exist. These several disturbances of function, which are essential to any given disease, I am in the habit of calling the pathological conditions of that disease. A careful investigation of the true order or succession of all the disturbances of function, which constitute the essential and accidental pathological conditions of disease, would effectually prevent the very common error of mistaking secondary links in the chain of pathological conditions, for causes of the disease. Pathological conditions can only be arrived-at through the medium of symptoms; and hence it is important to observe, investigate and ascertain all the symptoms as completely and as perfectly as possible. We must beware however of incorrect reasoning, and of incorrect inferences from the symptoms. The only security against this, is a complete and perfect knowledge of anatomy and physiology on the one hand, and finally a careful weighing and comparison of all the symptoms in the aggregate, and especially with that tact and true experience, which are always acquired sooner or later, by the custom, and even habit of critical and analytical study and research. The method here pointed-out is undoubtedly the main and principal method of arriving at the pathological conditions of diseases generally, the only method for which we ought to look and on which

we ought to depend ; though results obtained in this manner, may often receive very powerful confirmation, or be intirely disproved, by the proper observation of the effects of medical treatment, provided the observer has a thorough knowledge of all the powers and operations of all his remedial agents, and of the manner in which these powers and operations are modified by variations in dose, periods of repetition, accompanying regimen, and by the nature, character, and intensity of the disease, in which the remedial agents are employed. Indeed useful knowledge may be acquired in regard to pathological conditions, by means of all manner of errors, mistakes and casualties ; but these are never to be made, for the purpose of acquiring knowledge ; we are only to avail ourselves of them whenever they occur ; and we should never forget that their frequent occurrence must inevitably redound to our discredit and even disgrace. As appears to me, there are but few diseases whose true pathological conditions may not be certainly and unequivocally arrived-at by proper investigation, i. e. by investigation of the nature and character just described. If such is the fact, well may it be inquired why so many practitioners and so many authors are at variance, and entertain such a diversity of opinions upon the subject of pathological conditions on the one hand, or omit all specification of them on the other ? I believe that this is generally if not invariably occasioned by deficient study, investigation and research, and more especially by the adoption of preconceived notions and opinions derived merely from authority— notions and opinions, that are inconsistent and incompatible with the true and genuine facts. This latter cause exists almost universally.

It is important for the practitioner to know, and to understand distinctly and definitely, that every remedial agent in the *materia medica* is capable of producing some obvious and immediate operative effects, which are a test that the system is under just the proper degree of its medicinal influence, and that the duration of those obvious and immediate operative effects in a given case is a test of the proper interval, that should be suffered to elapse between the doses. It is important also for the practitioner to know that, as a general rule, in order to produce a given effect, with all remedies not evacuant, the doses require to be large, and the intervals short, more or less in proportion to the

violence of the disease, and the intensity with which the system is occupied by it. It is likewise equally important for the practitioner to know that, with reference to all those remedial agents, which are capable of producing morbid effects by too long continuance, there are certain definite and distinct operative effects, by which we may know accurately and precisely when they should be discontinued. Still further, it is equally important for the practitioner to know that remedies require very different management in chronic and acute diseases.

Arsenous Acid may be selected to illustrate some of these principles. When Arsenous Acid is administered in such doses, as to produce and keep-up continuous nausea, when it is given in such doses as to produce and keep-up continuous vertigo, and much more when it is given in such doses as to produce a sensation of a slight burning irritation in the stomach, the dose is certainly too large ; but in an acute disease, where it is to be continued but for a short time, and where its fullest effects are desired for the whole of this short time, it may be necessary to push it till it produces a slight degree of some one of these effects, and then to continue it in a quantity just short of its production. When each dose just falls short of producing any one of these operative effects, but the whole quantity taken in the twenty-four hours purges, either the periods of repetition are too short, or the doses are too large, though in a given case, it may be necessary to its successful use, that as much should be administered as can be taken without this effect. Regulating the quantity taken by these principles strictly, I have known as much necessary as one sixth of a grain every hour, and I have known as little as a sixteenth or even a twenty-fourth of a grain, repeated only four times in the twenty-four hours, fully adequate to accomplish every thing that was desired. It will be perfectly obvious, that upon the plan of giving what is commonly called a medium dose, at comparatively long intervals, nothing could be accomplished in either set of cases which I have specified, since it would be utterly inadequate to render any service in the former, and would be excessive in the latter. Arsenous Acid is one of those agents, which is capable of producing morbid effects, if it is continued in sufficient quantities for much too long a period. Now the symptoms which indicate that it has been continued long enough in a certain quantity,

are the occurrence either of a slight Ophthalmitis Tarsi, or of a slight puffy swelling under the chin, or about the lower part of the face, or about the wrists, or the backs of the hands. Even after the occurrence of these symptoms, it may be continued with perfect safety in diminished doses, or diminished quantities, or in conjunction with Papaver in some form or other, for a very considerable time; but when it begins to produce a slight but universal Hydrops cellularis, it must be discontinued, at least for some time, though it may be pushed to this extent with the most perfect safety. Now it is plain that in acute disease, in which what is done must be done quickly, because the disease will pass through all its stages in a comparatively short time, Arsenous Acid must be given with much more freedom than in a chronic disease, which can not be cured except after a considerable time. The doses and periods of repetition that would be absolutely essential to any benefit in the former, would produce such operative effects in the latter, as would render it necessary to discontinue the remedy in the latter, long before the disease could possibly be cured, according to the established laws of its progress, duration and recovery. Where such a method is pursued, the practitioner very speedily ascertains whether the course of treatment which he has adopted, is likely to prove successful or not, and he either continues it with just and well founded confidence, or he abandons it for some other; thereby saving the patient the suffering, the loss of time, and the disappointment resulting from a long and useless course of medication, to say nothing of the well founded reputation which the physician ought to acquire in the one case, and the just discredit which ought to fall upon him in the other.

But when doses of medicine are regulated by operative effects, we must beware of confounding one operative effect with an other. It is some times the fact that uncommon susceptibility to the influence and operation of one power of an article, will prevent its being pushed far enough to obtain the effect of an other power, which may happen to be indicated. For example, Sanguinaria vernalis may be indicated for its adenagic power, i. e. its power of operating in a peculiar manner upon the secernents and absorbents, or the glandular system; and yet the susceptibility to its emetic power may be so great, that a sufficient quantity to produce

a useful degree of adenagic effect cannot possibly be taken, without a constant emetic operation. For myself, I have never been able to produce any useful degree of an adenagic operation upon an adult, with the Tincture of *Sanguinaria vernalis*, unless the patient could take it in doses of about a fluidrachm without nausea or emesis; and yet I have known patients kept for weeks upon doses of eight or ten minims, to no purpose whatever, because more would nauseate and vomit. It is very often the fact that where the adenagic operation of *Sanguinaria* is desired, and at the same time, the susceptibility of the patient to its nauseating and emetic power is very great, the practitioner will injudiciously select that preparation, which is the most nauseating and emetic, and the least adenagic in comparison with each other, as for example the infusion or the decoction; thus inevitably frustrating his own purpose, either from deficient knowledge or deficient consideration. These remarks are equally applicable to *Veratrum viride*, *Lobelia inflata*, and other emetic and non-cathartic adenagics. It is some times the fact that *Colchicum autumnale* is indicated for its adenagic power, when the susceptibility to both its emetic and cathartic operations is so great, that enough of it can not possibly be taken to produce any useful degree of the first effect, without an injurious degree of the last two effects. Under such circumstances, it is perfectly fruitless to continue it in doses short of the nauseating and emetic or purgative points, with any hope of obtaining from it any beneficial degree of adenagy. These remarks are equally applicable to *Andira inermis*, *Phytolacca decandra*, *Apocynum pubescens*, etc. as well as to every other emetic and cathartic adenagic. In regulating the doses of an article given for a single power, we should always consider whether it may not produce some non-evacuant effect, which is contraindicated. In particular cases for example, a patient may be able to take such a quantity of *Colchicum* without nausea, emesis or catharsis, as might be adequate to produce its adenagic effects, and yet it may be positively contraindicated from its exhausting operation. The absolute doses of medicines then can never be specified to any useful and practical purpose, except in connexion with particular and especial therapeutic applications, and also in connexion with the temperament and peculiar susceptibilities of the patient under the existing disease, and for the time being. If

these are truly facts, and these principles are correct, the futility of treating all cases with a routine of what is called medium doses, at comparatively long intervals, will be sufficiently obvious, and there will cease to be the least cause for wonder, why there should be so much skepticism, as to the power of remedies, and the efficacy of medical treatment, among practitioners of such a stamp. I venture to assert that every one who will adopt these rules and principles, in regard to the doses and periods of repetition of all medicines not materially and not actively evacuant, will find that much more may be accomplished by medication, than he would otherwise be inclined to believe, and that much more may be accomplished in chronic diseases, than he would otherwise suspect; and he will find also, that active evacuants will grow more and more into disrepute with him (except in true and genuine phlogistic diathesis) while his success in the suspension and the removal of disease, after the manner enjoined by Asclepiades, viz. "*officium esse medici, ut tuto, ut celeriter, ut jucunde curet,* (*Celsus, Pag. 95, lin. 12 et 13*) will be commensurate with his skill and dexterity in the management of his remedies, according to this general method; and not at all in proportion to the quantity in which he bleeds, purges, vomits, sweats and salivates, as so many of the medical profession, at the present day, as I think, incorrectly inculcate.

It is often difficult to administer remedies in the best manner, on account of the prejudices of patients. I have very often had patients who refused to take *Elaterium* in the very small doses, and at the short intervals, which is necessary for its kindest and most effectual operation. They were unwilling to take any medicine so frequently, or to take so many doses, because they could not be persuaded but that the greater the number of doses, the greater of necessity must be the quantity taken in the twenty-four hours; and therefore, I have been constrained to give it in larger doses and at longer intervals; and consequently, with the production of more or less tormina, and more or less nausea, and sometimes even vomiting; and, at the same time, with a less beneficial effect, as an adenagic and hydragogue-cathartic. I have very often also had patients refuse to take *Papaver* as frequently as I desired; and therefore, in order to obtain a proper degree of its effects, I have been obliged to give it in much larger doses, than were capable of operating in the kindest manner.

I have known very many subjects that were always more or less alarmed whenever they perceived any degree of operative effect from any medicine that they were taking, and in such case, always considered it as disagreeing with them. As soon as they ceased to perceive any effect whatever from what they were taking, they would be content. Some of these subjects would imagine that every thing taken without any perceptible operative effects, because it agrees so well with them, must necessarily render service; and they will report to their physician accordingly, though in truth they are receiving no benefit at all. I have been in the habit of testing the truth of the patient's opinion, by substituting an absolutely inert article (of course without the patient's knowledge) such as a pill of dough or bread colored slightly with *Coccus Cacti* or something similar. If I found that this answered just as well, and satisfied just as perfectly, I have always concluded that the previous medicine was of no service. To use the common language of physicians, I have been as "successful" in the treatment of such subjects with dough or bread-pills colored with *Coccus Cacti*, as with any thing that I could induce them to take for any material length of time.

Ever since I began the practice of medicine, I have constantly been in the habit of being called to prescribe for patients that can not possibly tolerate any quantity of certain medicines without seriously distressing effects. To guard against false experience derived from erroneous conclusions, I have always endeavoured to test in some certain way the notions of such patients. Perhaps there is no agent that is so often supposed to disagree with certain subjects as *Papaver*. Ever since the *Alcaloid Morphinum*, and the *Salts of the Oxyd of Morphinum* have been in use, I have employed a powder composed of *Sulphate of Oxyd of Morphinum* one part—*Camphor*—*Liquorice-root*—and prepared soft *Carbonate of Calcia*—of each twenty parts in fine powder, equally mixed. I have likewise kept an other powder differing from the former only in containing no *Sulphate of Oxyd of Morphinum*. When one of these subjects, that can not possibly tolerate *Papaver*, even in the very smallest doses and quantities, without exceedingly distressing effects, has applied to me to treat any disease requiring *Papaver*, I have usually administered the second powder, taking care that the patient should have the impres-

sion that he or she (as the case happened to be) was taking Papaver. If the patient's appetite disappeared suddenly, if there was nausea, faintness and vertigo on motion and exertion, and even sometimes retching, and that perhaps from doses of a single grain repeated four times in the twenty-four hours, I always feel safe in concluding that there is as much imagination as reality in the patient's notions, in regard to the operation of Papaver on himself or herself, as the case may be. If in addition to this, I find that the patient is as favorably affected by Papaver as any body ever is, if it is only administered without its being at all suspected, I conclude that I have saved myself from a piece of false experience. If I find that one of these patients, who can not take even the smallest quantity of Papaver without the most distressing effects, can take any quantity of a Cob-web pill, ever required by any ordinary disease, in which the proportion of the Cob-web is merely nominal and the rest of the pill is an efficient dose of Papaver, and this without the least inconvenient or disagreeable effect, I always conclude that my own experience of what this patient will tolerate and what he will not tolerate, is as good as his own. If a patient consults me for a disease in which the Disulphate of Oxyd of Quininum seems to be proper, and at the same time informs me that he can not take this agent without very distressing effects; and if on trial of the dose of a quarter of a grain four times a day, the patient complains that it produces severe stricture across the thorax, harrassing tinnitus aurium with a good deal of obtuseness of hearing, together with parched or dry mouth and fauces, I always feel desirous of administering this medicine in a form so much disguised that the patient will not know it. If after this, the patient takes two grains of this salt at a dose, dissolved in sweetened infusion of Gentian, and repeats such dose four times a day, without knowing it, and without the least unpleasant effect what ever, I consider that I have saved myself from the false experience that I should otherwise have had, provided I had taken the patient's testimony without any investigation. If a patient consults me for an extensive and long-continued Impetigo Herpetica, at the same time informing me that he can not possibly tolerate Arsenous Acid in any doses and quantities, and bringing a statement from a physician that he has tried it, in all modes and forms, but

only with the most distressing effects, and if I obtain the patient's consent only with the greatest difficulty to make one more trial of it, and immediately put him upon three bread-pills a day, each not larger than an ordinary pin's head, and find these take the appetite wholly away, produce severe nausea and faintness, vertigo and head-ache, and prevent sleep; and if afterwards I suspend these supposed Arsenical pills, and substitute Syrup of Taraxacum, each dose containing five minims of Water of Disarsenite of Potassa, and repeated every three hours, not only with the kindest effects, but with a speedy cure of the disease, I am very apt to think, even though I do not give utterance to my thoughts, that there is very often great fallacy in experience.

But a far worse set of prejudices have been sedulously inculcated and diffused, having been foisted and crowded into the gilt-covered volumes that are made chiefly to ornament a parlor table, and also to occupy the five or ten minutes which the visitor must wait for the visited to adjust the dress, etc. I here allude to the tissue of nonsense which is so much insisted-upon, at the present day, by a set of quacks affecting the highest degree of knowledge and wisdom, viz. that it is the religious duty of every one "to study the laws or principles of our animal existence," and to use "a diligent care to live according to those laws," which is affirmed to be "living according to nature." All disease is affirmed to be only "the natural and inevitable consequence of living contrary to nature," either on our own parts or the part of our parents. This set of quacks constantly insist and inculcate that "the sickness which prevails" any where "may be directly traced to the violation of the great laws, which govern our present mode of existence." This set of writers inculcate further that "the general health of society depends far more upon the ladies than upon physicians;" "that medicine is an unnatural stimulus;" "that we are as much as possible to avoid taking medicine;" that "medicine is unfriendly to the human system;" that "its inevitable effect is to disturb the regular performance of the animal functions; that "it must inevitably impair the constitution in a greater or less degree; and that impaired health is to be restored by "a return to the regular system of living according to nature." According to this class of writers, "a return to the regular system of living according to nature," consists to a very important extent

in "making your meals of a single dish" because "mixed dishes are injurious;" in avoiding "a second course which will almost certainly lead to excess;" in eating old bread instead of such as is recent; in avoiding all "stimulating drinks;" (all drinks being reckoned "stimulating" except barely cold water); in avoiding all condiments; in eating as little as will barely sustain the system; in never eating any thing between regular meals; and at all events, in never taking food oftener than at intervals of about five hours, because this time is alleged to be necessary for digesting a meal; and because taking additional food, during the process of digestion "disturbs and impedes it and makes it more laborious;" and above all, in "letting the stomach rest after the digestion of a meal," because "the stomach always needs rest, before an other meal is taken." These writers always inculcate indirectly and sometimes directly that diseases are the effect of sin, either of ourselves or of our progenitors; and I have not infrequently met with individuals, among the readers of such books, who, under the view that disease is the penalty of sin, considered it a duty to endure it without resorting to any means of relief. They indeed called upon a physician but would employ no curative measures. The whole of the preceding is derived from a single chapter of a parlor table volume, and does not constitute one half of its absurdities and follies, in relation to the same subject. The same matter, and much more of the same character, is usually contained in books professedly upon phrenology, and upon various other "ologies" with which the public is inundated, for the purpose of gulling, and picking the pocket. This is a fair sample of the matter contained in books for the people on dietetics, physiology, hygiene, pathology and therapeutics, written by persons not physicians, and usually not even educated to medicine. I do not think that a single one of all these assertions is true; but this is not the proper place to discuss and refute them.

When once a patient's mind is possessed by these notions, it is impossible to treat him under disease to any useful purpose. Every thing operates in a manner totally different upon such a subject, from what is natural, and from what it does upon a subject with no such prepossessions. If it is only supposed to be medicine, a single dose of a few drops of pure water will produce pow-

erful morbid effects ; a pill of pure bread as large as the head of a pin, supposed to be medicine, will disorder the stomach for a week, so as to prevent any regular use of food ; and two or three grains of powder of the root of Liquirice with two or three drops of Tincture of Gentian if the patient takes it for a dose of Morphine, will produce either the most violent and unmanageable vomiting, or convulsions reckoned Tetanic, by the patient and the bystanding friends, though considered as Hysterical by the physician. I have been so situated as to be obliged to prescribe now and then, for such a patient, for many weeks in succession, and though I never directed any thing but mere placebos, perfectly inert articles, which the patient would not have taken at all, had I explained their real character, but which, taken as ordinary medicines, never failed of producing extraordinary morbid effects. If the physician does not understand such cases, he will be greatly deluded or imposed-upon, by what he might otherwise esteem to be true experience, worthy of perfect reliance.

There is one other class of patients, upon which medicine never operates normally, as I suppose, partly from their prejudices, partly from a subimbecility of mind, and partly from the unnatural condition of their systems, resulting from a preposterous mode of living. I now allude to those who imagine that every body is in the habit of eating to excess ; that food ought always to be weighed and measured ; that retrenchment as to the amount of food employed by every one, is necessary ; that vegetable food exclusively is greatly preferable to a mixture of animal food with vegetable ; that condiments (perhaps with the single exception of common salt) are injurious, and always to be avoided ; that the adoption of such a plan of diet is absolutely necessary for the best bodily health, and also for the greatest mental power and activity, etc. I have often been called to prescribe for patients, under various grades and in various stages of starvation, in consequence of their entertaining such notions and opinions, and living in conformity ; and I never found any medicines to operate normally in such subjects, nor do I think they can be made to operate so by any mode of management whatever. I never could render any service to such subjects, by any mode of medical treatment that I could myself devise, or that any medical counsellor ever suggested or advised. Indeed, all the counsellors that

I ever had, in any such case, always considered any benefit from medicine as just about hopeless, although the morbid symptoms might have the form of some ordinary disease, that is usually quite under the control of medical treatment. I have repeatedly watched the progress of such cases to death, in fact by starvation, though with the symptoms of some common disease, such as *Diarrhoea chronica*, *Phthisis*, *Struma* of some part or organ, *Marasmus*, etc. without being able to palliate the symptoms that are ordinarily quite under the control of medicines, as they exist in other cases. By the time a physician is usually called, many of these cases are incapable of returning to a proper diet, the tone of the stomach being too much impaired to tolerate it. A certain number of years ago, I met with several such cases, as did various of my professional friends, particularly among students, and all, I believe, were about equally unmanageable. Their numerous occurrence at that time, was considered to be due to certain quack books upon the subject, which then had a wide circulation among all those much accustomed to general reading. Since the popularity of these works has declined, the cases under consideration have become more rare; but now and then, one is met with, which is equally incapable of having any of its symptoms met and palliated by medicine, though the same symptoms by name, as they occur in other diseases, are greatly benefited by proper treatment. I do not suppose that the anomalous operation of medicine, in such cases, is by any means wholly due to prejudices, though such patients are always as full of them as possible. I believe there is always a peculiar imbecility of mind in all such cases, though the patient always supposes that his mind was never so clear, so active, and so powerful before; and I have commonly found this impression grow stronger, till the subject was very nearly in a state of *Amentia*. But the dysthetic condition of the system at large, undoubtedly greatly affects the operation of medicines; nor can I pretend to refer to each of the above-mentioned agencies, its proper and exact share in causing the operation of medicines to be abnormal, and in rendering the cases unmanageable and incurable.

The quantity of the most appropriate medicine, which is necessary to subdue and keep in subjection the most prominent and most urgent symptoms of a given case of disease, is the best test

of the violence and severity of such diseases, provided the medicine is managed in the most judicious manner. For example, in an attack of simple and pure Diarrhœa, the quantity of Papaver necessary to subdue it and keep it in subjection, may be considered as an absolute test of the intensity, violence or severity of the case, since Papaver is the most appropriate remedy at present known for this disease, a remedy which operates directly and immediately to counteract and overcome every pathological condition essential to simple and pure Diarrhœa. The quantity or amount of purgation that might, in a given case, suspend simple and pure Diarrhœa, or that might fail of rendering any benefit or service in a given case, would afford no clue to the violence and severity of the disease, because purgation is not an appropriate process for the relief of this disease, and does not operate directly and immediately to counteract and overcome it, but only indirectly, and by virtue of mere strength of impression, the direct and immediate effect being to coincide with and increase Diarrhœa, the pathological conditions of simple and pure Diarrhœa being identical with the conditions produced for the time being by a simple and pure cathartic.

There has been a vague and indefinite opinion, not only among those who have never studied medicine, but even among its practitioners, that no medicine of any material activity can possibly be taken with perfect impunity for any considerable length of time, even in a disease and general condition of the system to which it is most appropriate, and though it should be given in doses and quantities in the twenty-four hours, that are so small as not to produce the least instant or immediate inconvenience. In short, the prevalent opinion seems to be that all medicines of any material activity, in present use, are poisons, and therefore that they can not be taken in any material quantity, and for any considerable length of time, without poisoning the patient in proportion to the quantity taken, and the length of time they are employed. According to the views in question, disease is an evil, so that if the disease is light, and the medicine active, the medicine had better not be taken; and vice versa, if the medicine is feeble and the disease intense, it is best to employ it. The employment of medicine in the treatment of disease is merely and intirely a choice between two evils; and at all times, as little of

it as possible, should be taken. I have often been surprised at the extent to which these notions and opinions prevail, much more especially among men of better education than common, who are not at the same time physicians. They prevail likewise with a numerous body of physicians, particularly with those who in fact have few professional principles, except such as are perfectly hypothetical. Such views necessarily and inevitably affect the practice of those, who entertain them, very materially and disadvantageously, usually rendering it inefficient for the time being, and hindering it from being suitably persevering and protracted. I have often attended upon patients in consultation with such practitioners, and I have even had the practitioners themselves as patients, but I could never persuade them to meet and obviate any single symptom, however important it might be; or if by mistake of an attendant, a single adequate dose happens to have been employed, they will never consent to a repetition of such dose, till the symptom ceases to return. Such physicians seem to be afraid of every thing but death.

I once lived in the neighborhood of a practitioner of this sort, who by some means fell into the habit of always calling upon me whenever he desired a counsellor. I used often to review the cases that I saw with him, so often that I remember them with great distinctness. This gentleman was always willing and ready to follow my advice, so far as a choice of remedies was concerned, but I could never in a single instance induce him to administer doses large enough, or to repeat them often enough, or to persevere long enough in the use of the medicine, to render any material service, and much less to effect a cure of any single disease. If the case happened to be capable of spontaneous cure, though only after a much longer time than would have been required for a cure by proper medicine judiciously managed, the patient recovered, but not otherwise. I am confident that I have seen numerous cases under the charge of this gentleman, linger-along for a surprising length of time, finally wearing-out and dieing, that might have been speedily and perfectly cured by medicine. I make this decision from comparison with perfectly similar cases occurring about the same time, and treated exclusively by myself, or by physicians practising upon the same plan. Some of the cases that have died under the method of management that I am discussing,

I know certainly to have been perfectly controllable by medicine, because I have tried it, when by some accident the cases were left to my exclusive charge, for a day or two. Sooner or later, I believe that such practitioners of medicine universally become skeptics as respects the power, efficacy and utility of remedial agents—universally I say, unless they acquire a better mode of practice—a thing which I have found to be more rare with this class of practitioners than with any other that I now think-of.

SUPPOSED HAZARD FROM MEDICINES.

I do not think that the least hazard of injury in any degree can ever result from any course of medicine, that is decidedly and unequivocally indicated and judiciously and properly managed, unless such medication is either directly exhausting, or what I call neuragic. These are the only exceptions (as appears to me) that need be made. Whenever any appropriate course of medical treatment is supposed therefore to have proved injurious, I think we may certainly conclude either that there has been an error as respects the indications, or some fault in the management, or else that the supposed injurious effects are ascribed to the wrong cause, or are in fact imaginary. The classes of remedies that are directly exhausting, are antiphlogistics, nausiatics, cathartics and emetics. The degree of the exhausting effects produced by nausiatics, cathartics and emetics is very various, according to the individual agent selected for the production of the effect. Numerous articles not referrible to either of these classes, are directly exhausting, but in consequence of the possession of some other power or powers; can not be employed for the relief of phlogistic or entonic diathesis, and therefore can not be reckoned as antiphlogistics—can not be advantageously employed either for the production of nausea, the production of catharsis, and the production of emesis; and therefore can not be reckoned in these classes. All such articles I include in my general statement, although my general statement is not applicable to the whole of the classes, to which I refer these articles. Perhaps there are a few agents which are directly exhausting, and yet do not belong to either of the four exhausting classes that I have specified, and yet may not be prevented from being antiphlogistic by any other power which they possess in addition. I have never employed Tannic Acid (the sole principle of stypticity in plants) for any considerable

time, even where it seemed to be the most highly indicated, without impairing the tone of the organs of primary digestion most decidedly; and yet this article is not antiphlogistic. Is this substance prevented from proving antiphlogistic by its stypticity, or is it too weak an exhausting agent to be capable of subduing any degree of phlogistic diathesis? I can not answer these questions with certainty.

All exhausting agents, even when not given in inordinate doses, or in inordinate quantities in the twenty-four hours, and when employed in truly phlogistic or entonic diseases, I believe, always produce a greater or less degree of exhaustion, beyond that which was necessary for the obviation of the phlogistic or entonic diathesis, and beyond that which would result from the disease, if left intirely to itself, provided the case is not too intense to be capable of recovering spontaneously. It is nevertheless true that several classes of medicines, beside those that are directly exhausting, if given when not really indicated, or very injudiciously persevered-in, are capable of producing morbid effects. The leantics, (demulcents and emollients of authors) if given when not indicated, impair appetite much beyond what they nourish, and sooner or later materially impair the tone of the organs of primary digestion; and yet this is probably altogether the weakest class of agents in the whole *materia medica*. The above mentioned effects of the leantics I have often witnessed in several of the species of *Phthisis*, and also in *Dyspepsia* treated upon what was called *Broussaisian* principles. Notwithstanding these facts however, there is no just ground for the apprehension of evil from any thing like a judicious employment of leantics, nor is there any good reason for pronouncing them "an unnatural stimulus," and denouncing them as "unfriendly to the human system," etc.

That class of remedies which I have been in the habit of calling *neuragics*, (a name with which I am dissatisfied, but I have not been able to devise a better) a class of remedies of which Lead may be considered as the type, when ill managed, and their use injudiciously protracted, is indeed capable of producing very serious morbid effects; but there is never any difficulty in managing them so as to obtain all their useful medicinal effects, without the least inconvenience, not to say injury to the system. I never yet saw any of their ultimate and morbid effects produced

by their use in medicine, except in one case where it was intentional, the physician supposing (whether correctly or not, I will not now attempt to decide) that nothing short of this would make any impression upon the disease in which it was employed; and in a single other case in which the patient took the management of his case into his own hands, and used the medicine for a longer time than his physician thought to be either necessary or prudent.

I have no knowledge that any ill effect ever occurs even from the most protracted use of the simple and pure narcotics. Indeed there are few classes of disease which they would aggravate, even where they render no service. I do not think that the simple and pure narcotics are capable of rendering the least service in phlogistic diseases; and in the few cases in which I have seen them administered in diseases of this sort, I have been able to discover no aggravating effects. If the simple and pure narcotics ever do mischief by protracted use, in any thing like proper doses, i. e. such doses and such quantities in the twenty-four hours as produce no instant or immediate inconvenience, I have yet to learn what this mischief is. I am aware that all this is widely variant from both medical-professional and popular opinion, and yet I think it is never the less strictly true. I have seen that class of remedies which I am in the habit of calling *erethistics*, (of which *Ignatia amara*, *Strychnos Nux-vomica*, etc. may be taken as types) used protractedly, and always without the least apparent tendency to ill effects, provided the dose, and the quantity in the twenty-four hours, produce no instant or immediate inconvenience. I have often made inquiries of other physicians, for the ill effects of a protracted use of the *erethistics*, but no one had ever witnessed any. That class of remedies that I am in the habit of calling *euphrenics* (of which *Protoxyd of Nitrogen* may be taken as the type) if used very protractedly indeed, usually produces a greater or less degree of some variety of *Limosis Syncoptica*, whose chief manifestations are a very uneasy sensation mainly, but not wholly, referred to the stomach whenever the subject is not under *euphrenic* influence. This though an exceedingly uncomfortable sensation, does not appear to be attended with any danger to the health in any other respect, and much less to life. I suspect that the prejudices which exist so generally in regard to the protracted use of the *euphrenics*, have originated from the observation of the effects

of an euphrenic possessing several other different and distinct powers ; but this will be discussed in an other, and more appropriate place.

I have no positive knowledge of any ill effects resulting from a protracted use of that class of medicines which I am in the habit of calling oresthetics, and of which Capsicum may be taken as the type. Never the less, I have always been suspicious that they might be injurious, though I have never been able to find a single well ascertained fact, that favored my suspicions. In intertropical regions Capsicum is taken with the greatest freedom, as a condiment, and that, for a long life ; and so far as I have ever been able to ascertain, with the most perfect impunity. Various other articles belonging to this class of agents, as *Zingiber officinale*, *Amomum Granum-Paradisi*, etc. are used almost in the same manner, and apparently without any ill effects. The class of agents that I am in the habit of calling antisbestics, I can not speak of as definitely as of most other classes, because I am unacquainted with any simple and pure antisbestic (this being the only class that I recognize, which is in this predicament) i. e. with any article which possesses this power alone, without any other conjoined, though all the powers that are ever conjoined, very frequently occur without any antisbestic power in conjunction. The most simple form, in which we find antisbestics, is with an oresthetic power conjoined ; but with the oresthetic power predominating over the antisbestic, as in the medicinal *Rhoës* for example, viz. *Rhus venenata*—*Rhus vernicifera* — *Rhus perniciosa* — *Rhus pumila*—*Rhus Toxicodendron*—*Rhus radicans*, etc. If it were well settled (as it would really seem to be) that a protracted use of the oresthetics, in doses and quantities in the twenty-four hours, that are not productive of the least instant or immediate inconvenience, is incapable of occasioning any morbid symptom, and much less any definite specific disease, it would seem to be easy to ascertain whether an antisbestic managed in the same manner, will produce either definite and specific disease, or any morbid symptoms, by simply employing an oresthetic antisbestic. Now I have again and again employed the medicinal *Rhoës* for three and four months in succession, and in as large doses, and quantities in the twenty-four hours, as could be well taken without instant or immediate inconvenient or unpleasant effects ; and in every case with-

out being followed by the slightest morbid symptoms; and I have had testimony to the same effect, from several friends. All however that I can say upon this subject with certainty, is that I never met with any morbid symptoms that could possibly be ascribed to a protracted use of antisbestics; nor did I ever converse with any physician who supposed that he had ever met with any.

I do not think that simple and pure bitter-tonic medicines ever do the least injury, by the continuance of their use even for years, in cases in which they may be considered as indicated by the general condition of the subject, provided always that they are not given in inordinate doses, or excessive quantities in the twenty-four hours. I am perfectly aware that a different opinion has been very generally entertained, founded as I suspect, rather on certain universally received, but never the less erroneous doctrines in regard to what has been named indirect debility, than upon any observed facts. The once celebrated *Pulvis Portlandi contra Podagram* got an ill name, and was abandoned, without further investigation. A writer of the continent of Europe says of it—"dicunt autem, paroxysmos podagricos, ejus usu supressos, secutas esse affectiones morbosas, quæ morte terminatæ sunt." (*Pharmacop. Batav. Jo. Fried. Niemann Ed. ii. Lips. 1824, vol. poster. Pag. 466.*) A United States writer says of this preparation, "its long use has been attended with fatal consequences." But what is the *Pulvis Portlandi contra Podagram*? According to the *New York Formulæ Selectæ* (1818, *Pg. 61*) it is as follows, viz.

℞ Radicis Gentianæ luteæ—

———— Aristolochiæ rotundæ—

Summitatum Erythrææ Centaurii—

———— Teucris Chamædryos—

———— Ajugæ Chamæpityos— āā.

Redige ad pulverem crassum et misce.

The formula of the *Pharmacopœia Batava*, (*Lips. 1824*) differs from the above in not containing *Erythræa Centaurium*. *Gentiana lutea*, so far as my knowledge extends, is a simple and pure bitter-tonic, the primary manifestations of whose operation are in the organs of primary digestion. The same is equally true of *Erythræa Centaurium*, so far as I am acquainted with it. The *Pharmacopœia Batava* (*Lips. 1824*) however ascribes acrimony to

it, "amaritie acri celebris." The specimens that I have seen have always been destitute of this property, but it may have been lost by age. *Aristolochia rotunda* is described in the *Pharmacopœia Batava* (*Lips.* 1824) as "saporis amariacris, odoris fortis, nauseosi." This work adds "efficaciam hujus radicis dubiam esse." I do not think so. *Teucrium Chamædrys* is described in the *Pharmacopœia Batava* (*Lips.* 1824) as "saporis amari substringentis, odoris subaromatici siccatione pereuntis." *Ajuga Chamæpitys* is described in the *Pharmacopœia Batava* (*Lips.* 1824) as "odoris resinosi instar Pini, saporis amari." "Virtute æmulatur *Teucrium Chamædryn*." Now the bitterness of all these plants seems to be of one and the same character; and as to their acrimony, none of them have a sufficient amount to vary the powers of this mixture from that of a simple and pure bitter- tonic, primarily increasing appetite and digestive power, and secondarily increasing the tone of the system at large. But that cases of Gout treated by *Pulvis Portlandi contra Podagram*, have passed into "affectiones morbosas, quæ morte terminatæ sunt" I do not doubt; but that these were produced by this medicine, I utterly disbelieve, because I have repeatedly known the simple and pure bitter-tonics used for years in succession, not only with impunity, but with advantage; and because I have known cases of Gout to take the course described, when it has been treated in various ways; and also when it has not been treated at all, but left intirely to itself. The fact that the use of this medicine "has been attended with fatal consequences" I do not consider as in any degree proving that the "fatal consequences" were at all occasioned by the medicine.

I do not think that that group of bitter-tonics, which I have denominated styptic bitter-tonics, because in their crude state they are always accompanied with more or less stypticity, but whose really important peculiarities consist in the fact that the primary manifestations of their operation are always in the sanguiferous system, ever prove injurious in consequence of continuance for any length of time, if in other respects they are managed with reasonable judgment. If I do not misremember, (I have not the statement at hand for reference) Heberden affirms that he has prescribed *Cinchona* for twelve years in succession, with only a very few short intermissions in its use, and this not only with

impunity but with great benefit to the patient. I have done the same for twenty years in permanently feeble subjects, who could not be said to be affected with any individual disease in the whole nosology, unless perhaps a slight Coprostasis, which was always perfectly obviated by the Cinchona. It is not necessary to inquire in this place respecting any other of the groups of vegetable organic tonics, since they all consist of simple bitter articles, (the primary manifestations of whose operation are in the organs of primary digestion) with some other power conjoined, which power I have already considered or shall hereafter consider.

I can not say that I have ever known any ill effects from the protracted use of the simple and pure vegetable organic styptics under any pharmaceutic preparation of the crude article. I can not say the same however of Tannic Acid (alias Stryphnic Acid or Scytodephic Acid) for I have never been able to continue the use of that article for any considerable time, in its pure separate or uncombined state, without more or less injury to the tone of the stomach, manifested earlier in some cases, and later in others. On my first observation of this fact, I was much surprised at it, since I had seen no such effect from crude vegetable organic styptics; but repeated observation of the effects of a protracted use of Tannic Acid has only served to confirm my early results.

I have long supposed that none of the Oxygen-Acids of the compound-radicals of Hydrogen and Carbon except those which are decidedly sour to the taste, are antiphlogistic, or even exhausting in a degree short of being antiphlogistic. Perhaps this may be an incorrect generalization, and perhaps all the Oxygen-Acids of compound-radicals of Hydrogen and Carbon, that are not sour to the taste, may be exhausting even when not antiphlogistic. It will be observed that even this latter generalization would exclude Benzoïc Acid as it is commonly called (it would be more in conformity with the principles of chimical nomenclature if it were called Benzhylic Acid) and a large group of other Acids of analogous composition, which I presume can not be exhausting. It will be perceived that I here allude to the Oxygen-Acids of the compound-radicals of Hydrogen Carbon and Oxygen, or of Hydrogen Carbon and some other one of the eleven basifying and acidifying elements, which we must suppose ought to have other and different powers, when they have any powers at all.

There are no pure and perfectly simple styptics of chiminal inorganic origin, but all from this source possess other powers in addition. Those that are antiphlogistic, and those that are neuragic, may of course produce ill effects from protracted use; but I do not know that any of them are capable of producing any other ill effects when used in this manner, than those which result from their antiphlogistic and neuragic operations. Those salts of Zinc which possess the highest degree of styptic power, certainly produce a greater or less degree of an exhausting effect by protracted use; and yet it does not amount to an antiphlogistic degree of exhaustion. I am perfectly aware that this is at variance with prevalent medical opinion; but let the Protosulphate of Zinc, the Protacetate of Zinc, and the Pentaciprotacetate of Zinc be employed intirely alone and by themselves protractedly, in doses and quantities in the twenty-four hours, that occasion no uneasiness or disquiet, but still constitute an efficient employment of these articles, and if they are not found primarily to impair the tone of the apparatus of primary digestion, and secondarily of the system at large, then their operation will have been different from what it has been under my observation. It is true that if these salts are accompanied with a sufficient amount of simple bitter-tonics, or with aromatics and Cantharis, or with Wine or Alcohol, the general effect may be invigorating. I prescribed them myself for a number of years thus conjoined, and on the whole, with what I deemed quite a satisfactory result; but on employing them intirely alone and by themselves, I discovered to which articles all the invigoration that I obtained, was truly due. These salts of Zinc, beside their styptic power, are neuragic, suboresthetic, emetic, sub-cathartic, and unless their exhausting power results from some one of these, this must be added to the number.

I know of no simple and pure article of the class that I have been in the habit of calling adenagics, that is capable of producing morbid effects by continuous protracted use. But then the simple and pure adenagics with which I am acquainted, are far feebler as adenagics, than numerous articles that have other powers conjoined. On the whole, I know of no good reason to conclude that a mere and pure adenagic power of no greater intensity than is possessed by such simple and pure adenagics as I am acquainted with, is ever capable of producing morbid effects by

continuous and protracted use. However, I have a very strong impression that an intense adenagic power unconnected with any other power, would assuredly do mischief by protracted use. The morbid effects I should expect, would be some grade or degree of what I shall ere long describe as ultimate adenagy. Who, that has ever seen the obstinately vitiated secretions, which that dangerous agent *Colchicum autumnale* is so liable to produce, when it is given too freely, or continued too long, can possibly deny that an adenagic power of sufficient intensity, if exerted protractedly, may be productive of undesirable and troublesome morbid symptoms. But it is not through its adenagic power that *Colchicum autumnale* is such a dangerous agent—it is through its power of producing and its liability to produce an extreme exhaustion—one incapable of obviation—of all those subordinate parts of the system, which are dependent upon the nerve of chymical action, nutrition and reproduction. This it does by what would probably be an intense antiphlogistic power, if it were not for the additional powers that are conjoined with it. As it is, I think it quite possible that this agent might prove efficiently antiphlogistic, notwithstanding its additional powers, if we only had any phlogistic or entonic diseases, in which to employ it. But I have never witnessed any such effects from the protracted use of any simple and pure adenagic; and I come to this conclusion from what I consider to be only very strong probabilities.

I have now reviewed a sufficient number of the powers of the materia medica, and the classes of articles founded on these powers, to enable any one to form a sufficiently correct opinion as to the supposed danger from a protracted and continuous use of all medicines of any material activity, in doses and quantities in the twenty-four hours, that fall short of producing any disagreeable or inconvenient effects for the time being. But some thing additional upon this topic, will be found in my disquisitions upon the respective classes hereafter. A great deal of prejudice exists in regard to the use of powerful remedies, and if one physician wishes to undermine public confidence in another, this is the theme of declamation, which, as a general rule, will best enable him to accomplish his purpose. It is one of the easiest things in the world to produce a popular horror in regard to powerful remedies, and consequently in regard to a physician who employs

them. But what is understood by a powerful remedy? If one remedy produces a given effect in the dose of a hundred and forty-four grains, and an other produces the same effect both in quality and degree, in the dose of twelve grains, the latter is more powerful than the former; but is its use therefore attended with any greater hazard? Again, if a third remedy produces the same effect both in quality and in degree, in the dose of one grain, it is more powerful still; but is its use any more hazardous? Once more, if a fourth remedy produces the same effect both in quality and degree, in the dose of a twelfth of a grain, it is more powerful still; but is its use any more hazardous? Which of the foregoing articles is to be reckoned a positively powerful remedy—a remedy so powerful as not to be safe? Must practice be confined to those articles which require the largest dose to produce a given effect, in order to be safe, or may we use articles that are operative in less doses? If we may, where shall the line be drawn? Must we reject the article that operates in the dose of one twelfth of a grain, and retain that which operates in the dose of one grain, or must we reject that which operates in the dose of one grain, and retain that which operates in the dose of twelve grains? But perhaps it may be said that by a powerful remedy is intended every remedy that, under any circumstances, is capable of doing injury. If so, every true remedy in the world is a powerful remedy, since there is none which is capable of producing any remedial effects at all, which will not, by inappropriate or excessive use, do more or less injury.

A great deal of prejudice exists in regard to experiments in the practice of medicine; and if one physician wishes to undermine public confidence in an other, this is a theme of declamation, which, as a general rule, will very well enable him to accomplish his purpose. It is one of the easiest things in the world to produce a popular horror in regard to any physician, by charging him with making experiments in his practice. I have heard of a very distinguished and eminent physician's boasting of getting rid of a competitor for business, and that even from the very bed-side of the competitor's patient, by insinuations and accusations of his making experiments in his practice. But what is an experiment in the practice of medicine? Does it imply the

employment of an article in the treatment of disease, whose powers and operations have never been investigated or ascertained, and are consequently unknown? If it does, I do not think that any educated and intelligent physician ever made an experiment in practice. For my own part, I can not conceive of any possible motive, end or object, that any such practitioner could have, for such a course. It could not benefit the patient, or the physician, or any body else, in any conceivable way or manner; and nobody of common sense and common information, would be at all likely even to think of doing such a thing—not even an *ignoramus* and quack. Does an experiment in the practice of medicine imply the employment of an article in the treatment of disease, whose powers and operations have been well investigated and ascertained, and are consequently perfectly well known, but which has never before been administered by the prescriber? If this is what is intended by an experiment in the practice of medicine, an experiment is certainly a very harmless thing. For example, the powers and operations of Cinchona or Peruvian Bark have been perfectly investigated and ascertained, and are perfectly well known to every well educated physician. Is it not as safe a remedy, the first time any particular individual practitioner ever has occasion to prescribe it, as the twentieth or the hundredth? If any thing, beside what I have specified, is ever intended by an experiment in the practice of medicine, then I am ignorant of it.

MEANS OF ASCERTAINING NEW AND PREVIOUSLY UNKNOWN
MEDICINAL POWERS.

There are seven methods that have been used as means of determining or contributing to the determination of the medicinal powers of new and previously unknown articles, viz.

1. Natural history affinities.
2. Ordinary external sensible properties.
3. Proximate chymical composition.
4. Experiments on inferior animals.
5. Experiments on the diseased human subject.
6. Experiments on the healthy human subject.
7. The careful observation of the effects of articles taken by mistake or by accident.

The absolute and the relative value of these several means for

the purpose in question, and the manner in which they are to be used deserves careful consideration.

1. Natural History Affinities as a means of determining Medicinal Powers.

Sir John Hill (without specific reference to Class, Order, or Genus) says that "in general, those plants which agree in external aspect, agree likewise in their virtues." (*Hill's Fam. Herb. Bung.* 1812, p. 370.) Baptista Porta says, "tali forma tales vires conveniunt." Petiver says "herbs of the same make and class, for the generality, have the like value and tendency to work the same effects." Cæsalpinus (as long ago as the year 1583) suggested the idea that the virtues of plants are discoverable by their structure and alliance to each other. In his work "De Plantis" he says "Quæ enim Generis societate junguntur, plerumque et similes possident facultates," i. e. those plants which belong to the same Genus generally possess similar power. Linnæus says much more specifically, that "plantæ quæ Genere conveniunt etiam virtute conveniunt—quæ Ordine naturali continentur etiam virtute propius accedunt—quæque Classe naturali congruunt etiam viribus quodammodo congruunt;" i. e. plants which belong to the same Genus have the same virtues—those which are comprised in the same Natural Order, resemble each other in their powers—and those which are referrible to the same natural Class have a more distant agreement in their properties. (*Caroli a Linnè. Philosophia Botanica, Ed. tert. cur. Carol. Ludov. Willdenov. Berol.* 1793, p. 280, Sect. 340.) It is even claimed by some that the artificial Classes of the Linnean Method have a sufficient degree of natural or botanical affinity to be usefully employed as indicators of the properties and powers, operations and effects of new and previously unknown articles. It must be recollected however that the Linnean Classes frequently coincide with a Natural Order, as Syngenesia and Tetradynamia, for example. The Class Gynandria Orders Monandria and Diandria coincide with the Natural Order Orchidaceæ, the Class Monadelphia Order Polyandria, coincides with the natural order Malvaceæ, the Class Diadelphia, Order Decandria, contains nothing but Fabaceæ, and even the Class Hexandria Order Monogynia, consists mainly of several nearly allied Natural Orders; and I have not specified any where near the whole of such cases. I conclude therefore

that those who have made the claim that I am considering, for the Classes of the Linnean Method, must have had such cases in view, as I have just specified, so that it is a matter of Natural Orders, after all. Were these aphorisms true, Genus and Natural Order would furnish us with the best possible means of ascertaining the medicinal powers of a new and unknown article, provided the powers of any other Species of the Genus, or Natural Order to which such article belonged were known. But in all probability, the exceptions to these rules are very far more numerous than the examples upon which they were founded. The remarks of Cullen on this subject, are on the whole, so judicious that I shall quote them, in this place, more especially as it is so customary at the present time to neglect the study of this highly judicious author. These remarks it is true will oblige me to forestall some things that would more properly fall into an other place, and thus make what I have to say rather immethodical and desultory; but this is a matter of no great importance.

Cullen says, "it has happened (I think unfortunately for the materia medica) that the botanists have deemed it incumbent upon them, not only to distinguish plants from one another (as is their proper business) but also to point out their medicinal virtues; a task to which they are very unequal." Cullen adds "they have however commonly attempted it, and have done it in the most imperfect (and incorrect) manner, for they have commonly compiled merely from preceding authors, with very little choice or judgment, and have thereby only multiplied useless and erroneous writings." (*Cull. Mat. med. Bart. Ed. Philad. 1812, Part I. p. 89.*) One of Cullen's annotators says, in reference to these remarks, "surely the botanists have often, very often been equal to this task." "Witness the labors of Professor Murray, in his invaluable Apparatus Medicaminum—not to mention many other botanical physicians." (*B. S. Barton. Ibidem.*) But this annotator seems to misunderstand Cullen strangely and unaccountably. Cullen very evidently has reference to the mere botanists, those who are not physicians, and who, as botanists solely. i. e. from a mere knowledge of botany, and without being physicians, have pretended to give the medicinal powers of plants. Professor John Andrew Murray was a distinguished practical physician, and it was as a physician, and not as a botanist, that he

wrote a highly useful work on *materia medica*. Though a knowledge of botany is of great, very great importance to a physician, yet mere botany will never lead us to a knowledge of the medicinal powers and operations of plants, which is all that Cullen seems to mean. One not a physician can no more have a just appreciation of many medical truths and facts, and the evidence for and against them, than an autochthon of the great south cape of Africa or of New Holland, can appreciate the evidence for and against the Copernican system of astronomy. I doubt not that any number of these autochthones might be found ready to make oath (provided an oath should be explained to them) that they had had the evidence of their senses for ten, fifteen, twenty, thirty, or fifty years, (as the case might be) that the earth stands still, and that the sun revolves around it from east to west. Every practical physician constantly and daily meets with just such testimony as this—testimony equally valueless and absurd—from clergymen, lawyers, and highly educated literary and scientific men, professors in colleges, etc. These gentlemen at once take it in dudgeon if their testimony is not deemed conclusive, and you do not immediately adopt their views and opinions founded upon exactly this sort of evidence; and they can no more understand the reason why their testimony furnishes no sort of evidence, but carries absurdity on its very face, than the autochthon of the great south cape of Africa and of New Holland, can understand why the evidence of their senses is not perfectly conclusive as respects the fixed position of the earth, and the revolution of the sun around it. In regard to astronomy they can easily understand, because they have been taught that a certain amount of preliminary knowledge is necessary to a just and correct appreciation of certain physical phenomena; but they can not understand this in regard to medicine. I never knew the literary and scientific man, who was not an intelligent physician, that could detect a quack, if he only came to him in the garb, and with the manners of a gentleman of good general intelligence. Hence undoubtedly results the fact that such persons are the principal patrons of quacks, and furnish them with much the largest amount of certificates to as great absurdities in medicine—things as inconsistent with proved and thoroughly established facts—as the stationary position of the earth, and the revolution of the sun around it,

would be in astronomy at the present day. Now let any one of these literary and scientific gentlemen write a work on *materia medica*, or on the practice of medicine, and what must it inevitably be. Cullen doubtless understood well, and thoroughly appreciated such works, though he has said so little about them. I have repeatedly known literary and scientific men, who had never read a page of medicine, to make application to non-medical corporations of our colleges, for medical professorships, apparently without the least consciousness that they could not fill them as well as the best practical physicians. I have been informed that the department of *materia medica* was taught in this way, in one of our colleges, for a considerable time. With such instructors, Flowers's advertisement of his "*Mississippi Fever Tonic*" and Kendall's & Co.'s advertisement of "*The Electrical Febrifuge or Speed's Fever Tonic*" would be of as high authority as Pereira, Stephenson and Churchill, Woodville, Cullen or Lewis. It is enough for them that the articles entering into these nostrums have powers, and are used by physicians. As I have been informed even the principles and practice of medicine have been taught in our country, by such professors as I have just mentioned, who have, it must be admitted, always been men of talent and learning, though not physicians, or having any correct notions of any thing like judicious practical medicine. How is a mere botanist to judge what is truth in *materia medica*, and what is error? The views and opinions of a mere botanist on *materia medica*, are of no more value than the views and opinions of any man in no way belonging to the medical profession. Such a writer can not fail of collecting much that is wholly unfounded and valueless. I think therefore that Cullen is perfectly correct in his remarks upon this subject.

One of the ablest and most distinguished botanists in the world, who does not appear to be at all a physician, has associated a brief *materia medica*, without ascribing it to any physician, with two of his botanical works. The motto to that, which is more especially a *materia medica*, is "*certa feram certis autoribus; haud ego vates.*" Our author must intend this to have reference to the medical part of the work, for it can not by any possibility be considered true of the botanical part. Now our author certainly has not been very successful in selecting only the "*certa*,"

nor has he been any more successful in making his selections "certis autoribus." All this is so palpable that "haud ego vates" is altogether superfluous. It is however but justice to say that it is the best work of the kind by a mere botanist, that has been written within my knowledge since the Linnean era. Indeed I think that it was probably furnished by some professional friend of the botanical author, and quite likely a distinguished one; and yet I repeat that it contains much that I should by no means expect from a practical physician, and that ought to be rejected; and I think that it is destitute of a certain sort of wholesome and in fact necessary skepticism in regard to much that is contained in common books upon the subject, and much that is delivered from physician to physician, by tradition. In a word, there is certainly a great amount of error in the *materia medica* part I am considering, which is more obvious to me in what is said of the North American articles. These errors will be noticed in the proper place. But whatever may be the demerits of the *materia medica* part of the work, yet as a whole, it possesses great value, or rather is invaluable; and long may the author survive to labor for the instruction and benefit of the whole civilized world. An American work on *materia medica*, written and published by a mere botanist a few years ago, was immensely inferior and perfectly ludicrous, as respects its execution.

Cullen says that "when the botanists found that vegetables, by a similarity in the parts of their fructification, might be arranged under certain Genera, Orders and Classes, this arrangement established" (rather is founded upon) "what is called their botanical affinities." "This affinity has been shown to apply in a considerable degree, to a great number of vegetables, though not yet to the whole of them; but wherever it has been applied to Orders and Classes, so as to show a very great similarity and affinity amongst all the several species comprehended under them, these are properly considered as Natural Orders and Classes." Cullen continues, "after these Natural Orders came to be properly established, the botanists came to perceive" (rather imagine) "that where a great botanical affinity took place, there was generally also a remarkable sameness or affinity, amongst the several species with respect to their medicinal virtues." "This (says Cullen) "is in general well founded." I do not think that this is in gene-

ral well founded by any means. It may be commonly true of Orders containing from two to half a dozen species; and it is true of the Graminaceæ, the only large order of which it can be affirmed. But even in the Graminaceæ the conformity to the rule consists in inertness, or a negation of all positive medicinal powers. I venture to say that there is no other large Order, of which a twentieth part can be shown to be in conformity to any such generalization, or any such aphorism. It is truly surprising that botanists have suffered themselves to be so much deluded and imposed upon in such a manner. But Cullen says that "such a medicinal affinity does actually take place, not only in the species of the same genus, but also to a great degree, in the species of those Orders and Classes which may be properly considered as natural." "This gives an analogy whereby we may very often presume that an untried vegetable is of the same nature and qualities with those of the same genus and Order to which it is related by a botanical affinity." (*Cull. Mat. Med. Bart. Ed. Philad. 1812. Part I, Pages 89-90.*) Cullen says, "this, to a certain extent, is truly just, and applicable with some advantage;—but," (he adds) "it is by no means so universally applicable, as the Botanists would seem to insinuate, as there are, every where," (very) "numerous exceptions to be found." "Even among the species of the same genus there is often a great difference of qualities. (*Ibidem.*) However, "in the Natural Orders," (says Cullen) "the exceptions" to the aphorisms under consideration "are every where considerable." "In some of these Orders which consist for the most part, of the mildest vegetables, there are sometimes those of a deleterious kind; and in certain Orders which" (generally) "consist of the most active and powerful substances, there are those of a very inert and mild kind."

Graminaceæ. — Paris says that even "the System" (Method rather) of Linnæus, although in a great degree artificial, corresponds in a surprising manner with the natural properties of plants; thus a plant whose calyx is a double-valved glume, with three stamina, two pistilla, and one naked seed" (there is a pericarp adherent to the seed) "bears seeds of a farinaceous and nutritious quality." (*Paris. Pharmacology 2nd Amer. Ed. fr. 5th Lond. New York, 1824, P. 62, 63, Vol. I, Note.*) But is any Class

or Order of the Linnean Method ever supposed to correspond in a surprising manner with the natural properties of plants" unless it coincides with a Natural Order? In this case all the plants falling into the Linnean Class and Order Triandria Digynia, that Paris has reference to, fall also into the Natural order Graminaceæ, so they come under his aphorism in regard to Natural Orders. As Cullen cites an example, (to be soon mentioned) from the Graminaceæ I will consider that Order. There is no large Natural Order, that is to such an extent medicinally inert and esculent, as the Graminaceæ, none that even approaches in any degree to it, though it contains at least 3800 species, and probably many more. And yet there are prominent exceptions, even in this Order, which are already known, and doubtless many not yet known. There are enough exceptions known, to teach us caution in regard to the aphorisms of the botanists. "The *Lolium temulentum*" (Darnel) in the Gramina" (ceæ, says Cullen) is an instance" (of an article) "of a deleterious kind" (in an Order) "which consists for the most part. of the mildest vegetables." (*Cull. Mat. Med. Brt. Ed. Phil.* 1812, *Part I. Pg.* 90.) *Lolium temulentum* (Linn.) is undoubtedly a narcotic, and of considerable activity. "By boiling, and by twice baking" (says an annotator of Cullen, by way of extenuating this exception) the *Lolium temulentum* or Darnel, is rendered quite innoxious." (*Ibidem.*) This is equally true of almost every other article possessing narcotic powers, and therefore it is by no means a peculiarity of the *Lolium temulentum*. But *Lolium temulentum* is by no means the only medicinally active agent among the Graminaceæ. A variety of *Paspalum scrobiculatum* called Kureck in India, which is perhaps the Gohona Grass, is reputed to be a poisonous species, and is said to render the milk of cows that graze upon it narcotic and drastic. *Festuca quadridentata* is considered as poisonous in Quito, where it is called Pigonil. *Molinia varia* is said by Endlicher to be injurious to cattle, but in what manner I know not. *Bromus mollis* is reported vaguely by some to be unwholesome, but in what way we are not informed. By others, *Bromus mollis* and *Bromus Secalinus* are said to have narcotic seeds, like *Lolium temulentum*; but is not this doubtful, especially as respects the latter, which is so common in the U. S. A.? (See *Th. Green's*

Univ. Herb. Lond. 1823, *Vol. I, Page* 198.) *Bromus catharticus* (Vahl.) and *Bromus purgans* (Linn.) are universally received to be cathartic and emetic. *Donax Arundinaceus* is subacid and styptic. *Eleusine Indica* is reputed to be medicinal, since a decoction of it is employed in Demerara, in the Convulsions of Infants. The roots of *Agropyron repens* (Torrey.) (*Triticum repens* Linn.) *Triticum glaucum*, *Triticum Junceum*, *Cynodon Dactylon*, and *Cynodon linesare*, have a reputation as substitutes for *Sarsaparilla*.

From analogy with other Natural Orders, I venture to say that if the Graminaceæ were properly and thoroughly investigated, it is much more than probable that numerous other active articles might be found, and quite possibly types of almost every other class in the materia medica besides those which I have mentioned. I have never taken the pains to number the Graminaceæ that are employed for the food of man, and of brute animals; but the catalogue of the former is scarcely greater than that of the medicinal articles, though that of the latter must doubtless be considerably more extensive. Upon the whole, a very great majority of all the species of the Graminaceæ, (as is the fact with every other large Natural Order) is not yet known to possess any medicinal powers at all, or to be esculent, either by man, or any brute animal. At the same time, there are a sufficient number of active medicinal articles to prohibit the inference of inertness in regard to any uninvestigated species. Perhaps there is not a Natural Order in which there has been less search for medicines, so that all, which are at present known, may probably be said to have been discovered by mere accident, and yet fifteen species at least, are well known to be active. In these fifteen species, we find at least three different and distinct powers, beside five species which are said vaguely to be poisonous, without any specification of the manner in which they are so. Upon the whole, therefore, I can not discover how any analogies of the least use for the investigation of the powers of a previously unknown article of this Natural Order, can possibly be derived from such a state of facts in regard to it, as I have just specified.

Cucurbitaceæ.—"The *Cucumis Melo*" (Musk Melon) (says Cullen) "is very different in its qualities from the *Cucumis Colocynthis*," commonly called *Colocynth*. What is here called *Cucumis*

Colocynthis has been correctly separated from the Genus Cucumis, and arranged under the Genus Citrullus, of which Citrullus vulgaris, or Water Melon, is the type. This is a great improvement so far as mere botanical affinities are concerned, but it exhibits as great fallacies as respects dietetic and medicinal powers, since Water Melon is as different, in all but botanical affinities, from Colocynth, as Musk Melon. Since Cullen has mentioned two plants of the Natural Order Cucurbitaceæ, it may be proper to inquire in this place, how far this Order conforms to the botanical aphorisms in regard to medicinal powers, that I have quoted, and am now considering.

R. E. Griffith says—"the general" (medicinal) "character" (of the Cucurbitaceæ) "is acridity" (acrimony is English) "and a drastic-purgative power, which is found in some part of the plant; for although the fruit of many is bland and edible, the roots and leaves are usually active and dangerous." "In some cases, the fruit or its pulp is eminently powerful, as in Elaterium and Colocynth; and there is reason to believe that the edible species owe their freedom from acrimony to cultivation; for some of them, in a wild state, are active and poisonous." "The seeds are usually mild and oleaginous, and in one species, (*Telfairia pedata*) are very large, and are used as food in Africa." "They are said to be very agreeable, and when pressed, to yield an abundance of oil, equal in flavor to that of the Olive." (*Pg. 305, R. E. Griff. Med. Bot. Philad. 1847.*) It is true that the cathartic and emetic species of the Cucurbitaceæ are commonly more or less acrid, sometimes considerably so, and sometimes only in a slight degree; but this acrimony is not of such a character, or in such a degree, as to affect the medicinal operation in any way, so far as I know. It renders the articles disagreeable to the taste, and thereby offends, and even disgusts highly sensitive subjects; and this, as appears to me, is all that need to be said of it, when these agents are not given in inordinate doses in reference to their cathartic and emetic powers. As far as I have been able to ascertain, the roots and leaves of the Cucurbitaceæ are not more frequently active than the fruit. When the fruit is active, it is quite likely that the roots and leaves possess also a greater or less degree of activity; but when the fruit is esculent, I doubt whether the roots and leaves are active. But there may be instances, of which I have

no knowledge. For myself I very greatly doubt the opinions of Lindley and Griffith that the deficiency of activity, and the mildness in the edible species, is due to cultivation. When and where were *Citrullus vulgaris* (Water Melon) *Cucumis Melo* (Musk Melon), *Cucumis sativus* (Cucumber), *Cucurbita Pepo* (Pompion), *Cucurbita verrucosa* (Squash) etc. ever found to be acrid and drastic-cathartic? I know of no evidence that they were ever found in that state, and a priori this would seem to be sufficiently improbable. If there is any evidence of it, I should like to meet with it. The gentlemen, who make this statement, may be more in the way of obtaining the proofs of it, than I am.

Lindley and R. E. Griffith notice that "De Candolle states that none of the seeds" (of the Cucurbitaceæ) "are active or poisonous," but both add that "in this, he is mistaken, as the seeds of several species of *Feuillæa* are intensely bitter, and violent emetics and cathartics, and those of *Anisospermum Passiflora*, and *Hypanthera Guapeva* are stomachic in small doses, but are cathartic in large ones." (*Pg.* 305, *R. E. Griff. Med. Bot. Philad.* 1847.) Are not the *Feuillæaceæ* or *Nhandiobaceæ* an Order truly distinct from *Cucurbitaceæ*? I believe (but do not certainly know) that St. Hilaire and Turpin adopt such an order, and I have knowledge that Martius does. Such an Order, I suppose would comprise the genera *Telfairia*, *Feuillæa*, *Zanonia*, *Alsomitra*, *Actinostemma*, *Anisosperma* and *Hypanthera*, which make Lindley's Tribe *Nhandiobææ*. Now it is true that there is a far greater uniformity in the powers of the species of this Natural Order, than in almost any other large or moderately large Order. I believe however that the powers of the majority of the species are unknown, but what are best known are on the one hand esculent with much variety of character, or on the other hand cathartic and emetic, usually drastically so, and likewise with considerable variety of character. Besides a very great variation in the degree of the drastic-cathartic and emetic powers of a group of species of this Order, some individuals of the group are adenagic in addition. This is an important fact in reference to their therapeutic applications, and this constitutes the difference between *Ecbalium Elaterium* and *Citrullus Colocynthis*, rendering the former of the greatest value for the treatment of Dropsy, and various other diseases, for which the latter is comparatively worthless. But this subject will be much more fully treated-of in its proper place.

Some species of the Cucurbitaceæ are vaguely said to be poisonous, without any specification of the manner in which they are so. It would seem as if this could not have reference to drastic-cathartic and emetic powers, as these are never (to my knowledge) reckoned as poisonous operations. Some species of Cucurbitaceæ are said to be diuretic, I suppose of course without any drastic-cathartic and emetic powers. If any operate in this manner, I strongly suspect that they do it merely as a part of an adenagic operation. My grounds for this suspicion are that several of the Cucurbitaceæ are decidedly and efficiently adenagic, and that this power and operation has commonly been overlooked, and the articles possessing and exerting it, have been generally referred to the diuretics, the diaphoretics, the expectorants (incorrectly so called) or the emmenagogues, and some times to the whole at once. Some of the Cucurbitaceæ are said to be tonic. I should like much to know whether there is just foundation for this opinion. *Trichosanthes cordata* (Roxb.) at one time supposed to be the source of the officinal Kalumb, is one of the unequivocal Cucurbitaceæ that is reckoned tonic. I should like to know whether by a sufficiently large dose, it will prove drastic-cathartic and emetic. The seeds of the species of *Fenillæa* of the *Fenillæaceæ*, are often represented as even febrifuge-tonics preferable to *Cinchona*; and yet, in a sufficient dose, they are drastic-cathartic and emetic. Physicians seem to be disposed to reckon every thing, that is bitter, as tonic—a very great error in my opinion at least. In this way even *Citrullus Colocynthis* may be made-out to be a tonic, since it is intensely bitter. Some of the Cucurbitaceæ are reckoned as stomachic. Now the word stomachic always appeared to me to be a term of very uncertain import. When I have heard it used, I have often inquired what was intended by it; and I never got a like reply from any two persons. Swediaur applies it to what I should call the aromatic *Lamiaceæ*, as for example *Mentha piperita*, etc. and other analogous articles. But Cullen says that stomachics are “medicines suited to excite and strengthen the actions of the stomach.” If this is the true import of the term, it seems to me that it must include all of that group of bitter-tonics, the primary manifestations of whose operation are in the organs of primary digestion, and what else, I shall not stop to inquire in this place. Some of the Cucurbitaceæ are reckoned

as styptic. Some of the Cucurbitaceæ are reckoned as demulcent.

So far as is known, there are at least two hundred and ten species of the two hundred and seventy Cucurbitaceæ (inclusive of Feuillæaceæ) not known to be either medicinal or esculent; thirty-five species (exclusive of Feuillæaceæ) that are cathartic, and liable to prove emetic when employed as cathartics; four species of Feuillæaceæ that are of the same character; one species of the unequivocal Cucurbitaceæ, that is reputed to be merely diuretic, it being quite doubtful whether it possesses any power at all; one species reputed to be a simple bitter-tonic; seventeen species of unequivocal Cucurbitaceæ, that are esculent; and two species of Feuillæaceæ, that are esculent. If then the Natural Order contains at least two hundred and seventy species (and probably very many more) and we are acquainted with the powers of only about sixty of these, nineteen of which are esculent, and forty-one medicinal, and in this forty-one, we find nine different powers specified by authors, it may well be inquired how much, botanical affinities in the Cucurbitaceæ, will contribute towards the acquisition of the knowledge of the powers of a previously unknown species of this Order? Very little indeed, as appears to me.

Now if a physician has a new and previously unknown Cucurbitaceous plant, whose powers he is desirous of investigating, all he can learn from the preceding is to beware of being poisoned in some mysterious manner, or of being purged and vomited inordinately, which he ought equally to do if the plant belonged to any other Natural Order, since there is scarcely a large Order which does not contain cathartics; and he must be prepared, if he is neither poisoned nor purged nor vomited for either adenagic, diuretic, stomachic, tonic, styptic, leantic, or esculent properties, or above all, no properties at all. But there is a wide difference between an esculent and a drastic-cathartic and emetic; and quite as wide a difference between these powers and tonic, styptic and leantic ones, as exists between the majority of the classes of medicines. I can not but think that all this may be as well arrived-at, and would actually be arrived-at, without any knowledge of the botanical relation of Natural Order.

Solanaceæ.—It is said by Paris that “ a plant whose flower has five stamina, one pistillum, one petalum, and whose fruit is a

berry, may at once be pronounced poisonous." (*Paris. Pharmacol. 2nd Amer. Ed. fr. 5, Lond. N. Y. 1824, Pg. 62-63, Vol. I. Introd.*) When Paris wrote this, he undoubtedly had the Solanaceæ in view; and in all probability it did not occur to him that it would be equally applicable to genera belonging to the Natural Orders Vitaceæ Myrsinaceæ, Apocynaceæ, Loganiaceæ, Sapotaceæ, Ehretiaceæ, and various others, which it would be tedious to mention. The mere fruit of the Solanaceæ is certainly not poisonous in all cases; but very often it is perfectly esculent. I believe that very many more of the fruits of this Order are destitute of poisonous properties, than possess them; and I believe that very many more are esculent than are poisonous. But the characters of the fruits have not been as often investigated as those of the plants generally. It is difficult however to believe that the fruit can be poisonous, when the rest of the plant is not so. As the Natural Order Solanaceæ is so universally mentioned as one of the best illustrations of the connexion between medicinal powers and natural botanical affinities, I shall consider this Order extensively, at the risk of being very tedious since, if there is error in relation to this subject, it is quite important that it should be known and well understood.

One of Cullen's annotators says—"I may add the different species of the Genus *Solanum* or Nightshade." "How different the qualities, in a medicinal and dietetic point of view, of the esculent *Solanum tuberosum* or Peruvian Potato, and the *Solanum nigrum*, and most other species of the Genus. (*B. S. Barton. Cull. Mat. Med. Bart. Ed. Philad. 1812, Part I, page 90.*) Now I believe it is a fact that the stem and leaves, and the expressed juice of the stem and leaves of the *Solanum tuberosum* is quite as active medicinally, as the same parts of *Solanum nigrum*, and *Solanum Dulcamara*, the officinal species, and in fact more so; and I do not doubt that if *Solanum nigrum*, and *Solanum Dulcamara* had fleshy tuberous roots like those of *Solanum tuberosum*, they would, by proper cultivation, become equally esculent. This case therefore coincides in reality with the botanical aphorism, instead of contravening it. *Solanum nigrum* and *Solanum Dulcamara* are both feeble articles in medicine; and the Genus *Solanum*, (so far as I can obtain information) is mostly a very inert group of species. I do not know that there is a single

quite active species. *Solanum paniculatum* is said to be useful in what is called "obstructions of the bowels, and especially of the liver, and in *Catarrhus vesicæ*." "Obstructions of the bowels" seem here to imply almost any chronic functional disease, and "*Catarrhus vesicæ*" seems here to mean *Blennorrhœa vesicalis*. Now to be good in these diseases, probably implies that an article is what I call adenagic. *Solanum cernuum* is said to be sudorific, and good in *Urethritis Pyoblenorrhœica*, and in *Lues Syphilis*, which probably indicates that this species is also adenagic. *Solanum Jacquini* is said to be an expectorant. *Solanum Bahamense* is said to make a good gargle for "sore throats," and *Solanum* (Burabara), a spinose species, "is reported in Demerara to be an antidote for the bite of the Rattle Snake," from all which, I venture to conclude that there is not a species of the genus known, that possesses any material activity. If there were any truly active species, we should have more definite and precise accounts of them. It is not to be omitted that *Solanum Pseudo-Quina* is said to be bitter, and is reputed to be a substitute for *Cinchona*. Martius says of *Solanum Pseudo-Quina* (*St. Hil.*) "*cortex inter optima amara febrifuga numerandus.*" (*Pg. 39, Mart. Syst. Mat. Med. Veg. Bras. Lips. et Vindob. 1843. Ord. Nat. Solanaceæ.*) *Solanum Sodomeum*, a native of the South Cape of Africa, is said to have an acrid-narcotic fruit, occasioning head-ache, stupor and delirium, and acrid-bitter roots used in Dropsy by the Hottentots. *Solanum mammosum* is said to be diuretic, but Merian, however, says that it is "poisonous to man and beasts," which, in all probability, is a mistake, as it seems to have been used in medicine, and found to be only diuretic. If those who have used it as a diuretic found no other powers, it doubtless possesses no other, since in the use of it as a diuretic, they must have been detected, if they exist. But again Morin tells us that it contains Malate of Solanine, and in the form of Extract is given in Cardialgia and Lepriasis. *Solanum pterocaulon* and *Solanum Guineëse*, are said by Martius to be emollient, anodyne and diuretic. If they really possess these powers conjoined, they can not be very active. Probably they are anodyne only so far as an emollient operation is capable of relieving pain. A Wine of *Solanum undatum* is said to be cathartic, to arrest vomiting, and to be pectoral. The juice of *Solanum Violaceum* is said

to be good in diseases of the lungs, and a decoction of it, in cutaneous eruptions. *Solanum Caavurana* (Vell.) is reputed to be bitter, resolvent, mundificant of Ulcers, Wounds, etc. *Solanum Indicum* is thought to be efficacious in vesical retention of urine. *Solanum foetidum* is said to be employed "to cleanse ulcers," but I venture to say that good soap and pure water are far better. *Solanum coagulans* in the form of expressed juice of the fruit is used in Egypt to coagulate milk. *Solanum saponaceum* has a fruit, which is a substitute for soap. The fruit of *Solanum Vespertilio* is said to be good to paint ladies cheeks, and *Solanum Gnaphalodes* equally so—a highly important use! It is worthy of inquiry whether such valuable articles could not be introduced into the U. S. A.?

The tubers of *Solanum tuberosum* are well known to be esculent, as are said to be the tubers of *Solanum Valenzualæ* (is there not a mistake in regard to the last trivial name?) and *Solanum Bulbocastanum*. *Solanum insanum*, *Solanum Melongena*, *Solanum ovigerum*, *Solanum muricatum*, etc., have esculent fruits, as have also *Solanum album*, *Solanum Æthiopicum*, *Solanum Aguivi*, and *Solanum Quitense*. Martius says of *Solanum ovigerum* that "ab Æthiopibus" (I suppose incorrectly employed for Nigritis or Negros) "fructus in phyltra receptus." (*Ibidem*. Pg. 22.) *Solanum oleraceum* and *Solanum nodiflorum* are used as common pot-herbs.

Lycopersicum esculentum (Tomato) is well known to have an esculent fruit; and Martius says that "ab Æthiopibus" (incorrectly used for Nigritis Negros) "in phyltra receptus." (*Ibidem*.) I do not think that there is a single species of the genus *Solanum*, that possesses any material activity, or is of any material value in the materia medica, unless it may possibly be *Solanum Pseudo-Quina* (St. Hil.) and I think there is just ground for doubt in regard to what is said of this. It would be a thing of immense importance to obtain a perfect substitute for Cinchona, in the treatment of Intermittent, Gangrene, etc. Now if this article is really and truly "inter optima amara febrifuga," as is represented, it is very strange that the knowledge and use of it has not been extended. On the other hand the same power is falsely attributed to a great multitude of other articles; and the probability is that it is so, in the present instance. If we could trust what is found in books, we should be compelled to believe that there are hun-

dreds of articles better than Cinchona for the treatment of Intermittent, Gangrene, etc. and yet who relies upon any thing but Cinchona, in these diseases, notwithstanding its very high price, and the great distance from which it is obliged to be brought. If any of the much vaunted substitutes were really and truly such, we should have known more of them, long before the present time. As appears to me, these remarks are applicable to Solanum Pseudo-Quina. One species of the Solanaceous genus Physalis is commonly reputed, but not certainly known to be narcotic; the fruit of several species is esculent, and reputed to be diuretic, I suppose mainly because those who eat it always void urine afterwards; and this is the most that can be said of the medicinal powers of this genus. Never the less, several species are constantly mentioned in books of materia medica, as Physalis somnifera, Physalis Alkekengi, Physalis pubescens, Physalis viscosa, Physalis angulata, etc. I do not think that there is a single species of the genus Physalis, that possesses any material activity, or is of any material importance in the materia medica. The Solanaceous Nicandra Physalodes is reputed, but not certainly known to be diuretic. So far as I know, the powers of this article have never been investigated. The leaves of some of the Peruvian Sarachæ are reputed to be bitter, emollient and anodyne, (*R. E. Griff. Med. Bot. Philad.* 1847, *pg.* 480.) In all probability it is no further anodyne than emollients relieve pain. If it had been a narcotic-anodyne, we should have been informed. Any material degree of narcotic power is not easily overlooked.

R. E. Griffith says that Lycium umbrosum "is esteemed a remedy for Erysipelas, in New Grenada." Now if the Erysipelas of all the nosologists is here intended, I venture to say that the statement can not be true; but if any thing else is intended, I must know what that is, before I can form an opinion. The term Erysipelas has crept into popular use, and is employed as vaguely, and as variously, as any other popular term, as Salt-Rheum for example. In one place, where I happen to be well acquainted, it is applied to any cutaneous disease, but by way of eminence to Lichen Urticosus. In an other place where I am also well acquainted, it is applied to any sort of Lichen; but by way of eminence to Erythema vesiculare var. Rhœinum, which is a common affection in the place, because Rhus radicans grows about every body's

house, and in every body's garden, and extensively by the street sides. I might easily mention a dozen other applications of this term, but these will suffice. One species of the Solanaceous genus *Capsicum* viz. *Capsicum toxicarium*, is reputed to be narcotic, which is said to require confirmation, while the whole are acrid, and produce the effect that I am in the habit of calling orethhetic. The most active Solanaceæ are *Atropa lethalis* or *Beladonna*; several species of *Datura*; two or three species of *Hyoscyamus*; one or two species of *Mandragora*; *Himeranthus runcinatus* (which is said to be very analogous to *Mandragora*) and several species of *Nicotiana*. It ought to be added that nothing is certainly ascertained of *Himeranthus*, though R. E. Griffith says that it is considered by the aborigines of South America, "as an aphrodisiac, and as inciting to the passion of love," which on the whole, favors the opinion that it is inert. This is but a small number of truly active articles, for such a large Natural Order. If the Natural Order Cestraceæ (*Lind. Nat. Syst. Bot. 2nd Ed. Lond. 1836.*) Cestrineæ (*Schlecht. & Mart.*) is not retained as a distinct Natural Order, it will bring a few more active plants into the Order Solanaceæ. *Acocanthéra venenata* (or *Cestrum venenatum*) is said to be very active, *Cestrum macrophyllum*, and *Cestrum nocturnum*, are said to be quite active, and so are several other species; but in the second edition of his Natural System of Botany and in his Flora Medica, Lindley puts all these into an Order called Cestraceæ. Several species of *Cestrum* are represented as being febrifuge-tonics, and substitutes for *Cinchona*. Martius says that *Cestrum Pseudo-Quina* (Mart.)—"cortex fruticis amarissimus cortici Peruviano sæpe substituitur, nec in febribus solum sese commendat intermittentibus, sed in statu atonico universali, Anhæmia, digestionis debilitate, Hydrope rel." (*Pg. 40 Mart. Syst. Mat. Med. Veg. Bras. Lips. et Vindob. 1843.*) I suspect that what I have said of *Solanum Pseudo-Quina* may be equally applicable to this article. *Cestrum Hediunda*, *Cestrum auriculatum*, and *Cestrum Laurifolium*, are represented by Lindley as febrifuge tonics; and *Cestrum euanthes*, *Cestrum lævigatum*, *Cestrum corymbosum*, *Cestrum Parqui*, and *Cestrum bracteatum*, are represented by Martius as emollient, anodyne and antidysuric. But the Cestraceæ or Cestreæ, which ever they shall be ultimately decided to

be, are a very small group. Lindley names only four genera and specifies no number of species. Several other species of *Cestrum* are mentioned in various works on *materia medica*, but are not represented as very active. I have no knowledge that the other three genera of this Order, viz. *Vestia*, *Sessea* and *Fabiana*, contain any medicinal species.

R. E. Griffith says that "the properties of this Order are not uniform." For myself I do not know of any Order in which they are, unless it may be *Simarubaceæ* or some one which is too small to allow of much variety in this respect. I should have presumed a priori that the properties of the *Salvadoraceæ* might be uniform, since the Order contains only one genus, and this genus contains only two species. However we are told that one of these is epispastic, and the other purgative. So much for the correspondence of medicinal powers with botanical affinities. R. E. Griffith never the less says "the general character" (of the *Solanaceæ*) is that of the acro-narcotics" (acrid-narcotics, there being no such word as acro, either in English or Latin) "but the fruit of some species, and the roots of others are among the most valuable esculents, and the fruits of others again are purely stimulant." (*R. E. Griff. Med. Bot. Philad.* 1847, p. 479.) As far as I can ascertain the number of acrid-narcotics in this Natural Order must be exceedingly small. Perhaps *Solanum Sodomæum*, and possibly *Solanum mammosum* may be of this character. I must here say that I very strongly doubt whether this Natural Order contains a single true and proper stimulant, i. e. a single article which is capable of producing "a quickly diffused and transient increase of vital energy and strength of action, in the sanguiferous system," or in any other subordinate part of the animal economy. When Dr. R. E. Griffith mentioned stimulants in this Order, I suppose that he must have had reference to the single small genus *Capsicum*; but all the species are mere acrids without the least power of increasing vital energy and strength of action in any manner, or in any part. The properties ascribed by Lindley to the *Solanaceæ* are 1st, "Narcotic, or otherwise poisonous;" I think by far the smallest group; 2nd, "Tonic," consisting so far as I know, of a single species; 3rd, "Diuretic," which I think should be adenagic, few in number; 4th, "Pungent," and as I think, being oresthetic; one small genus; 5th, "Bland

and inert," and I think he should have added some times esculent. So far as is known this last group constitutes by far the largest portion of this Natural Order. The Solanaceæ in fact furnish only a few active medicinal agents, though the whole Natural Order comprises nine hundred species. As appears to me, if these are all the medicinal species of the Solanaceæ, and there is such a diversity of powers among them, and so large a portion are feeble agents, there has been "great cry and little wool," and the Solanaceæ will not contribute very much to confirm the aphorisms, that I am considering. But Cullen was merely speaking of the several species of a genus, which his commentator has extended to a Natural Order. Cullen however does not omit to consider how far the botanical affinities of Natural Order justify the inference of similarity of medicinal powers.

Scrofulariaceæ. — Cullen mentions "*Verbascum* among the *Luridæ*, or *Solanaceæ*," (as an example of an article) "of a very inert and mild kind," (though belonging to an Order which generally) "consists of the most active and powerful substances." "A better illustration of Cullen's idea" (says an annotator) "might have been offered." "*The Verbascum Thapsus* (Linn.) is not wholly an innoxious plant," (as Cullen seems to suppose). "It has at least, some thing narcotic in its constitution." "The leaves made into pills, kill fish" (this I doubt) "animals, it is true, very susceptible to the influence of narcotics; and I am assured that an infusion of the leaves is anthelmintic." (*B. S. Barton, Cull. mat. med. Philad. 1812, Part I. p. 90.*) Notwithstanding this observation of Cullen's annotator, it is still my opinion that *Verbascum Thapsus*, (as well as other *Verbascæ*) is a well selected illustration of Cullen's position, i. e. it is my opinion that not only *Verbascum Thapsus*, but also various other species, are medicinally inert, even though they should be admitted to belong to the Natural Order *Solanaceæ*, or *Luridæ*, as Linnæus calls it. Almost any vegetable infusion, or any vegetable powder, however inert it may be, if put into a large tub of water, or a small pond or stream, containing small fish, will first stupefy, and subsequently kill them, at least in most cases. I suspect (but do not pretend to know) that this is accomplished by some disturbance of the function of the branchiæ (bronchiæ or gills) i. e. by a hindrance of the arterialization or decarbonization of the blood and not

produced by a narcotic power, and possibly not by any true medicinal power. In this way I suspect that *Verbascum Thapsus* operates to destroy fish, when it does destroy them. However this subject has not been properly investigated, and therefore nothing can be said of it with certainty, one way or the other. A friend once undertook to prove to me that the leaves of *Comptonia Asplenifolia* (Aiton.) were narcotic by showing that they would stupefy small fish, in a tub of water. I caused him to try Oak-leaves in the same manner, and they proved equally effectual. Now I trust that the Natural Order Solanaceæ has already been shown to consist mainly of articles of a very inert and mild kind, at least so far as our present knowledge extends, and that the proportion of truly active articles, which it contains, is in fact very small in comparison with its whole number of species. *Verbascum* therefore agrees well enough with it, and as far as it goes, favors the aphorisms, that I am combating, provided we receive it as a Solanaceous genus. But the genus *Verbascum* does not belong to the Solanaceæ, but to the Scrofulariaceæ; an Order however, which contains the genus *Digitalis* with several active species, of which *Digitalis purpurea* is the best known. As therefore a genus belonging to the Natural Order Scrofulariaceæ, is mentioned by Cullen in connexion with this subject, it may be useful to inquire how far this Order favors or contravenes the aphorisms of the botanists which I am considering. So far as I have knowledge, *Digitalis purpurea* (Linn.) is the most active species of this Order. It is universally agreed that this agent is a powerful narcotic of a peculiar character; and that it is likewise diuretic. The real truth is that it is adenagic, being diuretic only as a part of an operation upon the whole secernents and absorbents or glandular system. If inordinate and excessive quantities are taken within a short time, it proves emetic and cathartic; but it can never be usefully employed in medicine for these operations and effects. *Digitalis ochroleuca* (Jacq.) *Digitalis lævigata* (Waldst. and Kit.) and *Digitalis ferruginea* (Linn.) are said to possess the same powers as *Digitalis purpurea*. In De Candolle's *Prodromus*, *Digitalis ochroleuca* (Jacq.) is given as a synonym of *Digitalis grandiflora* (Allion. and De Cand.) This is undoubtedly the species intended above. *Verbascum nigrum* (Linn.) is said to be slightly narcotic. I greatly doubt whether this article is really

and truly narcotic ; but it may possibly be slightly so, for aught that I positively know to the contrary. *Verbascum Lychnitis* (Linn.) is said to be a Mouse-poison. If this is true, I do not know by what power it could possibly destroy Mice, unless by a narcotic power. *Verbascum Thapsus* (Linn.) is said to be a fish-poison, I have already expressed my doubts whether this article is narcotic. Fish are so easily thrown into an anesthetic and acinetic state, by almost any thing that imparts any additional qualities to water, that being a fish-poison is no argument of any material activity. However if *Verbascum nigrum* and *Verbascum Lychnitis* are truly narcotic in any degree, *Verbascum Thapsus* may be so. It is quite desirable that this question should be investigated and determined or settled. If these articles are not narcotic, they are probably inert. I never even heard the suspicion that they are adenagic like *Digitalis purpurea*. *Buddleja Brasiliensis* (Jacq.) is said to have the same powers as Mullein. I suppose that narcotic powers are here intended. For myself, I know nothing about this article. *Gratiola officinalis* (Linn.) is affirmed by some, on apparently good grounds, to be narcotic, though this is not generally admitted by the medical profession at large. Its qualities as a narcotic are peculiar. It is not certainly much like *Digitalis purpurea* in this respect. This article (I believe) is generally admitted to be diuretic. The truth is that it is adenagic, being diuretic only as a part of its adenagic operation. If taken very freely, it is hydragogue-cathartic and emetic. But though its powers take the same name, as those of *Digitalis purpurea*, yet they differ very considerably in quality. *Gratiola Peruviana* (Linn.) is said to be cathartic and emetic. A species of *Gratiola* growing extensively in the U. S. A. has been alleged to be cathartic and emetic, by a writer in a periodical ; but I had used it for years before this publication, and never witnessed any such effects. I shall mention it in another place. Of *Brunfelsia Hopeana* (Benth? and De Cand.) or *Franciscea uniflora* (Pohl.) it is said—"cortex interior et omnes partes herbaceæ amaritudine nauseosa, fauces vellicante pollent"—"nimia dosi tanquam venenum acre agit"—"dosi parva resolvit"—"dosi majore exturbat"—"omnis planta, magna radix potissimum, systema lymphaticum summa efficacia excitat"—"particulas morbificas liquescit"—et—"sudore et urina eliminat"—"alvum et urinam ciet"—"abortum

mover"—"venenum a morsu Serpentum excutit"—"apud nonnullas Indorum gentes in regione Amazonica habitantes ejus extractum in venenum sagittarum ingreditur"—"magni usus in Syphilitide, ideo *Mercurio vegetal* a quibusdam dicitur." In short, this article is represented as nauseous bitter, as vellicating the fauces, as an acrid-poison, as a resolvent, as an alexipharmac in the bites of venomous Serpents, as an emmenagogue, as an abortifacient, as powerfully stimulating the lymphatic system, as liquifying morbid particles, as eliminating them by sweat and urine, as proving diuretic cathartic and emetic, as being used for an arrow-poison, as being valuable in the treatment of Syphilis, and hence as being called Vegetable Mercury. But what can all this be fairly reduced-to? This article is undoubtedly adenagic; it is probably either truly acrid-narcotic, or erethistic, I have no means of determining which, at least at present; and it is hydragogue-cathartic and emetic. Considering that, in other articles, this aggregate of powers usually has more or less direct exhausting power associated with it (indeed I can not now recollect an exception to this) I make no question that this agent has a direct exhausting power, though it is not likely to the extent to be antiphlogistic. This is probably the real aggregate of the powers, operations and effects, of *Franciscea uniflora*. *Franciscea uniflora* (Pohl) is now called *Brunfelsia Hopeana* (Benth. ? De Candolle Prod.) because it really belongs to *Brunfelsia* (Swartz) because Pohl's genus *Franciscea* has not a good foundation, and because there is an other genus *Franciscea*, that is different and distinct, but which De Candolle, in the Index to Vol. X. directs "*Franciscaria legendum*." As this appears to be an important article, I will add that it is *Franciscea Hopeana* (Hook.) which, with the above, constitutes its synonymy.

Scrofularia aquatica (Linn.) and *Scrofularia nodosa* (Linn.) are said to be cathartic and emetic. As far as I am acquainted with the *Scrofulariæ* through the Connecticut species, I have been in the habit of considering them as feeble adenagics, in addition to their cathartic and emetic powers. *Calceolaria pinnata* (Linn.) is alleged to be cathartic and emetic. *Linaria vulgaris* (Miller) is said to be cathartic and diuretic. I have long been satisfied that this article is adenagic, and diuretic only as a part of its adenagic operation. When taken to a certain amount, it is hydragogue-

cathartic and liable to vomit. *Linaria Elatine* (Miller) is said to be cathartic. The root of *Leptandra Virginica* (Nutt.) is a mere and pure cathartic, some thing like the root of *Ipomœa Purga*. This is *Veronica Virginica* (Linn. and De Cand.). *Scoparia dulcis* (Linn.) is said to be mucilaginous, cooling, laxative, and febrifuge, i. e. remedial of Intermittent. *Vandelia diffusa* (Linn.) is said to be emetic and febrifuge. It is also said to be cathartic and diuretic. *Calceolaria trifida* (R. & P.) is said to be a febrifuge-tonic. *Chelone glabra* (Linn.) is a simple bitter-tonic. *Gratiola aurea* (Muhlenb.) according to my own observations and experience, is a simple bitter-tonic, and nothing else, notwithstanding it has been alleged to have exactly the powers of *Gratiola officinalis*. *Linaria ramosissima* (Wallich) is said to be antiscorbutic and antidiabetic. *Linaria Cymbalaria* (Miller) is said to be antiscorbutic. *Torenia Asiatica* (Linn.) is represented as a specific in *Urethritis Pyoblennorrhœica*. *Herpestis Monniera* (H.B.&K.) is said to be useful as a topical application in Rheumatism, by which is probably intended Rheumatalgia. *Bramia serrata* is said to be used in baths, though it is not said to be useful in them. The genus *Bramia* (La Marck.) is now merged in *Herpestis* (Gært.) but I do not find this species under *Herpestis* in De Candolle's *Prodromus*, and I do not know how it has been disposed-of. Should it not have been *Bramia semiserrata* (Mart.) which is *Herpestis Gratioloides*, (Benth. & De Cand.) and *Caconapea Gratioloides* (Chamisso). *Herpestis amara* (Benth.) is said to be very bitter. This plant is now referred to the Genus *Curanga* (Juss.) It is *Curanga amara* (Juss.) *Gratiola amara* (Roxb.) *Herpestis amara* (Benth.) *Synphyllium Torenioïdes* (Griff.) but *Curanga amara* (Juss.) should take the precedence. *Picrorrhiza Kurroa* (Royle) is said to be very bitter. *Euphrasia officinalis* (Linn.) is said by some to be slightly acrid, by others to be slightly aromatic. I doubt not that the same external sensible property is intended by both terms. It is said also to be slightly bitter. This article has long held a place in the *materia medica*, but (as would seem) no body has been able to ascertain what it is good for. *Mimulus guttatus* (D.C.H.M.) is said to be eaten as a salad. I suppose that this is *Mimulus luteus* (Linn.) *Mimulus variegatus* (Loddiges.) *Mimulus rivularis* (Nutt.) *Mimulus lyratus* (Benth.) Hab. Western America from Unalaska to Chili, except with-

in the tropics. The Linnean name should have the precedence and actually has it, in De Candolle's *Prodromus*. *Melampyrum pratense* (Linn.) is said to be fed upon by Kine. The *Pediculares* are said to be acrid, but to be fed-upon by Goats. There are at least eighteen hundred and fourteen species of *Scrofulariaceæ* known and described. Of these (so far as I am informed) only thirty species are known or supposed to be medicinal, and three esculent, leaving seventeen hundred and eighty-one inert, or of unknown powers. To the thirty medicinal species, sixteen different and distinct powers are ascribed in books, to which I have added two additional ones. It is not necessary to discuss here the correctness or incorrectness of what is contained in the books, since it is all we have as a guide in the use of botanical affinities for the purpose I am considering. In the present state of our knowledge of the *Scrofulariaceæ*, I must say that I can not understand how a knowledge of the botanical relation of the species of this Order can possibly assist us in any respect or degree in investigating the medicinal powers of a new and previously unknown article belonging to this Order. Just so it is in almost all the Natural Orders.

Loganiacæ. Lindley recognizes twenty-two Genera, and perhaps twenty-four, in the Natural Order *Loganiaceæ*, and one hundred and sixty-two and perhaps one hundred and sixty-nine Species; while De Candolle recognizes twenty-six Genera and one hundred and seventy-six Species. Lindley says "it would be difficult to name a more venomous Order than this" (*viz.* *Loganiaceæ*) "of whose qualities the celebrated *Nux-vomica* may be taken as the representative," etc. (*Lindl. Veget. Kingd. Lond.* 1846, *p.* 603.) *Ignatia amara* is mentioned by Lindley as a medicinal species of the Order *Loganiaceæ*. It need only be said of this that it is believed to contain the same active principles as *Strychnos Nux-vomica*, and probably in about the same quantities, since some consider it as a little more active than *Nux-vomica*, and some as a little less so. Only seven species of *Strychnos* out of thirty, are definitely known to be medicinal; and of these seven medicinal species, there are three intirely different sorts of agents. I doubt not that other species will yet be found to have valuable powers, but whether they will coïncide with those of any species now known, or be entirely different, no man can possibly

tell. Lindley continues, "from the bark of the root of *Strychnos Tieuté*" an other frightful poison is prepared in Java, where it is called Tjettek or Upas Radja." "It acts like *Nux-vomica*, but in a more intense and violent manner." (*Ibidem.*) The powers ascribed by Lindley to those *Strychni*, of which *Strychnos Nux-vomica* may be taken as a type, are venomousness and fatality above all others. It is said to be intoxicating, febrifuge, tonic, antiparalytic, antidotal, anthelmintic, aphrodisiac, diuretic and cathartic. The powers of *Ignatia amara*, and all the *Strychni* of which *Strychnos Nux-vomica* is the type, seem to be identically the same, and for the best reason, viz. because they contain the same active principles, at least so far as their active principles have been ascertained. Lindley says also that "*Strychnos Pseudo-Quina* is said to be the best febrifuge in Brazil." "With the exception of the fruit (which is eaten by children without danger) all the parts, especially the bark, are extremely bitter and rather astringent." "It is universally employed instead of *Cinchona*, and is asserted to be fully equal to Peruvian bark, in the cure of the Intermittents of Brazil." "Vauquelin analyzed the bark, but could find neither Quinine, nor Vomicine, nor Strychnine." It is sold under the name of Copalche-bark." (*Ibidem.*) *Strychnos Pseudo-Quina* would seem to be a pure tonic, without any other power in addition. The medicinal *Strychni*, of which *Strychnos Nux-vomica* is the type, are unequivocally tonic; but the other powers, which they possess, make them totally different articles medicinally. Whether the tonic power of this article, and that of the group of which *Strychnos Nux-vomica* is the type, have any close similarity, I know not, never having had any opportunity to witness the effects of *Strychnos Pseudo-Quina*. Lindley adds, "an other virulent kind is *Strychnos toxifera*, which forms the basis of the celebrated poison called Wooraly or Ourari," etc. (*Ibidem.*) Lindley says, "in Dr. Hancock's opinion, the bark" (of *Strychnos toxifera*) "is one of the most potent sedatives in nature; and could it be safely managed, would no doubt become a valuable remedial agent, in the treatment of convulsive and spasmodic" (spastic) "diseases." (*Lind. Flor. Med. Lond.* 1838, p. 530.) An other author says that "the Urari or Indian Arrow-Poison will kill the strongest Bull, in four or five minutes." "Rats and Lizards wounded with it, die immediately." (*Litt. Liv. Age,*

25, Nov. 1848, No. 286, p. 349.) *Strychnos toxifera* is one of the narcotics that destroy life by suspending the functions of the nerves of expression, and when employed by inoculation, it does this with unparalleled rapidity. In reality, there would seem to be as much difference between the effects of this agent employed by inoculation and taken into the stomach, as there is in the case of the Venom of the *Caudisona horrida*. I do not think that *Strychnos toxifera* has a single power or property in common with those medicinal *Strychni*, of which *Strychnos Nux-vomica* may be taken as the type. They are in fact the very antipodes of each other. The only point in which they have the least approximation is in the fact that a sufficient quantity of either will destroy life very speedily, though in the most widely different manner possible. As appears to me, *Strychnos toxifera* differs from every other article of the materia medica (unless it may be the Venom of *Caudisona horrida*) in affecting the system so much more speedily, and so much more powerfully, when employed by inoculation, than when taken into the stomach. But *Strychnos toxifera* is a pure narcotic, while the Venom of *Caudisona horrida* seems to be a pure euphrenic.

It is very extraordinary that Martius should assert that *Strychnos toxifera* abounds in the same active principle as *Strychnos Nux-vomica*, and the other analogous species, when its whole powers, operations, and effects are so widely, indeed so totally different and distinct. It is still more extraordinary that Martius should make this assertion in 1843, (the date of his "*Systema Materiæ Medicæ Vegetabilis Brasiliensis*") when Bousingault and Roulin detected, separated and examined the active principle of *Strychnos toxifera* in 1828, and found it to be *toto cœlo* different and distinct from the active principles of *Strychnos Nux-vomica*. De Candolle says of *Strychnos? cogens* (whatever this may be) "*succus venenosus cum Strychno? toxifera non nunquam mixtus.*" The first part of this statement would seem to be error, since there is ample testimony to the inertness of this species, but the last part is doubtless correct. If we may trust testimony there are some species of *Strychnos* that are inert medicinally, and even some that are esculent and absolutely delicious. Lindley is either not aware, or he does not consider the evidence sufficient, that *Rouhamon Gujanense* (Aublet) and *Rouhamon Curare* (H.B.&K.)

(if the latter is truly a distinct species) possess the same powers as *Strychnos toxifera* (Schomburgk.) As appears to me, there is sufficient testimony to prove this. If such is the fact, it will undoubtedly be found that they contain the same active principle, that is contained in *Strychnos toxifera*. It is no more strange that the two species of Rouhamon, which I have named, should contain the same active principle as *Strychnos toxifera*, than that the medicinal species of *Strychnos*, of which *Nux-vomica* is the type, should contain the same active principles as *Ignatia amara*.

The Genus *Gelseminum*, of which there is only one species, is not mentioned any where either in Lindley's *Flora Medica*, or in his *Vegetable Kingdom*, but in the second edition of his *Natural System of Botany* (*Lond.* 1836) it is found in the Natural Order *Bignoniaceæ*, but without any recognition of its possessing any medicinal powers. This plant appears really to belong to the *Loganiaceæ*, as constituted by De Candolle, and with him, it is the only Genus and Species of his Tribe *Gelsemieæ*, and his Sub-order *Loganieæ*. I doubt not that it is here in its proper place, though George Don arranges it in the Order *Apocynaceæ*, and the Tribe *Gelsemineæ*, along with *Plectaneia* (Thouars) the latter placed by Lindley, with a mark of interrogation, in the Order *Apocynaceæ*, and the Tribe *Carisseæ*. I mention all these variations in the location of *Gelseminum* by different botanists, to indicate that no very certain analogy can be derived from such an uncertain species. Of *Gelseminum nitidum*, Lindley would undoubtedly say, as he says of the *Spigeliæ*, viz. that it "participates in the noxious properties of *Strychnos*," since it is a simple, pure and intense narcotic, without any other powers in addition, operating very much like the Water or Spirit of Cyanid of Hydrogen, unless it may be in its manner of destroying life (of which I know nothing) and it may be like it, even in this respect. In the quality of its narcotic power, I think that *Gelseminum nitidum* must differ very considerably from the *Spigeliæ*, since I think it has a much nearer resemblance to Water or Spirit of Cyanid of Hydrogen, than it has to the *Daturæ* or to *Atropa lethalis*, which the *Spigeliæ* resemble. At all events *Gelseminum* is much more active than the *Spigeliæ*. In as much as *Gelseminum nitidum* is far more active than the *Spigeliæ*, it must approach nearer in its qualities to *Strychnos toxifera* than the *Spigeliæ* do. In all

other respects however, it must differ widely in the quality of its narcotic power from *Strychnos toxifera*. Although I never knew a trial made, yet there is scarcely even a possibility that by inoculation, it could be made to affect the system at large in any degree like the operation of *Strychnos toxifera*.

Only three species of *Spigelia* out of thirty-three, are known to be medicinal. In all probability, a larger number will yet be discovered to have valuable powers; but whether such powers will be like the powers of those now employed, or widely different, no one can even conjecture with any approximation to certainty. Lindley says, "the *Spigeliæ* participate in the noxious properties of *Strychnos*." (*Lindl. Veget. Kingd. Lond. 1846, p. 604.*) I can not discover how the *Spigeliæ* can be said to participate in any thing that belongs to that group of medicinal *Strychni*, of which *Strychnos Nux-vomica* may be taken as a type. I have no knowledge of a single quality possessed by them in common. The *Spigeliæ* are simple and pure narcotics without any other powers in addition. As respects the quality of the narcotic power of the *Spigeliæ*, I think it approaches the nearest to that of the *Daturæ*, or of *Atropa lethalis* though the crude *Spigeliæ* are far more bulky in proportion to their power. In what manner the *Spigeliæ* would destroy life, if they were pushed to that extent, is not known. It is my impression that though *Strychnos toxifera* and the *Spigeliæ* are each simple and pure narcotics, yet the quality of their narcotic power must be essentially different. At all events, they differ very greatly as respects intensity of power. Lindley was not aware, or did not deem it worth mentioning, that *Picrophlœus Javanensis* (a genus of only a single species) has a bitter bark as indeed the name implies, and is reputed to be tonic, though, so far as I know for no better reason than because it is bitter. Beside the only species of *Ignatia*, seven species of *Strychnos*, and three species of *Spigelia*, Lindley mentions nothing as medicinal, except the only two species of *Potalia*, which do not seem to be considered as possessing much activity, and what they have being different, one being reputed to be mucilaginous and astringent, while the other is reputed to be bitter and sub-emetic. Thus it appears that in the Natural Order *Loganiaceæ* (De Candolle) there are seventeen or eighteen medicinal species, and a hundred and fifty-eight or nine not known

to possess any medicinal powers. To these seventeen or eighteen medicinal species, authors ascribe at least fifteen powers supposed to be different and distinct. It is not necessary for me to undertake to decide in this place whether this is correct or not. It is my opinion however that it is not more than one out of the way, if we reckon as different some cases that fall under the same denomination, but are so diverse in quality as never to be substitutes for each other, in any case whatever. And now how much benefit can be derived from a knowledge of all these facts, in the investigation of a new and previously unknown article of this Order?

Polygalacta medicinalia. Among the most common articles of the North American materia medica let us examine the Genus Polygala, for the purpose of ascertaining how far natural-history affinities, and those much nearer than Natural Order, indicate medicinal powers. According to De Candolle, (*Prod. Syst. Nat. Reg. Veg.* 1824) the Genus Polygala comprehends one hundred and sixty-three species. 1st. Polygala Senega (Linn.) adenagic, cathartic, emetic and directly exhausting. 2nd. Polygala grandiflorum adenagic, cathartic, emetic and directly exhausting. 3rd. Polygala polygamum (Walter) cathartic, tonic? 4th. Polygala vulgare cathartic? tonic? 5th? Polygala amarum cathartic? tonic? 6th. Polygala Nutkanum cathartic? tonic? 7th. Polygala Poäya (Martius) emetic like Cephaëlis Ipecacuanha, cathartic? 8th. Polygala glandulosum (H.B.&K.) emetic like Polygala Poäya. 9th. Polygala scoparium emetic like Polygala Poäya. 10th. Polygala Thesioïdes diuretic, very efficiently so. 11th. Polygala venenosum (Jussieu) Poisonous! Query! in what way? 12th. Polygala uliginosum (Reichenbach.) has no powers ascribed to it, root almost insipid, herb bitter, good in pulmonary complaints and spitting of blood. 13th. Polygala luteum said to be like Polygala Senega, very doubtful. 14th. Polygala Crotalarioides (D. C.) said to be like Polygala Senega, very doubtful. 15th. Polygala sanguineum (Linn.) said to be like Polygala Senega, doubtless error. 16th. Polygala Chamæbuxus (Linn.) said to be like Polygala Senega, which is more than doubtful. 17th. Polygala Cracasenum (H.B.&K.) said to be like Polygala Senega, but not as good, doubtful. Polygala tinctorium

used in Dyeing. These are all the species of the Genus *Polygala*, that are known by me to possess any medicinal powers. There are then only twelve, or at most seventeen known medicinal species, leaving at least a hundred and forty-seven, and most probably a hundred and fifty-one species either inert or not known to have any medicinal powers. Among these twelve, or at most seventeen medicinal species, there are at least seven different and distinct medicinal powers, and indeed several more, if we may rely upon the testimony of authors, beside great differences in the quality of powers taking the same name, as possessed by different species. In such a state of facts, I can not well discover how any analogy can be derived even from Genus, that can be made reliable, in researches after the powers of previously uninvestigated articles.

Polygalaceæ. According to Lindley, the Natural Order Polygalaceæ (inclusive of the Krameriaceæ of Martius) comprises nineteen Genera and four hundred and ninety-five Species. De Candolle only says of the Order Polygalaceæ "Succus lacteus præsertim ad radicem." "Cortex radixque amara. (*De Cand. Prod. Syst. Nat. Reg. Veg.* 1824, *Pars I.* p. 321). I am aware that Professor Lindley (of the University of London) gives what may perhaps be considered a different account of the Natural Order Polygalaceæ. He says of the Polygalaceæ that "bitterness in the leaves, and milk in the root, are their usual characteristics." He states that the "root of *Polygala Senega* is stimulant," which certainly is not true in any legitimate acceptation of this term; that it is "sialagogue, expectorant, emmenagogue, diuretic, sudorific, cathartic and emetic." I am satisfied that five of these are only one single power, viz. that which I call adenagic, and that this is not even the whole of the operation of the power in question. He says that according to Barton "*Polygala sanguineum* possesses similar properties." Now if it is Dr. B. S. Barton that he refers to, it ought to be known that he does not say exactly this, but perhaps he meant it. Dr. B. S. Barton's words are as follows, viz. "from some experiments which I have made with *Polygala sanguineum*, I am led to think that this species may be employed as an excellent substitute for the common species now in use." I recollect once making a statement to a professional gentleman (of which I observed that he made note

at the time) that I considered Opium and Botrophis Actæoides or Actæa racemosa in conjunction, as an excellent substitute for bleeding, purging and antiphlogistication, in acute Rheumatism; but I am sure I did not mean to be understood that I considered their operation and effects as even similar, and much less, as the same; but so I was actually understood, as I learned afterwards, by abundant testimony. According to De Candolle, the Natural Order Polygalaceæ, comprises eleven Genera and two hundred and sixty-five Species, viz. Polygala species a hundred, and sixty-three; Salomonina species four; Comesperma species nine; Badiera species five; Soulamea species one; Muraltia species thirty-seven; Mundia species one; Monnina species twenty-nine;? Bredemeyera species one; Securidaca species eight; Krameria species seven; all of which (except the first and last) are utterly unknown, in all common works on materia medica. (*Ibidem.*)

Of Polygala eleven species are supposed to be certainly known to be medicinal; and of Krameria one species, and doubtless more, though only one is officinal. Lindley mentions several Genera not recognized by De Candolle, in this Order at least, viz. Brachytropis (De Candolle); Catacoma (Bentham); Carpolobia (G. Don); Lophostylis (Hochstetter); Xanthophyllum (Roxburgh); Trigonia (Aublet); Moutabea (Aublet);? Hymenanthera (R. Brown). Whether De Candolle places these Genera in different Natural Orders, or whether they are new or not, having been constituted since he wrote upon the Polygalaceæ, I have no means of determining at this precise time, since I have not at present access to my own, or any body's copy of the Prodrômus. Of the Order Polygalaceæ (exclusive of the Genus Polygala) the following are all the species known to be medicinal, or useful in any way.

Polygalaceæ medicinales Polygalactibus exclusis. Soulamea amara (La Marck, De Cand.) very bitter, and consequently reputed to be tonic, though not known to be so. Monnina polystachya (Ruiz) Qu? adenagic? saponaceous, said only to be good in Diarrhœa and Dysentery. Monnina Salicifolia (Flor. Peruv.) the same as the preceding. Badiera diversifolia, adenagic? said only "to rival Guaiacum" in its effects. Mundia spinosa drupes esculent. Krameria triandra (Flor. Peruv.) styp-

tic pure and intense. Here then are supposed (not certainly known) to be three different and distinct powers in five different species! Deducting a hundred and sixty-three species of *Polygala* from four hundred and ninety-five (the whole number of species in the Order) the remaining number of species will be three hundred and thirty-two, out of which only four species are medicinal exclusive of the *Krameriaceæ*. There is actually only one species of *Krameria officinal*, but it is supposed that the whole Genus possesses pretty much the same power. In his *Natural System of Botany* (*Lond.* 1836) Lindley says there are ten species of *Krameria*. Without these, there will be only four medicinal species out of the three hundred and thirty-two, and with them, fourteen medicinal species, leaving three hundred and eighteen or three hundred and twenty-eight species, that are not known to have any powers. Assuredly no very reliable analogy can be derived from the known and investigated species of the Order *Polygalaceæ*, towards a knowledge and an investigation of the species, whose powers are not yet ascertained. The Natural Class, to which the Natural Order *Polygalaceæ* belongs, comprises a great number of the most heterogeneous articles of the *materia medica*; but I do not think it worth while to bring Natural Class into consideration, in connexion with this subject, if no dependence is to be placed upon Natural Order.

Apiaceæ. Linnæus says, "*Umbellatæ in siccis*" (*locis*) "*aromaticæ, calefacientes et pellentes; in aquosis autem venenatæ sunt; radice et seminibus pollent.*" (*p.* 381, *Sect.* 345, *Caroli a Linné. Philosoph. Botanic. Edit. tert. cur. Carol. Ludovic. Wildenov. Berol.* 1793.) Here Linnæus avails himself of Natural Order and habitat in conjunction. I have not found it to be the fact that those *Apiaceæ*, that grow "*in aquosis*" are any more "*venenatæ*" than the same are when they grow "*in siccis locis*." I have very often tried the culture of the narcotic *Apiaceæ* in dry places, and I never found that they lost any degree of their narcotic power. In fact some of the most active *Apiaceæ*, with which I am acquainted, I have never found, in our country, any where, except in dry places; but being exotic plants accidentally introduced, their being found only in such places may be due to the circumstances of their introduction, and they may grow in wet places, in their native regions. Again I

have cultivated the non-narcotic *Apiaceæ* in wet places, but I could never perceive that they either lost any of their previous powers, or acquired any new ones. Certainly I never knew any one of them to become poisonous in any way, or in any degree narcotic. The Natural Order *Apiaceæ* contains at least fifteen hundred species. Now by far the greatest portion of these are not known to be either medicinal or esculent. Some of those which are medicinal are narcotic, varying from a very slight, to a very intense degree of this power. The narcotic *Apiaceæ* are commonly said to be acrid, but this is not true of those with which I happen to be acquainted. I can of course say nothing of the European species, that have never got across the Atlantic. There are twelve, and possibly fourteen species of *Apiaceæ* that are narcotic in a greater or less degree. One of the narcotic species is definitely and certainly known to be adenagic; and it is believed that several others are, from the power that they possess over cutaneous diseases. There are fourteen and perhaps eighteen species whose inspissated descending saps are sub-euphremic, sub-adenagic, and sub-cathartic. There are three species, and perhaps a few more, that are said to be aromatic and tonic. As far as I am acquainted with them, I should call them sub-acrid rather than aromatic; and I should not consider them tonic at all. Instead of being tonic, they appear to me to be adenagic, cathartic or sub-cathartic, and emetic or sub-emetic. There are three species that are acrid and cathartic. There are eleven species which are aromatic-acrid, the root being the part employed. There are fifteen species that are aromatic-acrid, the seeds being the part employed. There are four species whose roots are aromatic without any acrimony. There are three species whose roots are said to be styptic, and nothing else. There is one species whose root is said to be diuretic, and nothing else. There are three species whose roots are farinaceous and saccharine, and by fermentation produce a Wine, which by distillation yields Alcohol. There is an other species that is used for the preparation of an intoxicating beverage, which, in all probability, belongs to the same group. There are about twenty species, which may be reckoned as pure esculents, some times the roots, some times the stems, and some times the leaves being the part eaten. There is one species which is said to be good for pasturage, a remarkable fact in the Natural

Order Apiaceæ. There are therefore fourteen hundred and ten species of the Apiaceæ, that are not known to be either esculent or medicinal, or useful for any purpose, while there are sixty-six that are medicinal, and twenty-four that are esculent, or used for the preparation of a beverage. Among the sixty-six species, that are medicinal, it is said that there are nine or ten different and distinct powers to be found. If then a new plant of the Natural Order Apiaceæ is to be investigated, are we to consider that there are only about sixty-six chances out of fifteen hundred that it will prove to be medicinal at all, and a certain number of chances out of the sixty-six, that it may possess any one of the nine or ten different and distinct powers? If so, we may as well investigate intirely independent of natural or botanical affinities. It may indeed be well to know that there are twelve species out of fifteen hundred, that are narcotic in a greater or less degree, some intensely so, and some only slightly, and therefore it may be well just to guard against a narcotic-poison so called. As appears to me, this is all the benefit that can be derived from the knowledge that a plant to be investigated belongs to the Apiaceæ.

Hexandria. Linnæus says, "Hexandriæ radices secundum saporem et odorem edules aut noxiæ sunt," "Edules sunt radices inodoræ." "Venenatæ secundum odorem virosum sunt," etc. (*Ibidem.* p. 281, *Sect.* 346.) Now it can not be said that there is any great natural affinity between the several genera and species of the Linnean Class Hexandria. In fact it seems to be odor and taste that indicate activity, and destitution of these that indicates inertness; and the Linnean Class Hexandria seems to have no connexion with the matter. I believe that the proposition of Linnæus would have been equally correct, if it had been made of bulbous and tuberous roots of what ever Linnean Class, rather than of the Class Hexandria. It is true that the Class Hexandria comprises some plants belonging to nearly allied Natural Orders; but it also comprises plants belonging to Natural Orders as diverse as possible. I can now think of Hexandrous plants belonging to the following Natural Orders, and if I were to refer to authorities, I have no doubt that I might double this number. Graminaceæ (Bambusa), Palmaceæ, Bromeliaceæ, Taccaceæ, Hæmodoraceæ, Hypoxidaceæ, Amaryllidaceæ, Musaceæ, Commelynaceæ, Juncaceæ, Orontiaceæ, Melanthaceæ, Lil-

iaceæ, Pontederaceæ, Frankeniaceæ, Sapindaceæ, Berberaceæ, Lythraceæ, Campanulaceæ, Cinchonaceæ, etc.

The inquiry whether there is any likeness of powers and properties among so many different groups of species, would be much like an inquiry as to what likeness of powers and properties may be found among plants taken at hap-hazard from the whole vegetable kingdom. The number of species of these Hexandrous Natural Orders of plants, whose powers are at all known, is by far too small to allow the general statement of Linnæus; and in addition to this, there are too many exceptions to it already known, to admit of its being true and trustworthy. But we are told "*edules sunt radices inodoræ*" of the Class Hexandria. There are at least a hundred and thirty species of Melanthaceæ. Some species are inodorous and tasteless without being either esculent or medicinal in any way. Some species are inodorous and tasteless and yet very actively narcotic. I will mention only one very prominent exception to the aphorism under consideration, out of a multitude doubtless, which, of itself ought to destroy the rule, viz. *Amianthium Musciticum*, which is Hexandrous and Trigynous, Melanthaceous and Veratrous. I have long been in the habit of using this plant in medicine, but as it has not grown where I have resided, I have always been indebted to friends for obtaining it for me. As I have received the root (the part that I have always employed) it has been insipid and inodorous, certainly it has not had an "*odor virosus*" a virose odor, and yet a saturated Alcoholic Tincture has (under my observation) been as active as Scheele's Water of Cyanid of Hydrogen, and much in the same way. One such case ought to destroy the rule, though I doubt not that there are many more such cases. Some of the species of Malanthaceæ, are simple bitter-tonics. The Taccaceæ, I believe, comprise only about eight species. The tubers of several species of *Tacca*, I believe, are quite acrid, and yet they are important articles of diet.

The number of the species of the Hæmodoraceæ is about fifty. "The natives of Swan-River live on the roots of *Hæmodorum paniculatum*, *Hæmodorum spicatum*, *Anigozanthus floridum*, etc. which are acrid when raw, but mild and nutritious when roasted." (*Lindl. Vegt. Kingd.* 1846, p. 152.) In the Hæmodoraceæ, the root of *Lachnanthes tinctoria* is valuable as a dye. *Aletris farino-*

sa and *Aletris aurea*, have "radices inodoræ," and yet they are by no means "edules." There are about sixty species in the Natural Order Hypoxidaceæ. Three species only, out of this sixty, are known to be either medicinal or esculent. The tubers of *Hypoxis erecta* are employed by the aborigines of North America against Intermittents, and in healing Ulcers." (*Ibidem* p. 154.) "The roots of *Curculigo Orchioïdes* are some what bitter and aromatic, and are employed in the East Indies in Urethritis Pyoblennorrhœica." (*Ibidem*.) "The tubers of *Curculigo stans* are eaten in the Mariane Islands." (*Ibidem*.) There are at least four hundred species of the Amaryllidaceæ. The viscid juice of the bulbs of *Brunsvigia toxicaria* is used by the Hottentots as an arrow-poison, and is said to be very active. *Amaryllis Belladonna* is said to be employed as a poison in the West Indies. It is even said to have been used as an arrow-poison by the aborigines. But this plant is a native of the Cape of Good Hope, and not of the West Indies. Martius tells us that the species of *Hippeastrum* have poisonous bulbs. *Crinum Asiaticum* (Roxb.) *Crinum toxicarium* (Roxb.) which is *Radix toxicaria* (Rumph.) has bulbs, which are powerfully emetic. The flowers and bulbs of *Narcissus Pseudo-Narcissus* are said to be poisonous and emetic. *Narcissus odoratus*, *Narcissus poeticus*, and *Narcissus Tazetta* have emetic bulbs. The bulbs of *Leucojum vernum* are emetic. The bulbs of *Galanthus nivalis* are emetic. The bulbs of *Pancreatium maritimum* are emetic. *Oporanthus luteus* is purgative. A cold infusion of the leaves of *Chæradodia Chilensis* is diuretic and purgative. *Alstroemeria Salsilla* is diuretic and diaphoretic. The root of *Agave Americana* is diuretic and antisymphilitic. *Bomarea Salsilla* is used as a substitute for Sarsaparilla. *Amaryllis ornata* is styptic. *Agave Americana*, *Agave Mexicana*, *Agave vivipara*, furnish a great abundance of sap which contains Sugar and mucilage and is fermented into a vinous liquor called Pulque, from which an Alcohol is obtained by distillation. The root of *Agave Saponaria* is a powerful detergent, and is employed in Mexico as a substitute for Soap. The expressed juice of the leaves of *Agave Americana* evaporated (I suppose) to dryness, is a useful substitute for Soap. *Alstroemeria pallida* furnishes a kind of Arrow-root i. e. fæcula from its succulent roots. *Alstroemeria edulis* (Tussac) has tubers filled with a nutritious fæcula. The

foregoing specifications have been made to show the great variety and diversity of properties and powers possessed by the plants comprehended by the aphorism that I am considering, and the utter impossibility of confiding in it for the purpose to which it is applied. It can hardly be said to have been necessary to give the aphorisms at the beginning of this particular topic, as much examination as they have already had, if it were not for the fact that they have been much insisted-on by botanists not physicians. In the time of Linnæus, but few plants comparatively were known, so that a generalization, which might have been correct then, may be wide from the truth at the present time, when a so much greater number of species has been ascertained.

Rosaceæ, etc. "A flower"(says Paris)"with twelve or more stamina, all of which are inserted into the internal side of the calyx, will furnish a wholesome fruit." (*Paris's Pharmacol.* 2nd Amer. fr. 5th Lond. Ed. N. Y. 1824, p. 62-63, Vol. I. Introduct.) Linnæus says, "Icosandriæ fructus pulposus est esculentus." (*Caroli a Linné Philosoph. Botani. Edit. tert. cur. Carol. Ludov. Willdenov. Berol.* 1793, p. 282, Sect. 348.) The Linnean Class, Icosandria constitutes the Jussieuan Natural Order Rosaceæ, including the more recent Natural Orders Pyraceæ, Amygdalaceæ, and also the Orders Cactaceæ, Myrtaceæ, Mesembrianthemaceæ, Loasaceæ, Philadelphaceæ, etc. The last mentioned five I will pass-by, but I will consider the Rosaceæ inclusive only of Amygdalaceæ and Pyraceæ. In these I think we shall find deviation enough from the aphorism to destroy its value wholly. The Linnean Icosandria (much better Calycandria, as Dr. William Darlington suggested) coïncides with the Jussieuan Natural Order Rosaceæ, comprising the more recent Natural Orders Amygdalaceæ, Pyraceæ, and some others. Among the Amygdalaceæ, are we not told that the "fruit of *Cerasus Lauri-Cerasus* is a virulent poison?" (*Lindl. Veg. Kingd. Lond.* 1846, p. 558.) Are we not told the same of the fruit of *Cerasus Padus*? (*Ibidem.*) The same is certainly believed and said of *Cerasus Caroliniana*, and is probably as true of several other species, as of these. I do not pause here to inquire whether this statement is in fact true, or not. There will be a more appropriate place for this inquiry. It is sufficient for my present purpose that the statement in question has been received and repeated, time immemorial.

Now one such exception, as each of these is claimed to be, is sufficient to destroy the aphorism under consideration. Two species of *Cerasus*, and only two, are cultivated for their fruit. One American species might possibly be worthy of culture; but its fruit is small. The fruits of three or four species of *Prunus* are valuable. The fruit of *Armeniaca vulgaris* is very valuable; and I believe that of one more species. The fruits of *Persica vulgaris* and *Persica lævis* are delicious and highly esteemed. The Natural Order *Amygdalaceæ* contains at least a hundred and ten species. But how far is there a conformity to the aphorism? I think there can not possibly be to exceed ten species of *Amygdalaceæ* out of one hundred and ten, that are in conformity to the aphorism, while there are admitted to be at least three species in positive and direct opposition to it; the remaining ninety-seven neither conforming to it, nor being in positive opposition to it. But how many of the *Pyraceæ* have "wholesome," or even esculent fruits, whether "wholesome" or not? Those fruits of the *Pyraceæ*, that are now the most valuable, were, in their natural and wild state neither "wholesome" nor edible, even if they were not poisonous. One species of *Pyrus*, one species of *Malus* and one species of *Cydonia*, have had their fruits so meliorated by long ages of culture that they may be said to be "wholesome." The fruit of one species of *Mespilus* is some times cultivated and eaten, being not unwholesome; but it is very little esteemed. The fruit of one species of *Amelanchier* is some times eaten; but it is not at all valued. Culture might possibly improve it some what. I have known children eat the fruit of *Adenorrhachis Arbutifolia*; but it is of no value as a fruit. What culture might make of it, I know not. Probably not much. The fruits of some *Cratægi* are some times eaten by children; but are considered as of no value. I think that there is one species of which some thing might be made by culture. The fruits of the several species of *Sorbus*, I do not think can be considered as "wholesome;" and certainly they can not be said to be esculent. I know of no other genus of *Pyraceæ*, of whose fruit any use is made. The Natural Order *Pyraceæ* contains at least two hundred species. There are then only about seven species, out of two hundred of the *Pyraceæ*, whose fruits can be reckoned as eatable, including the very inferior, as well as

the good. Let us next consider the Rosaceæ proper, excluding the Amygdalaceæ and the Pyraceæ. Perhaps the fruits of the Potentilleæ (excluding the Genera Rubus and Fragaria) might not be poisonous, but I doubt whether they would be any more "wholesome" than they would be palatable. Perhaps the fruits of the Spiræeæ may not be poisonous; but are they, in a single instance, eatable? The same remarks, in all probability, may be made of the Quillaieæ and Neuradeæ, that have been made of the Potentilleæ, and the Spiræeæ, which with the Roseæ, a tribe of two genera, many species, and one single very inferior esculent fruit about equal to the fruit of a common Cratægus or Hawthorn, make-up the Order. The Natural Order Rosaceæ made-up of the Natural Tribes Roseæ, Potentilleæ, Spiræeæ, Quillaieæ and Neuradeæ, has five hundred species, and only three Genera, that furnish eatable fruit, viz. Rosa, Rubus and Fragaria. I am not apprised that there is more than one species of Rosa, whose fruit is eaten, but the fruits of several species of Rubus and Fragaria are highly esteemed. Reckoning the esculent species of Rubus as ten, which is full large enough, the esculent species of Fragaria as five, which I should also think large enough, we shall have about fifteen esculent species out of five hundred. The esculent-fruited Rose hardly deserves mention. As appears to me, this statement barely, shows the utter worthlessness of the aphorism deduced from botanical affinities; and it shows that the Classes of the Linnean Method have far less conformity to it than Natural Orders.

Polyandria. Linnæus says, "Polyandria plerumque venenata est" (*Caroli a Linné, Philosoph. Botan. Edit. tert. cur. Carol. Ludov. Willdenov. Berol. 1793, p. 282, Sect. 349.*) This must have reference mainly to the Natural Order Ranunculaceæ; but it is very far from being true of a large number of these. Beside the very large number of Polyandrous and Ranunculaceous plants, that are not known to possess any powers, how can it be made-out that the several species of Hepatica, that Hydrastis Canadensis, Coptis trifolia, Nigella Damascæna, Nigella sativa, Aquilegia vulgaris? Aquilegia Canadensis? Pæonia officinalis, Xanthorrhiza Apiifolia, etc. are poisonous? I do not think that this can be affirmed even of Botrophis Actæoïdes, and probably of none of the Tribe Actæeæ. In short, if we could come accurately at the truth, I do not believe that one quarter (or even so many) of

the Ranunculaceæ are poisonous in the common acceptation of this term. The Natural Order Ranunculaceæ comprises at least one thousand species. As respects the Natural Order Papavera-ceæ, which is Polyandrous generally, *Papaver somniferum* and *Meconopsis Nipalensis* are the only species definitely known to be poisonous in the common acceptation of this term. Some may perhaps think that this may be affirmed of *Sanguinaria vernalis*; but I have known two fluid-ounces of the strongest Tincture that could possibly be made, taken at a dose, without any effect that could be called poisonous. Now if it is not poisonous in such doses, it is not worth while to reckon it a poison at all. I know of nothing else belonging to this Order, which comprises at least a hundred and thirty species, that is at all active in any way. The Natural Order Podophyllaceæ is Polyandrous, but is not poisonous, unless a cathartic power is a poisonous one. This Order comprises only a very small number of species, four or five at most, and perhaps not so many. The Natural Order Sarraziniaceæ, which however comprises less than ten species, is Polyandrous, though by no means poisonous, since *Sarrazinia* is only a little styptic. The Natural Order Dilleniaceæ, which is Polyandrous, contains about two hundred species; certainly none is poisonous. Some of its species are said to be styptic, some are said to be acid and antiphlogistic, some are supposed to be adenagic, and some are said to be moderately cathartic, but none possess any great degree of activity in any way. The Natural Order Anonaceæ is Polyandrous, and contains about three hundred species, none of which can be said to be poisonous. Some are pungent and aromatic, some are emetic, some are bitter and tonic, some exude a gum, some produce vertigo if taken in large doses, many are esculent, but the majority (so far as is known) are inert. The Magnoliaceæ are Polyandrous, and comprise about sixty-five species. A considerable number of these species are bitter and tonic, acrid-oresthetic and aromatic, but none are known to be poisonous. The Nymphæaceæ are Polyandrous, and the Order comprises about fifty species. The roots are styptic and leantic. They have been reputed to be aphrodisiac, doubtless an imaginary power of this Order. The seeds are esculent. No parts of these plants are poisonous. The Cabombaceæ are Polyandrous, but only three species are mentioned as belonging to the Order. These are said to

be slightly styptic and nutritious, and useful in Phthisis and Dysentery. They are certainly not poisonous. The Natural Order Nelumbiaceæ is Polyandrous. The seeds are esculent, and so are the tubers of the root. But there are not more than three or four species in this Order, and therefore it is by no means remarkable that they should agree in their properties. None of them are poisonous in any common acceptation of this term.

Simarubaceæ. As appears to me the Natural Order Simarubaceæ favors the aphorism of the botanists, that I am discussing, more than any other Order, comprising an equal number of species, though it is itself a small Order. The Simarubaceæ comprise ten Genera, and thirty-five species. Of these thirty-five species, the following are known to be medicinal. *Simaruba amara* (Aublet), *Simaruba versicolor* (St. Hil.), *Quassia amara* (Linn. fil.), *Picræna excelsa* (Lindley), *Nima Quassioïdes* (Hamilton), *Samadera Indica*, *Simaba Cedron*, *Simaba Gujanensis* (Aublet), *Brucea antidysenterica* (Miller), *Brucea Sumatrana* (Roxb.), *Brucea* ———* These are all simple bitter-tonics. But even in the small Natural Order Simarubaceæ, there is only about one third of the species that are known to be medicinal. If the rest possessed the same external sensible properties, with the same intense bitterness, it would undoubtedly have led to a trial of them in medicine; but as they have not been tried, or if they have been, did not prove active, it is on the whole to be presumed that they are inert, or possess different and distinct powers. However, I have no absolute knowledge of this. "An other observation to be alluded to," (says Cullen) "in employing the general analogy" (of Natural Orders) "is, that though the plants of the same Order may have a great resemblance in the general quality" (yet) "they have this in such different degrees, as by no means to admit of an indifferent choice, for the purpose of medicine." Cullen says, "a further observation, and of still greater importance, is that although there" (may) "be some resemblance in the qualities of the plants belonging to the same Order, yet in the several species, the resemblance is not only seldom exact, but more commonly there is a peculiar modification in each; and very often, with the quality belonging to the

* N. B. I do not recollect the trivial name of this species, and I have no access to my books, since in the course of about two years residence in Springfield, I have been unable to obtain a place where I can unpack and set-up my library. I write therefore without the use of it.

Order, there is associated an other, which is totally different, either from that, or from any other of the Order, and some times of a dangerous kind ; so that the heedless practitioner might be very much deceived in trusting to a botanical affinity alone." Cullen says, "it still further merits attention, that though plants of the same Natural Order commonly have the qualities belonging to the Order, similar in all their several parts, yet this is by no means universal." "Plants in general have the qualities of their several parts considerably different, so that the root is often of a very different quality from that of the leaves or seeds ; and the resemblance that may be in the fructification, which especially establishes their botanical affinity, is by no means to be extended to all the several parts of the plants agreeing in that affinity." "In their several parts, the common quality may not only be in a very different degree, but in some of the parts, there may be a widely different, and even a contrary quality." (*Cull. Mat. Med. Bart. Ed. Phil.* 1812, *Part I.* ps. 90-91.) By way of illustration of this statement, one of the annotators of Cullen says, "thus the leaves of *Podophyllum peltatum* are poisonous, the root a safe and excellent cathartic, and the fruit innocent and esculent." (*B. S. Barton, Ibidem.*) Now according to my investigations, the only way in which the leaves of *Podophyllum peltatum* operate, are that of a cathartic, and therefore the only way in which they can prove poisonous, is by producing hypercatharsis, a way in which they are much less poisonous than the root, and even than the fruit. According to my observations the "innocent and esculent fruit" of the *Podophyllum peltatum* is likewise cathartic, though in a much less degree, than the root. The fruit may not be sufficiently cathartic to purge every one, when it is eaten only in small quantity ; though, to my certain knowledge, it will purge many persons even in small quantity. In fact I have myself been purged by it, more than once. This statement of the annotator of Cullen (which was doubtless an adopted tradition) has often been quoted as a kind of medical paradox, and will doubtless continue to be so quoted. It is always best to ascertain the truth of such alleged, but highly improbable facts, before they are adopted and retailed. Cullen concludes that "from all these considerations, it will readily appear, that the botanical affinity of plants, though it may" (in some instances) "be of some use in investigating their medicinal qualities, can not be applied to the

ascertaining of these virtues, but with a great deal of caution, and can never afford any certain conclusion, without examining, at the same time, their sensible qualities; nor even then, except when the supposed medicinal virtue is confirmed by actual experience" (rather experiment?) "on the human body." (*Ibidem.*)

But much nearer natural affinities than those of Order, and even of Genus, are often marked by great diversity of medicinal powers. I have already mentioned the Natural Order *Salvadora-ceæ*, which comprises only one Genus of but two species; one of which is remarkably acrid and epispastic (it being the Mustard, incorrectly so translated, of Scripture) while the other is a mere cathartic. *Gratiola aurea* of North America, so closely resembles *Gratiola officinalis* of Europe as to have been long considered identical with, or a mere variety of it; and indeed I have never been able to discover any permanent difference between them, except the color of their flowers. But *Gratiola aurea*, according to my observations and experience, is a simple and pure bitter-tonic, with no other powers conjoined; while *Gratiola officinalis* is adenagic, narcotic, hydragogue-cathartic and emetic, without any tonic power, but on the contrary, with a greater or less degree of direct-exhausting power, though not sufficient to render it at all antiphlogistic. *Veratrum viride* of North America, which for a long time was not distinguished botanically, even as a variety, from *Veratrum album* of Europe, and which in fact differs but very slightly from it in its external characters, appears to be intirely destitute of any cathartic power; while *Veratrum album* is a drastic hydragogue-cathartic. Even those powers possessed by these two plants, which take the same denomination, appear to differ materially in quality. Both are adenagic, but the quality of their adenagic power differs quite appreciably. Both are narcotic (or perhaps erethistic) but the quality of this narcotic (or perhaps erethistic power) as possessed by these two articles, differs quite sensibly. Both are emetic, but the quality of the emetic power, as possessed by each, differs palpably. *Veratrum viride* is very much like *Sanguinaria vernalis* in its operations and effects, while *Veratrum album* is more like *Colchicum autumnale*. The narcotic (or perhaps erethistic) power of *Veratrum viride* is greater than that of *Sanguinaria vernalis*, and the sensations which it produces are very strongly like those produced by *Kalmia latifolia*, when taken to such an extent as to

produce its narcotic (or perhaps erethistic) effects. When I have prescribed *Veratrum viride* in Tincture, and in doses short of the production of any degree of nausea, to subjects that had previously taken *Kalmia latifolia* in Tincture, they have always insisted that they must be taking the latter article, because as they said their sensations were identically the same. In this connexion it may perhaps be asked what are the botanical differences between *Veratrum viride* and *Veratrum album*? To this inquiry I must answer that all, which I have ever been able to discover, consists in the facts that the edges of the segments of the perigone in *Veratrum viride* are slightly incrassated and contracted below their middle, which is not the fact in *Veratrum album*, and that the general aspect of *Veratrum viride* is coarser and rougher, and it is of a darker green color. These are small characters, but they appear to be permanent, incapable of change by accidental causes, such as climate, soil, culture etc. and are invariably continued by regular reproduction from seed, which is all that is necessary to render them diagnostic of true species. Now I doubt not that a great number of just such cases as these two, might be found if sought after; but these will answer my present purpose as specimens for illustration, which is all that is here necessary.

To show that my opinions are not altogether singular, I will make a quotation from a distinguished British naturalist and professor, with whom I can not but agree mainly as respects the utility of botanical affinities, for the purpose of determining medicinal powers. "We are told" (says Professor Rennie) "by those who boast of the Jussieuan, as the Natural System that it brings together the plants, which most resemble one another, in anatomical structure, in what are called affinities, and in nutritious (medicinal) or noxious qualities." "To show that I do not exaggerate a jot, in this statement, I refer the reader to Loudon's *Encyclopædia of Plants*, page 1052, where we are told (in the portion of the work contributed by Professor Lindley) that when the Natural Order of a plant is ascertained, many of its most important qualities, such as" (its) "medicinal powers" may be 'safely' inferred." "Now if this were so, I think nobody would dispute the high value of this natural system." "Unfortunately however this principle is virtually contradicted by what follows." "Thus under" the great division "Cellulares," (Class *Aphyllæ*)

"Order viii." (i. e. Lichenes) "Mr. Lindley gives us *Cetraria Islandica* etc. tonic and nutritive," along with "*Evernia Vulpina*, poisonous." Again under Vasculares, Order cxli." (i. e. *Urticeæ*) (to say nothing as to size, form and structure) of "the Fig the Mulberry and the Bread-fruit-tree being naturally, (common sense would say unnaturally) classed among worthless weeds" such as "the common stinging Nettle," "and shabby half herbaceous shrubs," such as "the Hemp and the Hop, what are we to think of "safely" inferring from the Fig, the Bread-fruit-tree, and the Sago-plant, the medicinal properties" of "the" (*Antiaris toxicaria* or) "Upas-tree, the inspissated juice of which" (to use Mr. Lindley's own words) "is a frightful poison." Mr. Rennie says, "were I the proprietor of this valuable work, I would not hesitate an instant to break-up the stereotype plates, in order to expunge such glaring contradictions and highly dangerous errors." "In this division also, among those especially called the true Nettles, (as if there could be in nature any false ones) we find the Mulberry-tree side by side with the stiff Hemp, and the light climbing Hop." "Now admitting that the seed and the flowers of all these agree in structure, as they do nearly, it must appear obvious that the plants are as incongruously and as unnaturally grouped as possible, in reference to their general form and habits; while if we look to qualities, what can be more incongruous than to rank the" (*Antiaris toxicaria* or) "poisonous Upas of Java, in the same Order with the Fig." "In the Order *Solaneæ* we find the wholesome Potato and the wild Shepherd's-club" (*Verbascum Thapsus*) "ranking with Henbane and Deadly Nightshade." "In the Order *Rubiaceæ* we find not only lofty trees ranked with dwarf-shrubs, and tiny slender herbs, but we have the Coffee ranked with the well-known emetic *Ipecacuanha*, and this again with Peruvian Bark."

Rennie says, "I could readily fill a volume with the similar discrepancies of this so preposterously belauded Natural System, which, if it have not to answer for the loss of human lives by poisoning upon principle, it is no fault of its promulgators." (*Renn. Alph. Bot. Lond.* 1833, p. 115-117.) It is however true that the same individual and identical proximate principles, in whatever agents they may be found, do in fact possess the same medicinal powers; and it is also true that the species of one Genus

are rather more likely to contain the same individual and identical active proximate principles than the species of different Genera; and the species of different Genera belonging to the same Natural Order, are perhaps rather more likely to contain the same active proximate principles, than the species of different Genera, belonging to different Natural Orders. But there is an immense multitude of very prominent exceptions to this rule (like the case of *Strychnos Tieuté*, *Strychnos toxifera* and *Strychnos Pseudo-Quina*) far greater than the instances which constitute the rule, or from which the rule is deduced. From a careful consideration of all these facts, I think we arrive at the irresistible conclusion that natural history affinities are of no absolute value, as a means of ascertaining the medicinal powers of new and previously unemployed articles; though they may be worthy of some, or possibly even much regard, as a mere auxiliary to other means. It is undeniably the fact, that they some times though rarely afford correct information, though probably in only a very small minority of cases. They may therefore be advantageously used as a clue merely to guide us, in our investigations by other means, though they are too uncertain to be relied-upon alone or independent of other indications. Whenever then the medicinal powers of a new vegetable article are to be investigated, it is always proper to ascertain its natural history affinities; and as a general (if not invariable) rule, to make research by other means, in the direction whither such affinities lead us; but it is never safe to repose the least absolute confidence upon the indications afforded by these affinities, till they have been confirmed and verified, by such researches. I have discussed this subject much more fully and thoroughly than I intended and perhaps have been tediously prolix upon it, because I very well know that the aphorisms of the botanists have had an influence far more extensive, than has been openly avowed or even admitted; and as I very well know, they have led to a great deal of error, which may be found floating in the form of tradition, or anchored in print from which it will be next to impossible to remove it. If such matter only once gets into books, it is next to impossible ever to get-rid of it. If one or two generations of compilers expunge it from their works, another generation is sure to revive it, and thus it is perpetuated. Errors that have been handed down from the time of Hippocrates,

and that have been exposed and refuted a hundred times, are every little while revived, and come out as bright as a new coin directly from the mint; and judging from the past, will continue to do so, for ages yet to come. It becomes important therefore to keep-out all such errors as far as may by any means be possible. At the very best we shall have more than enough of them. Indeed it is not in every man's power to detect such errors, and no great number ever even think of doing it, or of attempting to do it. I have often read communications in American medical periodicals, in which powers were assigned to indigenous articles, from their botanical affinities merely, and which were about as correct as Sir John Hill's botanical plates made-up from the descriptions of plants. I infer that the powers were so assigned, because they were perfectly identical with those commonly ascribed to nearly allied European articles, and in the same words to a considerable extent, though according to my own experience, and that of several years duration, the American articles in fact had none of the powers of the European, but an intire different set peculiar to themselves.

2. Sensible Properties, as a means of Determining Medicinal Powers. Sapor or Taste and Odor or Smell.

It is my full belief that analogy, similarity and identity of taste and smell, afford much more certain and unequivocal indications of analogy, similarity and identity of medicinal powers and operations or effects, than natural history or botanical affinities. Linnæus says, "vires plantarum a fructificatione desumat botanicus, observato sapore, odore, colore, et loco." Linnæus adds, "probant ordines naturales veritatem aphorismi." (*Caroli a Linné, Philosoph. Botan. Edit. tert. cur. Carol. Ludov. Willdenov. Berol. 1793, p. 280, Sect. 339.*) As appears to me much more certain conclusions are to be deduced from the taste, odor, color, etc. than from the fructification, and that in fact Linnæus always had far more regard to them than to fructification, or even Natural Order. To the botanical investigators of materia medica, fructification and Natural Order seem to be much like the Stethoscope (so called) to the pathologist. They get all the credit for whatever valuable facts are ascertained, though such facts were really arrived-at by other means of research. Unfortunately however, neither tastes nor smells can be described with sufficient precision and definiteness, to ena-

ble us to communicate in words any knowledge in regard to their peculiarities, and the information to be derived from them, which any individual may have acquired by long and close observation, and accurate and extensive experience; and yet, the diligent cultivator of the *materia medica* may, if he will be at the pains, soon acquire such a degree of tact and skill, in regard to these qualities, as to enable him to make them very highly useful, in most of his investigations and researches. It ought to be distinctly understood that though no peculiarities of taste or smell, can be so distinctly specified, or so accurately described in words, as to lead an unpractised observer to a sure conclusion that a new article possesses any particular and individual medicinal power; yet in a more loose and vague way, and to a practised observer, taste and smell may be considered as highly useful for the purpose of ascertaining the remedial virtues of new and unknown articles, more particularly when they are derived from the vegetable kingdom. But it is by comparison chiefly, that taste and smell may be made useful for the purpose under consideration. For example, if we find that a root (whose powers have not been investigated) has very exactly the taste and smell of the officinal *Kalumb* or *Cocculus palmatus* for example, this fact will afford a very strong presumption indeed that it possesses the same medicinal virtues, or at least analogous ones. Thus the root of *Menispermum Canadense* has to my judgment at least identically the same taste and smell as the root of the officinal *Kalumb* or *Cocculus palmatus*; and its medicinal powers are believed from thorough trial and experience, to be equally identical. The first time I ever met with this article was during my professional pupillage, but before I knew any thing of botany, or had ever heard of Natural Order; and for myself, I settled its powers and uses, without knowing that it had any relation to the officinal *Kalumb* or *Cocculus palmatus*. Indeed it was not known, at that time to what Genus and Species this latter article belonged, nor in fact from what country it was brought. The first time that I ever met with the root of *Cissampelos Pareira* was likewise during my professional pupillage, and I immediately came to the same conclusion in regard to that article and by the same means, though at the time I had no knowledge of its name and native country; and this conclusion I still believe to be as correct as that of the

books. It is however not a little remarkable that the root of *Menispermum Canadense* should have had the same popular reputation in our own country, time immemorial, that *Cissampelos Pareira* has in its native country. I suspect that there are few intelligent physicians but that are constantly arriving at valuable facts in the *materia medica*, in the same manner.

The most illiterate regular physician that I ever knew—a man who had the smallest number of books, and had read the least of any man in his station—but a man however of good powers of mind—I was formerly surprised to find in possession of a good knowledge of our indigenous *materia medica*. I knew that he could not have obtained this knowledge from reading, and he had never been in habits of intercourse with any one, who had ever cultivated this branch of the medical profession. On inquiry, I found that nearly all his knowledge had been derived from a careful study of external sensible properties, and a comparison of these with the external sensible properties of the articles kept in the shops; and I was again greatly surprised at the skill and tact which he exhibited in his researches. This man, in his total destitution of books and reading, had made many valuable discoveries in physiology and pathology, which might have immortalized his name, had they not been made by some other person, at least a hundred years previous. But indigenous *materia medica* was comparatively a new field, where there was no body to take the palm from him, since he was far more correct than those who have assigned powers to indigenous articles, from their botanical affinities merely. If we find a root having very exactly the taste and smell of that of *Polygala Senega*, it will very rarely fail of having analogous or similar medicinal powers. It must be similarity in quality, to afford certain indications. A variation in intensity is of comparatively small importance, unless it is extreme. Although its sensible properties are differently described by Elliott, (in his *Sketch of the Botany of South Carolina and Georgia*) yet, according to my judgment, the root of the *Eryngium Yuccifolium* (Michaux) has a closely similar taste and smell to that of *Polygala Senega*, and it has likewise equally similar medicinal powers. According to my judgment also, the roots of *Chiococca racemosa*, and *Chiococca anguifuga* (although differently described by authors) very closely resemble the root of *Polygala*

Senega, in taste and smell; and they certainly resemble it, with equal closeness, in medicinal powers and operations. If we find a bark having very exactly the taste and smell of either of the officinal barks of Cinchona, we may in general rely on its exerting the same powers. But there is less certainty in this case than in most others, because the external sensible properties of the bark of Cinchona do not seem to depend, at least wholly and intirely, upon their medicinal active principles. Whenever this is the fact, there must be more or less fallacy in judging of medicinal powers, from external sensible properties. And yet I have good reason to believe that even in the case of the bark of Cinchona there is a certain quality of flavor, by which that which is really and truly good may always be distinguished. Just before the discovery and introduction into practice of the Alcaloid Quina, and the salts of the Oxyd of Quininum, there was much bad Peruvian bark (so called) in the market, so that it became important to be able to distinguish the good from the bad. This the late Dr. Eli Todd, of Hartford, Ct. could always do, and without failure; and as he informed me, it was done by the perception of a peculiar quality of flavor, i. e. taste and smell. Dr. Todd was equally acute in the discrimination of good and bad medicines, of widely different sorts; and it was always accomplished in the same manner. I have so often seen the skill in this way of Dr. Todd tested, that no room for the least doubt in regard to it, remains in my own mind; and to me, it furnishes an excellent practical illustration of the utility of attention to external sensible properties for the discovery of the powers of new and previously unknown articles. It is true that every body can not be expected to attain to the tact and skill in this way, of Dr. Todd though many more may doubtless do it, than is commonly supposed. Now it will be obvious that if good Cinchona may be distinguished from bad, by the peculiar quality of its external sensible properties, new and previously uninvestigated articles may very reasonably be inferred to have the same occult medicinal powers, when they have the same external sensible properties, provided there is discrimination between those external sensible properties, which depend upon their essential active principles, and their non-essential active principles, such as Tannic Acid for example. It is clear that Dr. Todd must have exercised discrimination

in this last respect, in order to be able to distinguish good Cinchona from bad. But it must be admitted that there is scarcely a case in the *materia medica*, in which the whole medical profession have failed so egregiously in finding equivalents of any given article, as in the case of Cinchona. From consulting books merely, as I have before stated, we should infer that there are hundreds of perfect equivalents of this important agent, in fact hundreds of articles, that are preferable to it, for the cure of Intermittent, and of Gangrene; and yet, as a matter of fact, there is only a very small number of vegetable species that have the power in question, exactly how many, no man yet knows, or what these species are. It is certain that this power is not possessed by all the barks, that come to us under the name of Cinchona. There must then be some peculiar difficulty in this case, though one that may be overcome by peculiar skill in tastes and smells, as Dr. Todd's ability showed. As appears to me, all the examples that have been given to illustrate the detection of articles similar to Cinchona in power, by external sensible properties, and indeed by any other mode of investigating new and previously unknown articles, except by actual administration in Intermittent and Gangrene, have been intirely fallacious (whether from want of sufficiently accurate attention or not, I will not here undertake to decide) with the great body of the medical profession. A certain number of years ago a bark was brought under the notice of physicians, by the name of Alcornocco or Alcornoque-bark. It was first said to be the produce of *Alchornea latifolia*, (I suspect from mere similarity of the popular name to the botanical name of the plant to which it was referred) and afterwards of *Bowdichia Virgilioïdes*; but whether either of them is its true source or not, I am unable to decide. But this bark has been alleged to have exactly the external sensible properties of Cinchona and therefore has been supposed to possess the same powers. Now it never appeared to me that its external sensible properties were sufficiently like those of Cinchona to render it at all probable that it possesses the same powers, so that this generalization does not appear to rest on sufficiently good grounds. In addition to this, I have never heard of any case in which it has cured either Intermittent or Gangrene. Such hasty conclusions are more likely to be without foundation than not, and usu-

ally contribute to bring all attempts at the application of the estimative principles I am discussing, into disrepute. Still I think that Alcornocco or Alcornoque-bark is more like a group of articles that have been considered (as I think erroneously) to be similar, or at least analogous to Cinchona, such as Quercus, Alnus, Cornus, etc. These are indeed styptic bitter-tonics, which Cinchona may also be said to be, but I do not think that they are febrifuge bitter-tonics, which is the essential peculiarity of Cinchona. The external sensible properties of the two groups agree in general, but differ so far as respects the peculiarities, which depend upon its essential active principles, a sufficient difference to separate them widely. It would be a great and very important thing to discover one or more perfect equivalents of Cinchona, and in regions of country where the equivalent might be definitely ascertained both as respects Genus and Species, and where it could not be under the control either of savages or barbarians, and certainly not under the control of both. It appears to me to be highly discreditable to the discrimination and research as well as the knowledge of the medical profession, that so many crude articles have been announced, and continue to be mentioned as equivalents of Cinchona, which are proved to have no sort of claim to such a character, by the very fact that they are not at all used as substitutes for it, while Cinchona itself still continues to be so expensive, and not only Intermittent but even Gangrene, continue to be so common. Since some where about the year 1820, the time of the discovery of the Alcaloid Quinina, five different substances, viz. Syringine, Illicine, Salicine, Rodiæine and Nectandrine, have been respectively announced, and now are customarily mentioned as equivalents of Quinine; and yet, who has thoroughly investigated these articles? A few private individuals have indeed tried what is commonly called Salicine, and found it no better than an equivalent quantity of any simple bitter-tonic, such as *Gentiana lutea*, *Cocculus palmatus*, etc. and utterly incapable of making an impression upon Intermittent of any intensity, or upon Gangrene; and even this has not been published. As to Rodiæine and Nectandrine, the most promising of all these substances, no body seems inclined even to make trial of them, when they have appropriate cases. To me this seems strange, but it is never the less a plain fact. If we find a bark possessing very exactly

the taste and smell of that of the *Canella alba*, it will assuredly produce more or less similar if not exactly the same medicinal effects. The bark of the root of *Ptelea trifoliata* has considerable resemblance in taste and smell to the bark of *Canella alba*, and accordingly its effects are about equally similar. It is rather more bitter in proportion to its acrimony, and accordingly we find that it is rather more tonic, in proportion to its oresthetic powers. But in order to judge with any considerable degree of certainty by taste and smell, the person judging must be very accurately acquainted with the properties of the articles which he takes as his standard of comparison; and he must in addition have great delicacy of sensation as respects taste and smell. It is certain however that some very active articles (even from the vegetable kingdom, not to mention remedies of chymical origin) are some times destitute, or at least very nearly destitute of all taste or smell. This is believed to be the fact with the bark of *Andira inermis*, which is only submucilaginous, and sweetish to the taste, and only a very little disagreeable to the smell. It is also the fact with the roots of *Euphorbia corollata*, *Euphorbia gracilis* and *Euphorbia Ipecacuanhæ*, which are only very slightly sweetish (scarcely in an appreciable degree) and a little farinaceous. These roots may be said to be intirely destitute of smell. I speak of them in their recently dried state—the state in which I have always used them. Even *Elatina* (*Elaterina* of some) the active principle of the pepones of *Ecballium Elaterium*, has too slight a taste and smell to admit of any description, or of being named.

What Cullen says upon taste smell and color, as a means of determining medicinal powers, is upon the whole as well worthy of attention, as what he says upon natural-history affinities, for the same purpose. Cullen says “an other means proposed for judging of the virtues of different substances is, by attending to their sensible qualities of taste, smell and color.” “As we have already remarked that the operation of medicines is chiefly on the nervous system, so that as the sensations of taste and smell depend upon an action of certain substances upon the nerves of the tongue and nose, and their effects are very often from thence communicated to the rest of the body; so it may in some measure be presumed that those actions on the organs of taste and smell may be com-

municated to the whole of the nervous system, or may show an analogous power with respect to the system, when applied to the other nervous parts of it." (*Ibidem.*) I scarcely need pause to point out the absurdity of Cullen's notion that the system may be affected by a medicine through the medium of such nerves of special sensation as the olfactory and the gustatory. As well might we expect the system to be affected by a medicine through the medium of the other two nerves of special sensation, the optic and the auditory. When we find a medicine capable of affecting the system by means of being looked-at, then we may expect to find medicines capable of affecting the system through the senses of smelling and tasting. Cullen here does not distinguish between impressions made upon nerves of special sensation, and impressions made upon the nerves of common sensation, with which the organs of special sensation are always furnished; and likewise impressions made upon the sensor and motor nerves of the blood-vessels, through which medicinal impressions are very frequently (if not always) propagated to other parts of the system. The truth is that the nerves of special sensation are intended merely to convey certain external impressions to the hemispheres of the cerebrum, which are the organs of mental or intellectual operations. Each one of these nerves performs only a single function, without any of the variations to which common sensation seems to be subject, a function always very peculiar in character, and always widely different from any variety of what is called common sensation. Let it not be forgotten that medicinal impressions are never transmitted through these nerves. Never the less, there may be a regular and definite relation between the modes in which certain agents affect the organs of taste and smell, and the modes in which they affect the nerves of common sensation, and also the modes in which they affect the involuntary motor nerve of chymical action nutrition and reproduction, the two sets of nerves whose province it is to transmit medicinal impressions.

Cullen says, "I presume very confidently, to give it as a very general rule, that those substances which do not at all affect the taste or smell, and even those which affect these organs in a slight degree only, may be considered as inert and useless; and that all such substances should be rejected from the lists of the materia

medica, excepting a very few which, though without sensible qualities, may on this very account be of an emolient, demulcent, or nourishing quality." (*Cull. Mat. Med. Bart. Ed. Philad.* 1812, v. i. p. 91-2.) This rule is very nearly true of vegetable organic substances; but certainly among these there are some exceptions to it. I think also that there are some exceptions to it among animal organic substances; but there are many more among chymical inorganic substances. Cullen says, "Although physicians have not sufficiently attended to this general rule, they have however, at all times, from substances being endued with sensible qualities, presumed upon their activity in the human body; and from the state of their sensible qualities, have formed a judgment of their medicinal virtues." Cullen says, "It has indeed almost always happened, that from a similarity of taste and smell, in different substances, physicians have been ready to suppose a similarity of virtues." "Such a supposition" (continues Cullen) "is indeed well-founded in many instances; but it has been carried too far, and similarity of taste and smell in different plants has been supposed to point out, with some exactness, the same medicinal virtues." "Sir John Floyer, David Abercrombie, Hoffman, and several others since their time, have, upon this plan, given systems of the whole materia medica." "In the sequel" (says Cullen) "I shall have occasion to make many applications of this general doctrine, and shall endeavor to show how far it may be justly carried; but at the same time, it is very proper here to be at some pains in pointing out the fallacy that attends the universal application of it." "In the first place" (says Cullen) "there is a considerable difficulty in ascertaining the difference of tastes in different substances." "There are some" (says he) "such as the sour, the sweet, the bitter, and the styptic, which can be very well distinguished from one another, and about which, mankind are generally agreed; but there are many other tastes which can not be comprehended under any one general head." "It appears to me, that some general heads have been attempted, if not improperly, at least to very little purpose." "Thus to make a general class of tastes under the title of acrid; but this term expresses the force of impression, rather than any particular sensation; and it has always comprehended substances of otherwise very

different qualities, which we shall consider more particularly afterwards, under the head of stimulants." (*Ibidem.*)

For myself I do not believe that what is commonly called the taste of acrimony, at least so far as the acrimony is concerned, results in any degree from the mere force of impression upon the gustatory nerve exclusively, but depends intirely upon the peculiar quality of the impression, and that perceived not merely by the gustatory nerve, but also by nerves of common sensation, as is the fact with some other tastes commonly so reckoned. Here very evidently Cullen fails to discriminate impressions made upon nerves of common sensation, from those made upon nerves of special sensation; and this he appears always to have done, whenever he has supposed that medicinal impressions may be received and propagated to other parts of the system, by nerves of special sensation, as the nerves of olfaction and gustation. The same error can not be fallen into, in regard to the nerves of vision and audition. What is called an acrid taste is then a compound sensation having its seat partly in nerves of common sensation, and partly in a nerve of special sensation. The impression made upon the nostrils by gaseous Ammid of Hydrogen or Ammonia, is of a similar compound character, and likewise has its seat partly in nerves of common sensation, and partly in a nerve of special sensation. When there is a perfect anæsthesia (commonly but erroneously called a Paralysis) of the olfactory nerve, the inhalation of gaseous Ammid of Hydrogen will produce sternutation as readily as when the sense of smell exists in the greatest perfection, and the liquid Ammid of Hydrogen by topical application will blister the Schneiderian membrane as readily under a perfect anæsthesia of the olfactory nerve, as when the sense of smell is the most perfect. It is only in the cases of smelling and tasting, that special and common sensations are liable to be confounded.

"An other title employed with no better success in forming a class of tastes is the nauseous, which is manifestly too general, as comprehending many, which in general have a disagreeable but at the same time a peculiar taste; in other words, one different from any other, and therefore not to be brought under any general rule." "It is obvious likewise that the class of nauseous tastes comprehends many substances of very different virtues, and thus

must always give an insuperable difficulty in the arranging of virtues according to taste." (*Ibidem*, p. 93.) Now I have no hesitation in saying that unless we have some thing more definite and discriminating than all this, we shall never derive any benefit from attention to external sensible properties, in our researches for the powers of new and previously unknown articles. It is not sufficient to ascertain that an article is bitter merely. Some thing more precise than this must be ascertained. There is one quality of bitterness in Aloë, and quite a different sort from that of most other articles. If we find an article having the same peculiar quality of bitterness as Aloë, we shall have good grounds for suspecting it to possess similar powers. For example the peculiar quality of the bitterness of *Polygala polygamum* is very remarkably like that of Aloë, and accordingly we find that this article is in fact as near like Aloë in its powers and operations, as it is in external sensible properties. Few persons, I fancy, are able to distinguish the effects of the two. Again the extract of the root of *Convallaria Majalis* has a very near resemblance in the quality of its external sensible properties to Aloë, and its powers and operations are as nearly the same. But independent of acquaintance with at least one article having this peculiar quality of bitterness, and having the peculiar powers and operations of Aloë associated with it, we should have no right to infer such powers and operations. Colocynth is as bitter as Aloë, but its bitterness is of a very different quality. If we were not acquainted with Colocynth, we should have no right to infer that an article having this peculiar quality of bitterness would be drastic-cathartic; but being acquainted with Colocynth, we may expect similar powers in any new and previously unknown article, that has exactly the same quality of bitterness. Mere bitterness in general without regard to its peculiar and exact qualities, might indicate various other powers, beside that of a cathartic; and it is well known to belong to most, if not to all the tonics; and peculiar qualities of bitterness indicate peculiar qualities of tonic power, just as peculiar qualities of bitterness indicate peculiar qualities of cathartic power. As I have just stated, the peculiar quality of the bitterness of Aloë, and the peculiar quality of the bitterness of Colocynth are widely different, and their cathartic powers are equally different. Indeed every thing that is common to these two articles is

the bare and simple fact that both are cathartic. The manner of their operation in all respects is materially different. Again *Ignatia amara* is intensely bitter, but the peculiar quality of its bitterness is quite different from the peculiar quality of the bitterness either of *Aloë* or *Colcynth*, of *Cinchona* or *Gentiana*, etc. and its powers are equally different. But after acquaintance with the peculiar quality of the bitterness of *Ignatia*, it is very easy to perceive that *Strychnos Nux-vomica* has the same quality of bitterness, and if we were unacquainted with the powers of this latter article, we might infer with a very strong probability that its powers must be the same. This as appears to me, is the manner in which external sensible properties must be employed, in the investigation of the powers of new and previously unknown articles; and so employed, they are highly valuable, and afford more certain indications than any other means in our possession, but used in the loose way that Cullen mentions, they must contribute more to lead us astray than to guide us in the right way. I do not at all doubt but that Cullen himself in fact used them just as I describe, and not in the indefinite and vague manner of which he speaks. The mode of employing external sensible properties, as I direct, does not require greater acuteness, or nicer powers of discrimination than most people possess, but it requires cultivation of this acuteness and of these powers of discrimination, as well as perfectly accurate observations.

“Beside the general tastes,” (continues Cullen) “which we have said are tolerably well ascertained, there are many combinations of these, which give a variety of tastes, not to be exactly ascertained, nor always, so far as we yet know, to be taken as a mark of particular virtues.” (*Cull. Mat. Med. Bart. Ed. Philad.* 1812, v. i. p. 93.) By “general tastes,” Cullen probably has reference to those ordinarily specified in works on materia medica, such as are enumerated and pretended to be defined, (for example) in Swediaur, viz. “*fatua, aquosa, oleracea, mucilaginoso, pinguis, aromatica, acris, pungentia, calida, fervida, urentia, salsa, acida, austera, styptica, dulcia, amara, nauseosa, insipida*, etc. Now all this is just about useless in the researches for which I am attempting to give directions. It is not true that these are all the simple tastes in nature, and that all the rest are some compound of these. I repeat that unless we can have some thing more defi-

nite and precise than all this, external sensible properties can not be turned to any valuable use for the purpose I am considering. What is really useful and important, amounts to mere peculiarities of quality in taste, which, though they may be incapable of intelligible description in words, are never the less easily understood and employed, and that too with all the necessary accuracy.

“But further” (Cullen adds) “when we have collected a number of substances under any one of the general classes of tastes, we find the individuals possessing very different degrees of the same quality, and thereby of very different powers.” I do not agree with Cullen that “very different degrees of the same quality” ever give rise to “very different powers.” They may give rise to “very different degrees” of the same power, and this is the most that they can do. “In many instances, indeed where the quality of the class is prevalent in a plant, it has at the same time joined with it other qualities which give it different virtues from those of the general class.” Cullen says “it is needless however to insist further here on the fallacy of the general doctrine, because we shall have frequent occasion hereafter to take notice of it, and to point out the many exceptions, with which it is to be received.” “Bodies which give out a strong scent” (says Cullen) “whether agreeable or disagreeable, seem to be peculiarly fitted to act upon the nervous system and some very powerful medicines are remarkable for this quality.” He adds that “Linnæus however, carries the matter too far, when he maintains that odorous bodies act only upon the nerves, whilst sapid bodies act only upon the muscular fibers, for it is evident that sapids act also, and some times very powerfully, upon the nerves.” (*Ibidem.*) Now I can not at this time think of any vegetable organic medicine with a very strong fetid scent or odor, that is at the same time “very powerful;” and all very strong scented vegetable medicines that I can think-of are euphrenics (a class of which Protoxyd of Nitrogen may be taken as the type) and are feeble articles comparatively. *Chenopodium Vulvaria* (Linn.) or *Chenopodium olidum* (Curtis) and *Caroxylon foetidum* (De Candolle) or *Chenopodium Baryosmon* (R. & S.) are among the most strong scented fetid vegetable organic articles that, to my knowledge, are used in medicine; and yet they are rather weak euphrenics. Stephenson and Churchill mention them as true emmen-

agogues i. e. articles that act immediately directly and exclusively to increase the activity of the catamenial excretories of the uterus, without affecting any other part of the secernent and absorbent or glandular system. If they operate in this manner, they are the only articles of the materia medica that are hitherto known to do so. Stephenson and Churchill intirely overlook their euphrenic power, though it has been mentioned by old writers as a power of *Chenopodium Vulvaria* (Linn.) under the denomination antispasmodic, i. e. a remedy for some thing which is like spasm without being spasm. I suppose they considered these articles as possessing too slight an amount of this power, to be worthy of mention, if indeed they admitted them to possess any degree of it. Perhaps *Opopanax Chironium* (Koch) and *Narthex Assafœtida* (Falconer) and the group of agents of which these two are the type, have the next strongest fetid scent, of any vegetable organic articles of the materia medica; and yet the powers of this group are so feeble that it is has always been a serious question whether they have any or not; and if they have, what they are. Perhaps *Valeriana*, if used with sufficient freedom may be considered as a strong fetid-scented article, which is "powerful," if not "very powerful." As it is ordinarily employed however, no such thing as powerful effects are ever produced by it. But *Valeriana* is also a mere and pure euphrenic, so far as I am acquainted with it. The strongest fetid-scented article of the materia medica, which is of chiminal inorganic origin, is perhaps Sulphidrous Acid (i. e. Protosulphid of Hydrogen) and this is indeed active. I do not think that Bromine (whose very name implies stench) is either very strong scented, very fetid, or very active. I believe that all exquisitely fragrant or agreeable odorous articles are likewise euphrenic, but I do not know of a case in which such an article can be said to be an active or "very powerful" euphrenic. In fact I know of no such article, that is capable of producing any grade of euphrenic effect beyond very moderate exhilaration, and this only under the most favorable circumstances. Perhaps *Nardostachys Jatamansi* may be esteemed an exquisitely fragrant article (for I am not acquainted with its odor) and perhaps it may also be reckoned a "powerful medicine," if not a "very powerful" one. But it is a mere and pure euphrenic. *Andropogon* (*Cymbopogon*) *Ivarancusa* is strong scented and exquis-

itely fragrant, and a pure euphrenic; but it is by no means a "very powerful medicine." On the contrary, it is a feeble agent. Now I think that the operation of all the mere and pure euphrenics not only begins, but terminates in the nervous function, and therefore I agree with Linnæus that all highly odorous bodies, whether they are highly fetid, or highly fragrant, act only upon the nervous system, provided they have not other properties associated with their highly fetid, or highly fragrant ones.

But let it not be forgot that highly odorous bodies do not appear to produce their effects upon those parts of the nervous system in which we perceive the manifestations of their operation, by their impression upon the olfactory nerves. The subject of their operation indeed perceives their odor, but neither the obviation of languor or lassitude when it existed, the production of a calm placid and pleasant sensation, the production of a peculiar preternatural wakefulness, nor any degree of positive exhilaration, (all of which are grades of a euphrenic operation) depend at all upon the perception of the odor of the article. Some of the most efficient euphrenics produce all of these grades of effect, and even additional ones, without any odor at all, so that euphrenics can not always be supposed to operate through the medium of the special sensation of olfaction or smelling. It is said that there are a few individuals that fall into a state of anæsthesia (the next grade of the operation of a euphrenic after positive exhilaration) when exposed to the influence of an intensely fragrant article. This state is commonly called fainting, for want of a known popular name, though it is agreed to have nothing in common with Syncope, except insensibility to pain during its existence. Some have supposed that this state is occasioned by vitiation of the atmosphere by the perfume to such an extent as to render it incapable of adequately supporting life. That this can not be the fact however, is abundantly proved from the circumstances that only one person out of thousands has such peculiar susceptibility as to be affected in this manner, by the most fragrant articles known. This effect seems more like being produced through the special sense of smelling than any of the other grades of a euphrenic operation; and yet it is my opinion that it would take place upon such a subject from the influence of the same agent, even if the subject were affected by the disease called *Parosmis*

expers. The volatile principle or principles of the agent, whether it is fragrant or fetid must of necessity affect the nerves of common sensation, or there could not be an anæsthesia, since a mere affection of the nerves of olfaction, and that in such a manner as to produce a strong sensation either of fragrance or foetor, is the very opposite of anæsthesia; and how a strong sensation in the nerves of olfaction should occasion a privation of sensation in the nerves of common sensation, is not easy to be seen.

As to the opinion that all highly sapid bodies act only upon muscular fibers, I think with Cullen that it is a mere chimera altogether destitute of foundation. "I go on to observe" (continues Cullen) "that the judging of the virtues of plants, from their particular scent, is liable to still more fallacy than the doctrine of tastes." "Scents are of greater diversity than tastes; and it is still more difficult to reduce them to any general classes." "Indeed it does not happen that any other general division can be made of them, than that of the agreeable and the disagreeable." "It is true that each of these comprehends a great variety, but not to be assorted with any precision, under general heads." "Linnæus has attempted this, but it is enough to look at his general titles and his enumeration of plants under each, to perceive that they give no precise ideas, nor point-out any common qualities, but what arise from the general terms of agreeable and disagreeable, and that even these are considerably diversified in respect of power, and very often show different effects, according to the difference of the persons, to whom they are applied." "The analogy therefore, afforded by odors, is of exceedingly little use, in illustrating the materia medica." (*Cull. Mat. Med. Bart. Ed. Philad.* 1812, *V. I. p.* 92-3.) I consider all this as very little applicable to the subject under discussion, and I should agree with Cullen that odors, as specified and defined in books, are of no advantage towards ascertaining the powers of a new and previously unknown article. For example Swediaur in his materia medica specifies the following viz. fragrantia, suaveolentia, ambrosiaca, hircina, graveolentia, foetida, tetra, suspecta, inodora etc. I consider such specifications as of no value for our present purpose. Suppose that I am well acquainted and perfectly familiar with the peculiar quality of the odor of *Andropogon* (*Cymbopogon*) *Ivarancusa*, and that I should find an other plant (no matter

whether belonging to the same Genus or some other, no matter whether belonging to the same Natural Order or some other) that has exactly the same quality of odor and just about the same degree of it, would there not be good ground for concluding that it possessed the same medicinal power? Suppose I am well acquainted and perfectly familiar with the peculiar quality of the odor of *Valeriana officinalis* and that I find an other plant (no matter whether belonging to the same Genus, or some other, no matter whether belonging to the same Natural Order, or some other) that has exactly the same odor both in quality and in degree, would there not be good ground for concluding that it possessed the same medicinal power? Suppose a plant should be discovered, whose inspissated descending sap had exactly the same quality of odor as the inspissated descending sap of *Narthex Assafoetida*, should we not conclude at once that it must possess the same medicinal powers? Suppose we find a new plant, whose seeds possessed precisely the same quality of odor, as the seeds of *Abelmoschus Moschatus*, (W.&A.) should we not conclude that they must possess a similar or analogous medicinal power? Suppose a new animal substance should be brought to light, having exactly the same quality of odor as *Moschum Moschi Moschiferi*, and about the same degree of it, would not the conclusion that it must possess the same medicinal power be legitimate? Every intelligent physician (as I doubt not) constantly avails himself of odor, as well as taste, to assist his judgment in regard to any article new to him, to which his attention may happen to be called. In judging of the quality of almost every medicine of the shops, which is liable to deterioration, we avail ourselves instinctively of odor as well as taste. What physician does not know the value of odor for assisting us to judge of the quality of the root of *Rheum (officinale)* of the extract of *Aloe vera*, and other species? It is true there are far fewer medicines that have a decided and prominent odor, than there are that have a decided and prominent taste, so that the latter must always be of much more extensive utility than the former; but still the former are of great value and utility if skilfully and discriminatingly used. I doubt not that Cullen himself often turned it to better account than he admits, or than he was in fact aware-of.

Color. As respects color I do not know that, in the present state of our knowledge, it can be made very materially useful for

the purpose under consideration. Cullen adds finally that "Linnaeus, when he alleges that the virtues of medicines may be known from their sensible qualities, does, beside the taste and smell, suppose that the color likewise may give some indication of virtues; and accordingly, he has the following paragraph," (viz.) color pallidus insipidum, viridis crudum, luteus amarum, ruber acidum, albus dulce, niger ingratum indicat." But nobody possessed of the smallest knowledge of plants, can fail to mark so many exceptions to each of these, as to perceive that the attempt to establish such general positions is extremely frivolous and useless." (*Ibidem*, p. 93-4.) Provided even the subject of color were properly investigated and studied, I doubt not that some thing might be made of it, in reference to the detection of the powers of new and previously unknown articles; but as I have made no such investigation, and have bestowed no study upon it, I can not pretend to say any thing about it.

3. Chymical Composition, as means of Ascertaining and Determining Medicinal Powers.

At the present period a very considerable number of vegetable organic proximate principles are known, which possess valuable medicinal powers, and which, if they predominate to a certain extent over all the other proximate principles of a given plant, will enable such plant, either in its crude state or in some ordinary pharmaceutic preparation, to produce the medicinal effects of such proximate principles. When on analysis, any new and previously unknown article is found to contain such a quantity of any active proximate principle as to give power to any convenient doses of the crude article—such a quantity of such active principle as to prevent the crude article from being used for the effects of any other proximate principle, which it may contain in much smaller quantity, or which may require a much larger dose to procure a proper degree of effect—we may consider that we have succeeded in ascertaining the powers of such article, as it were a priori, and by the aid of chemistry merely. Now many of these proximate principles are not confined to a single species, but are common to a considerable number of species; and in addition to this, are easy of detection, even as respects quantity, in the crude plant, so that the powers, operations and effects of such plants may easily be determined a priori, as may even the proper dose, at least very nearly.

Hypocarbonic Acid (commonly called Oxalic Acid) is an antiphlogistic. Now this Acid exists in such abundance in the juices of several plants, either in a free state, or in the forms of Bi-hypocarbonate or Quaternhypocarbonate of Potassa, (both salts being better known by the names of Binoxalate and Quaternoxalate of Potassa) as to render the expressed juices of those plants quite efficient antiphlogistics. This Acid and its Salts, are sufficiently easy of detection in a crude plant; and the fact of its existence to a certain amount in any plant, is a sure indication that either the crude plant, or its expressed juice, or an extract made by inspissating such juice, may be made useful as an antiphlogistic. Here let it be particularly observed that expressed juices, or extracts made by inspissating such juices of those plants containing vegetable organic Acids, that are sour to the taste, have been found by much observation and ample experience, to be far preferable for medicinal purposes, to the perfectly pure Acids intirely separate from all other vegetable matter, as obtained by chimical process. The operation of an expressed juice, or an extract made by inspissating such juice, is not only more mild and kind, but it is far more effectual. Indeed such a preparation will often effect a cure where the pure Acid, as obtained by chimical process, will intirely, and as is alleged, invariably fail. For example, it is alleged that expressed juices and extracts made by their inspissation, which contain the vegetable organic Acids that are sour to the taste, will invariably cure *Porphyra nautica* or Sea-Scurvy, but that these Acids in a state of purity, as obtained by chimical process, will inevitably fail of accomplishing this purpose. This fact is all that is necessary in this place, the reason for it belonging to therapeutics, and resulting immediately from certain facts in pathology.

The Hypocarbonic or Oxalic Acid exists in such abundance, as to give antiphlogistic power, to the herbaceous part, and especially to the juice of *Oxalis Acetosella*, (Linn.) *Oxalis corniculata*, (Linn.) *Oxalis stricta*, (Linn.) *Oxalis Violacea*, (Linn.) and I doubt not numerous other species. It is commonly said that this Acid exists in these plants so combined with Potassa as to form Bi-hypocarbonate or Binoxalate of Potassa. This Acid is said to exist in the same combination, in the herbaceous part, and especially in the juice of *Rumex Acetosa*, *Rumex Acetosella*, (Linn.) *Gera-*

nium acidum, Cicer parietinum, etc. Scheele enumerates twenty-two roots and fifteen barks, in which he found Hypocarbonic or Oxalic Acid. In how many of these it existed in such quantity as to render the expressed juice, or the extract, or any ordinary pharmaceutic preparation, antiphlogistic, I know not. It must be recollected that Hypocarbonic or Oxalic Acid (as the former name implies) is Carbon acidified by Oxygen, its composition being $C^2 + O^3$.

But other vegetable organic Acids occur as frequently in the vegetable kingdom (and indeed much more frequently) that are sour to the taste, and consequently are as good antiphlogistics, as Hypocarbonic or Oxalic Acid. For example, all the vegetable organic Acids consisting of a compound-radical of H. C. acidified by O. and being sour to the taste, possess antiphlogistic power; and they impart this power to any crude plant, which contains a sufficient amount of any of them, either in a free state, or combined in the form of a super-salt with the Oxyds of some of the elements near the electro-positive end of the scale of elements, which are also antiphlogistic, as well as the Acids. There is a compound-radical of $H^2 C^4$ which so far as I have knowledge, has never been obtained in a separate state, but which by different degrees of oxygenation, makes at least four, and probably five different and distinct Acids, three of which exist in certain plants, either in a free state, or in the form of super-salts of the Oxyds of some of the elements near the electro-positive extremity of the scale, and in such quantities as to render the crude plant, or its expressed juice, antiphlogistic. Of these Acids the Tartaric, Racemic, Citric, Malic and Succinic are the best known. In fact there are at least five, and perhaps seven different substances composed of this compound-radical (viz. $H^2 C^4$) with a different number of equivalents of Oxygen, or such a difference of aggregation of the ultimate particles of the compound as to give different chymical habitudes and relations. There is first the compound-radical $H^2 C^4$ which according to the laws of the nomenclature, should be called Tartarogenium or Tartarogen. There is second $H^2 C^4 + O^1$ which is commonly called Peucedanina or Peucedanine, but which should be called Acidum Subhypotartarosum or Subhypotartarous Acid. There is no Deutoxyd of this compound-radical yet known, but it may be fairly expected that

one will yet be discovered. There is third $H^2C^4+O^3$ which is commonly called Acidum Succinicum or Succinic Acid, but which should be called Acidum Tartarosum or Tartarous Acid. There is fourth $H^2C^4+O^4$ which makes two different vegetable organic Acids, that are commonly called Acidum Malicum or Malic Acid, and Acidum Citricum or Citric Acid. One of them however should be called Acidum Hypotartaricum or Hypotartaric Acid, and the other Acidum Parahypotartaricum or Parahypotartaric Acid. There is fifth $H^2C^4+O^5$ which makes two or three different vegetable organic Acids, that are commonly known by the names Acidum Tartaricum or Tartaric Acid, Acidum Paratartaricum or Paratartaric Acid, and Acidum Racemicum or Racemic Acid. The names of the first two, as just given, are correct according to the principles of the chymical nomenclature; but if the third is really different and distinct from the two preceding, it should be called Acidum Metatartaricum or Metatartaric Acid.

There are two substances consisting of this compound-radical in combination with Chlorine viz. $H^2C^4+Cl.^3$ and $H^2C^4+Cl.^4$, but neither of these are vegetable organic proximate principles, nor are they known to be medicinal; and I just mention them only to illustrate the characters and habitudes of the compound-radicals contained in plants. Tartaric Acid is said to be contained in the pulp of the fruit of Tamarindus Indica (Linn.) in conjunction with Malic and Citric Acids; in the pulp of the fruit of Oxycoccus vulgaris (Persoon) in conjunction with Citric Acid; and I suppose also in the pulp of the fruit of Oxycoccus macrocarpus (Pursh) as is said by Dr. Thomas Thomson; in the external coat of the fruit of Rhus Typhina (Linn.) and I suppose also in the same part of the fruit of Rhus glabra (Linn.) though it has been affirmed by an American, that the Acid of these two plants is the Malic Acid. Tartaric Acid is likewise contained in the pulp of the fruit of Vitis Vinifera (Linn.) and Morus alba (Linn.) and I suppose, but do not know with certainty, Morus nigra (Linn.) and Morus rubra (Linn.) It is said also to be contained in the herbaceous part of Rheum Rhaponticum (Linn.) and in conjunction with Malic Acid in Agave Americana. It is said to be contained also in Rhus Coriaria, but in what part I know not. Malic Acid as is said unaccompanied with any other Acid, is alleged to be contained in the

pulp of the fruit of *Berberis vulgaris* (Linn.) *Prunus spinosa* (Linn.) *Prunus domestica* (Linn.) *Sorbus Aucuparia*, *Malus communis*, *Sambucus nigra*, etc. Accompanied with about an equal quantity of Citric Acid, it is said to be contained in the pulp of the fruit of *Ribes rubrum* (Linn.) *Ribes Grossularia*, *Cerasus hortensis* (Persoon) *Fragaria vesca* (Linn.) and I suppose also, but do not know, in *Fragaria Virginiana* (Linn.) and *Cerasus Avium*. With about an equal quantity of Citric Acid it is contained in the pulp of the fruit of *Vaccinium Myrtillus* (Linn.) *Rubus Chamæmorus* (Linn.) *Rubus Idæus*, *Pyrus Aria*. Citric Acid unmixed with any other Acid is said to be found in the juice of the fruit of *Citrus Limonum*, *Citrus Bigaradia*, *Citrus Aurantium*, and I suppose doubtless likewise in the juice of the fruit of *Citrus Decumana*, *Citrus* (Lima). Citric Acid is also found alone in the pulp of the fruit of *Vaccinium Vitis-Idæa* (Linn.) *Cerasus Padus* (De Cand.) *Solanum Dulcamara* (Linn.) and *Rosa Canina*. Dr. Thomas Thomson says that Citric Acid is found alone in the pulp of the fruit of *Oxycoccus vulgaris* (Persoon) but in an other place he mentions that this article contains Tartaric Acid. Now there are doubtless a multitude of other plants that contain these three Acids, either in a free state, or in combination with the Oxyds of some of the elements near the electro-positive extremity of the scale of elements, and contain them in such abundance as to render the crude article, or some ordinary pharmaceutic preparation of it antiphlogistic. The ordinary pharmaceutic preparations are either the expressed juice, or an extract made by its careful inspissation. The chymical tests for these Acids are very easy of application, and their presence may even be ascertained with sufficient certainty by mere taste. Whenever therefore a new fruit or herb is found, that contains these Acids in sufficient abundance, we may be sure that the expressed juice or an extract made by its inspissation, will be certainly antiphlogistic, and may be used accordingly.

I have begun my illustration of chymical composition as a means of ascertaining and determining medicinal powers, with antiphlogistic principles, because the antiphlogistics constitute my first class of remedial agents, and because I think that the study of this class is too much neglected, and therefore not so well understood at the present time as it ought to be. The value and im-

- portance of the expressed juices of these plants, or the extracts made by their inspissation, in the treatment of truly phlogistic or entonic diseases, can not by any possibility be thoroughly understood and appreciated, by those who have seen them employed only in the cases, which at the present time are mistakenly considered as phlogistic or entonic. It so happened that I began my professional studies before the change in the general diathesis, in the place where I studied, so that I had ample opportunity to witness the truly phlogistic or entonic diseases of a former period, as well as the effects of medicine upon them. The change in general diathesis, above alluded-to, took place in the region where I studied, during the latter part of my professional pupillage, so that I had opportunity to watch the phases of disease during the change, and after it was confirmed; and last, I began practice by myself, in a place where this change had not taken place, and therefore again had opportunity to witness the course and progress of the change. I have selected as my first example of the use of chimical composition as a means of ascertaining and determining medicinal powers, a truly simple and plain example, which can not by any possibility be misunderstood.

There can not be said to be any nauseating compound principles of inorganic or chimical origin; but certain elements seem to possess this power per se, and certain other elements, though not possessing it per se, invariably impart it to their compounds. The presence of one of these elements in any compound may therefore be always considered as indicating nauseating powers a priori, unless such compound contains some other principle so much more active, as to transcend and supersede any and all nauseating effects. Who would hesitate to pronounce any compound containing a notable proportion of Stibium or Antimony to be nauseating and emetic, even before it has been tried; and pretty much the same may be said of the compounds of Zinc and of Copper. As to vegetable organic nauseating principles, they are doubtless legion. It will be sufficient to mention one, viz. what is so improperly called Emetina or Emetine, as if it were the only nauseating and emetic principle in nature. It should be called Cephaeline or Cephaëlidine, since it was first obtained from *Cephaelis Ipecacuanha*, and is an Alcaloid. Any vegetable containing this principle in any notable proportion, in comparison with its other proxi-

mate principles, may be presumed with absolute certainty to be nauseating. The same may be said of a large number of other vegetable organic proximate principles.

There are seven (and more) leantic (i. e. demulcent and emollient) principles, viz. Acidum Pecticum, Gummi, Fæcula, Saccharum, Olea pinguis, (vegetabilia et animalia), Gelatina, Albumen, etc. When ever any one of these principles exists in a notable quantity, in any organic substance in proportion to its other proximate principles, we may with great certainty conclude a priori that such organic substance is capable of being used as a leantic.

As all the neuragics (of which Lead may be taken as the type) are of inorganic and chymical origin, except perhaps a single article, there are no compound neuragic principles; but certain elements possess a neuragic power per se, and impart it to their compounds, while certain other elements not neuragic per se, always produce compounds having this power. When ever we find a compound of either of these classes of elements, we may conclude a priori that by proper management, it will certainly be capable of producing some grade of neuragy, unless such effect is transcended and superseded by some other power. Thus by sufficiently protracted use, any compound containing Lead may be expected sooner or later to produce Lead-Neuralgia and Lead-Palsy in addition to the medicinal grades of operation, which usually precede these non-medicinal and ultimate grades of a neuragic effect.

With the exception of Unihydrite of Protoxyd of Etherogen or Alcohol, Binhydrite of Protoxyd of Etherogen or the active principle of Wine (neither of which are ever used for their narcotic effects) Protocyanid of Hydrogen and Benzhylid of Hydrogen and perhaps a few other substances, such as the narcotic principle generated during the production of Fæcula Amylum or Starch from the tubers of Solanum tuberosum or Peruvian Potato (incorrectly called Potato, since the name belongs to quite a different article) and that narcotic principle which is found in the stagnant water in which Cannabis sativa or Hemp has been macerated, or rotted as the popular term is, preparatory to dressing for cordage or cloth, the operation and effects of which are said to be intirely different from any thing naturally contained in Hemp, I believe that all narcotics are of vegetable organic origin, and that the

proximate principles in which narcotic powers reside are almost as numerous as the crude narcotics themselves. But there are some exceptions to this. *Strychnos toxifera*, *Rouhamon Gujanense* and *Rouhamon Curare* are said to contain the same active principle viz., the Alcaloid *Curarina* or *Curarine*. Who would hesitate to consider any new plant found to contain *Curarine*, as a substitute for either of these? All the medicinally active species of *Datura* are said to contain one single active principle viz. *Daturina* or *Daturine*; and it is said that this is also the active principle of *Atropa lethalis*, and that this latter plant contains it in greater abundance than any of the species of *Datura*. But whether the active principles of *Datura* and *Atropa*, are identical, or only have a near resemblance, but still are some what different, and constitute distinct Alcaloids, is not material to my argument. Surely no one would hesitate to use any new plant found to contain *Daturine*, as a substitute for the *Daturæ*.

There are no erethistics of inorganic and mere chymical origin, but all are organic vegetable. The active principles of but few of the erethistics are known, but those with which we are acquainted we may be said to know very well. One of the best known erethistic principles is the Alcaloid *Strychnina* or *Strychnine*. *Ignatia amara* and several species of the Genus *Strychnos* have already been found to contain it, and they are used in medicine accordingly. Who would hesitate to employ any other species of this Genus, or indeed any other plant, that shall be found to contain this substance? It is not a matter of course that all the *Strychni* contain it, since *Strychnos toxifera* contains a widely different but still more active principle, and *Strychnos Pseudo-Quina* must contain some principle also widely different, and far less active than either *Curarine* or *Strychnine*, since it may be used with about the same freedom, as the officinal species of *Cinchona*, and as is represented, with no more intensity of effect. Very much the same remarks may be made of *Vomicina* or *Vomicine* (some times improperly called *Brucina* or *Brucine*) which have been made of *Strychnine*, as it is an erethistic principle, with powers closely analogous to those of *Strychnine*, except that it requires much larger doses to produce a given degree of effect. Were *Vomicine* to be detected, as a predominant principle (as it is in the bark of *Strychnos Nux-vomica*) in any plant, whose

powers were previously unknown, no doubts could possibly be entertained as to its operation and effects, its safety as a medicine, and its therapeutic application. Aconitina or Aconitine is another erethistic principle, which if it should be found in a quantity predominating over other active principles, in any new plant, or in one whose powers were previously unknown, would warrant the use of such plant, in all cases in which Aconite of any species might be proper.

There are numerous euphrenics of inorganic and mere chymical origin, as well as of organic and vegetable origin. Those of inorganic and mere chymical origin are mostly compound-radicals of H. C. which, when liquid, constitute what are called Essential Oils; or they are non acid compounds of these Essential Oils with a small number of equivalents of Oxygen, or some other of the eleven basifying and acidifying elements, which, when they are liquid, are called the first class of *Æthers*, and when they are solid, are called Camphors; or they are liquid compounds of some of these *Æthers* with salifying principles constituting liquid salts, and called the second class of *Æthers*. Those salifying principles which are composed of the compound-radicals of H. C. united with several equivalents of Oxygen, or some other of the eleven basifying and acidifying elements, and which are sour to the taste, are not euphrenic, but possess widely different medicinal powers; but these have already been mentioned in connexion with this subject. I believe that the whole, which are known, of both the preceding classes of *Æthers*, are more or less euphrenic, though I doubt whether the Protocyanid of Etherogen could be used as a euphrenic, since as I imagine, its narcotic effects would transcend and supersede its euphrenic effects. Now as so many *Æthers* are already known, and as all without exception are euphrenic, there can be little or no risk in inferring that every new compound belonging to either of these classes, which may be discovered, will possess the power of a euphrenic. But it must not be overlooked that all the *Æthers* of both classes invariably possess at least two powers in addition to that which I call euphrenic; and with only two known exceptions, one of these additional powers is that of producing directly a greater or less degree of exhaustion (great from some of these articles, and smaller from others) in all of those subordinate parts of the system, which are depen-

dent upon the nerve of chiminal action nutrition and reproduction, i. e. the great sympathetic nerve. If it had been well understood and kept in mind that the Tritochlorid of Formicigen or Chloroform (*infeliciter sic dictum*) is a direct exhausting agent of very considerable power, exerting its exhausting influence more especially upon the sanguiferous system, the secernent and absorbent or glandular system, and particularly upon the parts of the latter, on which the arterialization of the blood depends, which is the very essence of respiration, I should think that fewer lives would in all probability have been destroyed by it. It is not a little remarkable that the two *Æthers*, which are not directly exhausting, viz. the Unihydrite of Protoxyd of Etherogen or Alcohol, and the Binhydrite of Protoxyd of Etherogen or the active principle of Wine, should instead possess a diametrically opposite power viz. the power of producing a quickly diffused and transient increase of vital energy and strength of action in the sanguiferous system, and probably in all parts dependent upon the great sympathetic nerve, which are susceptible of these effects. I have searched in vain among the analogues (as respects chiminal composition) of Alcohol and Wine, for other articles that are what I call antisbestics, instead of being directly exhausting. I hope that some one else will be more successful in finding such articles than I have been. I think we should look for such articles first among the Hydrites of the Oxyds of the compound-radical of H. C.

Euphrenic power in plants depends upon as numerous organic proximate principles, as a narcotic power, or a tonic power, etc., but when the euphrenic power of a given article has been ascertained to depend upon any specific principle, it is obvious that we may safely infer a euphrenic power in any new and previously unknown article, that is ascertained to contain such principle in a predominant quantity. It is not often however that the euphrenic activity of different vegetable articles depends upon the same identical proximate principle. There is one case however, in which several very widely different plants contain one individual proximate principle, and all of them are euphrenic. At present there is no other reason to suppose that this principle is that in which their activity resides, than the circumstances that they all contain it, and that no other principle common to the whole of them has yet been detected, nor any principle pecu-

culiar to each, on which their euphrenic power can, with any reasonable probability, be supposed to depend. A substance was obtained from the seeds of *Coffea Arabica* (Linn.) by Mr. Chenevix, in the year 1802, which was named Coffeïna or Coffeïne. A substance was obtained from the seeds of *Paulinia sorbilis* (Martius) by Theodore Martius in the year 1826, which was named Guaranina or Guaranine. A substance was obtained from the dried leaves of *Theä Sinensis* (Blackwall) by Oudry, in the year 1827, which was named Theïna or Theïne. On careful examination it was at last ascertained that Coffeïne, Guaranine and Theïne, were the same identical substance, an Alcaloid with feeble basic powers. More recently, as is said, Coffeïne has been detected in *Ilex Paraguajensis* (St. Hil.?) or Maté; and it is supposed to be contained in *Ilex Gongonha* (Lamb.) *Ilex Theizans* (Mart.) *Ilex vomitoria* (Linn.) or Yapon or Yaupon of the Southern United States, and perhaps in some other species. For the sake of illustration we will suppose that Coffeïne is in fact the active euphrenic principle of the seeds of Coffee, and that this was well ascertained at the time of its first detection; and we will suppose also that all the other articles containing it were new and previously unknown to the materia medica, when this principle was first discovered in them. Under such circumstances, would any one have entertained a particle of doubt that the new articles were euphrenic, like the old one? Under such circumstances it would indeed behove the analyst to ascertain that the new articles contained no other proximate principle or principles, of such different and so much greater powers, as to transcend and supersede the effects of the euphrenic principle. I believe however, that it is rarely (if ever) the fact that a vegetable contains two distinct proximate principles of materially different powers. It is far more common for one proximate principle to be endowed with several different and distinct powers.

When making the above remarks, I did not forget that Stryphnic, Scytodephic or Tannic Acid in small quantities, very often accompanied various other active principles in vegetables. This is the only exception (if in fact it is one) within my knowledge, to the preceding remarks. Stryphnic, Scytodephic or Tannic Acid seems to be one of the most common proximate principles in the vegetable kingdom, there being few crude vegetable medi-

cines, which do not contain larger or smaller quantities of it; but in general it exists in too small quantity to produce any medicinal effects; and even at the most, these are always moderate. Undue importance has undoubtedly been attached to this principle in therapeutics. Before the detection of the Alcaloids Cinchonine, Quinine etc. it seemed to be the prevalent opinion that all the peculiarities of Cinchona, in comparison with the simple bitter-tonics, were due to the fact that Cinchona contains a mere simple bitter- tonic principle with the styptic principle Stryphnic Scytodephic or Tannic Acid conjoined; and compounds of the simple bitter-tonics, such as *Gentiana lutea* for example, with the simple styptics, such as *Tormentilla erecta* for example, were recommended for the treatment of Intermittent and Gangrene, instead of Cinchona; and as a matter of course, were positively affirmed to be equally efficacious. If I do not misremember, this is laid down by Cullen, in his *Materia Medica*. In the U. S. A. this notion was speedily disproved by trial of such compounds in cases of considerable intensity, and the creed was amended by the declaration that an acrid or pungent principle was necessary to the compound, in order to render it equally effectual for the cure of Intermittent and Gangrene as Cinchona, and such articles as the bark of *Liriodendron Tulipifera* or Whitewood, were conjoined with simple styptics; or the same thing was supposed to be arrived-at, by conjoining *Capsicum* with the simple bitter-tonics and the simple styptics. But Cinchona could no more be made in this way, than by that previously mentioned. Cinchona is certainly destitute of any acrid or pungent principle. Here too much was expected from chemistry, and that in a much less advanced state than at present; for at that time, as I have already said, the true active principles of Cinchona had not been detected; and even after they were well ascertained, the solution of the problem turned upon the peculiar quality of the powers of these active principles, and not at all upon any combination of principles, which, in their separate state, were utterly inadequate to make any useful remedial impression either upon Intermittent or Gangrene, contrary to what Dr. Paris so strenuously maintained, even to the denial of the existence of any medicinal powers in Cinchonine, Quinine etc. by themselves.

Among the inorganic and chymical oresthetics, there are no particular principles that can be separated from the compound em-

ployed, and on which we can say that the oresthetic power depends, but the oresthetic operation and effect result from the intire compound. However the external sensible property of acrimony, as in the case of organic vegetable articles, always indicates the occult power. Chimistry therefore can never enable us here to decide a priori that any compound is capable of proving oresthetic, but this must always be ascertained by trial or experiment. But the case is materially different with respect to organic vegetable oresthetics. All vegetable proximate principles, that possess any considerable amount of acrimony or pungency, are always more or less oresthetic. This external sensible property seems to be essentially attended with the occult power in question. Under such circumstances, chimistry can not be considered as affording any assistance. But there are organic vegetable principles which, without any acrimony or pungency to the taste, operate as oresthetics. These can be determined to be oresthetics only by trial or experiment. When by the aid of chimistry, we have become acquainted with several such principles, we may be sure that every new and previously unknown article, which contains any one of them, must be oresthetic, and about in proportion to the quantity which it contains. What I have already said, I believe, will sufficiently illustrate the application of chimistry to the purpose of ascertaining when any new and previously unknown article possesses an oresthetic power, and therefore I pass-on to the consideration of the application of chimistry to the purpose of ascertaining a priori, when a new and previously unknown article possesses the power which I am in the habit of denominating antisbestic.

I consider the class of antisbestics (as I shall here after state more particularly) as the most meager in conveniently available articles, of any class in the whole materia medica. Unlike every other medicinal class, it does not contain a single article, which has no other power, except that on which the class is founded, though all the powers, that are ever associated with that of an antisbestic, not only occur unaccompanied with any degree of antisbestic power, but often intirely by themselves, and unaccompanied by any other power whatever. Phosphorum elementarium is an insulated article, which possesses an antisbestic power in a slight degree, and an oresthetic power in an intense degree. It

can scarcely be said to be available in medicine as an antisbestic, unless in some chronic cases. As this article is an element (in the present state of our knowledge)—as its medicinal powers depend upon it as a whole—and as there is no other analogous element, we can not expect, either by the aid of chemistry, or any other means, to find any thing like it. Such is the fact generally with elements, when ever they happen to be medicinal. In fact there are but two antisbestics, both of chymical origin, that have not some other power associated in such a degree with them as to prevent their use to the necessary extent, in all the cases in which the antisbestics are the most highly indicated, the most important, and the most indispensable. The two antisbestics to which I refer, are the diluted Unihydrite of Protoxyd of Etherogen or diluted Alcohol, and the diluted Binhydrite of Protoxyd of Etherogen or Wine, the latter of which is generally pretty much unknown in the U. S. A. except in one or two very limited regions, where a small quantity of it is made, and in the possession of a few wealthy gentlemen, who import sufficient for their own use directly from some foreign vineyard. That which is obtained from the Wine merchants is any thing, and almost every thing, but what it professes to be. As it always contains Alcohol, it is not inert; but Alcohol can always be better employed in some other state of mixture. The antisbestic power of Alcohol and Wine does not depend upon any individual specific principle—does not depend upon Etherogen merely—upon the Oxyd of Etherogen merely, since this has been ascertained to be directly exhausting—nor upon the Hydrous Acid or Water merely, which exists in them in strict chymical combination, but it results from these respective compounds, each as a whole. The only manner in which chemistry can be useful in leading us to the knowledge of new and previously unknown articles possessing the same powers as Alcohol and Wine, is by leading us to search for them among analogous compounds, i. e. among the Unihydrites and Binhydrites of Oxyds of compound-radicals of H. C. But investigation to rather a limited extent has been made, in the direction in which chemistry leads, though without success. Perhaps after investigating a considerably greater number of such compounds, some one or more may be found having an antisbestic power. At all events, the discovery of new and additional antisbestics is sufficiently desira-

ble to induce us to continue our researches as long as there are analogous substances to be examined. Further, I can not perceive how chemistry can assist us in ascertaining antisbestics of inorganic and chymical origin. There is a group of at least thirteen, and probably a much greater number of vegetable articles, of which *Rhus venenata* (De Cand.) may be taken as the type, which are slightly antisbestic and efficiently oresthetic, so much so, as not to be available to any extent in medicine as antisbestics merely. These articles are all so much alike in their operations and effects, as to leave no doubt that their powers depend upon a single active principle. But this principle has not yet been ascertained by the chimists, though when it is, all articles containing it will doubtless be found to possess the same powers, to exert the same operations, and to produce the same effects. I have heretofore observed incidentally, that the vegetable kingdom affords no antisbestics. By this I do not mean to say that it does not furnish any article containing a particle of antisbestic power, but only none that are available where antisbestics are indispensable. The articles that I have just mentioned, though actually possessing a slight degree of antisbestic power, are very few in number, and as I have said, have so much oresthetic power, that they are incapable of answering the indications for antisbestics in any case where such agents are indispensable, as for example, in the sudden and extreme exhaustion without inanition, which sometimes occurs at the outset of a very malignant disease; or occasionally though rarely, on the fourteenth day of an utterly neglected Typhus nervosus. So utterly unimportant are these articles as antisbestics, that I hesitated about mentioning them in this class.

There is a considerable number of Insects of the Beetle Tribe, belonging to the Genera *Cantharis*, *Lytta*, *Meloë*, *Mylabris* etc. that are moderately antisbestic and intensely oresthetic, so much indeed as to prevent their being employed to such an extent as to be of any material value for the former effects; and all of these are supposed to contain a principle which has been incorrectly called Cantharadine, since it is certainly not an Alcaloid, to the names of which the termination *ina* or *ine* belongs exclusively. It should have been called Cantharidia, since the terminations *ina* or *ine* and *ia* or *a* imply as much difference as the terminations *idum*

or *id* and *uretum* or *uret*. Upon this principle the activity of some of these Insects certainly depends, and in all probability the activity of the whole. Now any other Beetle, indeed any other Insect, whether in the Larve, Pupe or Imago-state, that shall be found to contain Cantharidia to any prominent amount, will undoubtedly be found to possess the same aggregate of medicinal powers.

The tonics of inorganic or chiminal origin would seem at first view to be in general like the articles of most other classes from the same source, i. e. their tonic power would seem rather to depend upon the compound as a whole, than upon any principle that enters into their composition. This however is not considered as being the fact. Iron may be taken as an illustration. As is commonly supposed elementary Iron is tonic per se. This point I will not discuss here, but for the present, I will take the commonly received opinion for granted. If this is true, all its compounds must be tonic by virtue of the Iron which they contain. I believe that it may be admitted that nothing which enters into the composition of any of the supposed tonic compounds of Iron possesses any tonic powers by itself. This very strongly favors, though it does not absolutely prove, that Iron is the sole tonic principle in all its compounds that are tonic. I believe that all compounds of Iron are commonly reckoned tonics, which do not contain some other principle, that gives a different power, which transcends and supersedes the supposed tonic power of the Iron, as the equivalent of Potassa and the two equivalents of Tartaric Acid transcend and supersede the supposed tonic power of the equivalent of one or the other of the basic Oxyds of Iron that enters into the composition of the two Tartrates of Potassa and Iron. When Chlorine, Bromine and Iodine are combined with Iron, they often prevent its being taken with sufficient freedom for it to prove tonic. The powers of Chlorine, Bromine, and Iodine are not in opposition to those of Iron, like those of Potassa and Tartaric Acid. The Diferrite or Disoxyferrite of Sesquicyanid of Iron (Prussian Blue) can not be used as a tonic, because the narcotic activity of the Cyanogen which it contains, prevents its being taken in a sufficient quantity to operate as a tonic. Chemistry therefore will enable us to judge correctly in regard to the tonic powers of any new and previously unknown compound of Iron, provided we carefully ascertain every thing,

with which it is combined. In the case of the preparations of Arsenic, this element is the principle of their tonic activity, when they are so managed as to produce a tonic effect; and there is scarcely any thing that can be combined with it, that would hinder this effect, hardly even Cyanogen. We can then decide by the aid of chemistry, respecting the tonic activity of a new and previously unknown compound of Arsenic, if we are willing to be at the pains of calculating the relative proportion of the Arsenic in the compound, in comparison with any other active principle or principles, that it may happen to contain.

The tonic power of such compounds as Protiodid of Quininum—Protocyanid of Quininum—Dicyanoferrite of Protocyanid of Quininum, and the analogous compounds of Cinchoninum etc. may be inferred with certainty from chemistry; and even their appropriate doses may be arrived-at by calculating the relative proportion of the respective principles, which enter into their composition. I prepared all these, and various other similar compounds several years before there were any accounts of any of them in print. The views which I entertain of their exact composition, and the formulæ which I employed, do not however agree very exactly with those which were first published. What the publisher of the first formula for Protiodid of Quininum (called by him erroneously Iodid of Quinine) pronounced to be Deutiodid of Quinine, I had considered, and still consider to be Protiodid of Quininum in an amorphous, instead of a crystalline form. But a true Deutiodid of Quininum, I think, may be easily prepared, and I believe, has been prepared, by one of my private pupils viz. William Manlius Smith, M. D. of Manlius, New York. The doses, which I inferred from calculation of the relative proportions of the respective proximate principles, proved on trial, to be correct. All this is founded upon laws that have been already laid-down in a preceding part of this Introduction. By the aid of Chemistry we may infer with all reasonable certainty that the Iodoquininate of Iodid of Potassium is of much less value as a tonic, than if it did not contain one equivalent of Potassium and two equivalents of Iodine to one equivalent of Quininum; and that its dose must be regulated rather by the amount of Iodine which it contains, than by the amount of Quininum. This preparation was also made by my pupil Dr. Wm. M. Smith. It would

seem from such chymical researches as have already been made, as if the active principles of the Cinchonaceæ and Cinchoneæ, the truly febrifuge-tonics, are always Alcaloids having a greater or less analogy with Cinchonine, Quinine etc. As many as eight of these (all from the Cinchonaceæ, but perhaps not all from the Cinchoneæ) have been supposed to be ascertained by the chimists. Indeed I believe that the whole of the tonics of organic and vegetable origin, may be considered as owing their tonic and all other powers, either to some Alcaloid, or to some Oxyd of a compound-radical which they contain. The great class of tonics is by far too numerous for all of them to have been thoroughly analyzed; but so far as adequate analyses have been made, they justify the foregoing generalization. Now it is not infrequently the fact that different individual articles contain the same active principle. It would seem to be the fact that the small group of Simarubaceous tonics all depend upon one single active principle; but what this is, I do not think has been properly ascertained. I think that all the numerous tonic species of *Gentiana* contain only one single active principle. I am inclined to think that all the tonic species of *Cocculus* and *Menispermum*, of which I have any knowledge, depend upon one single active principle. I doubt not that all the species of *Populus* and *Salix* depend on the same active principles viz. *Populine* and *Salicine*. All the tonic species of *Ignatia* and *Strychnos* are well known to depend upon the same active principles, viz. *Strychnine* and *Vomicine*, and I very strongly suspect something else. It is believed that all the tonic *Amygdalaceæ* and *Pyraceæ* depend upon the same active principles, from their very strong similarity in flavor. It is most probable that all the tonic species of *Magnolia* depend upon the same active principles; and it is not improbable that the same principles exist in *Liriodendron* *Tulipifera*, and even in the tonic species of *Illicium*. The same may probably be said of the several tonic species of *Drymis*. As appears to me, the tonic species of *Aristolochia* are sufficiently alike to have one single essential active principle, how much so ever their odorous principles may vary. The febrifuge and styptic bitter-tonics of the Genus *Cinchona* are well known to depend essentially upon one or more of a certain set of Alcaloids. The Stryphnic Scytodephic or Tannic Acid, which they contain, is of no importance for the purposes for which they are

employed. Now when any tonic active principle has once been obtained and investigated, there can never be any hesitation in ascribing its powers to any vegetable article, that contains a notable and predominating quantity of such principles, as ascertained by chemistry merely.

There are a few styptics of chemical and inorganic origin, all but one of which are of no great importance. These do not depend upon any principle or principles, which they contain, but their stypticity results from each compound as a whole. In such cases, chemistry can be of no service towards ascertaining new and previously unknown styptics of the same character; but there is a simpler and easier way viz. by the taste, since a styptic taste is always a sure indication of a styptic power. As there is but one true and proper styptic principle in the whole vegetable kingdom viz. Stryphnic Scytodephic or Tannic Acid, any plant that shall be found by chemistry merely, to contain a sufficient quantity of this principle, without any other active principle that will transcend and supersede a styptic operation, may at once be employed for this effect. As this principle in very small quantities, is however more widely diffused than almost any other medicinal principle, we must take care not to ascribe styptic powers to every article that manifests traces of this Acid on the application of the proper chemical tests. Very many articles of the materia medica are now uniformly said to be styptics, which do not contain a sufficient quantity of this Acid to produce even the slightest effects.

Electricitas Galvanica seems to be both orethetic and adenagic. Some of the elements are adenagic per se, as Chlorine, Bromine, Iodine, Sulphur, Aurum or Gold and Platinum. It will at once be obvious that chemistry can be of no service towards ascertaining adenagic power in new and previously unknown articles such as these. Many of the inorganic and chemical adenagics depend upon a simple or elementary principle for their adenagic power, as for example the adenagic compounds of Chlorine, Bromine and Iodine, which depend upon these elements respectively. Chloroxynitric Acid and Chlorohydric Acid, in all probability, depend upon the Chlorine, which they contain, for what little adenagic power they possess, if indeed they possess any at all. I do not think that Nitric Acid possesses any adenagic power

at all, though effects have been attributed to it, which, if they were truly produced by it, would indicate adenagic power. But for myself, I never could obtain any such effects from its use. The adenagic compounds of Aurum or Gold, depend upon this element for their adenagic power. The adenagic compounds of Platinum depend upon this element for their adenagic power. Certain elements, which are not medicinal per se, always impart adenagic power, when they enter into combination, as Arsenic and Hydrargyrum or Mercury. The adenagic compounds of Arsenic depend upon this element for their adenagic power. The adenagic compounds of Hydrargyrum or Mercury depend upon this element for their adenagic power. So far as the adenagic power of a new and previously unknown article depends upon a principle, whether elementary or complex, entering into the constitution of the article, so far chemistry may be useful towards ascertaining its powers, as it is capable of detecting the active principle, and showing us its relative proportion in the compound. As respects the adenagics of organic and vegetable origin, they are in much the same category as the tonics, i. e. their adenagic power depends upon one or more of their proximate principles, which are either Oxyds of compound-radicals of H. C. or Alkaloids. There are undoubtedly many instances, in which nearly allied species of the same Genus contain the same active principle; and in all probability there are some instances, in which the species of nearly allied Genera of the same Natural Order do the same. In all such cases, the detection by chemistry, of an already known active principle, in a predominating quantity, in a new and previously unknown plant, decides its medicinal powers; and if the proportion in which it exists, is accurately ascertained, and is in a good degree uniform, it even decides its ordinary dose. This last however, is a matter of no sort of consequence. This appears to me to be all that chemistry can do, towards assisting to a knowledge of the powers of new and previously unknown articles in the class of adenagics. By this review of all of the first nexus or alliance of classes, in the system of materia medica, which I adopt, I trust that every mode and variety, in which chemistry is capable of being employed or used for the purpose of arriving at a knowledge of the powers of new and previously unknown articles, has been displayed and illustrated, and that

nothing further would appear in this subject, even if the examination were to be extended to all of the remaining classes, and therefore I leave this topic here.

4. Experiments upon Brute Animals as a Means of Determining Medicinal Powers.

Of late, particularly among the toxicologists so called, much importance has been attached to experiments of four sorts upon brute animals, as a means of ascertaining the powers of new and previously unknown articles ; but in my opinion, three out of four of these methods that have been thus employed, are equally frivolous, futile and barbarous. The methods here referred-to are

1. Putting the article under investigation into incisions or wounds made in the flesh, which is such a measure as we should naturally expect from the amount of knowledge, and the wisdom of an American savage.

2. Forcing it into the stomach, and then cutting so as to put a ligature around the œsophagus, and some times an other around the duodenum, a measure not less barbarous, and quite as futile, as the preceding.

3. Injecting it into the blood-vessels.

4. Causing the article to be taken into the stomach, and that without being preceded, accompanied or followed by any wounds, mutilation or violence, such as must inevitably disturb and derange the functions of the system in general, at least in a greater or less degree.

It is only since the publication of Orfila's toxicology, that the project for getting the remedial effects of medicines, by means of injections into the urinary bladder, has started into notice; otherwise without doubt, this method would have been paraded, recommended and employed in experiments upon brute animals, for the determination of the powers of new and previously uninvestigated articles. I believe that neither of the first two of these methods originated among English physicians, or has been practised to any material extent by them; which I am happy to be able to state, for the honor of the people, from whom the first European inhabitants of New England, and of a large portion of the rest of the U. S. A. must claim descent.

1. Putting the article to be investigated into incisions or wounds made in the flesh.

It can not but be obvious at once, that this method can never be employed in the actual practice of medicine, for obtaining the remedial effects of the ordinary articles of the *materia medica*; and therefore it behoves us to ascertain carefully whether medicines used in this manner produce the same effects both in quality and degree, as when taken into the stomach. There are a very few pure and intensely active articles of the Classes *Narcotica*, *Euphrænica* and *Erethistica*—in truth I know of but two narcotics, one euphrenic and one erethistic—that will produce powerful and even fatal effects, and upon the whole system, when employed by inoculation, or insertion into incisions made in the flesh. The best known of the only two narcotics within my knowledge, that will affect the system speedily and powerfully by inoculation, is the extract of the barks of *Strychnos toxifera*, *Rouhamon Gujanense* and *Rouhamon Curare*, which depend for their activity on the supposed Alcaloid *Curarina* or *Curarine*, to which I have already had occasion to refer more than once. According to Mr. Brodie, Mr. Waterton and others, the *Curare*, when employed by inoculation, causes the subject to die in a few minutes, without any symptoms except a rapid failure of all the actions dependent upon the involuntary nerves of expression and respiratory motion, the action of the heart and blood vessels continuing for some time after the total cessation of all respiration. But this is by no means the way in which this article operates, when it is taken into the stomach. Dr. Thomas Thomson says that “*Curara* may be taken into the stomach with impunity; but when introduced into a wound, it occasions death in a few minutes.” (*Th. Thomson, Chim. Org. Bod. Veget. 7th Edit. Lond. 1838, p. 286, Artic. Curarina.*) By the term *Curara* as above used, Dr. Thomas Thomson intends an extract from the bark of either of the three plants just mentioned; while by *Curarina* or *Curarine* he intends the active principle of these barks. The Extract however is more commonly called *Curare* than *Curara*, and it is likewise known by a great multitude of other modifications of this term, some where about forty, if I do not misremember. Martius says that “Indians” (aborigines of America) “wishing to commit suicide have swallowed large doses, without perceiving any effect.” He adds “the Indians consider it a strengthener of the stomach.” According to Mr. Brodie, brute animals inoculated with the inspiss-

sated juice of *Antiaris toxicaria*, generally died in a few minutes, without any symptoms except a rapid failure of all the actions of those parts dependent upon the nerve of chemical action nutrition and reproduction, more obviously manifested by a suspension of the actions of the heart and blood-vessels, the respiratory motions of the lungs continuing for some time after the cessation of all sanguiferous action, and of course without any arterialization or decarbonization of the blood. Now this article can not be made to operate at all in this rapid and powerful manner when taken into the stomach.

The only euphrenic within my knowledge, that will affect the system speedily and powerfully by inoculation, is the Venom of the *Caudisona horrida* or Rattle Snake, and that of other venomous Serpents, which seem to produce essentially the same fluid. This Venom when used by way of inoculation, produces a sudden, intense and rapidly increasing exhaustion of the whole nervous system, but much more especially of the nerve of chemical action nutrition and reproduction, which almost (if not quite) always destroys life, if not prevented by the interposition of appropriate medicine. In addition to this, it produces a topical Erythema with a strong Gangrenous tendency, and of a very peculiar character in all respects, which extends about equally in every direction, but whose progress is rather slow in comparison with the general exhaustion. But when taken into the stomach, this Venom produces the most pleasing, and as is said, even the most delightful euphrasy or exhilaration. From the effects of this Venom employed by inoculation, would it be by any means possible to infer its effects, when taken into the stomach?

The only erethistic within my knowledge, that will affect the system speedily and powerfully when employed by way of inoculation is the recent juice of the bark of *Strychnos Tienté*, or *Tjettick*, or an extract made from this bark. If this article contains no other active principle than the Alcaloid Strychnine, as is commonly supposed, this Alcaloid ought to be capable of producing the same effects, by inoculation. I do not however recollect ever to have seen any evidence adduced to show that the Alcaloid Strychnine is capable of producing its regular effects upon the system at large, when it is applied or employed by inoculation. I presume that experiments have been made with it, in this way, in

connexion with the experiments made with the Juice or Extract of *Strychnos Tienté*, but for the reason heretofore stated, I am unable to make any reference to authority upon the subject. I can however say with certainty, that I never witnessed any constitutional effects from Strychnine applied to any part of the surface denuded of its cuticle by a vesicatory of *Cantharis*, or applied to a cutaneous ulcer. But under such circumstances, the regular sympathies with other parts of the system of the blistered and ulcerated patches, may be fairly considered as interrupted by the morbid conditions produced by the vesication and the ulceration. However I have known the topical irritation, i. e. the smarting, the pain and inflammation, which is some times produced by such applications to blisters and ulcers, as the *Alcaloids*, mistaken for valuable medicinal effects. I have even known the application of Sulphate of Oxyd of Morphinum, to a blistered or an ulcerated surface, to irritate, produce pain and more or less Phlogosis. Veratrina or Veratrine the active principle of *Asagrea officinalis*, and as is supposed by some of *Veratrum Sabadilla*, is alleged to be capable of vomiting and purging, when applied to blistered and ulcerated surfaces. Now I have seen Veratrine so applied, till the patient said it felt like fire; but I never saw any vomiting and purging follow such application. Perhaps there was not enough of it used, but there was as much applied as the patient would consent to. All the effect that I have ever seen from Veratrine thus employed has been a mere local irritation. I think I have seen a topical discutient effect from it, when rubbed upon an enlarged and inflamed lymphatic gland, when such gland was near the surface, and the Veratrine was applied freely to the sound skin immediately over it. Used in this way, it has always produced more or less rubefaction of the part on which it has been rubbed. In short I imagine that there is only an exceedingly small number of agents, that by topical application, either to a blistered or an ulcerated surface, are capable of affecting the whole system, though there may be a few more, that will do this when applied to the sound skin. It does not appear to me at all probable that an article, which will not affect the system at large, through a blistered or ulcerated surface, or through the sound skin, would be at all likely to affect it, by being put into an incision, and much less, when employed by mere inoculation. By far the greatest number even of the most active remedies, when put

into incisions in the flesh, produce no other effects than a topical irritation, passing into Erythematic Inflammation, and some times into Gangrene, if the agent is applied in a considerable quantity, and its application continued for a comparatively long time. If the Erythematic Inflammation is sufficiently extensive, and sufficiently intense, it will occasion an irritative constitutional febrile affection, of that type formerly termed putrid.

But far, very far the greatest number of medicinal agents, when employed by inoculation, or insertion into gashes or incisions in the flesh, operate in an intirely different manner, and produce effects of a very different character from those which result from taking them into the alimentary canal. If any one chooses to doubt this, on the ground that a sufficient number of experiments may not have been made to justify this conclusion (which doubts I do not for myself think would be at all well founded) I would still urge that it is to be particularly observed that the effects of the Extract of *Strychnos toxifera*, *Rouhamon Gujanense* and *Rouhamon Curare*, as well as of the Extract of *Antiaris toxicaria*, and also of the Venom of Serpents, are widely and very essentially different, when operating by inoculation, from what they are when operating through the medium of the stomach and upper and smaller intestines; while the effects of the Juice or Extract of *Strychnos Tieuté* are said to be the same, in which ever of these modes administered. If such is the fact, the investigation of the effects of articles employed by inoculation, or insertion into gashes or incisions in the flesh, will not supersede the necessity of investigating their effects when taken into the stomach, since, without trial, we can never be certain that they will be the same, from both methods of employment. But if we have investigated their effects when taken into the stomach, what need will there be of investigating their effects as produced by inoculation, or insertion into gashes or incisions in the flesh? Unless we undertake to revive the art of preparing and using poisoned arrows, I can not discover the utility of ascertaining the effects of articles employed by inoculation. Possibly such knowledge might be of some little service for the purpose of assisting to keep us from accidental inoculation by the very few articles, whose operation in this way might be dangerous or fatal. I could never discover any good reason for employing a medicine, by way of experiment, in order to ascertain the power or powers of a new and previously unknown

article, in any manner in which it can never be employed in actual practice, and in the treatment of disease. Who in his senses, could seriously contemplate gashing the flesh of a patient, for the insertion of medicine into the wound, as often as a patient might require a dose? This would be a mode of practice that even the inhabitants of New Zealand, of the Feejee Islands, and the aborigines of America, trained as they are to endure torture, would reject.

Upon the whole then, I do not attach any value to inoculation with medicines, or even to inserting them into gashes or incisions in the flesh as a means of ascertaining the powers of a new and previously unknown article. I consider such a method as not only worthless, but as absolutely barbarous; and therefore I would reject it wholly and intirely. Not but that I attach a considerable degree of value to the knowledge of the difference in the effects of the Venom of *Caudisona horrida*, and of the Juice or Extract of *Rouhamon Gujanense*, *Rouhamon Curare*, *Strychnos toxifera* etc. when employed by inoculation and when taken into the stomach; but it is only their effects when taken into the stomach, that belong to the *materia medica*. Their effects as produced by inoculation belong to pathology, and as pathological effects, we shall acquire the knowledge of them as rapidly as we acquire a knowledge of other pathological effects, and as rapidly as we need them. There can be no need of experiments for ascertaining them, any more than for ascertaining any other pathological facts. Indeed I believe that a knowledge of such pathological facts always precedes, and that too by a long period of time, the pharmacological facts resulting from the same agents.

2. Forcing an article into the stomach, and then cutting-down and putting a ligature around the *oesophagus*, and some times doing the same by the *duodenum*.

It appears to me that it can hardly be necessary, to discuss in detail, the merits of a method of ascertaining the powers and operations of remedies, so palpably absurd as this; and yet it is a method much extolled, and extensively employed upon brute or inferior animals, by certain individuals of great notoriety, and celebrity and in the highest esteem with some, at the present day. It appears to me that by such a mode of administration it will always be impossible to distinguish many of the effects of medicinal agents, from the effects of the factitious lesion of cutting-down and tying the *oesophagus*. It appears to me

that such a lesion must absolutely prevent the production of the operative effects of very many medicinal agents, must heighten and exalt the operative effects of others, and must, at all events modify very materially all the operative effects of the rest. After forcing the medicine into the stomach, I think that we might just about as well give the brute animal a fatal blow upon the head, so far as any reliable information is to be obtained from the experiment. I am sure I should never venture to repose any confidence in any results so obtained. I will therefore dismiss this method of attempting to obtain a knowledge of the powers of a new and previously unknown article, without any further comment, since the common sense of every man can not fail of giving him a just appreciation of such a method.

3. Injecting the article under investigation into the blood-vessels.

It must be particularly observed that employment by way of inoculation, or insertion into incisions in the flesh, is by no means the same as injection into the blood-vessels. As I have already inculcated, I have no belief that any thing employed by way of inoculation, or inserted into incisions in the flesh, in one case in a hundred, ever reaches the blood-vessels; and in the very few cases, in which such is the fact, it doubtless produces all its effects long before it reaches the blood-vessels. From the fact that the lining membrane of the blood-vessels, like the lining membrane of the alimentary canal, is a mucous membrane (for such it really is, authorities to the contrary notwithstanding) and likewise from the fact that all its actions depend upon the involuntary motor nerve of chymical action nutrition and reproduction, and all its sensations depend upon the nerves of common sensation, that accompany the motor nerve just mentioned, it will perhaps be concluded a priori that remedies must produce the same medicinal-operative effects, when applied to the inner parietes of the blood-vessels, as when applied to the inner parietes of the alimentary canal. It is indeed true that a few remedial agents will produce their ordinary operative effects when injected into the blood vessels; but it has never been ascertained to be the fact that a majority of the remedies in common use are capable of doing this. For example, an infusion of the root of *Cephaelis Ipecacuanha*, it is said, will nauseate and vomit, when administered in this way. Here

then is both sensation and action produced by an agent injected into the blood-vessels. It is said likewise that the oil of *Ricinus communis* will purge, when injected into the blood-vessels. Scheele's Water of Cyanid of Hydrogen is said to exert all its narcotic power, and to produce all its narcotic effects, when injected into the blood-vessels. But when applied after this mode, each of these agents, though destitute of any acrimonious qualities, never the less produces troublesome and even hazardous effects, to all appearance from its mechanically irritant operation, when received into the cavities of the organs of the circulation of the blood. How then can we expect articles that possess positively acrimonious and other irritant qualities, which usually make a strong impression upon external organs, to be tolerated within the blood-vessels? How can we expect them to produce kind, salutary and truly medicinal operations, and especially of the same character as are produced when they are received into the alimentary canal? The alimentary canal seems to have been intended for the reception of all manner of crude things which are capable of contributing to the nutriment of the body; but the blood-vessels do not seem to have been intended for the reception of any thing which has not been thoroughly elaborated by the organs of primary digestion, and in fact vitalized in a low degree; for I take it for granted that nobody at the present day can by any means doubt the vitality of the crassamentum of the blood. It is true that water, a mere inanimate and inert substance, is found in the cavity of the blood-vessels, as it is also, in a larger or smaller quantity, in every cavity of the body, because it is necessary for the preservation of the softness and flexibility of all membranous parts. If the blood-vessels had been intended for the reception of any thing, whether nutriment or medicine, that has not passed through the organs of primary digestion, and been thoroughly elaborated by them, there would have been some other avenue to their cavity—they would assuredly have been provided with a mouth or with some thing equivalent. But the blood-vessels appear to be far less nervous (as respects common sensor nerves at least) and therefore far less exquisitely susceptible (at least in that manner best fitted to receive primarily remedial impressions) and they appear to have far fewer, and far less intense sympathies with other parts of the system, than the stomach and smaller intestines. It

must be the nerves of common sensation that receive the primary impressions of medicines either in the blood-vessels, or in the alimentary canal, since they only propagate influence from their extremities to their origin, while the involuntary motor nerve of chymical action nutrition and reproduction (the great sympathetic nerve so called) propagates action only from its origin to its extremities; and these are the only two sets of nerves received by the blood-vessels and the stomach and upper and smaller intestines. But at all events, I consider it as certain that the structure, susceptibilities and functions of the organs of the circulation of the blood, are of such a nature and character, as not to tolerate with impunity, except in very peculiar circumstances of extremely rare occurrence, the mechanical impression of any substances foreign to the blood. The few instances referred-to, in which injections into the blood-vessels have been made with impunity, are cases in which the system is so powerfully occupied by some extremely intense severe and violent disease, a disease of such a nature and character as to enable it to bear, not only without injury, but seemingly without any effect, both processes and agents, that under other circumstances, would be extremely hazardous or even fatal. As examples of such cases of disease, we may mention those instances of Epidemic and Malignant Cholera, in which a pint of brine was injected into the blood-vessels without any apparent disturbance, and certainly without the least benefit; for nothing short of a miracle could ever render such a perfectly mad measure of the smallest service or utility. Some cases of Rabies, some cases of Tetanus, and some cases of Mania, are as insusceptible of influence, either for good or for evil, as the cases of Cholera here referred-to. I have repeatedly seen cases of Pneumonitis Typhodes-notha that were of a similar character. Nothing that could be thought-of, would do them either good or hurt. I doubt not that many more of such cases occur, than is commonly known or understood, and that they some times give rise to very injurious false experience.

Although man was very evidently intended by Divine Providence to live artificially, yet I can not discover why we may not reasonably conclude, from anatomical structure, that it was not the intention of the all-wise Creator that any thing should ever reach the mass of the blood, without undergoing the primary

digestive process, which is performed in the stomach, the upper and smaller intestines, and probably to a certain extent, by the lacteal absorbents, and in the mesenteric glands, or without undergoing the decompositions and recompositions produced in the absorbent system. Assuredly the exquisite care, with which the avenues to the mass of the circulating fluids are guarded, indicate design and object. It is even the fact that when substances pass out of the stomach into the small intestines, without undergoing the process of digestion, they occasion very considerable irritation, even in the upper intestines. *Oleum Ricini*, in order to operate kindly as a cathartic, must be more or less digested. When from any cause, it passes unchanged through the intestines, it always produces severe tormina, and other troublesome and even distressing symptoms. Now this is much more eminently the fact in regard to the blood-vessels. But at all events, I believe that no man can be found, who has ever witnessed the operation and effects of remedial agents injected into the blood-vessels, in any considerable number and diversity of cases, that still remains an advocate for this process, either as a therapeutic measure upon the human subject, or as a means of ascertaining the powers of new and previously unknown agents. I am fully aware that there are some distinguished medical gentlemen, at least in our country, and I think also in Europe, who still commend this process (I suspect as a matter of speculation merely) in their instructions and writings, but who never the less, as I am informed, never make use of it in their practice, whether from the unwillingness of their employers to submit to it, or from some other cause, I know not. One gentleman in particular (now dead, but who formerly filled a professorship in one of our largest institutions) as I have been repeatedly informed, was accustomed to commend this process in the strongest terms to his pupils; and yet he once admitted to a private pupil of mine, who was under his public instruction, that he had never employed this measure in his own practice. All the arguments of this gentleman in favor of this method of administering medicines (as far as I can learn) were derived from the reports of experiments made in various parts of Europe, and at various periods of time, which when brought together, it is true make a considerable array, but which when examined in detail, intirely fail of affording any evidence either of its convenience,

or of its utility, or even of its safety. As is probably well known, an eminent physician of New England once made trial upon himself of this method of taking medicines, and he did indeed escape with his life, though it seemed to be a bare escape. I venture to say that the Doctor was not in haste to employ this method again, either upon himself or any of his human patients; nor will any other practitioner be in haste to try it, who will be at the trouble of reading carefully the report of the case in question. I suspect that the experiment has been much less frequently made, than the advocates for it would fain have us suppose; and if a full account of all the experiments that have been made were published, I believe that much less would be said of the process than we now hear. In every instance of the employment of this process, of which I have had particular information, it has been a mere experiment made solely for the purpose of witnessing the operation of a medicine employed in this way, or it has been a fool-hardy measure for the purpose of ascertaining what a hopeless case of disease will tolerate with impunity. In my opinion every thing done for either of these purposes merely, is absolutely to be reprobated. What benefit could any physician in his senses, expect from injecting a pint of brine into the blood-vessels of a patient in a hopeless state of disease? Brine makes no part of the mass of the healthy circulating fluids; and an antiphlogistic cathartic can not be useful to a person already in an irremediable state of exhaustion. This process can never be employed in the actual practice of medicine, because patients will never submit to the trouble and pain of the operation as often as they require a dose of medicine, because none but an expert surgeon could perform the operation properly and safely, because it would be necessary for the surgeon to devote his whole time to a single patient so long as the disease continued, because not one patient in a thousand could afford to make adequate compensation for such exclusive and protracted attendance, and above all because no one who has once been the subject of the process would ever submit to it a second time, on account of the violent and truly dangerous effects which are its immediate result. If this process is not capable of being employed in the actual practice of medicine it is not worth while to employ it in the investigation of new and previously unknown articles. It is not yet

ascertained that the operation of any considerable number of medicines is the same when injected into the blood-vessels as when taken into the stomach. A priori there is as great a probability that their effects should be different, as that they should be different, when put into gashes in the flesh, and when taken into the stomach. To settle this matter involves an investigation of their operation and effects when taken into the stomach. But if we have this knowledge what need we more? Of what possible consequence can it be to the practitioner of medicine how they operate when injected into the blood-vessels? I would therefore intirely reject this process as a means of ascertaining the powers of a new and previously unknown article.

4. Causing the article to be taken into the stomach, and that without being preceded, accompanied or followed by any violence of any sort or degree.

In my opinion, it is only when remedies are taken into the stomach, and that without being preceded, accompanied or followed by any violence, that we can expect to derive any valuable information, or to obtain any results at all worthy of the least confidence, from experiments upon the inferior animals. But even when managed in this manner, there are numerous sources of fallacy from this sort of experiments, the most important of which is the great diversity in the operation of individual agents upon different, inferior or brute animals, and particularly the difference in their operation upon such animals, and upon man.

Experiments on Fowls or Birds. "The following case" (says Professor Bigelow, M. D. of Harvard University) "happened under my own observation." "A large Eagle (*Falco ossifragus*) intended for a Cabinet of Natural History, was subjected to a variety of experiments, with a view to destroy him, without injuring his plumage." "A number of mineral" (inorganic chemical) "poisons were successively given him without effect, even in large doses." "At length a drachm of Corrosive Sublimate of Mercury was inclosed in a small fish, and given him to eat." "After swallowing the whole, he continued perfectly well and free from inconvenience." "The next day an equal quantity of" (white?) "Arsenic" (Arsenous Acid) "was given to him without any better success; so that in the end, the refractory bird was obliged to be put to death by mechanical

means." (*Big. Amer. Med. Bot. V. I. p. 164, Bost. 1817.*) I wish that *Strychnos Nux-vomica* (Strychnine was not discovered till 1818, whereas Dr. Bigelow's Work bears on its title page the date of 1817) had been tried in this case, and its effects recorded. I wish likewise that a sufficient quantity of some of the simple or pure and highly intense narcotics had been tried. From ten to fifteen grains of the recent root of *Cicuta maculata* collected just as the top of the plant began to decay, has been known to destroy an adult man. How would this bird have been affected by this agent? From ten to fifteen grains of the bark of *Gelseminum nitidum* has been known to destroy a young person a dozen or fourteen years of age. The root of *Amianthium Muscitoxicum* is about equally active with the bark of *Gelseminum nitidum*. How would this bird have been affected by a sufficient quantity of these agents? How would it have been affected by a sufficient quantity of Cyanid of Hydrogen? I think it could not have resisted the influence of Curare Woorara or Wourali, even if it had resisted the preceding articles.

We are informed that birds constantly eat the fruit of *Rhamnus cathartica*, without any cathartic effect, though Withering tells us that "the flesh of birds that feed upon the berries is said to be purgative." (*Wm. With. Syst. Arrang. Brit. Pl. 5th, Ed. Birm. 1812, Vol. II. p. 327.*) It is perhaps worthy of inquiry how it is ascertained that wild birds are not actually purged by eating this fruit. In regard to caged and domesticated birds, there can be no difficulty; and perhaps it has been by observations upon these, that the conclusion has been arrived-at. It is to be observed however that the flesh of wild land birds of the larger sort is extremely liable to be more or less cathartic, particularly to those not much accustomed to it, and that, in regions where no fruit of *Rhamnus cathartica*, nor any other purging fruit is known to grow. Such being the fact, I can not discover how it can be determined when the purgative power of bird's flesh is due to eating this fruit and when not, except in the case of caged or domesticated birds. Withering says of the flowers of *Sambucus nigra*, that "they are fatal to Turkeys." Again Withering says that "the berries are poisonous to poultry." (*Ibidem, p. 397.*) I suppose that this is not lightly said, but is undoubtedly to be relied-on, since, as I suppose, there is no better

authority than Withering. It is a widely prevalent opinion almost every where in our country, an opinion, to the truth of which I have received a great deal of specific precise and positive testimony, from those who professed to have been many times eye-witnesses, that Chlorid of Sodium or common Salt is a fatal poison to the common dung-hill fowl. I always felt strongly inclined to doubt the statement, though I am aware that there is no good ground for such doubt. The Tetraö Umbellus, and probably other species of the same Genus of Birds, regularly eat the fruit of *Rhus venenata*, which is poisonous to man. The same bird appears to eat more or less of the leaves of *Kalmia latifolia* and *Kalmia angustifolia*, both of which are deadly to Sheep, and active upon man. Almost any amount of cases might be adduced, in which articles not poisonous to man are considered as poisonous to birds; and vice versa, in which articles poisonous to man are wholesome to birds; but those that I have already mentioned I trust will be sufficient.

On Quadrupeds. "Brute animals"(says Dr. Bigelow)"are frequently less susceptible of the influence of poisons than mankind." (*Big. Amer. Med. Bot. V. I. p. 164, Bost. 1817.*) "There is scarcely any narcotic plant which is not devoured by some species of quadruped." (*Ibidem.*) "Horses, Goats, Sheep and Swine, are said to eat the *Hyoscyamus niger* without injury." (*Ibidem.*) Withering however makes a somewhat different statement. He says "Horses, Cows, Sheep and Swine refuse it" according to Linnæus; but he quoted the authority of Mr. Ballard to the effect that "Sheep some times eat it when young." (*Wm. With. Syst. Arrang. Brit. Pl. 5th Ed. Birm. 1812, V. II. p. 322.*) I am confident that I have seen Sheep eat it in the flowering season. I imagine that there are few herbaceous plants, that Sheep will not eat; and it seems to matter little to them whether their food is poisonous to man or not. Withering says of *Datura Stramonium* that "Horses, Cows, Goats and Sheep refuse it." (*Ibidem, p. 321.*) Now whatever may be the fact, in England, and with *Datura Stramonium*, Sheep, in New England, will devour with the greatest avidity *Datura Tatula* (very certainly a distinct species from *Datura Stramonium*, and I think some what more active) as I have many times witnessed. I once knew a very large barn-yard, in which *Datura Tatula* always grew-up, every summer, as

thick as it could well flourish. The truth was that the earth of this inclosure seemed to be full of the seeds of this plant, and the usual spring-clearing of the yard brought them into a situation and exposure suitable for germination. Some of the plants growing in this place were perfectly gigantic for *Datura Tatula*. As soon as the capsules had attained moderate size, but before the seeds were mature enough for growth, this farmer would always turn into this yard a part of his flock of Sheep, after which it would be no long time before every plant was eaten to a very small stump. The thorny capsules with their inclosed seeds, seemed to be the favorite parts of the plant with the Sheep, and were always devoured first. I never saw brute animals eat any thing with so much apparent relish, and with such avidity as these Sheep used to eat this article. Of course, it was never productive of the least injury to the Sheep.

“Modern travelers” (says Dr. Rush) “have discovered that Tobacco constitutes the food of a solitary wild-beast well-known in the deserts of Africa by the name of Rock-Goat.” (*Benj. Rush. Ess. Lit. Mor. & Philos. Philad. 1798, p. 273.*) I wish that this statement had been a little more specific or definite. I have no recollection of ever meeting with the statement that either *Nicotiana Tabacum* or *Nicotiana rustica* or any other species of this Genus grows in the deserts of Africa. Indeed I do not now recollect that Africa naturally produces any species of *Nicotiana*, except one at the Cape of Good Hope, which has been called *Nicotiana fruticosa*, which is not likely to grow in the deserts of Africa. Again I should like to know some thing more of the Rock Goat, whose food is so peculiar. For my part, I do not recollect that naturalists ascribe any Goat to the deserts of Africa, unless it is *Capra Jaëla* (Ch. Hamil. Sm.) which is found on the mountains of Abyssynia, in Upper Egypt, on Mount Sinai, and probably in Persia. This whole statement has altogether too much of an apochryphal savor to be worthy of reliance, though Dr. Rush seems not to have doubted. I wish he had given us his authority for it. But I am confident I have seen Sheep eat the living and growing leaves of *Nicotiana Tabacum*. It is a question however whether such leaves are in fact active. I believe that no active principle has ever been detected in the recent leaves of Tobacco, nor even in the dry leaves, till they have undergone a

kind of fermentation. They indeed contain the compound-radical of the Alcaloid Nicotina or Nicotine, viz. $H^8 C^{10} N^1$ but this requires to be converted into an Alcaloid by combination with Oxygen, before it becomes active. This Oxydation takes place when the leaves, "after being dried, are moistened with water, tied together in small bundles, and placed in heaps" (when) "a peculiar process of decomposition" (rather change of composition) "takes place." A kind of "fermentation commences and is accompanied by the absorption of Oxygen." "The leaves now become warm and emit the characteristic smell of prepared Tobacco." "When the fermentation is carefully promoted, and too high a heat avoided, this smell becomes more delicate, an Oil-like Nitrogenized" (and Oxydized) "Alcaloid is found in the leaves." "This substance Nicotine, which possesses all the properties of a" (salifiable) "base, was not present before the fermentation." (*Lieb. Org. Chim. Cambr. U. S. A., 1841, p. 311.*)

Similar facts have been ascertained in regard to the Alcaloid Indigotina or Indigotine, the essential principle of Indigo. This substance does not exist in any living plant, or any where in nature, but only its compound-radical $H^5 C^{16} N^2$ which requires to be combined with O^1 to become Indigotine, the essential principle of Indigo. This combination is always effected factitiously by the manufacturer. I suspect that many of the active principles of other plants are often produced in the same way, and that this is the explanation of the perfect impunity with which many articles are eaten by brute animals, that are so active upon the human animal under their best pharmaceutic preparations. But at present this is mere conjecture, which requires verification or disproof by accurate chymical research. However we are told that all the species of the Genus Capra eat with impunity unprepared and unmanufactured Tobacco; but I have never seen any mention of their eating this article after it has been moistened and fermented. If they will eat this latter, I should think that it would establish the general fact that brute-animals may eat with perfect impunity that which would be absolutely poisonous to man. We are told also that all of the species of the Genus Capra, eat with impunity what is called "Cicuta or Hemlock," (*See Griff. Cuv. Anim. Kingd. Vol. IV. Lond. 1827, p. 299, & Turt. Linn. Syst. Nat. Lond. 1806, Vol. I. p. 116*) by which, I suppose, is intended Conium macu-

latum. These two plants, viz. Cicuta and Conium are too diverse, and differ too much in the quality and degree of their powers, to be thus confounded with any safety. It is to be observed that the popular name Hemlock (i. e. Border or Road side Plant) belongs exclusively to Conium maculatum, and not at all to any Species of Cicuta, to which it is wholly inapplicable. In America, I have never seen Conium maculatum growing any where, except by road-sides, or the contiguous borders of fields, unless under culture; but I never saw Cicuta in such a situation. Now I strongly suspect that living Conium maculatum, like living Nicotiana Tabacum, contains only the compound-radical of the Alcaloid which is its active principle viz. $H^{14} C^{12} N^1$ except the membranous part of the leaves in the fullest flowering season, and the full-grown but still perfectly green seeds. The sudden appearance of the Alcaloid in these parts, at these times, and its equally sudden disappearance, is very singular upon any other hypothesis. But let it not be forgotten that this is a mere hypothesis not yet at all proved. Should it ever be substantiated I think it not at all improbable that the leaves collected at other times than the fullest flowering season may be rendered active by the same process which renders the leaves of Nicotiana active. The compound-radical above given requires only O^1 to convert it into the Alcaloid Coniine. But this will be further considered under Conium maculatum. If these conjectures should prove true, it will explain why some brute animals can eat this plant with such perfect impunity.

It is said that "an interesting observation was made" (by James Blundell, M. D.) "viz. that a Dog can not be poisoned by very large doses of Opium in its different forms." (*Petigr. Med. Port. Gall. Lond. Part IV. p. 14, Life of James Blundell, M. D.*) We are told that a German physician, who wished to dissect the Erinaceus Europæus or Hedge Hog, gave one of these animals a large quantity of Opium without killing it (I wish it had been mentioned how much) and then what is called Prussic Acid (a vague name which has been applied to five intirely different and distinct articles) but as is said, it produced no effect. Then he gave Arsenic (I suppose Arsenous Acid) and finally Corrosive Sublimate (of Mercury) and all without any disturbance of the animal, or at least without any danger to his life. This seems

like being proof against all sorts of things commonly called poisons. I have been informed, on unquestionable authority, that, in one instance, twelve grains of Strychnine were administered to a Dog without any apparent effect. I have also been informed upon equally good authority, of another instance, in which twenty-four grains of Strychnine were likewise given to a Dog with no effect. I have known various intermediate quantities between these two taken with the most perfect impunity by Dogs. On the other hand I have known much smaller quantities than the first mentioned to kill medium sized Dogs. Thus it would seem as if no reliance could be placed upon producing any uniform and definite effects upon brute animals, even by as active an agent as Strychnine; and that a small quantity may kill speedily, and a large one may fail of producing any apparent effects, not even that of vomiting the animal.

The whole Genus *Capra* also eats with impunity species of *Euphorbia* of the most active character. (*Griff. Cuv. Anim. Kingd. Lond. 1827, Vol. IV. p. 299.*) Withering says of *Sedum acre*, — “it is very acrid,” — “applied externally it blisters,” — “taken inwardly” by man of course, “it excites vomiting,” and yet — “Goats eat it.” (*Wm. With. Syst. Arrang. Brit. Pl. 5th Ed. Birm. 1812, Vol. II. p. 525-6.*) Withering says of *Ranunculus acris*, — “it is very acrid, and easily blisters the skin,” — “Horses and Cows leave this plant untouched, though their pasture is ever so bare,” — “Swine refuse it” while “Goats and Sheep eat it.” Withering says of *Ranunculus sceleratus*, — “the whole plant is very corrosive,” meaning epispastic, — “beggars are said to use it to ulcerate their feet, which they expose in that state, to excite compassion,” and yet, — “Goats eat it.” (*Ibidem, Vol. III. p. 621.*) Withering says of *Ranunculus arvensis*, — “three ounces of the juice killed a Dog in four minutes,” and yet, — “it has lately been said Horses, Cows and Sheep in Italy eat it greedily, though it is so acrid as to poison the last.” (*Ibidem p. 626.*) Withering says of *Ranunculus Flammula*, — “it is very acrid,” and — “applied externally it inflames and blisters the skin.” He adds — “its acrimony rises in distillation.” He says — “some years ago, a man traveled in several parts of England administering vomits, which like White Vitriol operated the instant they were swallowed.” “The distilled water

of this plant was his medicine, and from the experience I have had of it, I feel myself authorized to assert that in the case of poison's being swallowed, or other circumstances' occurring in which it is desirable to make a patient vomit instantaneously, it is preferable to any other medicine yet known, and does not excite those painful contractions in the upper part of the stomach which the White Vitriol some times does, thereby defeating the intention for which it is given." "Of this plant" says Withering "Cows, Goats, Sheep and Swine refuse it," while "Horses eat it." (*Ibidem*, p. 619.) There are some things in this statement that require comment. I have been in the habit of using the distilled water of *Ranunculus Flammula*, but such a preparation as I have had has never been any where near as acrid as the plant itself. If the distilled water, that I have had, has been held in the mouth for a while, a slight sensation of warmth has usually been perceptible; and the same has been the fact in the stomach if some of it has remained there for some time. I never knew a disagreeable amount of it to occur. If such is commonly the fact, it will hardly justify the statement that "its acrimony rises in distillation." It would be more correct to say that a small portion of its acrimony rises in distillation. Withering represents this distilled water as vomiting the instant it is swallowed. Now I have used it a great number of times, and I never knew it vomit under five minutes, and often not under ten minutes. This I call a quick emetic, but by no means an instantaneous one. According to my observations, the peculiarities of the operation of this distilled water are, first its speed, though it is not instantaneous; second the slight amount of uneasy sensation that it produces in the stomach, both before and after it vomits, for many times it produces no nausea at all; third the thoroughness with which it clears the stomach; and fourth the general kindness of its operation. Its effects pass-off about as speedily as they come-on. I speak of doses sufficiently large to produce the fullest desirable effects. I join Dr. Withering fully in his commendation of this article, though I never witnessed an instantaneous effect from it, or any thing approximating to it.

"Professor Pallas has stated that the Hedgehog," (*Erinaceus Europæus* Linn.) "devours Cantharides by hundreds, without inconvenience, whereas" (a single) "one of these insects may

occasion serious trouble to a man." (*Big. Amer. Med. Bot. Vol. I. p. 164, Bost. 1817.*) Withering says of *Asarum Europæum* that "the root finely powdered and taken to the amount of thirty or forty grains produces vomiting," but "if it is coarsely powdered it generally purges;" that "an infusion of one or two drachms in Wine vomits;" and that "the powder of the leaves is the basis of most of the Cephalic-Snuffs," (so called) "which occasion a considerable discharge of mucus from the nostrils without much sneezing;" and yet he adds "Cows eat it." (*Wm. With. Syst. Arrang. Brit. Pl. 5th Ed. Birm. 1812, Vol. II. p. 529.*) Withering says of *Senecio vulgaris* that "a strong infusion of the plant vomits." This statement, according to others, hardly does justice to its emetic activity. "Swediaur recommends this plant as an anthelmintic, and the juice is given internally to discharge Botts from Horses." "Horses and Sheep refuse it" according to Linnæus; and "Cows are not fond of it;" and yet "Goats and Swine eat it." (*Ibidem, p. 888.*) Of *Eupatorium Cannabinum*, Withering says "an infusion of a handful of it vomits and purges smartly," and that an ounce of the root in decoction is a full dose" etc. "Horses, Cows, Sheep and Swine refuse it" though "Goats eat it." (*Ibidem, p. 871.*) In the Farmers Magazine it is stated that "the botanical professor at King's College" (London) "said that Horses will not touch Cruciferous Plants, but will feed on Reed Grasses, amidst abundance of which Goats have been known to starve; and these latter again will eat and grow fat on the Water Hemlock" (*Phellandrium aquaticum*) "which is a rank poison to other cattle." "In like manner, Swine will feed on Henbane" (*Hyoscyamus niger*) "while they are destroyed by common Pepper" (*Piper nigrum*) "and the Horse which avoids the bland Turnip" (*Brassica Rapa*) "will grow fat upon Rhubarb," Rheum?—what species is here intended?

Such statements as the preceding, and from the best authorities, might be multiplied indefinitely. Those that I have adduced have been taken as they happened to fall within my cognizance, and without any selection of those most favorable to my own views. Indeed my views are purely the result of a careful consideration of these, and a great number of other similar observations. That every individual of them is strictly true I can not show; but I can not entertain the least doubt that a sufficient

number of them may be perfectly relied-on to establish all the conclusions that I deduce. Even if one half of the multitude of such statements, that are already on record, are admitted as facts, it will at once be obvious that very little reliance indeed can be reposed on experiments made upon brute animals for the determination of the powers of new and previously unknown articles, even when the article is taken into the stomach, and that, without being preceded, accompanied or followed by any violence of any sort or degree. But it is almost invariably the fact that the only effects of most medicines, that can be observed upon the inferior animals, are their ultimate effects—effects which are never required for remedial purposes—effects which are considered poisonous, when produced upon the human subject. That grade or degree of operation which is useful or proper for remedial purposes, can be perceived in the inferior animals, only from a very moderate portion of remedies. It is true that we can some times very safely infer what will be the medicinal grade of the operation of an individual agent, from a knowledge of the ultimate or poisonous grade; but this can be done only in a limited number of cases. A distinguished writer upon physiology and zoölogy generally, very justly says — “never indeed should we forget for an instant, that all arguments drawn from analogies between man and brutes ought to be received with the utmost caution.” “Their specific differences are so numerous, extend to such a variety of particulars, and often present such striking points of opposition, that in reasoning from one to an other, the chances are generally against the correctness of our inferences.” (*Griffith? Cuvier's Anim. Kingd. Class Mamm. Suppl. Hist. Man. Lond. 1827, Vol. I. p. 141.*) Dr. Bigelow of Harvard University says “in the experiments which have been made on the inferior animals, to test the effects of doubtful medicines, the positive evidences of activity which they furnish are in general more to be depended on, than the negative; i. e. if an”(inferior)“animal suffers from the action of any substance, a man would be more or less likely to suffer some what in the same way; yet if the”(inferior)“animal escapes with impunity, it does not follow that the man would be equally fortunate.” (*Big. Amer. Med. Bot. Vol. I. p. 164, Bost. 1817.*) Further I believe it is very generally the fact that when a given agent produces the same effects upon a considerable num-

ber of the inferior animals, it will be likely to produce them upon man. But still I would strongly advise that very little confidence should be reposed in this method of arriving at the knowledge of the powers of new and previously unknown articles. In short I do not think it deserves sufficient confidence to render it worth while even to institute a regular and systematic course of experiments of this sort, and for this purpose; though such facts as accidents present may be worthy of record.

5. Experiments on the Diseased Human Subject, as a Means of Determining Medicinal Powers.

I think it should be adopted as a rule never to be deviated from, that we are in no case whatever to make experiments upon the sick with new and intirely unknown articles. It is never justifiable to omit the use of remedies of known and established powers and efficacy in any case of disease, that may be committed to our care, for the employment of any agents of previously unknown powers and efficacy, that we may by chance be desirous of investigating. Such a course of proceeding as that to which I object, would constitute one of the most flagrant abuses of the trust and confidence reposed in the medical profession by the non-medical community that I can well imagine, more especially as it would be of little or no use to the interests of humanity, even if it were to be practised. How would it be possible under such circumstances to distinguish the operations of a new and previously unknown article from the effects of disease; since it is well established that though all cases of the same identical species of disease agree in the general outline of their symptoms and phenomena; yet every individual case differs from every other individual case very considerably, in the filling up of the picture? How then I repeat would it be possible to distinguish under such circumstances, the operations of a new and previously unknown article from the effects of disease? In my opinion, it is no more justifiable, nor capable of affording any more information, to accompany the remedies of known and established powers and efficacy, in any case of disease, with agents of unknown powers, since in these circumstances it would be utterly impossible to determine what belongs to the disease—what is the effect even of the known remedies—and what is occasioned by the new and previously unknown articles. It is now well ascertained that two or more medicinal agents, differing more or less in their powers,

when administered in conjunction, often (though it is true not always) so modify each others' powers, as to produce effects very materially different from the effects of either alone; and likewise that diseases and remedies act and react upon each other, in such a manner, that unless we are familiarly acquainted with all the possible diversities in the course of the disease, and all the operations of the remedial agents, it will be impossible to refer the phenomena which may occur, with any certainty, either to the disease or the medicinal agents employed. Upon the whole therefore I would intirely reject experiments upon the diseased human subject as a means of determining the powers of new and previously unknown articles, on the ground that such experiments would be not only wholly unavailable and unjustifiable, but also very useless, indeed absolutely worthless for the desired purpose. Though such a method is some times commended, yet I never heard, much less knew of its being adopted by any physician whatever.

6. Experiments on the Healthy Human Subject, as a Means of Determining Medicinal Powers.

Experiments upon the healthy human subject appear to me to be the most satisfactory, the most perfect, the least inconvenient and the safest means of determining the medicinal powers of new and previously unknown articles. In this opinion I am not singular, for Haller is said to have declared "that to be able to judge of the effects of different remedial agents, experiments ought to be made with them, upon persons in health, the results of which might then be assumed as the foundation of some thing stable and certain, for directing their employment in disease." Sir John Hill justly says that "it any suppose the trial" (of new articles, for the purpose of ascertaining their medicinal powers) "is at all dangerous, they mislead themselves; and to encourage so laudable an undertaking, I shall observe how little is the hazard, and how considerable the advantage, from what we know already." (*Sir John Hill's Fam. Herb. Bung.* 1812, p. 370.) But it has been objected to experiments upon persons in health, for the purpose of ascertaining the powers of new and previously unknown articles, that the effects of remedial agents in health, are materially different from their effects in disease; and that indeed there is but little analogy between their operations under such different circumstances. This is true in certain respects, and it is untrue in certain other respects.

It is certain there can be no antiphlogistic operation where there is no phlogistic diathesis ; but if an article possesses the power of directly diminishing, in a prominent degree, both vital energy and strength of action in the circulating system, in perfect health, and at the same time possesses no other power, that would aggravate phlogistic diathesis, we may safely infer that it is more or less antiphlogistic. There can be no such operation as that of a tonic, where there is no loss of tone, or in other words no atony, or exhaustion or general debility of the parts dependent upon the involuntary motor nerve of chymical action nutrition and reproduction ; and yet an article that is capable of producing a direct augmentation of the appetite and digestive power in health, may be expected to produce the same effect in some diseases at least. An article that is capable of producing a slow, gradual and permanent increase of vital energy and strength of action, primarily in the circulating system, and secondarily in the muscular and other systems in health, will assuredly produce the same effects in some moderate and chronic cases of atony, debility or general exhaustion. An article that is capable of producing a quickly diffused and transient increase of vital energy and strength of action, of a peculiar character indeed, in the circulating system in health, will assuredly produce a greater or less degree of the same effect, in some doses and quantities, and in some diseases. A given article may indeed have a sufficient degree of these powers to produce the effects in question in health, and in moderate diseases, but not in very severe and violent diseases ; but we are not to expect that absolutely every thing in regard to remedies, can be learned, by experiments upon the healthy.

There can be no such operation as that of an antirritant where there is no morbid irritability and irritation—no morbid sensibility and sensation—no morbid mobility restlessness and jactitation ; and yet, we may certainly infer that an article is antirritant, provided we find that it diminishes the natural and healthy susceptibilities in a material degree. An anodyne effect can not be produced where there is no pain ; and I know of no method of arriving with certainty, at a knowledge of the anodyne powers of a new article, by experiments upon a person in health ; but if the article is found to be an efficient narcotic, it will justify the trial of it for the relief of pain, but all active narcotics are not necessarily anodyne in any material degree.

There can be no such thing as an adenagic operation where there is no topical Inflammation—no Parabysma—no cutaneous eruption—no glandular enlargements—no suspension of activity in any part of the secernent and absorbent or glandular systems, etc. and yet, if an article is found to increase materially both the secretions and excretions generally, such as those of the salivary glands, the bronchial and other mucous membranes, the liver, the kidneys, the uterus, and the skin, or even a majority of them, in a healthy subject, it will afford a strong presumption that it possesses an adenagic power. In this manner, was the adenagic power of the several American species of *Apocynum* first discovered, as well as that of numerous other articles. If an article increases the activity of a single excretory only, without affecting any of the rest, as for example, the kidneys, it may be inferred to be diuretic or uragogue, or the excretories of the skin, it may be inferred to be diaphoretic or hidrotagogue, etc. If by observation we find that a new article greatly diminishes the natural secretions from the mucous membranes in health, it may be safely and certainly inferred to possess more or less power of diminishing the excessive secretions from this texture, in such diseases as *Blennorrhœa nasalis*, *B. faucialis*, *B. bronchialis*, *B. intestinalis*, *B. vaginalis*, and *B. urethralis*. In this manner was the power of *Creasote* over excessive secretions from the mucous membranes first discovered. A remedy may be safely inferred to be capable of contributing to the relief of *Paruria superflua* (i. e. excessive secretory activity of the renes, and excessive discharge of urine not containing saccharine matter) and even to the relief of *Paruria Diabetes*, provided it has the power of greatly diminishing, and much more, of nearly suspending the urinary secretion in health. The power of *Narcotina* or *Narcotine*, in these two diseases, was ascertained in this manner.

A remedy may be safely inferred to be capable of contributing to the relief of *Diarrhœa*, provided it directly diminishes the peristaltic action of the intestines in health and also directly diminishes very greatly the natural secretory activity of the mucous follicles, so as to produce comparatively dry discharges. An article that will nauseate and vomit in health may fairly be expected to do so in many diseases. An article that will prove cathartic in health, may certainly be expected to produce the same effect

in many diseases. It is true, we can not always arrive at the ultimate operations of new agents, by experiments upon those in health, because it might, in many cases, be incompatible with perfect safety to push certain articles to such an extent, and certainly it would not be convenient. We may however be well acquainted with the medicinal grades of the operation of a given agent, without knowing any thing of the ultimate grades of its operation, or those which transcend and supersede the medicinal grades. It is true that it is always desirable that we should know what the ultimate grades may be; but for this knowledge, we may well be content to be indebted to casualties, which unfortunately are constantly occurring, and doubtless will continue to occur, to the end of the world. It is to these that we have hitherto been mainly indebted for all we know on the subject under consideration. But the ultimate operation of a new article, may often be inferred with sufficient certainty from its primary operations. If a new and previously unknown article is capable of producing upon the surface of the body, i. e. the skin, any grade or degree of an epispastic effect, even that grade or degree that falls short of rubefaction, so called, it certainly possesses that power, which I call *oræsthetic*. Indeed that grade of operation, which I have just specified, is in fact medicinal *oræsthesi*s, and the grades of rubefaction (*Erythematic Phlogosis*), vesication, ulceration and suppuration, would be ultimate and morbid grades of *oræsthesi*s, if produced internally, though they may be medicinal grades, when produced upon the skin. Sphacelation, even upon the skin, is always the very last grade of *oræsthesi*s, and should never be produced, even though an exceedingly superficial sphacelation of slight extent, might not be attended with any danger. It would involve sloughing and an unsightly scar. If an article acts powerfully upon the mucous membrane of the alimentary canal and obviously increases its susceptibility to a preternatural degree, we may safely and certainly infer that it would not only obviate torpor and insusceptibility, but that, in sufficient quantities, it would be likely to produce topical irritation, and *Erythematic Inflammation*, and might even produce ulceration, and suppuration, and perhaps even sphacelation.

If we find a new article to be capable of producing powerfully soporific effects we may surely infer that if pushed still

further it will likewise produce that aggregate of effects constituting ultimate narcosis. An article may indeed be capable of producing very slight soporific effects without being capable of producing ultimate narcosis; but such an agent must be a feeble one, and can not be of much importance in the *materia medica*. A certain degree of ultimate narcosis may however be produced in a person in health, without the least hazard, and scarcely even inconvenience. For example, any narcotic may always be pushed so far, not only with impunity, but without the least risk, as to produce confusion of head, vertigo, some affection of the sight, epigastric uneasiness, and various other symptoms, which will be decisive that the article possesses true narcotic powers. It is never worth while however to push a narcotic so far, as to be able to decide what sort of convulsions it will produce, though I have often known this done without any inconvenience or hazard, with those supposed narcotics, really *erethistics*, which produce tetanic spasms, gradually and regularly increasing the doses and quantities, and taking them at regular and short intervals. Fortunately, the ultimate effects of active agents are seldom or never necessary for remedial purposes; and it is only desirable to be acquainted with them, in order to be able to recognize and remedy them, as excessive and inordinate, or as the popular phrase is, *poisonous effects*.

But such is the propensity of unprofessional people to tamper with medicines, that no agent can have been long in use among physicians, without having been taken to excess by some rash and ignorant person, so that the medical faculty seldom remains ignorant of the ultimate effects of any agent, for any great length of time after it is introduced into the *materia medica*. It may be considered as quite certain, that all the effects which a given agent is capable of producing in health, it will likewise be capable of producing in disease, provided the doses and quantities are proportioned to the modification of the susceptibilities which the disease occasions. Certain diseases are well known to heighten the susceptibility to certain operations of medicinal agents; while certain other diseases diminish, some times even in a powerful degree, the susceptibility to particular operations. Particular diseases some times increase the susceptibility to particular operations of medicinal agents, while at the same time, they diminish

the susceptibility to other operations. Feeble remedies will sometimes intirely fail of producing their specific operative effects in diseases that greatly diminish the susceptibility to impressions from their peculiar powers; while in diseases which exalt the susceptibility to their specific impressions they may prove comparatively efficient agents.

When experiments are to be made upon a human subject in health, for the purpose of ascertaining the medicinal powers of a new and previously unknown article, let the new and unknown agent be taken at first, only once a day, beginning with a dose exceedingly small in relation to its probable character, and let it be gradually increased in its quantity, till it begins to produce decided operative effects, and even till as much is taken, as can be tolerated without inconvenience or with perfect impunity. In this way, its minimum and maximum doses will be very easily ascertained, as well as the ordinary operative effects of such doses. Let the new and unknown agent be taken next in moderate and uniform doses some where between the minimum and maximum and be repeated at regular and short intervals, and be continued for a considerable time—gradually increasing the dose, or shortening the intervals, or both, till it is pushed as far as it can well be, with perfect impunity, or at least without any material inconvenience. Let the experiments, if possible, be repeated upon several different subjects. Next let all the various pharmaceutic preparations, of which the agent is fairly susceptible, be tried in succession. Few articles indeed will be conveniently susceptible of the whole, that hold a place in our Pharmacopœiæ; and this fact will greatly lessen the labor of research. Besides, the trial of two or three of them will enable us to judge of the comparative eligibility of the rest, at least with all the precision necessary for a selection in particular cases. The pharmaceutic preparations, a greater or less number of which may be proper, though never the whole, for any single article, are 1 Powder; 2 Pill; 3 Infusion; 4 Decoction, (in general a very bad form); 5 Tincture; 6 Wine; 7 Water; 8 Extract (by expression and inspissation); 9 Extract (by exudation and inspissation); 10 Extract (by decoction and inspissation); 11 Extract (by tincture and evaporation); etc. In my opinion, that form which is termed *Acetum* or *Vinegar* is not worthy of being retained in pharmacy,

as I should urge with reasons, if I were to prepare a work on pharmacy, or in other words a Pharmacopœia; and I think that the same may be said of that form, which has been termed an *Æther*. What has been termed a Confection or Electuary, should likewise be rejected if the form of Powder is retained, since a Confection or Electuary is only a Powder with Syrup (or something equivalent) for immediate administration. The forms of Emulsion and Trochiscus I purposely omitted to mention because it is applicable only to such a small number of articles.

I will give a brief illustration of some of the grounds upon which one pharmaceutic form is to be preferred to an other. The Disulphate of Oxyd of Quininum may well be given in powder mingled with Syrup of Sugar. In this however more or less of an ordinary dose is liable to be lost in the mixing, or by means of its adhering to the vessel or instrument in which it is mixed. This constitutes some what of an objection to this pharmaceutic form. Some patients however refuse to take it in this form, on account of its great bitterness to the taste. This agent may well be given in the form of pill. If this form is selected, we must take care that the pills are recently prepared and soft, so as to be easily soluble in the stomach. Some apothecaries constantly keep pills containing a medium dose of this article ready prepared, and of course in a very dry and often hard state. I have some times seen no effect produced by such pills, and on due investigation, found that they passed through the alimentary canal undissolved. But some patients will not take a pill at any rate. This salt might well be given in tincture, were not the intensity of its bitterness so much exalted by solution in a dilute Alcoholic menstruum, as to render it intolerable to very many persons, after a short time. This salt is too little soluble in water to permit a Water of it to be an eligible form. Some convert the common di-salt into a neutral, by the addition of Sulphuric Acid, the neutral salt being much more soluble. I do not think that the operation of the neutral salt is as good as that of the di-salt, but it is much less bitter than the tincture, and therefore more readily taken. But in converting the di-salt into the neutral, almost all apothecaries and even physicians add an excess of Acid. This renders its operation not only unkind, but often absolutely injurious. I therefore consider the Disulphate of Oxyd of Quininum

diffused in Syrup of Sugar, or recently made into pill, as the most eligible forms for administration, for the reasons just hinted.

Strychnos Nux-vomica can not well be administered either in the form of powder or pill, on account of the extreme difficulty—indeed almost impossibility of reducing it to a sufficiently fine powder for this purpose. Infusion or decoction is worthless because water is an inadequate menstruum of its active principles. Tincture is a highly eligible preparation, with the single exception that it is so intensely bitter, that some patients will not take it on that account. Alcoholic extract, or extract by tincture and subsequent evaporation of the menstruum, has always, under my observation, been of very variable and uncertain strength, so much so that some times an eighth of a grain is a sufficient dose, and some times two grains are required. In addition to this, in all cases within my knowledge, the extract has been so thin as to require about five grains of inert matter and often more, to be added to a dose to give it a suitable consistence for a pill. This inert matter is always much worse than useless, and together with the variability of its strength, renders it nearly, and often quite impossible to regulate the doses by operative effects. I never knew the physician, who prescribed the extract exclusively that ever accomplished much with this agent. I do not believe that Strychnine is the sole active principle of *Strychnos Nux-vomica*, and therefore I can not consider it as a substitute for it, in all cases. All things considered then, I am of the opinion that the Alcoholic tincture of *Strychnos Nux-vomica*, is on the whole the most eligible pharmaceutic form for its administration.

Let the effects upon all the functions, be carefully observed and recorded, and sensations as well as actions, of all sorts; and also the times when they took place; and likewise the difference in these effects according as the experiments are made upon different subjects, and with different pharmaceutic preparations. Let it be remembered that the functions to be observed are 1 Those of the involuntary motor nerve of chemical action nutrition, etc. 2 Those of common sensation; 3 Those of the involuntary motor nerves of expression; 4 In a few instances, those of voluntary motion; 5 In a few instances, those of special sensation; 6 In a few instances, the functions of the hemispheres of the cerebrum.

The functions of the hemispheres of the cerebellum being intirely unknown can not therefore be observed.

In experiments of this sort, an undue importance is commonly attached to the pulse. Some years ago, such experiments were made by simply administering a certain moderate dose of the article under examination at comparatively long intervals, often without diminution or increase, and of course, without any means of knowing whether the system was really and truly under its influence. The pulse was then numbered, every ten, fifteen, twenty, or thirty minutes, and if its frequency was a little greater, than at the outset of the experiment, it was inferred that the agent under examination was a stimulant; but if the frequency was a little less, why then the article was the very opposite of a stimulant. Such experiments were absolutely worse than worthless, since they positively misled all those who did not know enough to appreciate them properly. How any physician of ordinary intelligence could have countenanced such an inference, from such premises, is a mystery to me. The loss of two thirds of the blood in the body would have been far more stimulant. From great Hemorrhage I have known the frequency of the pulse increased to two hundred beats in a minute; therefore great Hemorrhage must be very powerfully stimulant.

It will at once be obvious without further explanation or comment, that this method which I am now recommending of investigating the powers of new and unknown articles, must be first perfectly safe as respects the subjects of the experiments, and not even productive of any material inconvenience; and second more certain, extensive and complete, and in all respects more decisive and satisfactory, than any other method hitherto proposed. And yet there is one set of cases for which it will not answer. The Class Ecbolica (if there is really just foundation for such a Class) is an exception to the preceding statements. I can not conceive of any mode of determining whether an article possesses ecbolic power or not, except by trial upon a gravid female, either of the human race, or of some brute animal species. Even if an article should prove ecbolic upon a brute animal, I do not think it would by any means follow as a necessary consequence, that it would produce the same effect upon the human subject. Ever since I have known any thing of medicine, I have been ac-

quainted with the reputation of *Nesæa verticillata* or *Decodon verticillatus* (which ever may be the earliest name, the former being that however on which Lindley decides, a plant commonly known by the not very refined name of Slink-weed) which is to the effect that it possesses the power of producing ecchymosis in all domesticated herbivorous animals, more especially in Sheep, when it is eaten by them with hay. To its operation in this way, I have had the strongest testimony, that can possibly be given by farmers on a medical subject, and as large an amount of it as can possibly be desired; and yet I have never been able to devise any means of ascertaining whether it is capable of operating in the same way upon women. A company of my private pupils once undertook to ascertain whether this article possessed any other power or powers, by experiments upon themselves; but before any thing was definitely determined, I was obliged to undertake a journey, which interrupted my superintendence of the experiments; and before my return, the young men were dispersed, as they had previously finished the studies of their professional pupillage, so that the investigation was never resumed. In this way or by this method of management a very adequate knowledge may be obtained, of the powers and primary or medicinal operative effects, and even of the doses of new articles that would fall under most of the Classes of Medicines, that I recognize. Investigation in this manner, and to this extent, may be made by such experiments, almost without trouble or inconvenience. It is true that the ultimate effects of medicinal agents, those effects which transcend their remedial operations, can not as I have already said, be arrived at in this manner; nor can they perhaps in any other manner, except by the observation of the effects of accidents, and of poisonous doses of a few articles taken for the purpose of suicide, or otherwise feloniously administered; but this only leaves us, in relation to new and previously unknown articles, just where we are, in relation to a large number of old ones.

Without an accurate and precise knowledge of the exact powers of an article as evinced by its operative effects upon the human system, all use of it must be absolutely empirical. It is sufficiently obvious that without a knowledge of the operations of a given agent, it can certainly never be rationally, but only empiri-

cally known, whether it is well adapted to counteract any particular morbid condition or not. But as disease and medicine mutually act and react upon each other, and modify each other's effects to a greater or less extent, I think it is quite clear that a state of health is much more suitable for experiments for the purpose of determining the true powers of a new and previously unknown article, than a state of disease; and that we can not consider ourselves as completely acquainted with the powers of any new agent, till a thorough course of experiments have been made with it upon a person in health.

7. The Careful Observation of the Effects of Articles taken by Accident or Mistake as a Means of Determining Medicinal Powers.

Important information in regard to the powers and operations of previously unknown articles has often been derived from the observation of their effects, when taken by accident or mistake; but that such information should be of any value, it is necessary that the observations should be made by a scientific and practical physician of sound judgment and competent professional skill in all respects. In this way a knowledge of the ultimate effects of medicinal agents is often acquired—a knowledge which can not be obtained, at least to any extent, by experiments upon healthy human subjects, without pushing the agent to an inconvenient, and in some instances perhaps to a hazardous extent. In other respects however, the knowledge thus obtained is far less extensive and complete than that which is obtained by experiments upon healthy human subjects, since it is well known that when so large a quantity of a given article is taken at once, as to produce directly its ultimate effects, very few of the ordinary effects will occur which result from smaller doses, and especially moderate and uniform doses at regular and short intervals. For example, *Cicuta maculata* of our country has been taken many hundreds of times by mistake, in many instances in such quantities as to destroy life speedily, in others, in such quantities as to produce the most violent effects a subject could well suffer consistent with recovery; and yet how little knowledge useful in the *materia medica* has been acquired in this way in regard to this article. In this way we have learned simply that *Cicuta maculata* is a very active narcotic; that retching and vomiting are a part of its

ultimate effects when it is taken in inordinate doses; that the convulsions which it produces are of the clonic or Epileptic sort, and that in all probability it is one of the most active agents of the indigenous materia medica of the U. S. A. We have not yet obtained, nor shall we ever obtain in this way, any knowledge of the degree of its antirritant, anodyne and soporific powers; nor any knowledge whether it is euphrenic, antisbestic, adenagic, diaphoretic, diuretic etc. all of which are powers that are frequently associated with those of a narcotic; nor have we in this way learned any thing of its medicinal doses or the duration of its effects under any circumstances, of all of which we should know more than can possibly be learned in such a manner, before we could be justified in treating any serious disease with it.

I was once called to see a patient who by the recommendation of a female empiric, had taken an infusion of *Sium latifolium*; but the utmost that I could ascertain from this case was merely that the article taken was more or less narcotic, and from the best judgment I could form, very considerably so. I could not learn any thing in regard to the strength of the infusion, as it was not made by weight and measure, and there was not sufficient notice taken of quantities, for a probable guess. Even the quantity of the infusion taken, and the length of time before it produced any perceptible effects, could not be ascertained at all satisfactorily. From the statements of the bystanders I was inclined to think that the patient had had more or less convulsions before my arrival, and of the subtonic or common sort; but nothing could be ascertained either with any reasonable degree of precision or certainty. The subject was in a state of great languor and lassitude or in a word prostration (not exhaustion) but how much of this was due to the fright and how much to the *Sium* could not possibly be ascertained. I did not reach the bed-side soon enough to witness the most urgent symptoms, and the attendants could tell me nothing at all satisfactory.

I was once speaking of the great acrimony of the full grown but unripe seeds of *Dirca palustris*, in the company of the late Dr. Nathan Smith of Yale College, when he informed me of a case that he had seen in which some of them had been swallowed, and had produced effects, which were considered alarming. He had been called upon the occasion, and had found what he judged

were the effects of a narcotic, and one more or less like *Datura*. Feeling a strong interest in the subject I made many inquiries of Dr. Smith, but he told me that he had been able to ascertain nothing more than the above. He could obtain no information that would justify even a probable conjecture 'as to the quantity taken, the length of time before any effects were produced, whether there were indications of any other power beside that of a narcotic or not, etc. He observed none of the effects of an acrid. The powers of this article have commonly been considered as analogous to those of *Daphne Mezereum*. Now if such men as Dr. Smith could obtain no more information from such a case as this, how can the great body of the profession be expected to do it? I have many times obtained about the same amount of information from various medical gentlemen of great acuteness of observation and research, in regard to cases to which they had been called, and in which certain articles had been taken in inordinate quantities by some mistake or error. I have no recollection of ever arriving at any thing myself that was any more definite or important.

A very intelligent man of my acquaintance once took a large dose of a watery preparation of the root of *Veratrum viride* instead of a similar preparation of *Ictodes foetidus*. It produced violent effects, and occasioned some alarm not only with the patient but with his friends. Being well acquainted with the powers, operations and effects of this agent, I availed myself of the occasion to find how much valuable information could be obtained of such a man in regard to an article so taken; and I found it was very little. The patient noticed a considerable amount of vertigo, a great deal of nausea, and much vomiting. He was unable to tell me any thing else. If I had had no other means of judging of the character of *Veratrum viride*, I should have found no reason to conclude that as a medicine it could be any thing but a simple and pure emetic. The fact is that the physician seldom reaches the bed-side of the patient till the most urgent symptoms have passed-by, or till he has been dosed by the friends or the bystanders, some times judiciously and some times the contrary; and between the discomfort and the fright (if the latter occurs) no accurate observations are made and of course no accurate information can be given. This I imagine is about as

much as is commonly ascertained, or as much as we can expect to ascertain, as a general rule, from such cases; so that it will be sufficiently obvious that the knowledge of the *materia medica* can never be much advanced in this manner; and yet the little information that may be thus obtained should not be neglected.

OF WHAT THE MATERIA MEDICA SHOULD CONSIST.

Previous to the commencement of my account of the Classes of Medicines which I shall adopt, of the individual agents which these Classes will comprise, and of their application to the treatment of particular diseases, it may be proper, perhaps necessary to state the principles upon which my *materia medica* will be made-up, and of the nomenclature in this department which I shall adopt. According to my views, the catalogue of the articles of the *materia medica*, that every physician ought to have at his command, should comprise

1. Every indigenous remedial agent which is definitely known to be really capable of being useful in the treatment of any cases of disease, whether it is active or mild in its effects, whether it is of primary value and capable of being relied-on exclusively in the management of severe complaints, or is only of secondary importance, and merely proper as an auxiliary, and whether it requires officinal preparation, or only extemporaneous; and

2. Every exotic article of the same general character that can be conveniently obtained and easily kept.

To those who are inclined to adopt the supposed opinion, so often quoted, of Lord Bacon, that "a multiplicity of remedies is the child of ignorance," who suppose that the body of medicinal agents now in the hands of physicians does not need an increase in the number of its articles, that the *materia medica* is already encumbered with many superfluous drugs, that redundancy rather than deficiency is always its defect; that even the very active substances are more numerous than can be of use to any physician, that retrenchment is now more necessary than augmentation, and in short, that but few medicines are necessary for the best management of all diseases, and perhaps that the *Lancet*, *Tartrate of Antimonia* and *Potassa*, and *Dichlorid of Mercury* are amply sufficient for most cases, such a catalogue may appear preposterously extensive. It has repeatedly been suggested to

me by some of my professional acquaintance, that my *materia medica* is much too extensive; that the profession at large can never become acquainted with so many articles; that if the great body of physicians could actually do this, they would never have occasion to employ a twentieth part of the list, etc. But I know from personal inquiries and investigations that this is a great undervaluation of the capabilities of the medical profession. The elderly practitioners, with whom I made acquaintance while I was engaged in the studies preparatory to the practice of medicine, and with whom I continued intercourse, as long as they survived, had as extensive a *materia medica* as I have, and I never heard them mention any difficulty in regard to their attainment of it, and I am sure that I never found any myself. The same has been equally true of the medical students, with whom I have been best acquainted. It has been particularly insisted that medical students can never acquire a knowledge of so many articles, and can not fail of being embarrassed and perplexed by such an extensive catalogue; and yet I have known a large number who had readily and easily made themselves familiar with the powers operations and effects of all the articles that I ever treated-of in my instructions; and if they were botanists, of a considerable additional number of articles. This has been the fact generally with my own private pupils, and not a few have done it, who only attended my public instructions. A stronger and more valid objection than this must therefore be found to an extensive *materia medica*. In fact it requires but a very little labor and pains to acquire a knowledge of a limited and small *materia medica*, and this I suppose is the principal reason why such a *materia medica* has been so popular with so many of the profession.

In favor of the opinion that but few—very few remedies are ever necessary, it is said that an immense host of the greatest names in the medical profession may be readily and speedily arrayed. It is an unquestionable fact that some of the ablest men of the medical profession at the present day, as well as in former times—ablest as respects mental and intellectual endowments, but not by any means in point of practical skill, more particularly the practitioners in the department of surgery, have entertained and practised upon these principles, or the principles here referred-to. “*Hoffmannus eam condat legem*”—“*perpaucis selectis med-*

icamentis ad morbos, et sanaudos, et arcendos, utendum"—"paucæ enim et simplicissimæ sunt morborum causæ, ideoque iis etiam removendis pauca sufficiunt adminicula." (*Præfatio Editoris Parisiani Consultationum Medicarum Hermannii Boerhaavii Parisiis 1750.*) "Ex vero scribit Johannes Langius"—"ego dico desipere eos medicos, qui pluribus medicamentis aliquid expediunt quod paucioribus transigi potest." (*Ibidem.*) "Egregium profecto Damasceni est consilium"—"pharmaca pauca tenenda tibi sunt, et quorum operationes ac potestates multoties expertus es; totius enim multitudinis notitia incomprehensibilis est, et cum per singulas volueris inquirendo discurrere, multiplici diversitate distentus, nescies cui debeas fidem adhibere." (*Ibidem.*) "In hanc rem scribit Wedelius." "Res se habet hæc instar amicorum." "Magis æstimamus unum vel alterum amicum, quam centum notos." "Multi etiam amicitiam offerunt, pauci merentur." "Ita etiam curiosum, selectum et expertum medicamentum, reliquis omnibus præferendum est." "Qui omnes habet amicos, neutrum habet; qui omnia novit, neutrum novit." (*Ibidem.*) A celebrated physician of Boston (Mass.) a native of Scotland, and I believe educated at its most celebrated school, says "A proper regimen and diet, and not exceeding a dozen notedly efficacious medicines, properly applied, is the true effectual materia medica." (*Wm. Dougl. M. D. Hist. Brit. Settlem. in N. A. Bost. 1749, Vol. I. p. 175.*) In a letter from Joseph Celestine Mutis whose name is inseparably connected with the materia medica, to the younger Linnæus, dated "Mines Ybagua 12 September 1778," this gentleman says "with regard to the remedies, of which I make use in my own practice, I would remark by the bye, that I believe the whole of practical medicine lies in a small compass, in which opinion the most eminent men concur." "The more simple and free from a jumble of numerous articles it is, the better, as you and every intelligent practitioner must soon discover, in spite of the preconceived opinions of the vulgar herd of physicians." "The materia medica to which I have recourse in my practice, is therefore of the most simple kind." "The reputation I have acquired among the Americans is such, that I am beset with a crowd of sick people, who flock after me, even in my rural retirements, having learned by experience that it is possible to be well cured of their diseases at a very moderate ex-

pense." "A European could scarcely believe how little these people would spend upon physic, if the Apothecaries' shops were (happily to the advantage of humanity) banished from the country." (*Vide Select Correspondence of Linnæus Lond.* 1821, Vol. II. p. 584.) A writer (unquestionably a medical one) (in the Quart. Journ. Lond. Ed. Vol. IV. p. 8) says of Mr. Abernethy that his "greatest fault was an incurable skepticism respecting the power of drugs." "He condemned and despised, and scornfully treated the whole materia medica." "Apparently it would have given him no deep concern, if all the articles kept at Apothecaries' Hall, had been washed away in a general deluge of pharmacy." "The Blue pill, Plummer's pill, the infusions of Gentian and Senna, and the decoction of Sarsaparilla, were almost the only preparations which he would have admitted into the Ark." "His maxim seemed to be 'one disease, one medicine.'" "A prejudice which so narrowed the labors of prescribing and compounding, was but too welcome to many of Mr. Abernethy's disciples, who applied his alterative treatment" (as he called it) "even to acute cases, and loudly professed, at the outset of their career, that they had no faith in any other method of proceeding, that is to say, no regard for medicines, the various properties of which had never occupied their attention." "Some of them also naturally became too regardless of local remedies, where such remedies would have been useful."

But the whole materia medica has very often been alleged to be very nearly a worthless department of medicine—one of not sufficient importance to justify the bestowment of much if any time and attention upon it, either in the way of private study, or of attendance upon public instruction. Mr. Lawrence says "general anatomy and physiology furnish the principles by which we are guided in our attempts to preserve health, to alleviate and remove disorder, and to cure disease." "From this quarter we must expect the future improvement in our profession; not from the addition of new medicines to a catalogue already too long; not from fresh accessions to that mass of clinical observations which lie unread on the shelves of our medical libraries." (*Law. Lect. on Phys. Zoöl. and Nat. Hist. Man, Salem.* 1828 p. 57.) I have very often indeed heard and read just such statements as this from the anatomists. For myself however, I can not dis-

cover how "general anatomy and physiology" can possibly furnish a single principle of therapeutics; or how a principle or principles can be substituted for remedial agents. I can not understand how the most minute, and the most profound knowledge of anatomy and physiology, or any principles deduced from this knowledge, without a *materia medica*, can ever enable us to cure even a single individual disease. How can anatomy and physiology, or any principle or principles deduced from them, ever enable us to cure Intermittent, or Syphilis, or Scabies, or Diarrhœa, or Cholera, or any other specific disease in the whole nosology? Notwithstanding this broad and unqualified assertion from one reckoned among the great men of the present age, it appears to me that we must still be indebted to clinical observations for our pathology, and to the *materia medica* for our means of curing all diseases, except a few that fall exclusively within the province of surgery, and require nothing but mere mechanical measures and processes. Indeed I can discover no reason why such measures and processes should not be reckoned a part of the *materia medica* as much as a substance that is swallowed into the alimentary canal. But upon this I do not by any means insist, not deeming it of the least importance one way or the other. That much which has been called pathology, and has been alleged to be founded upon clinical observation, has been nothing more than a record of the mere imaginings of the physician, constitutes no valid objection to accurate and correct pathology founded upon true clinical observations. The counterfeit always proves the value of that which is counterfeited. An American writer says "you ought to cultivate with unwearied industry, first of all, and most of all, pathology." "In this department, more than any other, lies the 'great strength' of a physician." "A man so familiar with morbid phenomena as readily to detect the seat, character and stage of a disease, and able with tolerable certainty to anticipate its progress and result, possesses an enviable talent, and is a giant in his profession, whatever may have been his opportunities in early life." Our medical writings are in fact full of such sentiments as the preceding from Lawrence and others. But to use the language of the *Medico-Chiurgical Review*, "the *materia medica* after all furnishes us with the powder, shot and cannon by which we assail the citadels of disease; and without this mate-

riel of war, all our fine-spun rules of gunnery and fortification are of no avail." *Review. A. T. Thom. Elem. Mat. Med. & Therap. in Med. Chirurg. Rev. for Apr. 1833, Numb. 52, Vol. XXII. p. 321.*

It is commonly and constantly said in favor of a small materia medica, that it is better to understand thoroughly even a very few articles, than to be acquainted superficially and imperfectly with a considerable number. In short, from the meagerness and small extent of the catalogue of the materia medica of some, and even many practitioners of medicine, it would seem that they are guided by the old maxim in regard to wealth, viz. "non qui multa possidet, sed qui paucis indiget dives est." It appears to me however to be very certain that no knowledge, be it ever so thorough, can ever render a very few articles of medicine either the most proper, or the most useful for the exclusive treatment of a great variety of cases, and of the most diverse diseases. It appears to me that the educated, graduated, licensed, or as he is so fondly called, the regular practitioner, who habitually treats every variety of case of all species of disease, with a very few articles of medicine, is in no respect better than the uneducated, non-graduated, unlicensed and irregular quack, who does precisely the same thing; and perhaps he is even worse; the thing, as I insist, being precisely the same, whether practised by the educated or uneducated, the graduated, the licensed or the unlicensed, the regular or the irregular practitioner; and in my view no salvo can either excuse or justify it. Indeed, as I have just said, I think it less excusable in the educated than in the uneducated man. As appears to me, no knowledge however perfect, of the powers, operations and effects of Depletion of Blood, Tartrate of Antimonia and Potassa, the antiphlogistic cathartic salts, the non-evacuant antiphlogistics, Disoxyd and Dichlorid of Mercury, Senna and Sarsaparilla can possibly render these articles either proper or useful in nearly all diseases. If to these we should add Ipomœa Purga, Polygala Senega, Urginea Squilla, and half a dozen analogous articles, such a materia medica could never make even a tolerable practitioner; and yet I have known a considerable number of physicians, some of whom had attained to very considerable distinction and eminence, whose materia medica was pretty exactly what I have specified, and not by any

means of a greater extent; and I have very often been informed of numbers of such practitioners of medicine. It appears to me that the circumstances that such a physician has been educated, graduated or licensed, and that he is considered as being in regular standing among his professional brethren, does not in the least either change, or in any way affect his true character, though it may indeed affect his reputation and his business. I have known many men who were much better acquainted with a considerably greater number of far more valuable agents, but who were held in no estimation, in consequence of a deficiency of some of the forms of education now fashionable, or from not having attended public lectures at certain institutions. Some of the most learned and accomplished physicians that I have ever known have had private educations merely. As appears to me, the opinions in relation to a very limited materia medica now under consideration are as unfounded and in fact as false, as any with which indolence—indeed sheer laziness ever attempts to palliate its own deficiencies and consequent misdeeds. I fear however that the opinions which I am combating are but too popular, and forever likely to be so, with the medical student and the young practitioner, of many of whom; we may use the words of a clerical writer *mutatis mutandis*, in regard to theological students and young clergymen. “Short as is the term of our professional study, the youth of our land are disposed practically to make it shorter.” “Under the specious plea” (of poverty and necessity) “they take a short cut in *medicine*, and run before they are sent.” “They find” (there is reason to believe that some never make the discovery) “when it is too late, that they have deceived themselves, and robbed their minds of that knowledge and experience, by which they might have been thoroughly prepared for their professional duties.” (*Amer. Bib. Repos. Jan. 1838, Numb. 29, Vol. II. p. 36, J. Packard on Stud. Classics.*) The prevalence of such opinions, it is quite evident, is much more convenient for the indolent practitioner than useful for the sick. Dr. Good observes to the same effect, that “there are some practitioners who think that all the articles which are of real use in the cure of disease, lie within a small compass, and may be learned without burdening the memory.” “This remark” (Dr. Good continues) “may be allowed to those who are limited to a portable dispen-

sary, as in traveling or on ship-board ; but when uttered under other circumstances, it savors less of wisdom than of indolence." He adds, " we may easily indeed substitute one medicine for another, but it is very rarely that we can hereby obtain an integral representative—a remedy possessing not only the general, but the particular qualities of that, whose place is supplied, so as to be adapted to the exact state of the disease, and the express character of the idiosyncrasy." The justice of Lord Bacon's remark elsewhere quoted, depends intirely upon the age, in which it was made, and the circumstances from which it doubtless originated ; as at that time it was not uncommon to find twenty, fifty and even seventy or more, heterogeneous, and often incompatible articles mingled in one prescription. I once requested a physician of very high character and eminence to suppose that the materia medica was to be reduced to thirty articles, and to give me a list of the thirty, that he would retain. The reply was that he had endeavored in vain to make out such a list. He did not think that medicine at the present day could be practised with so small a number of articles. I also requested an other physician of equally high character and eminence to suppose that the materia medica was to be reduced to a hundred articles, and to give me a list of the hundred, that he would retain. The reply to this was that he had bestowed much thought upon the subject, and had attempted such a catalogue, but had by no means succeeded to his mind, in making-out one, and therefore he did not send one. He said that though he might not use one quarter of this number of medicinal agents in his ordinary daily practice, yet extraordinary cases required extraordinary remedies, so that when he began to cast about and make provision for cases of this character, as he had met with, in the course of his professional career, his list soon overwent a hundred. He added that he could discover no good reason why the materia medica should be restricted within such narrow limits. For his own part he said he should be glad to possess at least half a dozen articles of the same general character as *Papaver somniferum*, half a dozen articles of the same general character as *Cinchona lanceolata*, half a dozen articles of the same general character as *Alcohol officinale* or *Spiritus Vini Gallicus*, with no more difference than exists between very nearly allied articles of a natural group of medicinal agents.

He observed that he always considered it very unfortunate in the *materia medica*, to have only one article of a particular character, as in the case of the articles above named. Though there is frequently great similarity and affinity between many of the vast multitude of diseases, to which the human race is subject, yet it is confidently believed that from difference in climate, season and period—sex, age and temperament—diathesis, type and duration—idiosyncrasy, habit and even prejudices—as well as proëgumenal and procatactic* causes, together with various other accidental circumstances, no two instances, even of the same disease, and much less of diseases commonly considered to be specifically distinct, are ever to be considered as precisely alike both in quality and degree. In fact, the regimen and medication of the early stages of every case, if at all efficient, must always essentially modify and cause peculiar variations in the condition and symptoms of the secondary stages.

In cases which pediluvium and slight rubefacients to the extremities, conjoined with weak diaphoretic infusions, are capable of curing, it would be worse than useless to employ active and energetic remedies; and on the contrary, when the most powerful agents in our possession are scarcely sufficient to resist the violence and arrest the progress of a severe and dangerous malady, surely it would be worse than culpable—it would be actually criminal, to confide in weak and comparatively inefficient articles. Many medicines, like diseases, agree in their general operation; but yet, every individual agent doubtless has its peculiarities; and if it is important to discriminate shades of disease, it must be equally so, to adapt the remedy with the same accuracy to the condition of the case. It is not supposed that cases of acute disease often require at one time, any considerable variety of medicines; but those which are employed should be selected with judgment, and be the most appropriate to the existing circumstances. In chronic diseases however, their long continuance, their frequent obstinacy, the great changes which they sometimes undergo during their progress, and the well known tendency of the human system to become in a short time insusceptible of

*I use the terms proëgumenal and procatactic in a strictly technical, precise and restricted or limited sense, and by no means in the vague and unlimited acceptations in which predisposing and exciting are always employed at the present day.

impressions from a long employed agent, while it continues to be capable of being acted-upon, by an other article, perhaps even of inferior power, but with some shades of variation in quality, often render a much greater variety, and a much longer succession of remedies absolutely necessary. Now such a variety and such a succession can never be supplied from a small and limited catalogue of articles.

But even on the ground of substitution, an extensive catalogue may be not only useful, but necessary. The pecuniary circumstances of a patient, the situation of a physician remote from an adequate supply of such articles as he might perhaps prefer, or even some sudden and special emergency, may render judicious substitution absolutely indispensable. I have often heard it said that the very best medicines in the abstract for the case, without reference to price, should always be prescribed even for the most indigent patient, on the ground that it will be the cheapest on the whole; and that if it should not be, health and life are not to be risked for the difference in the cost of different medicines. All that I have to say to this is that I have often had patients that would dispense with medicines in preference to obtaining those which were expensive, even though they might not be absolutely unable to pay for them; and that I have still oftener had patients that were wholly unable to pay for expensive medicines however necessary they might be, for the restoration of their health. I believe that it is the usage in hospitals, dispensaries, alms-houses, and in all charitable institutions, to prescribe the cheaper article in preference to that which is more expensive, provided it can by any means be made to answer the purpose, even in a far inferior manner, and after a considerably longer time. I have more than once had knowledge of a physician's receiving heavy censures from the non-medical overseers of such institutions as I have mentioned above, for prescribing Disulphate of Oxyd of Quinum instead of crude bark of Cinchona, and even cheaper articles, and that even to patients who paid a moderate but fair compensation for their attendance, and under circumstances in which the physician thought that nothing else but what he directed would by any means have answered the desired purpose. I once happened to be in a region where Intermittent was endemic, without any Cinchona or Quinine in my possession, or any salts of the

Oxyds of Cinchoninum or Quininum, and without being able to obtain any of these articles within about a thousand miles. I had an abundance of the best crude bark of Cinchona, that has been obtainable since the introduction of its active principles into common use; but the disease was too intense to yield to any quantity of this, that I could induce any patient to swallow. I likewise had Arsenous Acid, but the disease would no more yield to this agent, than it would to crude bark of Cinchona. Neither would it yield to these two in conjunction. There was an abundance of *Cornus florida* in the vicinity, but I had previously ascertained its utter impotence in any thing like intense Intermittent. Here true and adequate equivalents of Cinchonine and Quinine, and of the Salts of the Oxyds of Cinchoninum and Quininum, were much needed, but not to be had. But the subject of substitutes for all the important agents of the *materia medica* is rendered very important by certain considerations resulting from the character of the medical profession, as it is now constituted in our country, and as it must continue to be constituted so long as our political institutions remain what they are at present; for (what may at first view seem very strange and incredible to some) our political institutions have a very great influence upon the practice of medicine and upon the success of candidates for this practice, in obtaining and retaining an establishment. There are very many instances in which it is undoubtedly best to omit the administration of the most eligible remedies for a given case of dangerous disease, and in their stead to employ articles of inferior efficacy, and less perfect adaptation to the exigencies of the malady. Where the true powers and operations of a remedy under its several modes of administration are not at all understood, but in fact wholly misapprehended by the non-medical public, and particularly by the relations, friends and immediate attendants of the patient, and where at the same time, there are the most fixed and inveterate prejudices and the most erroneous opinions in regard to its effects, such as will cause all, or nearly all of the inconvenient, distressing and dangerous symptoms that belong exclusively to the disease, and even the death of the patient, when this happens, to be ascribed to the medicine, thus bringing not only the medicine, but the physician prescribing it, into wholly unmerited disrepute, and therefore greatly lessening, and often even destroy-

ing the usefulness of both. I do not think that any physician is under any obligation to employ such medicine. On the contrary it is not only the part of wisdom, but of duty also, to omit altogether the employment of such agent in his whole practice, till the prevalent popular errors are sufficiently obviated to admit of the articles' being used without the consequences heretofore mentioned. It may be said perhaps, that it is the duty of the physician to administer such articles in a concealed form. This however can rarely if ever be done, in the case of medicines in regard to which highly erroneous notions, and very strong prejudices prevail. In the first place, most people, under such circumstances, will become acquainted with, and easily recognize both the external sensible properties and the ordinary operative effects of all agents respecting which there are the popular feelings, that I have specified. Beside this, the physician will always be directly questioned as to whether he is giving the odious and offensive agent, and he will be obliged to utter a deliberate falsehood, with the certainty of being detected sooner or later, if he gives sufficient to produce any useful effect. It may also be said that a physician ought to give-up a patient, that he is not allowed to treat in the manner which he judges best. If this should be done however, no practitioner of medicine could retain any business, if he has dishonorable competitors, since such competitors would infallibly propagate false notions in regard to the powers, operations and effects of all the most important medicines, and kindle prejudices against their use, for the express purpose of depriving a man (who acted conscientiously and upon such principles) of business, and of getting rid of him. Neither of these methods of proceeding can therefore be acted-upon. When first called to prescribe for an individual case, it has not been uncommon, even before I had made any investigation as respects the signs and symptoms, or the nature and character of the disease, for the patient or some of the bystanding relations and friends to attempt to stipulate, as the condition of my attendance, that I should prescribe no Dichlorid of Mercury, nor any other Mercurial; that I should direct no Papaver in any shape or form; that I should employ no Disulphate of Oxyd of Quinum, and no intoxicating liquor, and more especially no Alcohol and Wine. By different persons other different articles have been objected-to,

and very often all Poisons have been specified. Now, if one physician will not engage to humor all these caprices, there are enough others to be found who will; and no one does well to enter the medical profession who has not made-up his mind to comply with all this. I know very many physicians who, since the years 1830 and 1840 have scarcely, if at all, prescribed the Dichlorid of Mercury. This utter discontinuance of its use has resulted from the very strong popular prejudices against it, which have been produced by the incessant declamations against it of Beachites, Thompsonians, Homeopathists, Hydropathists, etc. (commonly so called) and the consequent nearly invariable custom of almost every one not belonging to the medical profession, and of persons erroneously considered by the public universally as belonging to it, of ascribing every undesirable, inconvenient and dangerous symptom of every disease intirely to Mercurials, and mainly to Dichlorid of Mercury. In such a state of things, it is neither expedient, proper nor right that a physician should destroy his own reputation, and consequently his usefulness in all respects, by persisting in the employment of an agent, in regard to which there are such apprehensions, and such prejudices, even when they are wholly unfounded, which is not altogether the fact, as Dichlorid of Mercury is actually employed by many practitioners of medicine who style themselves regular physicians.

In very many cases where I considered Papaver as very strongly indicated, and very much needed, I have been constrained to omit its use, because there was so much fear or dread of it that every unpleasant and every undesirable symptom of the disease would inevitably be ascribed to it, if it were used at all, and even death itself, provided this should happen. All this will be the fact, even where the patient, and his relations do not undertake to make any stipulations beforehand; where they deny that they have any objections to, or prejudices against any single article of the whole materia medica; and where they profess to wish that you should prescribe according to your own best judgment. I have intirely lost many a good employer, in consequence of curing an almost desperate disease in some individual of the family, mainly by means of a remedial agent, in regard to which apprehensions were entertained, though intirely disavowed and denied. I have lost the business of more than one family in consequence of arrest-

ing attacks of what is commonly called Puerperal Delirium (after the cases had been pronounced incurable and inevitably fatal, by an other physician) on the ground that I accomplished this by what was deemed too free a use of Papaver, though only sufficient to produce quiet sleep for about three hours. In two instances, I have lost the business of a family, because an Apothecary informed the male head of it that I had used Camphor at the rate of two drachms in the twenty-four hours, in the case of his wife, while affected by a severe, urgent and dangerous disease, at the beginning of which it was stipulated that I should administer no Papaver. I have lost employers in consequence of having been discovered to have used Datura Tatula. I have lost employers in consequence of having been discovered to have used Veratrum viride. I have repeatedly lost employers in consequence of having been discovered to have used Strychnos Nux-vomica. I have lost employers in consequence of their discovering that I had used Tincture of Cantharis internally, beside being much slandered and abused in addition. I have lost employers in consequence of having been discovered to have used Acidum Arsenosum, and have known all this happen to other physicians as well as to myself.

Such a state of things has been produced by the fact that very many physicians, who are rival candidates for professional employment, are in the habit of treating competitors exactly as the political parties in our country treat the candidates for office of their opponents, i. e. slander and traduce them almost without any limit. The measures that are used without scruple in politics, and that are attended with a greater or less degree of success, are employed without hesitation in small and merely personal matters; and this is the way in which our political institutions affect even the practice of medicine. I have known a medical professor of a distinguished institution to call upon a family that were perfect strangers to him, one of whose number lay dead in the house at the time, to inform them that the deceased was undoubtedly killed by an other physician who had made the patient two visits, at one of which a bare laxative dose of Tincture of Aloë and Myrrh was prescribed, and at the other eight Dover's powders of five grains each by actual weight were left, one of which was directed to be taken every three hours. The particu-

lar affection for which this prescription was made was Catarrhus communis. The patient however had an inveterate disease of the heart. He had no attendance during the night but took three or four of his powders at the prescribed times. In the morning he fell suddenly into what is called a dieing state, and actually expired in a very few minutes. This case is mentioned only as a sample of hundreds probably, which have given rise to the state of things specified, and which must inevitably occasion such a state. If the medicine taken had been any thing else, no matter what, the same charge would have been made, and had actually been made of a great number of the most useful, and the most valuable agents of the materia medica, begetting a most inveterate prejudice against them all, so that if employed at all, the fact must be carefully concealed, if it should be by any means practicable, or the physician prescribing them will be abandoned. Now in such a state of things, what course ought a practitioner of medicine to take? In brief, I would on the whole advise all physicians, who intend to live and prosper by the medical profession, never to employ medicines in any case, as to which either the patient or his attending friends and relations have very strong fears or very strong prejudices. Under such circumstances, it is always best to select agents for the treatment of patients against which they have no such objections. Such a state of things very clearly evinces the importance of an extensive materia medica. If our catalogue of articles could be as extensive as is to be desired, and if we had perfect equivalents for all the most important articles, the difficulty here described would thereby be very materially diminished. The most successful physician that I ever knew—the most successful in the only way by which professional merit is now estimated, viz. by his attainment of great popularity, made himself thus successful by never prescribing any thing that the patient or his attendant relations and friends were afraid-of, or had any prejudices against; and by always prescribing a greater or less quantity of any thing that the patient was desirous of taking, or his attending relations and friends were desirous that he should take.

A catalogue founded upon the principles here contended-for, would of course exclude many mild and only moderately efficient foreign articles, which are difficult to be obtained and preserved,

as well as all those both indigenous and foreign whose powers are doubtful or uncertain. However, medicines of moderate powers are undoubtedly too much neglected at the present day. I would not by any means inculcate that such agents are ever to be exclusively relied upon, in severe and dangerous diseases, but only that as a general rule they are the most appropriate in moderate cases. It is true many active and powerful agents may be rendered moderate and mild by a suitable diminution of their dose; but this is not by any means always the fact. There are cases assuredly, in which the active narcotics for example will fail of producing salutary effects, but in which even the mild euphrenics will accomplish all that is required. In like manner the mere eccoprotics or the laxatives often render service, when the purgatives, and much more the drastics, even in greatly reduced doses will inevitably do injury. But it is supposed by some that a widely extended catalogue of medicines may, by its multiplicity, perplex the student and burden the young practitioner, and thus in part hinder the necessary discrimination in regard to such a number of articles. However, after a suitable education and other due qualifications for practice, there can be no sort of danger of this. To assist the mere student in acquiring and retaining a knowledge of such a catalogue of articles, and to aid his discrimination in the application of them, I would recommend that he as well as the young practitioner should in his own mind distribute them into two great classes, the one to consist of efficient or active remedies, upon which reliance may be placed in the severest diseases; the other to comprise such as possess inferior powers, suited only to the treatment of mild cases, or useful as auxiliaries merely to the former class, in the more violent complaints. It ought to be no objection to an extensive catalogue of remedies that particular practitioners have been more or less successful, as much so as their professional neighbors, and whether successful or not, have attained to high reputations, who have employed only a small number of articles, or even that the majority of the physicians throughout the country employ only a few. Individuals (perhaps often from accidental causes) some times become more minutely acquainted with the powers of certain articles, than the great body of their brethren, and from frequent employment of them, acquire a skill and dexterity in their management, under which

they may in fact be preferable to medicines in more general use and in fact intrinsically more appropriate, but which are managed with less of the skill and dexterity above referred-to, and therefore are inferior in their effects.

Galen says "*medicus omnium stirpium (si fieri potest) peritiam habeat, sin minus plurium saltem quibus frequenter utuntur,*" i. e. the physician should be thoroughly acquainted with the whole *materia medica* (if it is possible) but if not, he should at least be well acquainted with every thing in general use. It is said by Oribasius that "*simplicium medicamentorum, et facultatum quæ in eis insunt cognitio ita necessaria est, ut sine ea nemo rite medicari queat;*" i. e. the knowledge of (all) simple medicines and of their powers is so necessary that without it no one can properly cure diseases. It must be obvious that no mechanic can be expected to work well without good implements and a sufficient number of them; and on a little consideration, it will be equally obvious that no physician can cure disease without good remedies and an abundant supply of them. The *materia medica* unquestionably stands in very much the same relation to the physician as tools to the mechanic—arms to the warrior—and machinery to the manufacturer. The greatest theoretical attainments in other respects, the minutest knowledge of physiology and pathology, and the most accurate judgment, without a knowledge of the instruments, constitute the mere amateur; it is skill and dexterity in the use and application of means, in addition to the other qualifications, that forms the valuable practical man. Nothing so strikingly exhibits the preëminence of civilized society, as the numerous facilities which it affords for furnishing the comforts, conveniences and necessities of life. Since the middle of the eighteenth century a new and surprising impulse has been given to the efforts for furnishing the means of the improvement of almost every department, which regards the happiness or prosperity of man. The profession of medicine has perhaps had its share of the spirit, which distinguishes the present age; and in many of its branches, has kept pace with the rest of the world. The present state of chemistry, botany, anatomy, physiology and surgery, is an ample demonstration of this fact. There are some other branches however, in which it is very doubtful whether there has been a proportional advance in the scale toward perfection,

though they have unquestionably been at least some what improved. Among these the materia medica is very obviously to be ranked. A few new articles comparatively have been occasionally discovered, and pharmaceutic chemistry has added a few useful preparations, but not many new properties of medicines have been ascertained and investigated, or new principles and rules for their application have been established, at least since the days of Cullen and his immediate successors. This state of things, if rightly considered, instead of being a hindrance, really offers the most animating encouragement for the pursuit of materia medica. Many other fields of science have been so fully explored that comparatively only a few unproductive gleanings are to be anticipated; but here, though the main roads have long been opened, new paths may be cleared and traversed in every direction. Besides, an important part of the surface has only been glanced at, but never accurately surveyed. Indigenous materia medica is comparatively a new region. Most of it has been traversed only in a very casual and superficial manner. It is to this that our attention ought to be particularly turned.

So far as the cure of disease is concerned, all the other departments of medicine would be absolutely valueless without the materia medica. The most minute knowledge of anatomy and physiology, the most perfect acquaintance with all that is known of pathology, with all the subordinate topics which these departments comprise (to say nothing of chemistry, mineralogy, botany and zoölogy, commonly reckoned as auxiliary branches of medicine) will never enable us to cure a single disease without materia medica and pharmacy. However different it may be from the ordinary estimate, materia medica is the crown of the whole, without which, all the rest would be comparatively valueless. To all this, perhaps some one may adduce surgery as constituting an exception; but it is by no means such. Mechanical measures and processes belong as much to the materia medica as fortuitous lesions and deformities belong to pathology and nosology.

In my subsequent continuation of this work, it will be my endeavor to bestow particular attention upon indigenous articles of medicine, at least where they possess any thing like first rate activity, and where their powers have been ascertained with any considerable accuracy. At the same time I shall endeavor not to

bestow an undue degree of attention even upon these. As is commonly believed, there are but about four articles of the *materia medica* from the vegetable kingdom, for which there are not either good, or at least tolerable substitutes growing in North America. The most important of the articles for which there is no American substitute is the exuded and inspissated descending sap of *Papaver somniferum*, commonly called Turkey Opium. It has long been supposed that *Lactucarium* possesses powers and produces effects very similar to Opium, only on the whole rather preferable. I commenced the practice of medicine with this notion derived from books, and from practitioners of various ages. Being altogether disappointed in my own practice, as respects the expected narcotic operation of this substance, I have been induced to experiment with it, at least at half a dozen different times, in the course of the forty-two or forty-three years, that I have been engaged in the practice of medicine. In these experiments I have commonly employed an imported article reputed to be, and having every appearance of being of the very best quality—an article constantly employed by some of my professional neighbors, apparently with great satisfaction, in doses of half a grain—a grain, and for aught I know, two grains in some cases. I have caused this article to be taken in doses of a drachm—two drachms—three drachms—and even four drachms, with no more narcotic effect than was produced by as many grains, with the exception only, that the quantity of two hundred and forty grains i. e. half an ounce Troy weight, commonly produced more or less nausea, but never to the extent to occasion retching, and much less vomiting. This effect was always inconsiderable and transient. As I failed of producing any narcotic effect from the imported article, I repeatedly prepared a *Lactucarium* from the American *Lactuca elongata*, and from the cultivated *Lactuca sativa*, by collecting and inspissating the descending milky sap in the full flowering season; but I could perceive no difference between the article prepared by myself and that which was imported. The imported article, which I have had, was reputed to have been brought some times from England, when it was supposed to be the product of *Lactuca virosa*, and some times from Continental Europe, when it was supposed (on what authority I know not) to be the product of *Lactuca Scariola*. But all the exuded inspiss-

sated juice wherever obtained has always been of much the same character. I have likewise tried the inspissated expressed juice, or at least what was understood to be such, and also the decoction and the infusion. Some times as many as half a dozen young men have taken the article at the same time, and some times not more than one, beside myself. The last experiments that I made were during the summer of 1851 upon a single pupil in the latter part of his professional studies, and upon myself. As near as I can recollect (for I have not at present access to my notes) it was pushed only to the dose of two drachms, though in previous experiments it had been extended to four drachms, and in each case without any thing even like a narcotic operation. From such facts, it will readily be inferred that I do not consider Lactucarium of any value as a substitute for Opium. Without the least doubt the Poppy might be successfully and profitably cultivated in either or all of the great sections of the U. S. A. and a better Opium might be prepared from it than we now obtain; but such small work (as it is termed in this country) as collecting the sap from a plant like the Poppy is universally distasteful and irksome to all U. S. A. laborers, so much so that they will never be inclined to engage in it, so long as coarser and rougher employments, which require less attention, care and nicety of manipulation can be obtained. Could some means be devised for obtaining Morphine, Codeïne, and whatever other active principles there may be in Opium, from the crude Poppy-plant, our laborers might be induced to go into the culture of the article; but I suspect not without. Several years ago I made a number of experiments upon a small scale to accomplish this purpose, but intirely without success. From such an utter failure in all my results I was much inclined to suspect that the active principles of Opium do not exist in their perfect state in the crude Poppy plant but only as compound-radicals of H. C. N. which are alcalized by combination with Oxygen, during the inspissation of the exuded sap, in analogy with the oxydation and alcalization of the compound-radical of H. C. N. in the leaves of *Nicotiana Tabacum*, by a process to which they are submitted long after being dried. This is a subject worthy of thorough and speedy investigation since the products of the Poppy constitute undoubtedly the most important agents of the *materia medica*.

The second article in importance, for which we possess no North

American substitute, is *Cinchona*, of which I speak as a unity, since all the species in actual use seem to depend on Cinchonine and Quinine, or rather upon the salts of Cinchoninum and Quininum, for all their powers and all their therapeutic operations and effects. *Cornus florida* and some other species of the same Genus growing in the U. S. A. have been much vaunted by many as North American equivalents of, and substitutes for *Cinchona*, but I think without just grounds. I have made a number of trials of the crude bark of *Cornus florida* in Intermittent, but without the least success. So far as my observations go, it is no better in this disease than the bark of *Quercus alba*; and if it is good for nothing in Intermittent, it is probably worthless in Gangrene. But negative testimony should never be admitted on any thing like light grounds, so that I hope many more thorough trials will be made with *Cornus*, before its true value in Intermittent and Gangrene is considered as settled. The trials which I have made with *Cornus* seem to me to have been sufficient to establish an affirmative, but by no means a negative. I do not know of any thing else that has ever been supposed, on any good ground, to be an equivalent of, and substitute for *Cinchona*, which grows within the limits of the U. S. A. except *Pinckneya pubens*; and so far as I know, this has been no more perfectly investigated than *Cornus*.

The third article in point of importance, for which we possess no North American substitute, is *Cephaëlis Ipecacuanha*. This agent is of vastly less importance in the *materia medica* than either of the two preceding articles, since they could not possibly be dispensed with, while this might be, without any extreme inconvenience. Nevertheless I think it is altogether the best agent of its kind that we possess. *Gillenia trifoliata* and *Gillenia stipulacea* are commonly mentioned as the best North American substitutes for it, but I have never seen satisfactory evidence that such is the fact. But these two articles are so scarce, and so difficult to be obtained, that it is next to impossible to get enough of them for a fair trial. Such a circumstance as this would seem to be an insuperable obstacle to their being substituted for it. In a number of instances, I have seen a recent very fine powder of the best root of *Sanguinaria vernalis* operate so nearly like *Cephaëlis Ipecacuanha*, that the effects of the two were not to be distinguished; and I have received much testimony to the same effect. The emetic

operation of the infusion and tincture of *Sanguinaria vernalis* is certainly very materially different. Besides, the root of *Sanguinaria* is a very troublesome article to reduce to a sufficiently fine powder for an emetic, and it does not keep well when it is so pulverized. I have seen a recent very fine powder of the leaves of *Verbena hastata*, and of *Verbena Urticifolia*, collected at the right time and properly dried, operate very much like *Cephaelis Ipecacuanha*; and others have had the same experience with these two articles. I think that the operation of an infusion of these two species of *Verbena* is materially different. Several other articles may probably be mentioned, that would answer about as well as these *Verbenæ*; so that *Cephaelis Ipecacuanha* might be dispensed with without any great inconvenience.

The fourth article in point of importance, for which we possess no North American substitute, is *Rheüm (officinale)* and I know of nothing that is at all like it; but happily it is an agent of no great importance, very good in its place, but far from being indispensable. Indigenous articles possess a great superiority over foreign ones, in the circumstances that there is a possibility dependent upon ourselves of collecting them at exactly the right season; of rejecting dead and partially decayed specimens, which oft times can be done only while the article is recent; of properly cleansing, drying and keeping them; and of renewing them every year, when they do not perfectly retain their powers for a longer time. I say there is a possibility dependent upon ourselves of doing all this, though I have never been able to have it done except by myself and a very few professional friends. I never saw in any Apothecary's shop any *Aristolochia Serpentaria*, that even approximated to its best state. I once exhibited a small quantity of this article of my own collection and preservation to a professional friend, who was exceedingly partial to this medicine, and who prescribed it very often; and so superior was my specimen, that he did not recognize it either by sight, smell or taste; and when I named it, he would hardly believe that it could be his favorite medicine. I never saw any thing like first rate *Botrophis Actæoïdes* in more than one Apothecary's shop. Indeed so inferior is this article as found in the shops, that as a general rule, nothing can be accomplished with it as a medicine; so that it has no character with almost all physicians who have prescribed it

from the shops only. I never saw any first rate *Sanguinaria ver-nalis* in more than about three shops. In fact so inferior is this article in the shops, that I believe it has no character with very much the largest portion of our physicians; certainly nineteen twentieths within my acquaintance, and perhaps ninety-nine hundredths in the country at large. *Spigelia Marilandica*, as found in the shops, can scarcely be recognized by comparison with that of one's own collection and preservation. In short scarcely any even indigenous article, that is capable of deterioration, is ever found in our shops in any thing like the good state in which a scientific and careful physician may easily collect and preserve it for himself. In all probability there is always a greater or less degree of the same sort of deterioration of a large portion of the foreign articles kept in our shops. This it is impossible for us to prevent or remedy; but it is not so with indigenous articles. These we can either collect for ourselves, or some times cause them to be collected and preserved exactly according to our directions. These and various other facts and circumstances furnish strong grounds of preference for indigenous articles in comparison with foreign, whenever they possess a due degree of activity.

It may be proper to state in this place that, as appears to me, a degree of knowledge of an individual agent, which would not entitle a foreign article to our notice, may render an indigenous one worthy of a place in this work. Even when my knowledge of an article of our indigenous *materia medica* is quite incomplete, I think it is important to record all that happens to be known. A connected summary of detached and traditional facts in regard to indigenous articles, whose powers, operations and effects have never been systematically or thoroughly investigated, can not but be more or less useful. One individual physician may be acquainted with only a single fact in regard to an indigenous article, which fact perfectly alone or by itself may be of but little importance; and yet all the various insulated facts that may be known by many different individuals of the medical profession, if recorded, brought together and arranged, may in the aggregate amount to quite a full account of the article. By recording single insulated facts that I have met-with, in the course of my reading, or derived from several different friends, I have often within the period of two or three years, collected quite a full account of a

considerable number of indigenous articles, which account I have then verified by trials upon a healthy subject, and still subsequently have further confirmed my results by a very advantageous employment of the agent in the treatment of disease. In short I am of the opinion that what Dr. Hancock, a British physician who has distinguished himself as a successful investigator of the materia medica, says of the indigenous plants of Great Britain, may be said with equal truth of the indigenous plants of the U. S. A. viz. "it is well known that there is a great number of exceedingly useful remedies amongst the indigenous vegetables of England; but in general, these are too much neglected by the members of the" (medical) "faculty, who however eminent in other respects, for exalted talents and profound medical skill, seem on the whole to evince rather too exclusive a preference to" (foreign articles and to) "the chymical-mineral remedies at present in vogue." (*Vide Journ. Phil. Coll. Pharm. Vol. II. p. 45, Hancock on Sarsaparilla, etc.*) As appears to me, these remarks are even more justly applicable to the physicians of the U. S. A. than to those of Great Britain.

The great advantage of investigating the properties of new medicines more particularly if they are indigenous, is in reality too obvious to require any argument in its favor. By such researches, we are often led to the discovery of articles that possess well known and highly valuable medicinal qualities in a more concentrated form, or in a new and different state of combination, by which they are rendered more manageable, and more useful. Such researches often render us less dependent upon foreign countries, and as I have already said, afford us cheap and recent substitutes for expensive and perishable articles; and by the extension of the materia medica, they furnish us a greater choice of agents for the management of long continued and obstinate modifications of disease. If we look back to the materia medica that was in use two, three or four centuries ago, we shall find that it was quite different from, and far inferior to that in present use. Now by what means has this great change and this prominent improvement been effected? I think very obviously by means of a continued investigation and substitution of new and better articles for old and decidedly inferior ones. I can not discover why the arguments used against any further extension of the materia

medica might not have been employed with equal propriety and force in the time of Dioscorides, or even of Hippocrates, and then we should have retained the *materia medica* of these fathers of medicine quite unchanged, which would have saved a vast amount of labor, research, discussion and controversy. A writer (say some physicians) should take the *materia medica* just as he finds it. He should not controvert the utility of any thing, since a work on *materia medica* should not be controversial; neither should he add any new articles to it, since he should display the *materia medica* just as it is received, and in established use, by the medical profession in general. Upon this plan we should be still copying from Dioscorides, or even Hippocrates, to this very day. For my part I say, away with all such notions as these. Let us examine, discuss, controvert and reject less valuable and inferior articles, and substitute newer and better agents. I would overturn and overturn till all worthless articles are rejected, and as large a catalogue of the very best articles is substituted in their places, as the most extensive knowledge of the times will permit, no matter how great changes we make in arriving at this. If it were not for wholesome and salutary changes, all things would become stagnant and corrupt in medicine, as in many other things. Provided changes are sound and on good foundation, I care not how many are made, nor how often they are made; the more there are the better. The sooner any error is corrected, or any improvement made the better it is. There can never be any improper time to do either of these things.

At first view, it seems not a little singular that it should be easier to obtain medicinal agents from India, the Eastern part of Africa, the Great South Cape of Africa, etc. than from our own Western, Southern, North-Western, South-Western and even Middle States I mean for New Englanders; and yet such appears to me to be the fact. I suppose that this must be mainly due to the circumstance that there is an established commerce in medicinal agents between Great Britain and these several parts of the Eastern Hemisphere which I have just mentioned, and no regular commerce in such articles between the several sections of the U. S. A. An other circumstance doubtless has much influence upon this subject, viz. that there are many more naturalists to be found on the Eastern Continent to identify the articles desired and em-

ployed than in the U. S. A. but whatever may be the cause, I have always found it peculiarly difficult to obtain the medicinal articles of the several sections of our country. In fact there are many valuable and sufficiently common United States medicines that I have never been able to obtain, during my whole professional life. The less common articles, such as grow only within very circumscribed limits, such for example as *Pinckneya pubens*, the southern species of *Actæa*, etc. I have never even seen. In truth, the easiest way to obtain the greater part of our own indigenous medicines is to import them from England. This ought not to be the fact, though it is actually such. I shall only add, in this connexion, that I have never been able to obtain any of the numerous valuable medicinal articles that so much abound in Spanish, British, Dutch, French and Portuguese Guiana—that great island formed by the connexion of one of the tributaries of the Amazon (Rio Negro) with the Orinoco by means of the *Casiquiare*—any more than if the country were upon an other planet.

Whenever a remedy is recommended in any considerable number of diseases, most physicians immediately conclude that the article is much overrated, and that the principle part of the powers ascribed to it are merely imaginary. This view is supposed to be sufficiently confirmed, if disappointment follows a few trials made either in a different sort of cases, or after a different preparatory course, or with different auxiliaries, or with different preparations of the medicine, or at least under different general management as respects doses, periods of repetition, accompanying regimen, or perhaps the whole of these circumstances in conjunction. In this manner, for example, much of the valuable experience of Stœrck, with respect to certain narcotics, has been rejected, and that even by persons who have never read his works. The same is equally true with respect to the experience of Fowler in reference to Arsenic and Tobacco; of Withering in reference to *Digitalis purpurea*; of Percival in reference to *Cocculus palmatus*; and some time ago, an attempt was made in the same way, to prove the complete medicinal inertness of *Spermœdia* (*Acinula* or *Sclerotium*) *Clavus*, in opposition not only to the experience of Dr. Stearns, but also to the united experience probably of a majority of all the physicians of

the United States. Others are ever ready, from mere speculative prejudices, to reject a remedy recommended in the manner above specified, and requiring discrimination, vigilance and care, for its successful employment. To such nothing need be said, as plain matter of fact is too tame a thing to make any impression upon their minds; but to those who, from a small number of trials, and that under unskilful management, perhaps for want of suitable directions, have been disappointed in the effects of a new article, a serious attention to the following sensible observations of an author, is earnestly recommended. "How few medicines preserve the good character which is given of them by their first promulgators?" "It is not that the promulgator has any intention of deceiving the public, nor that he has even deceived himself." "It is that he has studied the properties of the medicine so closely, as to have acquired a knowledge of its action, which he in vain attempts to teach others." "We say in vain, because they cannot learn it, unless they study it as intensely as he himself has done." "There are few medical men, who do not possess some remedies, with which they can produce effects that differ from the" (hasty and imperfect) "experience of their neighbors." Ferriar has justly remarked that "the management of a remedy which requires care and delicacy is not to be immediately acquired;" and he subjoins that this "does not imply any assumption of superior" (talent and) "skill, but merely of" (superior) "attention" to the subject, and therefore of superior knowledge, and therefore of superior dexterity in the management of the article, and of adapting it to the exact pathological conditions.

It should be particularly remembered that a considerable number of diseases, which seem at first view to be materially different in many important respects, and which in fact are really so, have never the less some thing in common, as regards their pathology; and this something may even constitute the most essential part of these diseases; so that an individual medicine, which exerts only one power, and which therefore produces only one effect, may be of more or less service in the whole; the particular variations of each individual disease from the rest, requiring only variations in the auxiliary remedies, the mode of managing the principal article, and the accompanying regimen. A writer on the *materia medica* says (and I fully adopt his sentiments)

“from the misunderstanding of a proposed remedy displayed by some; for the perverted views of it taken by others; or for the ignorant use and abuse of it by a still larger class, I can not consider myself accountable.” “Want of management in the mode of administering it, want of perseverance in its use, and want of care in its selection” (and preparation) “must interrupt its advancement.” (*Vide Chas. Turner Cooke on White Mustard Seed, N. Y. Edit. 1827, p. 5-6.*)

There are other sources of disappointment in regard to the character which is given to new medicines by their first promulgators. As most persons manage the collection, preservation and preparation of vegetable medicines, if they are at all delicate articles, they are exceedingly liable to be rendered worthless or very nearly so. I will mention two cases by way of illustration. The root of *Botrophis Actæoïdes*, if not collected at the proper period of its growth, and at the right season of the year exactly, is extremely liable to be very nearly worthless. If this root is washed, not speedily dried, and not kept subsequently secluded from the influence of light and a free circulation of atmospheric air, it is exceedingly liable to be spoiled. If the Alcoholic menstruum in which it is prepared is not strong enough, and if a sufficient quantity of the root in proportion to the menstruum is not employed, the preparation is very liable to be nearly valueless. Some thing has been known of this article time immemorial. In the summer of 1810 (the last year of my professional pupillage) my especial attention was first turned to it; but I did not arrive at that knowledge of it, which I suppose myself to possess at present, till 1815 or 1816, though I was more or less engaged in the investigation of its qualities, the whole intervening time. Since that period I have learned very little, if any thing additional, in regard to it. Now from that time to the present, I have never seen a first rate pharmaceutic preparation of it, unless I made it myself, or unless it was made by somebody who had had the results of my researches, and who made it as I make it. In the whole of this time, I have never seen a pharmaceutic preparation not made as I make it, that would not disappoint just expectations as derived from my own experience with it. In the whole of this time, I have never met with a single physician, who has not been disappointed in its effects, unless he has had a preparation made by me, or made after the manner that I employ.

I consider it quite certain that what some would doubtless consider small circumstances will assuredly spoil this article. Let no one understand me as denying that others may have had a good preparation of this article; but only as saying that I have never met with one, unless made by myself, or made as I make it. After the Tincture of Botrophis Actæoïdes began to be kept in the shops of Hartford (Ct.) a medical gentleman, then of that vicinity, obtained several different preparations of it, and employed it in his practice in the doses and quantities which I had recommended, but with complete disappointment. He therefore abandoned it for a time, but at last determined to try it in much larger doses and quantities. When the dose amounted to half a fluidounce, he found it produced all the effects that I had specified; but such a dose was too inconvenient for most patients, so that he did not employ it often. Being himself subject to violent paroxysms of Rheumatalgia acuta after certain exposures, which he thought he could not well avoid, he tried this medicine upon himself, and found that one dose of half a fluidounce uniformly arrested and cured the paroxysms above mentioned, and therefore it became a standard medicine with him for this disease. At last happening to obtain a preparation made by me, he took one of his customary doses just at evening. The night following he had what were considered such alarming symptoms that a physician staid with him about twelve hours, it being feared that he might not survive the night. In the morning however, very much to his surprise, all his morbid symptoms rapidly disappeared, and he recovered speedily from them. No suspicion was entertained that Botrophis had any instrumentality in their production, till several weeks afterwards, when an other such dose was taken with exactly the same consequences, which were then recognized as obviously the effect of the medicine. I have had personal knowledge of one such case, and have had others reported to me. Thus it appears that when of the best quality, this agent is very far from being destitute of activity, notwithstanding a bad preparation so often disappoints expectation.

Sanguinaria vernalis is exceedingly liable to be either worthless, or of little comparative value, if attention is not paid to circumstances, which many would probably consider unimportant in reference to its collection, preservation and preparation. I began

the investigation of the qualities of this article in the year 1810 and spent full twice the time upon it, that I devoted to Botrophis Actæoïdes. Now I have never known any body except my own pupils and friends, those who have had the results of my researches either from me viva voce, or have seen me employ this agent, who could verify all my experience in regard to it; but there have been persons enough of this description, who have habitually accomplished with it, all that I have done, so as not to leave me any ground for doubt that my own experience with it is true and genuine; and yet out of the particular circle of persons mentioned, I have never met with any physician who appeared to have a proper estimate of, or value for this agent. I once knew a gentleman who began the use of the Tincture of this article, as it is found in the shops, but being disappointed in the effects of the common doses ordinarily recommended, he gradually increased their size, carefully watching for operative effects, till they reached an ordinary Wineglassful - i. e. about two fluidounces. At this time he first began to accomplish what he expected from it. After some time he happened to obtain a first rate Tincture, of which he gave his customary dose, and was very much surprised to find it produce what he considered as alarming symptoms. Some may perhaps imagine that the amount of Alcohol in such a dose of a Tincture must be seriously in the way; but I have been long satisfied that the Alcoholic part of Proof-Spirit so called, in such a Tincture, is to a great extent wasted, partly by evaporation, but mainly by being absorbed by the insoluble parts of such a bulky root as that of *Sanguinaria vernalis*. Such Tinctures are always very weak as respects the Alcohol which they contain. But even if they were not so, the basis of this Tincture is always (at least in my opinion) contraïndicated by any degree of phlogistic diathesis; while in atonic diathesis, the Alcohol will be of no disservice. But I would not employ a Tincture of *Sanguinaria vernalis*, that required such doses as two fluidounces for the production of its effects. It was always a mystery to me how any patient could ever be induced to take such a bulk of an article so nauseous; but by admixture with a sufficient quantity of water so much sweetened as to be almost a syrup, it seemed to be taken with but little more inconvenience than a dose of a fluidrachm.

WHAT THE NOMENCLATURE SHOULD BE.

We need not look far to find the amplest and the strongest testimony from almost all writers on any and every department of knowledge, to the importance of correct and accurate language, when it is common, when it is technical, and when it is nomenclatural or denominative, in all works of science. An American writer in a medical periodical says very forcibly—"so well are we convinced of the extensive influence of words and forms of speech in transmitting the crude and false notions of former times, that we should esteem the man who had succeeded in abolishing all accustomed words and phrases involving falsehood and hypothesis, and in substituting for them such and such only as should convey literal truth, as the greatest benefactor medical science ever received." "It is true, language is not susceptible of all the precision we could wish, when employed as the medium of medical truth." "We are often obliged from a paucity of words, to use comparison and circumlocution, when speaking of the phenomena of disease," etc. "In such cases a door is always left" (open) "into which error may find its way." "But language, as used in medicine, admits of vast improvement as respects the directness and precision of terms." "Much of the laxity and verbiage, which at present belong to it, and which unfit it for the purposes of accurate scientific research, might be readily dispensed with." "The number of these verbal falsehoods, with which we are every day familiar, might be greatly reduced." "This has already been done, if we compare the present age with the past." "To be convinced of this we need only consult the medical writings of those who lived a hundred years ago." "Among them we may often detect a literal error, or an unfounded hypothesis, in almost every sentence." "This is more rarely the fact now, in more modern works, though it is unnecessarily frequent even yet." "Language is imperfect and equivocal enough at best." "For the purposes of medical science, it should have all the precision of which it is susceptible." "Facts should never be obscured or misrepresented by words which involve a hypothesis, or verbal falsehood, or which have acquired a fixed and technical meaning in some science having no analogy with medicine." "Truths should be expressed in terms as brief, as direct and as unequivocal as possible." "A figure should never

be admitted, when accuracy is required." "That which some times gives a charm to eloquence and poetry, is the bane of true science." (*H. Bronson, M. D. Rev. of Abercromb. on Intellectual Powers etc. Vide Amer. Journ. Med. Sciences, Feb. 1833.*)

The Abbé Condillac says "we think only through the meaning of words." "Algebra, which is adapted to its purpose in every species of expression, in the most simple, most exact and best manner possible, is at the same time a language and an analytical method." "The art of reasoning is nothing more than a language well arranged." (*Pref. to Lavois. Elem. Chim. fr. 5th Edin. Ed. translated by Kerr, N. Y. 1806, p. xiii.*) Lavoisier says "while I thought myself employed only in forming a *nomenclature*, and while I proposed to myself nothing more than to improve the language of chemistry, my work transformed itself by degrees, without my being able to prevent it, into a Treatise upon the Elements of Chemistry." (*Pref. Kerr, Transl. of Lavois. Elem. Chim. fr. 5th Edinb. Ed. N. Y. 1806, p. xiii.*) Lavoisier says further—"the impossibility of separating the *nomenclature* of the science from the science itself, is owing to this, that every branch of physical science must consist of three things—the series of facts which are the objects of the science—the ideas which represent the facts—and the words by which these ideas are expressed." "Like three impressions of the same seal the word ought to produce the idea, and the idea to be a picture of the fact." "And as ideas are preserved and communicated by means of words, it follows of necessity that we can not improve the language of any science, without at the same time improving the science itself; neither can we, on the other hand, improve a science without improving the language or nomenclature which belongs to it." "However certain the facts of any science may be, and however just the ideas we may have formed of these facts, we can only communicate false or imperfect impressions of these ideas to others, while we want words by which they may be properly expressed." (*Ibidem, p. xiii-iv.*)

These quotations sufficiently evince the importance which each of these authors attached to correct, precise and accurate language of all sorts; and the last particularly enforces the importance of correct nomenclature. I need not multiply authorities upon this subject; and I quote these only to prove that my own opinions

are in no respect singular, but are sanctioned by some of the greatest names. I shall therefore consider myself abundantly justified in introducing into the *materia medica* any of the changes in nomenclature, which the progress of natural history and chemistry have made necessary in those sciences, since I should esteem it as almost criminal to deprive the *materia medica* of any improvements which it can derive from two branches with which it is so nearly connected, and upon which it so much depends. Lord Chesterfield said that "geography and chronology are the two eyes of history." It is certainly no less true that chemistry and natural history are the two eyes of the *materia medica*.

The nomenclature of natural history ought always to be the nomenclature of what are called the simples of the *materia medica*, while the nomenclature of chemistry ought always to be that of all chymical compounds, and such will be the fact in the succeeding work. From the earliest records of these several branches, such has always been the fact, and must ever continue to be the fact. From a deficiency in the records of antiquity it is impossible to determine which began first in manner and form, natural history or medicine; but it is clear that there must have been observation and recognition of natural objects, before there could be remedies, and that very soon there must have been preparation of some sort or other of the articles employed as remedies, and doubtless a part of such preparation must have been what would now be considered as chymical, though it must have been chemistry as a mere art, and not at all as a science. But it is not necessary to go into inquiries of this sort, and it is sufficient to state that ever since natural history and chemistry have been recognized as distinct branches of knowledge, their nomenclature has been the nomenclature of the *materia medica*. But in modern times it has been made an objection to this community of terms in these departments of knowledge, that improvements in natural history and in chemistry (by which I understand the correction of errors, and the addition of new facts) are constantly requiring improvements in terms, which seems to be considered an evil not to be endured in medicine. Although these objectors do not say so expressly, they never the less seem to suppose that medicine is perfect, and ought now to remain for ever unchanged and unchangeable. They would have what they call an

officinal nomenclature i. e. some antiquated and exploded natural history and chymical nomenclature, which, from its original and primitive defects, or from the regular progress or advance of science, has been found to be so erroneous, and so defective, as no longer to be capable of subserving the purposes of the department of knowledge for which it was originally framed. Every distinct *officinal nomenclature* so called, with which I have ever been acquainted, has always been merely an antiquated and exploded natural history and chymical nomenclature, or parts of two or more such. Now is it reasonable to suppose that a set or system of terms, which from the progress of the department of knowledge for which it was contrived, has been found to convey erroneous ideas of affinity, to obscure analogy, to be deficient in precision and correctness, and in short to be altogether inadequate to the purposes for which it was intended, can be better adapted to the materia medica and to pharmacy than it is to the branch for which it was originally contrived and from which it has been rejected—better than a nomenclature which is far more precise, far more correct, and far better adapted to the existing state of the science? As a general rule however, these objectors, in their selection of an antiquated and exploded natural history and chymical nomenclature for what they call an *officinal nomenclature*, do not wish to go farther back than to the nomenclature of natural history and chemistry which was in use during their youth, and their professional pupillage. Some favorite terms of a much older date however they often desire to retain, such for example as *Calomelas* or *Calomel* in application to the *Dichlorid* of *Mercury*, a word which signifies good or beautiful black, and which is said to have been originally a name of the *Disulphid* of *Mercury*, to which by import it is sufficiently applicable. This term is said to have been first transferred to the *Dichlorid* of *Mercury* by a French quack,* who was a favorite of Charles II. of

*I have had occasion to employ the term quack before; and from certain *small indications* I imagine it is doubted in some quarters, whether there are any such persons at the present day. A definition may assist in throwing some light on this *obscure* subject. By a quack I understand one who prescribes for disease without any true knowledge of the pathological conditions of such disease, or any correct knowledge of the powers, operations and effects of the remedies which he prescribes. I consider this definition as covering all those who disguise common medicines, in use by all, under the form of nostrums, which they claim to have discovered, and of

England, and consequently was patronized by him. This man is said to have been persecuted and driven from Paris, whereupon he went to England. His crime in France was the employment of *Antimony* in his practice. He seems to have been peculiarly partial to medicines of chymical origin. While residing in England he is said to have published some sort of essay upon *Dichlorid* of *Mercury*, which he called for the first time *Calomelas* or *Calomel*. Of course every body in Great Britain must follow such a court favorite. Thus we are told this name was transferred from the *Disulphid* of *Mercury*, for which it had some shadow of appropriateness, to the *Dichlorid* of *Mercury*, to which it is as inappropriate as possible. If an antiquated and exploded term (I mean antiquated and exploded in chemistry) must be retained for this substance, I should greatly prefer *Aquila alba* i. e. *White Eagle*. *Panchymagogum minerale* a still older term, would be equally appropriate in import, and it would have the advantage of being more pompous.

That the nomenclature of the natural history and of the chemistry of the present day, whatever faults it may have, is greatly preferable to that of any former period, as appears to me, does not admit of a doubt; and accordingly it has been adopted by all the most approved authors of the latest times. Indeed the student in medicine, the practitioner, the pharmacist and the druggist can become thoroughly acquainted with the articles of the materia medica only through the medium of the study of natural history and chemistry, in which they must of necessity become familiar with the terms of each science. This circumstance alone

which they pretend to the exclusive knowledge, recommending them for more than ten times as many cases as they can possibly be applicable to, and demanding for them often more than ten times what they might be bought for, in any other shape, at any apothecary's; which nostrums are intended to be taken, without the advice of a physician, by persons not having the least knowledge of the diagnosis or pathology of any disease, or of the operations of any medicines. To this list of those whom I consider quacks, may be added all those educated, licensed or graduated physicians, who favor and promote the last mentioned form of quackery, by recommending or prescribing the nostrums in question, or who adopt the ordinary course of conduct pursued by the preceding sorts of quacks to obtain professional employment, and to supersede their professional brethren. From this explanation, I trust that every one will be able to understand what I mean by the term quack, and to judge whether there are any or not, at the present day.

is conclusive in favor of a unity of nomenclature in these branches, and in the *materia medica*, as the trouble of acquiring, and the inconvenience of employing two sets of terms must far more than counterbalance any of the supposed trifling advantages that are so often alleged to result from a peculiar officinal nomenclature i. e. some antiquated and exploded natural history and chymical nomenclature. It seems to be admitted that what is called an officinal nomenclature must necessarily be accompanied with the received nomenclature of natural history and chemistry, in order to render it perfectly intelligible and unequivocal; but that the latter alone is adequate to every purpose of the former, has been proved by actual trial for a long period of time.

It has been correctly said by an author that "every science requires a systematic nomenclature." "That this is especially the case in relation to chemistry, has been fully proved by the confusion which prevailed before the adoption of the happy idea of Guiton de Morveau." "The nomenclature which has been in use since 1780 is the fruit of the labors of Guiton de Morveau, directed and aided by Lavoisier, Berthollet and Fourcroy." "The great advantage of this system is to be found in the fact that as soon as we are made acquainted with the composition of a body we can tell its name without having had any previous knowledge of it." "Thus the memory is not burdened with"(mere)"names." "A systematic nomenclature is moreover the expression of a theory, so that while the theory assigns a name, the name expresses the theory." "To this connexion of nomenclature and theory, the objection has been urged that the nomenclature must undergo changes whenever theories change, which would not be the case if names were arbitrary." "Since however all changes of theory tend toward greater simplicity" (and greater correctness) "such change of nomenclature facilitates, instead of retarding the advancement of science." "In general nothing which tends to render any of the parts of a science stationary can be beneficial to it—all its parts should advance as discovery and information multiply." (*Vide Amer. Journ. Sci. & Arts, No. I. Vol. XXII. p. 249, July 1832.*)

This is a good testimony in favor of a systematic nomenclature; but it must be observed, that the word theory is here used in a different sense from that in which it is employed in medicine.

Not with standing changes in theories are here spoken-of, yet theory seems here to intend a logical deduction from perfectly established premises (which would not appear to be liable to much change) rather than a mere hypothesis, which is the customary, if not the invariable import of this term in medicine. But whether this is correct or not, justice is not here done to the modern chymical nomenclature, at least when it is constituted upon its own strict principles. When these principles are perfectly understood, a specification of the composition of a given substance instantly suggests the name; and vice versa, the mention of the name instantly suggests the composition of the substance. For example water is composed of Hydrogen and Oxygen, in the proportions of one part of the former, to eight parts of the latter by weight. Now if we consider 1 as the equivalent of Hydrogen and 8 as the equivalent of Oxygen, water as a salifiable base (which function it often performs) is Protoxyd of Hydrogen, the Greek ordinal number *protos* implying that there is but one equivalent of the electro-negative element of the compound, to one equivalent of the electro-positive element; the change in the termination of the word Oxygen denotes that it is the electro-negative element, and this element is indicated as one of the eleven basifying and acidifying elements by its taking the termination *id* instead of *uret*. But water often performs the functions of an *acid* or *salifying compound*, and as such, its composition at once suggests the name *Hydrous Acid*—*Hydrous* instead of *Hydric*, because there is an other compound of *Hydrogen* and *Oxygen* in the proportions of 1 Hydrogen to 16 Oxygen by weight. Now in *acids* or *salifying compounds*, it is the name of the electro-positive element, which has its termination changed, and such termination is *ic* in the *acid* which contains the greatest amount of the electro-negative element in proportion to the electro-positive element, and it is *ous* in the *acid* which contains the smallest amount of the electro-negative element in proportion to the electro-positive element. But in what respect can any change of theory vary these names according to the principles of the present nomenclature of chemistry, while the composition of water is demonstrably as I have stated?

It will be observed that I have said above—if we consider 1 as the equivalent of *Hydrogen* and 8 as the equivalent of *Oxygen*

—the name will be as I have said. Now this is the view taken by British and U. S. A. chimists, and as I think, on good and just grounds. But if we consider 0.50 as the equivalent of *Hydrogen* and 8 as the equivalent of *Oxygen*, this change of theory will vary the appropriate chimical name, and will require it to be *Disoxyd of Hydrogen*, the Greek multiplicative numeral *dis* implying that the compound contains two equivalents of the electro-positive element to one equivalent of the electro-negative element. This is the view at present taken by the chimists of continental Europe. This is the only way so far as I know, in which the modern nomenclature of chimistry is “the expression of a theory,” and in which “the theory assigns a name,” the only way in which “the nomenclature must undergo changes whenever theories change.” I can not view this as constituting even the smallest objection to the modern chimical nomenclature, since if a man knows any thing about the atomic weights or the equivalents of the elements, he will necessarily know any diversities of opinion, that may have been entertained upon the subject, and he will not be able to fail of thinking at once of the respective names that such diversities of opinion respectively require. As to the diversity of names to which erroneous views of composition have given rise, they will necessarily be corrected by a correction of views in regard to composition. Assuredly no one can possibly be in favor of retaining an error as respects composition, because the correction of such error unavoidably suggests a correction and a change of the name. When it was supposed that we possessed two substances composed of *Oxyd of Mercury* and *Muriatic Acid* then so called, could any one acquainted with the principles of the modern chimical nomenclature fail of calling them (at least in his own mind) *Muriates of Mercury*? But these two compounds differed essentially in their external sensible properties, of course giving rise to a question as to the causes of this difference. When it was supposed that the difference between these two compounds was owing to the circumstances that one was a neutral salt, and that the other contained an excess of the same base, could any one acquainted with the principles of the chimical nomenclature as then recognized and received, fail of calling the former *Neutral Muriate of Mercury*, and the latter *Submuriate of Mercury*, at least in his own mind? When it

was supposed that one of these compounds consisted of *Oxyd* of *Mercury* and *Oxygenized* or *Oxymuriatic Acid*, and the other of the same *Oxyd* of *Mercury* and *Muriatic Acid*, could any one acquainted with the principles of the chymical nomenclature as then recognized and received, fail of calling them respectively (at least in his own mind) *Oxygenized* or *Oxymuriate* of *Mercury*, and *Muriate* of *Mercury*? When it was believed that one of these two compounds consisted of *Red Oxyd* of *Mercury* and *Muriatic Acid* and the other of *Black Oxyd* of *Mercury* and *Muriatic Acid* could any one acquainted with the principles of the nomenclature of chemistry as then recognized and received, fail of calling them (in his own mind at least) *Muriate* of *Red Oxyd* of *Mercury* and *Muriate* of *Black Oxyd* of *Mercury*? Now what inconvenience resulted from all these changes of name of these two compounds, since such changes of name were necessarily, inevitably and involuntarily suggested to all persons acquainted with the principles of the present nomenclature of chemistry, by the changes of view in regard to their composition? But all these opinions as to the composition of these two compounds were confessedly hypothetical, because the composition of *Muriatic Acid* then so called, was intirely unknown. Now that all difficulties are obviated, and all doubts removed, in regard to the constitution of these compounds, and one of them is fully proved to consist, by weight, of 100 parts *Mercury* and 35.48 parts *Chlorine*, a basifying and acidifying element, while the other is fully proved to consist, by weight, of 200 parts *Mercury* and 35.48 parts *Chlorine*, can any one acquainted with the principles of the present chymical nomenclature fail of having the appropriate name suggested involuntarily and immediately? If 100 is the atomic weight or equivalent of *Mercury*, and 35.48 of *Chlorine*, the first mentioned of these compounds is *Protochlorid* of *Mercury*, and the last mentioned *Dichlorid* of *Mercury*. For the benefit of those not acquainted with the principles of the nomenclature of chemistry, I have already explained the import of the prefixes *protos* and *dis*, as well as that of the termination *id*; though we ought to presume that no medical reader of this work can need such explanation. If any other atomic weights or equivalents should be adopted for *Mercury*, or for *Chlorine*, the appropriate names, which such variation will require, can not but be obvious

to every one acquainted with the principles of chymical nomenclature.*

There is always a great clamor whenever a writer changes a long employed term, or introduces a new term without disturbing any old one; and yet terms have been constantly undergoing change, ever since human knowledge has been systematized, and committed to writing, and they will doubtless continue to be changed so long as knowledge continues to be progressive and is cultivated by the human race. For myself I have never been able to discover the mighty evil of a change of terms, or the introduction of new ones, when the progress of science clearly required it. Those who object to changes in chymical nomenclature do not seem to be aware that they are, in fact, objecting to the progress of science; but this is actually the case. Under our present system, as I have already said, the name should exactly express the composition of the article, so that knowing the composition may at once suggest the name; and knowing the name may exactly indicate the composition. Now I presume that no one will pretend to advocate the suspension of scientific progress in the discovery of the composition of substances, or the correction of any errors in relation to this point, into which we may have fallen, from imperfection of knowledge. But whenever we correct an error in composition, we must correct the name also. This is a matter of course, and it is not productive of any trouble to those who understand the composition, since a knowledge of the composition instantly suggests the name. The only trouble about the matter is in the correction of the error in regard to the composition. The *Chlorids* of *Mercury* already mentioned will very fully illustrate this statement. It is said by the opposers of all change, that nomenclature is not science. This is not correct if the nomenclature is what it should be, according to our present system, viz. the expression of the composition of a substance. But if nomenclature is old and justly exploded terms—terms

*Should it seem to be called for, or desired, by any number of persons, I will here say that I am prepared to publish the principles of the nomenclature of chemistry, as an appendix or supplement to this work; and that I am prepared also to show the true classification (as I believe) of all vegetable organic principles at least, that have been thus far sufficiently investigated; and to show still farther that they all may be named in conformity with the names of inorganic principles.

which, in the progress of science, have been found to convey erroneous ideas, nomenclature certainly is not science, but some thing directly opposed to science, it is positive error. But those gentlemen who are so much opposed to changes in nomenclature, in order to be consistent, ought to adopt the first name ever applied to a substance. To this however, they would positively object. Science, as I have already said, must remain stationary at the precise point where it was when they received their educations. They have no sort of objection to the possession of more knowledge than was possessed by their predecessors; but they are unwilling that those who come after them shall have more than they possess; and they are equally unwilling to study sufficiently themselves, to keep up with the regular progress of science. But the world will not wait for the indolent. As Mr. Lawrence so justly observes, "the current of knowledge and improvement rushes on so strongly, that they who hesitate to commit themselves to it, will soon be left far behind, and serve only the disgraceful purpose of enabling us to measure the force and rapidity of the stream." "Beware, I entreat you" (says Mr. Lawrence) "of this shameful apathy—this fatal indecision; and strain every nerve to advance all branches (whether immediate or auxiliary) of the profession you have chosen." "You will thus enjoy the greatest pleasure, which upright and honorable minds can receive—that of increasing the usefulness, and thereby raising the credit and respectability of the body, to which you belong." "You will" (thereby) "prepare for yourselves, at all times, a pure source of the most satisfactory reflections." (*Lawr. Lect. Salem 1828, last Lect. p. 483.*)

The alleged prolixity of many of the natural history and chymical names has often been made a great objection to their adoption into the *materia medica*, and into pharmacy. But this objection is far more specious than real, or well founded. Assuredly what is not burdensome in natural history and chymistry, can not be intolerable in *materia medica* and pharmacy. The length of a name is in fact of very little consequence; but even if it were, it may very easily be obviated, since the system even of unequivocal abbreviations so universally practised in extemporaneous prescriptions, removes all difficulty on this score.

I. When only a single part of an article of vegetable or animal origin is officinal, such part need never be specified in an

extemporaneous prescription. The application of this rule is too obvious to need an illustration.

II. When the trivial name of an article of vegetable or animal origin is a substantive, which is applied to no other species in either kingdom, the generic name may always be omitted. For example in an extemporaneous prescription we may always write for *Napellus* instead of *Aconitum Napellus*; for *Tatula*, instead of *Datura Tatula*; for *Stramonium* instead of *Datura Stramonium*, etc. But it would not be proper to write for *Serpentaria* without a generic name in conjunction, because there is an *Aristolochia Serpentaria*; a *Nabalus* (*Harpalyce* or *Prenanthes*) *Serpentaria*; a *Botrophis* (*Cimicifuga* or *Actæa*) *Serpentaria*; etc.

III. When the trivial name of an article of vegetable or animal origin is a mere attribute, of course incapable of standing by itself, and but one species of the genus is employed in medicine, the generic name alone is sufficiently unequivocal. I have actually seen *lutea* written in a prescription instead of *Gentiana lutea*. I once remarked upon such a prescription that this term was a mere attribute denoting *yellow*, and could not therefore stand alone and by itself. The reply was "yes it can, for there is no other name in the materia medica, of which it makes a part." It went accordingly to the apothecary, and he, acting upon this principle, as I suppose, put up the right article. Nothing is more common than to find the cultivators of foreign ornamental plants, which of course can have no proper English popular name, calling them by mere attributes, and such as are common to numerous other species, and this intirely without the generic name. For example, we constantly hear *Polianthes tuberosa* called *tuberosa* alone, without the generic name; and it is beginning to be common to hear this epithet corrupted into *tuberoze*, though scarcely any flower can be less like a *Rose* than this is. I never yet heard *Solanum tuberosum* (*Peruvian Potatoe*) called *tuberoze*; but this would doubtless soon be common if the popular name of *Batatas edulis* had not been transferred to it. Every cultivated ornamental plant that from its native country has the trivial *Japonica*, I believe is invariably called popularly by this attribute alone. When I have heard persons speaking of the *Japonica*, I have several times inadvertently asked—Japanese what? I could only apologize for such inadvertence, but

could not explain, as this would only have made the mistake still greater.

I very often hear physicians in high places—professors in our medical schools—declaiming against the study of the dead languages etc. I always think it is much to be regretted that (according to the very homely old saying) these gentlemen can not have their noses brought to the grindstone. If they could only be once made respectable Latin and Greek scholars, they would immediately perceive the very great advantage of having all the language of science from these two sources, so that it can be common to every civilized nation in the world, these languages being already studied by every one. In addition to this, these languages possess a flexibility and a facility of combination, so as to produce new and properly significant terms, that belongs to no modern language within my knowledge. I used formerly to suppose that sooner or later, the Latin and Greek languages would be exhausted as respects this purpose; but it is now a considerable time since I have been satisfied that they are absolutely inexhaustible. An other advantage of the Latin and Greek languages for this purpose, is the fact that every civilized nation of Europe has a method of converting Latin and Greek words into its own language, without destroying the identity of the word. In English, this is accomplished mostly by omitting their articular terminations. An other advantage still, to all those who speak the English language, is the fact that their own language can be mastered only by a thorough study of the Latin and Greek. The fact that the Latin and Greek languages have ceased to be living or spoken languages, is an important fact in their favor for their use as the language of science, because a living language is always used loosely and vaguely by the great mass of the people, which looseness and vagueness will inevitably creep into science; and because a living language is always undergoing slow and gradual changes. Of this any one may be easily satisfied, who will be at the trouble of comparing the received version of the Scriptures into English, with the English language as it is now spoken and written.

IV. Even when both the generic and the trivial names of an article of vegetable or animal origin, are necessary to avoid ambiguity, the very common practice of abbreviating the terms themselves affords an easy method of avoiding all supposed inconve-

nience from their length. Indeed so commonly and so long have the names of medicines been abridged in extemporaneous prescriptions that very many apothecaries suppose that the abbreviation is the intire word, and the intire word fully written-out, is unintelligible to them. For several years I had been in the habit of writing for the root of *Coptis trifolia* in the abbreviated form of *Copt.* Upon a particular occasion I accidentally wrote *Coptidis trifoliæ*. The apothecary to whom the prescription went said that Dr. Tully very well knew that he never kept any such article. It has almost always been my custom in extemporaneous prescription to write *Hydrargyri Dichloridi*. On a particular occasion I wrote *Calomelanos*, I do not now recollect why. The prescription was taken to four apothecaries, all of whom said that they did not keep the article. If I had written *Cal.* this abbreviation would have been understood at a glance. In fact in several places where I have been acquainted, this article, in common parlance, has been invariably called *Cal* merely, as if this were the whole name. I am also acquainted where *Cephaëlis Ipecacuanha* is called *Hippo*, and is thus written in extemporaneous prescriptions. When such abbreviations are used, great care should however be taken that they are neither obscure nor equivocal. I have much oftener seen obscurity and equivocality in abbreviations of chymical terms, whether recent or old, than in the abbreviations of the terms of natural history. I have often seen in prescriptions the abbreviated terms *Sulph. Potass.* Now these abbreviations stand equally for either of the five *Sulphids of Potassium*, for *Sulphite of Potassa*, or for *Sulphate of Potassa*. We very often see in prescriptions the abbreviations *Sup. Carb. Sod.* How is an apothecary, from this to know whether *Sodæ Sesquicarbonas*, or *Sodæ Bicarbonas* is desired, since both of these salts are equally *Supercarbonates*. I once had knowledge of a friend's sending to a large city for a considerable quantity of *Bicarbonate of Potassa*. The apothecary took him at his word, and forwarded the genuine article beautifully crystallized of course, with a large bill. The physician who ordered it, did not know what he had got, when it reached him. If he had ordered *Supercarbonate of Potassa*, the same article in relation to the thing ordered might have been sent. I suppose that it was *Sesquicarbonate of Potassa*, that was wanted, though the gentleman ordering it did not know it.

There is a *Unicarbonate of Oxyd of Ammonium*, a *Sesquicarbonate of Oxyd of Ammonium*, and a *Bicarbonate of Oxyd of Ammonium*. These are all called *Carbonate of Ammonia*, very incorrectly indeed; but it is the second which is commonly obtained, when the last name is found in a prescription. There is likewise a *Unicarbonate of Ammid of Hydrogen*; and I suppose also (though I do not know it) a *Sesquicarbonate* and a *Bicarbonate*. Now *Ammid of Hydrogen* is but an other and a more correct name for *Ammonia*. It appears to me these salts should be called the *Carbonates of Ammonia*, if these names are retained, rather than the *Carbonates of Oxyd of Ammonium*.

An Apothecary of the U. S. A. sent an order to Europe for a large amount of *Morphine*. The European druggist took him at his word and sent the *Alcaloid*. None of his customers would purchase it of him, never having used it, and not being at all acquainted with it. I have been in the habit of seeing prescriptions in which *Morphine* is directed to be dissolved in Water, and these sent to an Apothecary, who kept the *Alcaloid*, and not infrequently put it up, at the prescription of some of his customers, not however in solution in water. I have very often seen in prescriptions the abbreviations *Sp. Nit.* or *Spirit. Nitri*, or even the whole of the words *Spiritus Nitri*, for which the Apothecary ought to put-up *Nitric Acid*, which I have some times seen done, even when the quantity of the article directed in each dose of the mixture was a fluidrachm. To a physician it was obvious that *Spiritus Nitri dulcis* was intended, i. e. *Hyponitrous Æther*, or *Hyponitrite of Protoxyd of Etherogen*. I believe that when this article is intended to enter into a prescription, it is oftener written-for by the name *Spiritus Nitri* or some abbreviation of this than by any other denomination.*

I have frequently examined apothecaries' shops, in which I could not find a single correct label. The terms would be grossly

*By the bye, all that I have ever known sold for *Spiritus Nitri dulcis*, *Hyponitrous Æther* or *Hyponitrite of Protoxyd of Etherogen*, has always appeared to me to be a mere mixture of a little *Nitric Acid* with *Alcohol*. An intelligent apothecary of my acquaintance once supplied himself with a quantity of the genuine article, but he could never sell it, either to physicians or non-medical persons. All seemed to prefer the subacidulated *Al:ohol*; and I am inclined to think that it accomplished the purposes for which it was administered, better than the genuine article. Pure *Alcohol* would probably have done better still.

misspelled, or they would be part Latin and part English, or they would be almost any thing but what they should be. I once knew an apothecary, who, upon removing into a new and elegant shop, was so unaccountably singular as to apply to a neighboring medical gentleman—both an accurate linguist, botanist and chimist—to furnish him with correct labels, which were accordingly supplied, and very handsomely put-on. In a short time however he sold the establishment to a young gentleman educated, trained and brought-up to the business of an apothecary, in a neighboring large city, who was exceedingly shocked at the accurate spelling, the good Latinity and the correct natural history and chimistry, which he found, so much so, that he thought it necessary to apologize to most of his customers for it, and to assure them that he should have it removed as soon as he could by any means make it convenient. I have received the Terebinthinate essential oil of *Abies Canadensis* from shops kept by graduates of our colleges of pharmacy (I suppose they were Doctors of Pharmacy, for I do not know that any other degree could have been conferred) resident in our large cities, labeled *Oleum Conii*. I have received the essential oil of *Gaultheria procumbens* labeled *Oleum Pyrolæ*. This plant is popularly called *Wintergreen*. If we look for the name *Wintergreen* in European Medical Dictionaries we find it explained by *Pyrola*, and hence *Oil of Wintergreen* is *Oleum Pyrolæ*. I have received the root of *Statice Caroliniana* under the label *Radix Rosmarini* (not *Rorismarini*.) I have received the fruit of *Cornus sericea* under the label *Baccæ Rhamni catharticæ*; and perhaps I could fill a sheet with similar errors. A certain number of years ago it was the fashion to prescribe *Tincture of Resin Acroides*. By this was understood a *Tincture* of the *Resin* of *Zanthorrhæa Hastile*, which, in works of chimistry, is called *Resina Acaroïdis resiniferæ*, though I have never seen the name *Acaroïs resinifera* in any work on botany even as a synonym.

From all this I infer that a great deal more of Latin and Greek; a great deal more of chimistry and natural history, are needed in the profession. In short, if medicines were as active and as dangerous as many suppose, and if prescriptions were accurately and correctly followed as they are written and sent to the Apothecaries, more persons would be killed than cured by them. When-

ever I have suggested this, the objection has always been that no man can obtain an adequate remuneration, in our country, for the time and the toil necessary for scholarship in the medical profession, those without this qualification uniformly succeeding best in obtaining popular favor and patronage; all of which I am inclined to think is very accurately the truth. At all events a classical education preparatory to the study of medicine has been constantly becoming less and less common ever since my attention was first turned to the subject. Not a year has passed since I became a candidate for the practice of medicine, that I have not heard learning mentioned as a disqualification for a physician. Nothing is more common than to hear it said of particular physicians, that they have too much learning to be good practitioners; and this comes from persons reputed to be highly intelligent. There seems to be a wide spread kind of belief that ability in the practice of medicine must be congenital, or perhaps intuitive; and hence a quack that pretends to this, always (wherever I have lived) receives a great deal of credit for skill, and has the unbounded confidence, even of the highly educated part of the community, at least in general. One of the coarsest, the rudest, and the grossest quacks I ever knew, was accustomed to go about boasting that he had never read a medical book, that he had never seen even any part of a human body dissected, that he had never set his eyes upon a human skeleton etc., and yet he was in high popular estimation, not only with people of mere common-school education, but with men of classical educations, clergymen, lawyers, professors in our colleges, universities, etc. I was once describing this man to a clergyman, and mentioning the fact that I had heard him boast that he could take a cat in hand, and dislocate every bone in her, so that if he laid her down she could not move a hair's breadth; that he could then take her up and replace every bone so that the animal could run-off perfectly well. I then stated that this man had the full confidence of many highly educated persons, among whom were clergymen, lawyers, professors of colleges, etc. The gentleman with whom I was conversing denied this charge wholly, as respected clergymen, and reproved me severely for bringing such an unfounded charge against his professional brethren, a charge that he wholly and absolutely denied and repudiated. I immediately named the quack, when

my friend at once pronounced him one of the brightest lights of the medical profession and not in the least a quack, a little uncultivated and coarse indeed, but truly belonging to the first rank among surgeons and physicians; and then he reproved me for my envy of such a man, and for my calling him a quack. This clergyman was many years older than myself. The whole affair reminded me of a physician of my acquaintance who always perseveringly denied that a case of Croup could ever be cured. He always took care never to make a diagnosis of a case till it was decided. If the case recovered it was not Croup; if the patient died it was Croup; and therefore no case of Croup ever recovered; truly a short way with skeptics.

The nomenclature of medicines employed by a physician should always be unequivocal and definite. These two points should always be regarded above all others. After this it may be rather desirable that it should be concise and brief so far as may not be incompatible with the preceding; but this is a point of very inferior importance and not worthy of sacrificing any thing else for.

Thus, as seems to me, it is clear that what is called a peculiar officinal nomenclature, i. e. that an antiquated and exploded natural history and chymical nomenclature, is not at all necessary for the *materia medica*, is without the advantage of facilitating extemporaneous prescription in any way, and may in every respect be considered as worse than useless, since it inevitably increases the labor of the student, the physician and the apothecary or pharmacist, and beside this contributes to conceal those natural relations which exist between different medicines, which it is always useful to keep in view, and which the modern nomenclature of natural history and chemistry so happily displays.

The difficult subject of popular names for the articles of the *materia medica* demands a few passing remarks in this place. It is commonly supposed that every individual medicine must necessarily have its popular name. For myself however, I could never perceive this necessity. I could never perceive why the natural history and chymical names of what are called the simples of the *materia medica*, might not answer all the purposes of the physician, as well as of the naturalist, and the chemist. At all events, there are very many cases in which these names do answer all purposes, since there are no other. With many, there

seems to be a kind of mystery about, and a kind of horror of a Latin word, a kind of feeling that it will not by any means answer for common use. Such persons do not consider how many pure and unmodified Latin words contribute to make-up the English language—words which are in every body's mouths, every day. With many, the use of a Latin appellation of a medicine is deemed unpardonably pedantic; and thus, between want of familiarity with the Latin language, and wholly unfounded prejudices in regard to its use, an absurd system (if it can be so called) of having popular names, in a department of knowledge, which never was popular, and from the very nature of things never can be, has been kept-up and will perhaps be perpetuated. But the great difficulty in regard to popular names in the U. S. A. is to induce the inhabitants of different sections, or even those of two adjoining towns, to apply the same names to the same articles. In one place a given article will perhaps be called Stink-weed. In the next place immediately adjoining, it will perhaps be called Night-shade. In the next still, it will perhaps be called West-end-Pink. In the next still, it will perhaps be called Henbane. In an other place it will be called Jimson-weed—a corruption of Jamestown-weed—while in books, the popular name given is Apple-Peru (the plant being exclusively of East Indian origin, and utterly unknown in any part of America, till carried thither by Europeans) and Thorn-Apple; and even all this is not absolutely intelligible without the natural history name *Datura Tatula* (Linn.)

The preceding specifications are actual usage that has fallen within my own personal knowledge. In addition to this, all of these names will be applied to numerous and widely different plants in different places, the same name often not being applied to the same plant in two different places. As a general rule, all the English popular names of plants are to be found in the U. S. A. but applied to wholly different articles. A few examples will illustrate this sufficiently. The English name Centaury is every where applied to the several species of the genus *Centaurea*, whether in England or the U. S. A. In England the popular name Centaury is applied both to *Erythrœa Centaurium* and to *Chlora perfoliata*. In Philadelphia, and as I am informed, in various other parts of Pennsylvania, the popular name Centaury is applied to *Sabbatia angularis*. It is however commonly abbrevi-

ated into Centry. In New Haven Ct. the popular name Centaury is applied exclusively to *Polygala polygamum*. In various other parts of Connecticut the popular name Centaury is applied only to *Hypericum mutilum*. If the matter were to be thoroughly investigated, I doubt not that it would be found that this same popular name Centaury is applied to at least half a dozen other plants, within the limits of the U. S. A. In England Wake-Robin is the popular name of *Arum maculatum*; while in the U. S. A. where this plant is not found, Wake-Robin is the popular name of one or more species of *Trillium*. A few years ago, much was said in certain periodicals about the medicinal value of Extract of Artichoke, to which was appended by way of rendering it intelligible, the natural history name *Cynara Scolymus*. I happened to come to the knowledge of the fact that a considerable number of U. S. A. physicians made, as they supposed, this preparation, and employed it in their practice, but without any useful effects. Several of these gentlemen finally consulted me about the matter, when I found that they had made an Extract of *Helianthus tuberosum*, the only plant to which, within their knowledge, the popular name Artichoke is applied in this country. Some of these gentlemen I had great difficulty in convincing of their mistake.

In the U. S. A. it would be utterly impossible to convince persons of ordinary educations, or the majority of the apothecaries,*

*A friend has repeatedly admonished me, that I apply the term apothecary, in a manner which is not English. I will here state that those, whom I call apothecaries, in the first place prescribe for the sick, though more especially for chronic cases. They do it however in their own shops, rarely visiting the patient at his or her residence. I have known apothecaries, with no other education than that obtained at a common school—men who had stepped immediately from a mechanic's shop to that of an apothecary, to prescribe for more chronic cases than any M. D. within ten miles. In the second place, those whom I call apothecaries, put-up the extemporaneous prescriptions of the neighboring physicians; and they make-up all sorts of officinal preparations. In the third place, they sell all sorts of crude and unprepared medicines, beside many drugs which are not medicinal. Thus I think they perform all the functions of an English apothecary, except visiting patients at their own houses. It may perhaps be said that the educations of the American apothecaries are inferior to those of England. This may be true, but it does not exclude the Americans from the ranks of the profession. I know of no reason however why this term may not be applied in the U. S. A. in a manner different from what it is in England. At any rate, this definition will explain the American application of the word apothecary.

that *Carthamus tinctorius* is not the true Saffron. I have often attempted to do this, but have failed in a majority of cases. I have known a number of physicians who employ *Solanum nigrum* as Belladonna, because this is its popular name in the section of country where they reside. They would not be persuaded that the popular name could possibly mislead them. *Celastrus scandens* is constantly employed for *Solanum Dulcamara* in some parts of our country, because it bears the popular name Bitter-sweet, in those regions. I have even known the leaves of *Abies Canadensis* used for the leaves of *Conium maculatum*, because the former is called Hemlock as well as the latter. Numerous other articles are in the same predicament in the U. S. A. I do not believe that there is a physician in this country, who ever uses indigenous articles, and is not at the same time a scientific botanist, who is not occasionally misled by popular names. What can be known certainly from such a name as Snake-root? I once made a catalogue of a very large number of plants bearing this name, which were as unlike each other as the species of widely different genera usually are. The English popular name Dog-wood is applied to *Erythrina Corallodendron* in Jamaica and some other parts of the British West Indies, and to *Gardenia Randia** in Barbados, and doubtless in some other islands; while in the U. S. A. it is applied to *Cornus florida* and other species, and also to *Rhus venenata* and *Acer striatum*. I can perceive no particular appropriateness in the name Dog-wood to any of the plants to which it has been applied. It is true I am not acquainted either with *Erythrina Corallodendron* or *Gardenia Randia*; but I have no knowledge that any reason has ever been given why this name is peculiarly adapted to either. I can perceive no sort of appropriateness to *Cornus florida* in the popular name Dog-wood. The name Cornel is the received English appellation of more than one species of the genus *Cornus*. Why not call *Cornus florida* by the popular name of Flowering-Cornel? The popular name Swamp Sumac is far more appropriate than Dog-wood for *Rhus venenata*, and ought therefore to be always preferred. I doubt whether any good reason can be given why *Acer striatum* should be called Dog-wood. Why not call this plant Striped-Maple?

*Doubtless *Randia latifolia* (La Marck & De Candolle) *Randia aculeata* (Linn. & Roemer & Schultes.)

This last would be sufficiently appropriate, and so far as I know, it is not now applied to any thing else. However should it ever come into use as the popular name of *Acer striatum*, it will soon be applied to other things. *Acer striatum* is often called Moose-wood; but this name is oftener applied to *Dirca palustris*, so that at all events Moose-wood is rendered equivocal as a popular name. The English popular name of *Ficus Sycomorus* of Egypt, Palestine, etc. is Sycomore, to which only it is truly appropriate, and to which it should be confined exclusively. But in England this name is applied to *Acer Pseudo-Platanus*, while in the Southern U. S. A. it is applied to *Platanus occidentalis*. Assuredly the popular name Sycomore can be appropriate to no other plant but *Ficus Sycomorus*. The word *Sycomorus* or Sycomore means Fig-Mulberry, because (as has been said) the fruit is a Fig while the foliage of the tree resembles that of a Mulberry. Some times the natural history name of one plant is the popular name of an other plant. *Aloë* is the natural history name of a genus, and it is the popular name of all the species of *Agave*. *Aloë* is given by some authors as the popular name of the species of *Aletris*. *Syringa* has been the long established name of a genus, and yet it is constantly employed as the popular name of all the species of *Philadelphus*. *Sanguinaria* is the natural history name of a genus, and it is the popular name, or one of the popular names of *Polygonum Aviculare*.

If what are called popular names are to be tolerated in the *ateria medica*, I think that a great reformation is necessary. Very many appellations constantly put-down in books as popular, certainly never were so, since they are mere translations of the natural history name of the plant, and that according to its etymological signification, without the least reference to the circumstances, which led to its application. These translations are often as absurd as if we should convert the name *Michauxia* (from the two botanists father and son named Michaux) into the Half-Lime-plant, because Half-Lime is the etymological signification of the name Michaux. The name *Lysimachia* derived from the name *Lysimachus* (as some say a King of Thrace and a companion of Alexander, but as others say a King of Sicily) has been translated into Loose-strife, because *Lysimachus* is made-up of *Λύσις*, "the act of loosing; the act of setting at liberty," etc. and *μάχη*, "strife,

conflict," etc. And yet, strange to tell, this very appellation Loose-strife is given in popular American books upon botany in the English language, as the popular name of *Gaura biennis*, a plant as unlike any species of *Lysimachia* as any thing I can well mention. Now this case seems to prove either that popular names, made by authors of popular books on botany, are received and adopted by the people, or that such books contain names, which are not in fact popular, though given as such. In some of the American popular works on botany, we find the term Dragoness as the popular name of *Clintonia borealis*. This word appears to be a mere translation of *Dracæna*, the name of a genus to which this plant has been some times referred. But whoever made this word made also a great blunder. There is certainly no such word as Dragoness in the English language, nor any room for such a one, since Dragon is common gender, as much feminine as masculine. I do not think that Dragoness was ever used as a popular name of the plant I have just mentioned. If it ever was, it will contribute to prove that popular names may be imposed by popular writers upon botany. Popular works on botany give Follicle-vine as the popular name of *Periploca Græca*, which I do not think was ever so used. It will be observed that follicle was the Linnean name for the pericarp of this plant, though conceptacle is the name employed by the French school of botany. It is true that the pericarps of this plant are prominent organs, in the right season, and might give it a name; but I do not think that the populace would be at all likely to derive one of their own appellations from a technical term in the Latin language. If *Periploca Græca* ever has been called popularly Follicle-vine, it furnishes an other proof that popular names may be imposed by authors.

These specifications of the errors and absurdities connected with, and resulting from what are called popular names, in a department of knowledge that can never become popular, will perhaps suffice (though I have not mentioned a tenth part of their varieties) to support my recommendation of using no other names in the *materia medica*, than those of the natural history and chemistry of the present day, even though a great cry of pedantry should be set-up. Such a cry (as was once said by an American author on a very different subject) will neither pick a man's pock-

et, nor break his leg. Every new and merely popular book of descriptive botany, of course written in English, for the use of the numerous academies, institutes and colleges for young ladies, in our country, always manufactures a host of new popular names of plants, a considerable portion of which become sufficiently popular to get into use, by the aid of the ladies. The fact that popular names are some times introduced by popular authors, furnishes a strong argument in favor of the attempt to reject and discard some of the most faulty and objectionable ones at present in common use, and of an attempt to introduce less equivocal and more truly distinctive ones. Such a fact sufficiently evinces that by the right sort of effort in relation to the circumstances of the case, a very useful degree of reformation and improvement may be made, even in this unpromising field. It is true I should very greatly prefer to abandon all attempts at any thing like popularity, in a department of knowledge utterly incapable of it; but as this, in the present state of things, may be utterly impracticable, we must do the best we can, viz. amend as far as possible what seems to be fixed upon us. When therefore I come to give the popular synonymy of the articles of the *materia medica*, I shall make all the strictures that I think the subject requires; and for myself I shall make but small use of any other than the natural history and chymical names. If any body chooses to call this pedantry, I will say beforehand that it is a sort of pedantry of which I shall be proud.

PHARMACEUTIC PREPARATIONS.

It is my present intention to accompany what are commonly called the simples, with Formulæ for all the pharmaceutic preparations, that are suitable to be kept in the shops, or that can ordinarily be needed or required as officinal preparations. But I do not intend to confine myself to the Formulæ of any individual Pharmacopœia at present extant. As appears to me, many of the Formulæ for generally received officinal preparations are susceptible of improvement, and I can discover no good reason why they should not be improved. There are many even comparatively old Formulæ in our Pharmacopœiæ that unquestionably need reformation, which in some, if not many cases I shall attempt to reform, with what success, let my readers judge. For example, I

suppose that the reason why a Water of Arsenite of Potassa is directed instead of a Water of Arsenous Acid is that Arsenous Acid is so very slightly soluble in water, that such a preparation must inevitably be too weak. But a neutral combination with Potassa renders it some what more soluble, though still not sufficiently so. The Formula of the Pharmacopœiæ does not make wholly either a Unarsenite or a Disarsenite of Potassa. The quantity of Anhydrous Carbonate of Potassa employed is deficient gr. 25.20, for the Disarsenite, and it is in excess gr. 19.40, for the Unarsenite. Of the salt contained in this Water, 0.44 is neutral, and 0.56 is a disalt. Now as the neutral salt is but little more soluble than the Acid, and at all events is not soluble enough; and as the disalt is considerably more soluble, I shall reject the formula of the Pharmacopœiæ for this preparation, and shall give a Formula for a Water of Disarsenite of Potassa. But while I do this I shall not vary the strength, as a preparation of Arsenous Acid, so that the dose will remain the same. In the Formula of the Pharmacopœiæ there is always more or less precipitate as soon as it becomes cold, whereas in that which I propose, the solution remains perfect. I consider it therefore as an improvement of the previous Formula.

There are a number of comparatively recent preparations, Formulæ for which, I think, may be advantageously introduced into our Pharmacopœiæ, and which I shall at all events give in this work. It is my present intention to give several Formulæ for preparations of Arsenic that will be new to the Pharmacopœiæ, and as I trust both convenient and useful to the practitioner of medicine. The preparation introduced by Donovan, as is said under the name "*Liquor Hydriodatis Arsenici et Hydrargyri*" is doubtless well worthy of a Formula in our Pharmacopœiæ, and a place in our shops. Mr. Donovan's views, as respects the composition of the salt contained in this preparation are undoubtedly very far from being correct, and it is therefore no matter of surprise that his Formula requires amendment, which I shall endeavor to make. As appears to me, Mr. Donovan is utterly mistaken in supposing that this preparation contains Sesquoxyd of Arsenic or Arsenous Acid, and in supposing that this substance performs the function of a base in the salt formed. I do not think any compound of Arsenic with Oxygen is ever basic, but on the contrary,

all such compounds are always salifying. In truth I have no knowledge that any compound of Arsenic with any one of the eleven basifying and acidifying elements, is ever basic. I believe that all such compounds are always salifying if they perform either function. As appears to me, Mr. Donovan is utterly mistaken in supposing that this preparation contains Protoxyd of Mercury. I do not think that the salt of which this preparation is a Water, i. e. an aqueous solution, contains a particle of Oxygen. As appears to me, Mr. Donovan is intirely mistaken in supposing that the salt which is formed in his preparation contains a particle of what he calls Hydriodic Acid, but which he should call Iodohydric Acid, since Iodine is its basifying and acidifying element, and not Hydrogen. I consider it as well determined that Iodohydric Acid, and all other Acids having Hydrogen for their base, and being acidified with any other basifying and acidifying element except Oxygen, are intirely incapable of entering into combination with any Oxyd, without immediate decomposition. Now if Mr. Donovan's supposed Hydriodate of Sesquoxyd of Arsenic and Protoxyd of Mercury does not contain a particle of this Acid, or of either of these two Oxyds, or even of Oxygen, we may well suppose that a Formula for its preparation, founded upon such erroneous views of its composition, must need some reformation. Mr. Donovan's Formula undoubtedly makes an intirely different salt, from what he supposes, and the proportions of his ingredients are such as to be intermediate between those exactly right for two different salts, precisely as is the fact with the Formula of the Pharmacopœiæ for the Water of the Arsenite of Potassa.

Ever since the medicinal Alcaloids have been in any thing like common use in the practice of medicine, I have been in the habit of making various, and indeed numerous new compounds of them with other active ingredients. After employing these compounds for a longer or shorter time in my own practice, and communicating my knowledge of them to a greater or less number of my pupils and friends, there has usually appeared in some of the periodicals a more or less incorrect Formula for the same compound, but put forth with different views as to its composition, and with a different name. Long before there was any published account of any such compound, I had prepared, communicated to

many of my friends, and determined the powers, effects and proper doses of what I had considered, and still consider to be a Protiodid of Quininum. After I had been in the habit of employing this compound somewhere between five and ten years, a Formula for it was published, in which the crystalline form of it was considered as a Protiodid of Quinine, and the amorphous and resinoid or resiniform substance was considered as a Deutiodid of Quinine. As near as I now recollect, the new Formula for it was not strictly correct, though I have not the periodical at hand in which it was contained, and therefore can not now refer to it, so that it is possible I may be mistaken in regard to this last. I shall however state what it is exactly, when I come to give my own Formula. In the subsequent work I shall give my own Formula, my views of its composition, what I consider to be its correct name, together with its powers, doses and therapeutic applications, as ascertained by myself, wholly disregarding the *awful* fact that all this will involve a deviation from something that has been once printed, and above all, a change of an erroneous published name for one that is correct.

Very early after the chymical habitudes and relations, both of the compound radical Cyanogen, and those of the Alcaloid Quina or Quinine began to be in any degree well understood, I made first what I considered to be a Protocyanid of Quininum, and subsequently what I considered to be a Dicyanoferrite of Cyanid of Quininum. After a longer or shorter time, more or less incorrect Formulæ for both of these compounds were published, and they were called by quite different names—names which either failed of expressing the true composition with any definiteness and precision, or which conveyed incorrect notions of their composition. I shall of course give my own Formula, my own views of composition, the name which most accurately expresses this, etc. without the least regard to the fact that this may involve a change of some erroneous and absurd name that may have got into print. I never had the least respect even for the most antiquated errors, and certainly none for very recent ones; and I have no more respect for an error that has been printed, and that even in a popular book, than for any other. It is my present intention to add several Formulæ for preparations of Cyanogen that will be new to the Pharmacopœiæ, and as I trust both convenient and

useful to the practitioner of medicine. All highly active medicines, of which any material variation in the dose might be productive of danger, at least to children if not to adults, I shall be very careful to keep of the same strength of the active ingredients, as near as possible as it has hitherto been, to prevent all danger from its use by those accustomed to old preparations. If the preparation were now to be made for the first time, it would be perfectly immaterial whether the officinal Tincture of Opium should be half, or double its present strength; but as it has been so long in use of a given strength, and as so many non-medical persons have learned how to manage it with perfect safety, both for adults and for children, it would now be hazardous, indeed quite dangerous for such persons, to increase its strength; and it would often be productive of disappointment as to its effects in the hands of non-medical persons, if its strength were to be materially diminished. But Tincture of Opium is too active, and too much concentrated, to be convenient or altogether safe, under the most careful management by non-medical persons, for young children. The Compound Tincture of Opium is so contrived as to meet and obviate this difficulty. It is a preparation of about one twelfth the strength as an Opiate, of Tincture of Opium, at least as determined by its use. Now it would be hazardous, indeed dangerous, to vary the strength of this preparation. There should be no officinal liquid preparation of Papaver or any of its products, of strengths as Papaver varying from the officinal Tincture of Opium, and the officinal Compound Tincture of Opium, at least for the safety of children if not of adults. If therefore we have an officinal Tincture of the Alcaloid Morphine, let it be of the same strength as a preparation of Papaver, as the Tincture of Opium. If we have an officinal Compound Tincture of the Alcaloid Morphine let it be of the same strength as a preparation of Papaver, as the Compound Tincture of Opium. If we have any officinal Water of the Sulphate of Oxyd of Morphinum, it should be of the same strength as a preparation of Papaver, as the officinal Tincture of Opium. Now I think that a Tincture of Morphine, a Compound Tincture of Morphine, and a Water of Sulphate of Oxyd of Morphinum, all of the regular and standard strength of the long received preparations of Papaver, and so made as not to undergo any speedy change, even in the warmest

weather are much required as officinal preparations, and I shall accordingly give Formulæ for such.

There is an officinal Compound Powder of Opium. Now as universally as the preparation alluded-to seems to have met with the approbation of the medical profession, and as long and as extensively as it has been generally employed by physicians, still I think it is ill adapted to the purpose for which it is intended, and in some points of view, is quite exceptionable. Although I shall retain the Formula of the received Pharmacopœiæ for Compound Powder of Opium, yet I shall propose as a substitute for it a Compound Powder of Sulphate of Oxyd of Morphinum, a preparation which I made as soon as the salt which is its principal basis was introduced into the materia medica, which I have used ever since, and which I consider as a much preferable preparation. But if we have an officinal Compound Powder of Sulphate of Oxyd of Morphinum, let it be of the same strength as a preparation of Papaver, as the officinal Compound Powder of Opium. My Powder is accordingly made-out upon this plan. Some times, perhaps frequently, I shall give specimens of Formulæ for mere extemporaneous preparations, when otherwise the proper proportions for such preparations might not readily be known; but I shall always keep officinal and extemporaneous preparations under distinct heads. The statements which I have now made will be sufficient to give my readers a sufficient notion of what I shall endeavor to perform in Pharmacy, and therefore I need say nothing further upon the subject in this place. What I have mentioned is only a specimen of what I shall try to do.

NOMENCLATURE OF THE PHARMACY.

Perhaps a few statements in regard to the nomenclature of pharmacy which I shall employ, may be needed, if they are not absolutely necessary. As to the simple crude articles, or compounds not prepared by the pharmacist, but only by the manufacturer, I have already stated what their nomenclature is to be. All regular pharmaceutic preparations I shall name after their forms, according to the principles of authors; though I shall follow these principles much more strictly than the authors themselves do, who lay-down the principles referred-to. For example I shall have

nearly all the pharmaceutic forms commonly adopted by all the best Pharmacopœiæ of the present period, such as

1. Pulveres et Pulveres compositi.
2. Confectiones et Confectiones compositæ.
3. Pilulæ et Pilulæ compositæ.
4. Trochisci et Trochisci compositi.
5. Syrupus sachari officinarum, syrupi et syrupi compositi.
6. Extracta aquosa, Extracta spirituosæ, Extracta expressa inspissata, et Extracta exudata inspissata.
7. Mucilagines.
8. Emulsiones.
9. Succî et succi compositi.
10. Infusa et Infusa composita.
11. Decocta et Decocta composita.
12. Misturæ et Misturæ compositæ.
13. Aqua distillata Aquæ et Aquæ compositæ.
14. Acetum distillatum, Aceta et Aceta composita.
15. Vinum Vitis Viniferæ, Vina et Vina composita.
16. Spiritus officinalis, spiritus et spiritus compositi.
17. Tincturæ et Tincturæ compositæ.
18. Ætheres et Ætheres compositi.
19. Olea essentialia.
20. Olea pinguis vel unguinosa.
21. Linimenta et Linimenta composita.
22. Unguenta et Unguenta composita.
23. Cerata et Cerata composita.
24. Emplastra et Emplastra composita.

As appears to me Mel Apis Mellificæ should take its place among the simples, and that those pharmaceutic forms named Mella et Mella composita should be merged in Syrupi et Syrupi compositi.

I have many doubts whether Mucilagines and Emulsiones are worthy of being retained as distinct pharmaceutic forms; but I do not know what to call them in conjunction, or what other form or forms to associate them with. It is even doubtful whether they are worthy of being retained at all.

Succi et Succî compositi constitute pharmaceutic forms not commonly found in our Pharmacopœiæ; but I am satisfied that they ought to have a place in them. I refer to the expressed

juices of certain recent plants with just *Spiritus officinalis* enough added to them to prevent any fermentation, or any other change, except what necessarily results from long keeping. I have long been in the habit of using several such preparations, and none more frequently than *Succus Conii maculati* for example.

I have long had doubts whether the *Decocta et Decocta composita* are worthy of being retained in the *Pharmacopœiæ*, since I consider the *Infusa et Infusa composita* as always preferable.

I have also long entertained doubts whether the *Aceta et Aceta composita* are worthy of retention. I think they are always inelegible preparations, and that the *Vina et Vina composita* might always be advantageously substituted for them.

If the name Spirit is retained as a pharmaceutic form, it appears to me that we should retain the name *Spiritus officinalis* instead of *Alcohol officinale* for the Unihydrite of Protoxyd of Etherogen of the specific gravity of 0.835 i. e. 85 per centum Alcohol and 15 per centum Water. On the same ground what is called *Alcohol Ammoniatum* should be *Spiritus Ammoniaë* or rather *Spiritus Hydrogenii Ammidi*. If we retain the name *Alcohol officinale* for the Unihydrite of the Protoxyd of Etherogen of the specific gravity 0.835 i. e. 85 per centum Alcohol and 15 per centum Water, the pharmaceutic forms in which it is the menstruum should be denominated *Alcohola* (if indeed the word is declinable) et *Alcohola composita*, in analogy with *Vina et Vina composita*; *Aceta et Aceta composita*; *Aquæ et Aquæ compositæ*, each respectively from *Vinum Vitis Viniferæ*; *Acetum distillatum*, *Aqua distillata* etc. I am much inclined to think that if the *Tinctures* had been merged in the *Spirits*, it would have been better still. There is certainly a great difficulty of finding a well marked line of distinction between them, and there have always been certain preparations in regard to which pharmaceutists never could agree, some calling them *Spirits* and some calling them *Tinctures*. I think it well that *Elixirs* were rejected as a distinct pharmaceutic form, and that they were joined to the *Tinctures* as compound forms of them. The *Æthers* and *Ætheres compositi* are comparatively unimportant pharmaceutic forms; but there are a very few of them that can not be dispensed-with, and therefore I retain them.

But the Pharmacopœiæ in common use are full of anomalies and deviations from what seem to be their own principles. For example, according to the laws of the nomenclature of pharmacy, when a pharmaceutic preparation has only a single base, we are told that its name should be made-up of the name of the pharmaceutic form, followed by the name of the base in the genitive case and nothing else. But how does usage conform to this law? We find *Tinctura Opii Acetata* as the name of a simple or noncomposite preparation of Opium, because the menstruum is Vinegar twelve fluidounces and officinal spirit eight fluidounces. As appears to me, there is room only for the question whether this preparation is an *Acetum Opii* or a *Tinctura Opii*; and as the Vinegar predominates over the officinal Spirit in the menstruum, I think it should rather be called an *Acetum*. When the menstruum is compound, I should suppose that the predominating ingredient of it should give the name. We find *Tinctura Valerianæ Ammoniata* applied to a Tincture containing nothing but root of Valerian, as I suppose, because the menstruum is *Spiritus Ammoniacæ*, or *Spiritus Hydrogenii Ammidi*. We find also *Tinctura Guaiaci Ammoniata* applied to a Tincture containing nothing but the resin Guaiacum, because, as I suppose, the menstruum is *Spiritus Ammoniacæ* or *Spiritus Hydrogenii Ammidi*. Subsequently we find the same form of language employed to denote quite a different thing, viz. to denote the secondary base in a Tincture with two bases, or in other words, a Compound Tincture formerly called an Elixir, as *Tinctura Opii Camphorata*, or still more incorrectly *Tinctura Camphoræ Opiata*. Now *Camphorata* and *Opiata* as applied to what is really *Tinctura Opii composita*, are not analogous in their import to *Acetata* as applied to *Acetum Opii*, or to *Ammoniata* as applied to *Tinctura Valerianæ Ammoniata*, and *Tinctura Guaiaci Ammoniata* as applied to Tinctures whose menstruum is *Spiritus Ammoniacæ*, or *Spiritus Hydrogenii Ammidi*. According to the laws of the nomenclature of pharmacy, when a pharmaceutic preparation has more than one base i. e. is what was formerly called an Elixir, its name is to be made-up of the name of the pharmaceutic form, followed by the name of the principal base in the genitive case, with the addition of the term *compositus*, agreeing in gender with the name of the pharmaceutic form. But does general usage conform to these rules? In the Pharmacopœia of the

U. S. A., for 1830, we find but three Tinctures with the term *composita* annexed, viz: *Tinctura Gentianæ composita*, which is not a compound Tincture, since it has only a single base; *Tinctura Cinchonæ composita*, which is a true compound Tincture since it has a double base; and *Tinctura Sennæ composita*, which is also a true compound Tincture since it has a double base. Several other true and proper compound Tinctures are contained in this Pharmacopœia, but they are not named in conformity to the law of the nomenclature, but in some irregular and anomalous way, not in uniformity among themselves. Instead of *Tinctura Aloës composita*, which the law requires, we find *Tinctura Aloës et Myrrhæ*. Instead of *Tinctura Rheï composita*, which the law requires, we find *Tinctura Rheï et Aloës*. It may perhaps be inquired why this is not the most eligible mode of naming what are directed to be called *Tincturæ compositæ*? If there were never any Tinctures with more than two bases, I should be inclined to answer this question in the affirmative; but if there are no such in the Pharmacopœiæ, I have often seen them in the shops, and in the office of the practitioner who dispenses his own medicines. To a Tincture with two bases, viz. Rhubarb and Senna, in the proportions of an ounce of the former and a quarter of an ounce of the latter, we find the name *Tinctura Sennæ aromatica* applied. As the Rhubarb is the principal base, this preparation, according to the laws of the nomenclature, should have been called *Tinctura Rheï composita*. There is actually another Tincture consisting of Senna and Jalap, in the proportions of three ounces of the former to one ounce of the latter, to which the name *Tinctura Sennæ composita* is correctly applied, which contains a greater quantity of aromatics, and might with far more fitness have been called *Tinctura Sennæ aromatica*, if such a name were only authorized by any of the laws of the nomenclature of pharmacy. Here the mere popular name, viz. Senna, of the principal base of a Compound Tincture is used instead of the natural history name either of the genus or of the species. Now the word Senna is brief and euphonous and admits very easily of declension according to the laws of the Latin language, but with the exception of these circumstances, the name *Tinctura Sennæ composita* is no better—certainly it is no more conformable to the laws of pharmaceutic nomenclature, than *Tinctura Bloodrootiæ composita* would be for

a compound Tincture of Sanguinaria. The superior euphony is all the real advantage that the former has over the latter. Thus, out of six Compound Tinctures contained in the Pharmacopœia before me, only three of them bear the name, while it is incorrectly applied to one mere simple Tincture.

A Pharmacopœia now before me, contains fifteen preparations called Liquors. Now what is a Liquor? Most of the preparations called Liqueores or Liquors are Aquæ or Waters, but not all of them. I can not discover why the term Liquor is not equally applicable to Aquæ, Aceta, Vina, Spiritus, Tincturæ, Ætheres, Infusa, Decocta and Succī; and it would not be very far from correct if it were to be applied to Mucilagines, Emulsiones, Olea essentialia, Olea pinguis, Syrupi etc. In a Pharmacopœia before me, I find the name Liquor Arsenicalis. Here is an anomaly as regards the name of the base of the preparation. From this name merely, who could even conjecture what this preparation might be; but who could fail of understanding the name Aqua Potassæ Disarsenitis or Water of Disarsenite of Potassa? This scheme of nomenclature does not distinguish preparations of salts from preparations of elements. According to this method Water of Ammonia should be Liquor Ammonialis; Water of Chlorid of Barytum should be Liquor Barytalis; Water of Calcia or of Protoxyd of Calcium, should be Liquor Calcialis; and I can not discover why Water of the Chlorid of Calcium would not take the same identical name.

In a Pharmacopœia now before me, there are eleven preparations called Solutions. But what is a Solution? I can not perceive any good reason why the term Solution should not comprise just about as many different and distinct pharmaceutic forms as the term Liquor. Indeed all that I have mentioned in connexion with this latter term, may be and often are Solutions. Mucilages and Emulsions are frequently made to hold very numerous medicinal articles in solution. I have known various articles administered in solution in Syrup of Sugar. I have often known Phosphorum administered in solution in a Greasy Oil. Various articles might certainly be administered in solution in the Essential Oils. In fact the Pharmacopœiæ in common use are full of needless anomalies and deviations from the principles by which they should be regulated. These specifications will serve as specimens

of the numerous objectionable things to be found at present in many of our Pharmacopœiæ, which, as appears to me, require amendment, and which therefore I shall take the liberty to amend as far as I am able. I shall endeavor to avoid as many of these as practicable, and shall conform my language to the laws of the nomenclature, rather than to these irregular and often very equivocal usages.

COLLECTION AND PRESERVATION OF THE MEDICINAL PARTS OF PLANTS.

Of annual plants the leaves are usually the most active parts. Some times however their principal virtues reside in the seeds. It is believed to be a general rule—a rule however to which there are doubtless exceptions—that the seeds of those plants whose powers reside in a colored descending sap, instead of some other part of the plant, are commonly destitute of the virtues of such colored descending sap, and indeed are often intirely inert—as is the case with *Papaver somniferum*. *Sanguinaria vernalis* and *Lobelia inflata* are considered to be more or less exceptions to this rule. How many more there may be I am unable to say. But the seeds of those plants whose virtues reside in some other part beside the colored descending sap, generally possess the medicinal powers of the plant, and often in a concentrated form, as is the case with *Datura Stramonium*, *Datura Tatula* and other medicinal species of this genus.

More rarely the medicinal powers of annual plants are found in their flowers. As a general rule, flowers of whatever plant, are cathartic, but they some times possess some other power in addition, as for example a narcotic one, which may prevent their being pushed so far as to purge or even to prove laxative. But I wish to have it understood that I give this as a commonly received opinion wherever I have practised medicine, not as any thing I have ever verified. I have known flowers to prove cathartic often enough, but I never administered any that ever proved narcotic. I should think that the flowers of *Gelseminum nitidum* might prove narcotic; but I never prescribed them, or witnessed their effects when prescribed by any other person, or when taken by accident. If the flowers of as active a plant as this are not narcotic, I should not know in what case to expect such a thing. I should expect that the flowers of *Aconitum Napellus* might

possess the same powers as the rest of the plant, whatever names these powers may take ; but I have never verified it.

The stems of annual plants usually possess much the least, and often no activity. In some instances however, they possess exactly the same powers as the rest of the plant. The more perfectly herbaceous they are, the more likely are they to be active. The roots of annual plants are commonly fibrous, or even subligneous, and altogether worthless, though there are doubtless exceptions to this. These are given as general rules merely, to be followed only in the absence or deficiency of definite and specific information in regard to the individual plants ; but they are doubtless true in most cases, and would very seldom lead to error. There are exceptions however as I have repeatedly said, but these must be studied in connexion with the individual article, to which they relate.

The medicinal parts of annual plants should always be gathered when their vegetation is most vigorous, which is commonly in the early part of the flowering season, though some times in their fullest flowering season. But at all events, leaves, whether of annual, biennial, persistent or frutescent plants, are always best when in the fullest vigor ; and this is usually, though not always, when the plant is in flower. Flowers should be collected as soon as they are fairly expanded. Seeds, as is said, should generally be collected when they are ripe, but some times they are the most active when they have just attained to their full growth, and before they can be said to have ripened. This I suppose is equally applicable to annual, biennial or persistent plants. I suspect that the latter precept in regard to the collection of seeds is much oftener the fact than is known or recognized.

Fruits of whatever plants, which do not consist intirely in seed, are commonly collected when they have arrived at full maturity. Some times however they are preferable in an immature state. What are called in the shops *Aurantia Curaslaventia* or *Curaçoa* Oranges, are the fruit of *Citrus Bigaradia*, collected when about the size of a Hazelnut, and some times as large as a Filbert. They are selected of this size mostly for employment in *Fonticuli* or *Issues* ; but they are also used as an aromatic bitter-tonic, all their parts in the aggregate being taken in conjunction. The parts

or appendages of the axis ascendens of biennial plants are usually, and probably always the most active, during the second season of their growth, and at the time of their flowering. The several parts of the axis ascendens of biennial plants are generally of the same relative value as those of annual plants; though indeed the root or axis descendens is of much more comparative importance. The leaves of *Conium maculatum*, a biennial plant, are affirmed to possess no activity, except in their fullest flowering season. They are declared to be inert both previous and subsequent to this period. Even at the time of their activity, all their power is stated to be confined to the membranous part of the leaves. Of course the leaves of this plant can be of no value the first year of its growth. The seeds of *Conium maculatum* are affirmed not to be active, till they have attained their full growth; and they are declared to lose their activity, when they are so much ripened as to become brown.

I was long in the habit of prescribing the ripe seeds of *Heraclium lanatum*, a biennial plant, without ever witnessing any other effects than what are vaguely and indefinitely called carminative. I recollect laughing at the suggestion of Dr. Jacob Bigelow that "they should be used with caution." (*See Seq. to Pharm. U. S. A. Bost. 1822, p. 203.*) I had seen them used a hundred times to an unlimited extent, without any thing like a narcotic effect. But I was subsequently called to prescribe for a patient under such a degree of ultimate narcosis produced by them, that a distinguished gentleman, at least twenty years older than myself, who happened accidentally to be with me, thought that the patient must inevitably die. This patient had taken a very considerable quantity of as strong an infusion as could well be made of the full grown, but unripe and perfectly green seeds of this plant. On the employment of Alcohol, all the symptoms of ultimate narcosis, intense and dangerous as they seemed to be, vanished carminatively i. e. "like a charm," as many physicians are in the habit of saying, showing conclusively that the case was not as desperate as it seemed at first view. Subsequently I had an opportunity to see two other cases of ultimate narcosis produced by these seeds collected as I have described. In both, the symptoms yielded with equal speed to the use of Alcohol. Since this case, all the seeds, whether of annuals, biennials or persistent

plants, that I have collected myself, for medicinal purposes, I have collected at the period of their fullest growth, but always before they have changed color in consequence of what is termed ripening; and in no instance hitherto have I failed of having a more active article.

When the root of a biennial plant is the part employed in medicine, it should usually, if not invariably be collected in the autumn of the first year, or at any time after the decay of the top, and previous to its sprouting or shooting the second season; for most of these plants grow more or less even during the winter, so as to give the autumn of the first year a great preference over the spring of the second for their collection. The perennial roots of persistent plants should always be collected when vegetation is not active, and therefore in the autumn or winter, or after the decay of the top, and before its growth again commences. Under such circumstances, the secretions are concentrated in the surviving part, and they contain much less watery matter. It is true these roots are not commonly inert at other seasons, though at such times, their powers are feebler and liable to be much more variable and uncertain. In some instances (probably a much greater number than physicians are apprised of) a good deal of particularity is necessary, in order to obtain these roots in their greatest perfection. As a general rule, they are not of the best quality till they have had at least three years' growth. I doubt whether many of them are worth much after nine or ten years duration; but this is a much more difficult point to ascertain, than the period when they begin to manifest their greatest activity, and therefore I can not pretend to lay any thing down with regard to it, with any thing like absolute certainty. At all events it has seemed to me best to reject the roots of persistent plants, that I know to be more than ten years old. However I have no means of ascertaining this, except in cultivated plants. But in the case of wild plants, it is always necessary to examine the roots carefully, and many specimens will be found inferior, and apparently worthless, as would seem, from too great age.

The perennial roots of persistent plants should never be washed. This produces a very material deterioration. They never dry and keep well after it. When first taken out of the ground, all the earth should be jarred or knocked off, as far as is practicable;

and when they are dry to a certain extent, the rest should be removed by a brush. In this way they may be cleansed as effectually as if they are washed, and without any injury to their medicinal qualities. They are better dried by a free exposure to a dry air than in the sun. Exposure to a bright sun for any considerable time, certainly injures some articles very decidedly; and probably it injures a greater number than it has been definitely ascertained to do. The Malate of Coniine, the alleged active principle of *Conium maculatum*, is said, when in a detached state, to undergo speedy decomposition in strong sun-light. If we look through a magnifier at a little of it upon a piece of paper, and in a bright sunlight, it is said that we may see minute bubbles of air separate from it, which are affirmed to be Ammid of Hydrogen or Ammonia, of course composed of the Nitrogen of the Alcaloid united with a part of its Hydrogen. When all the Nitrogen of the Alcaloid is detached, there is said to remain a brown resinoid substance, perhaps a true and proper resin composed of $H.C.+O$. and supposed to contain exactly $H^{11}C^{12}+O^1$ i. e. the Alcaloid minus one equivalent of Nitrogen and three equivalents of Hydrogen. That this or some other decomposition of the Alcaloid takes place while it remains in its proper cells in the plant, when it is kept for a sufficient time in a strong sun-light, is sufficiently proved by the fact that the active parts of the plant soon become inert, when placed in such circumstances. Now the Alcaloid Coniina or Coniine is believed to be much more readily decomposable than many of the Alcaloids, yet this example may very properly be selected to illustrate the manner in which active substances become inert when exposed to the intense rays of the sun.

As respects the exsiccation of the medicinal parts of plants, it is a general rule that an article should always be so well dried as not to be at all liable to become musty or mouldy. More than this is injurious. Herbaceous medicines should never be strongly pressed into cakes, after the manner of the articles collected and put-up by the Shakers. In order to retain the shape into which they are pressed, they must be put-up before they are quite dry enough. It is rare that the pressed medicines of the Shakers are not more or less musty. In addition to this, strong pressure, when the plant is not perfectly dry, ruptures the cells into which the active principles of plants are secreted, and in which they should

remain, unless separated by art for distinct preservation. These cells being ruptured the active principle exhales, or undergoes decomposition. I have never seen a medicine pressed into a cake, that was good for any thing, if its activity depended upon a Volatile Essential Oil of any sort, or a Camphor of any species; not even *Thymus vulgaris* and *Majorana hortensis* or *Origanum Majorana*. Even *Satureja hortensis* becomes worthless when put-up in this manner.

The barks of deciduous-leaved shrubs and trees are invariably to be collected after the ascent and descent of the sap has ceased, in consequence of the fall of the leaves. At this time, their secretions are perfected and accumulated in the liber, the part of the cortex in which the activity of barks mostly, if not invariably resides. Evergreens may have their medicinal parts collected at any time; but they are best when in the fullest vigor of vegetation. This may be laid down as a general rule. But more specific directions are required for the collection of their leaves. The leaves of sempervirent or evergreen plants, that are collected for medicinal purposes, should always be of the growth of the year in which they are collected, and they should not be collected till they have attained to their full size. The old leaves are sometimes, but not always inert. I have found the old leaves of *Ledum latifolium*, as far as I could discover, inert. But the old leaves of *Kalmia latifolia* I have found usually to possess more or less activity, though not as much as the new leaves. Those which become inert, and those which retain their power to the second year of their growth, can be determined only by special observation. It would not be worth any man's while to be at the trouble of making such observations, were it not for the fact that an additional supply of an article is some times needed, before the new leaves have acquired their full growth.

None of the medicinal parts of plants should ever be washed at the time of their collection. As I have already said of roots, they never dry as well after washing, as without it; and when they become dry after being washed, I have always found them to be more or less deteriorated, more in some cases and less in others. In all cases when an article is liable to deterioration by keeping, a new supply should always be obtained every year, and what has been kept over the year should be thrown away. There

may be an exception to this, when the article is either brought from a great distance, or is very expensive, since a deteriorated and inferior article may be better than none.

Drawers shoving into boxes that are as near air-tight as possible, some times with the addition of a cover sliding closely in a groove in the sides and near the top of the drawer, which may be pushed backward when the drawer is opened but is closed when the drawer is pushed into its box, are highly useful for the best preservation of some of the most delicate medicines. I once had a case of drawers made in this manner, with the addition of a pair of close doors in front of the drawers. So tight was the whole work that not even the scent of the medicines was ordinarily perceived in the room in which the case was kept; and one medicine was not contaminated by the odor of the rest. In this way the medicines were kept more satisfactorily than in any other, with which I was ever acquainted. Were I to live my professional life anew, and be obliged to dispense my own medicines, I would never keep them in any other manner. As medicines are kept in the shops of our druggists and apothecaries, it is no wonder that they so often disappoint expectation. The wonder is rather that they do not disappoint far oftener than they actually do. When a medicinal vegetable substance is properly dried, it should at all events be kept as perfectly from the influence of air and light, as is possible.

I have repeatedly been informed by retail druggists and apothecaries themselves, that when they made their purchases of the wholesale dealers, they never pretended to make any examination and selection of articles, but simply presented a list of the things wanted, and always had them put-up by the wholesale dealer, who would do it at the cheapest rate, without the least reference to quality or purity, or in other words, freedom from adulteration, or deterioration from long keeping and exposure to atmospheric influence and to light. Such being very often, if not generally the fact, I think it must be evident that it is high time that there should be a reformation. If there ever is a reformation, I think there can not be the least doubt that it must depend primarily upon the action of practising physicians.

SOURCES OF MANY MEDICINES UNKNOWN.

It is a remarkable fact, of which no physician should be ignorant, that there are still many articles of the materia medica, some of which have been in use from the earliest records of medicine, whose source is not known, and whose natural history has been in no degree investigated. An author notices this fact in the following terms. I quote mutatis mutandis, for the writer all along has reference to the arts and diet, though his remarks are equally applicable to medicine. He says "here we should remark the exceedingly imperfect state of our knowledge with regard to the" (natural history of plants, which produce many of the articles of the materia medica, so extensively employed in the practice both of Europe and America.) "It might be supposed that there would be no more difficulty in finding the botanical names" (and habitats) "and" (in) "deciding the species" (and genus) "of the foreign" (articles in general use) "than in pointing out the Oak that is used in ship-building, and the Pine" (that is employed) "in the construction of houses; but the contrary is the fact." "The attention of the botanists" "has not been" (very particularly) "directed" to this point, and the commercial dealers in these" (articles) "have paid no regard to it." "It would be well, in this age, when natural history is so much cultivated, if naturalists" "would combine their experience upon this subject, and supply the deficiency." Very little) "knowledge of the matter can be procured in books; and we have consulted practical botanists, and commercial men, without obtaining any information that could be depended upon, though both agreed in mentioning that a subject of such general interest should have been left so intirely without investigation." Even) "though no important results to science should procede from such inquiries, it is certainly humiliating not to be able to tell with precision where those" (articles) "are produced, and what vegetable species furnishes them, which are so useful" (in our daily practice. (*Libr. Intert. Knowl. Prt. I. Vol. II. Vegt. Timb. Trees, p. 177-178.*) I do not think that the greatest difficulty of such investigations exists exactly where this writer seems to suppose; nor do I think that it is so much a combination of the researches of botanists that is needed, as it is time, and patient perseverance of individual botanists. For example—before the source of the officinal

Jalap was known, what would have been the proper and necessary course to obtain the desired knowledge? The botanist who undertook this would of course have been obliged to embark for Vera Cruz, the Mexican port at which European traders obtained the article under investigation. Here it would have been ascertained that the article was brought to Vera Cruz immediately from Jalapa or Xalapa. When at Jalapa or Xalapa it would have been found that it was brought to that place by some of the aborigines, from a considerable distance in the interior of the country, no body would have known where, and only at a certain season of the year. The botanist would then have been obliged to wait till this time of the year arrived, and he would have accompanied the aborigines bringing the article to market, into the interior. Perhaps the time of the arrival at the place of the growth of the plant would not be the proper time to find it in a state of active vegetation. For this he would be obliged either to wait, or he would be compelled to return home, and subsequently make a new journey thither.

Because this article has the popular name Jalap, it is supposed that it must of course grow about Jalapa or Xalapa; but I know of no sort of evidence that such is the fact. Jalapa or Xalapa is merely the place to which the aborigines, who collect it, bring it to market. Its habitat is said to be "on the eastern declivity of the Mexican Andes near Chiconquiaco, and near San Salvador on the eastern face of the Cofre de Perote, at an elevation of about 6000 feet above the sea" (and) "on the mountains near Orizaba." (*Lindley*.) Now where these places may be, I know not, but I think they can not be near Jalapa or Xalapa. It is but a short time since the source of the officinal Jalap was ascertained, and perhaps even now there may be some doubt as to the genus to which the plant producing it actually belongs. Wenderoth, Zuccarini and Nuttall referred it to *Ipomœa*, and Lindley adopted this reference in his "*Flora Medica*" (London 1838) but in his "*Elements of Botany*" (London 1841) he refers it to *Exogonium*. This course, or some thing very like it, would have been necessary in order to ascertain the source of the officinal Jalap, before there was any clue to its habitat. Now an individual botanist can not well afford to employ so much time and labor, and to expend so much money upon researches for a single plant.

Before the source of the officinal Kalumb was at all known, a botanist would have been obliged to go to the port of Ceylon, where European traders are said once to have obtained it exclusively, and thence to have accompanied the traders westward again to Mozambique in Africa, with how many intervening trading stations I know not.

No scientific naturalist has ever seen the plant which produces the officinal Rhubarb commonly so called. It is not even certainly known that it is a *Rheum*. It is only presumed to be the product of some species of this genus, from its strong resemblance to several well known and well ascertained species. If the investigation of the source of the officinal Rhubarb were now to be undertaken, it would be necessary to follow it in the course of trade, till we should arrive at some place where it is purchased from those who actually collect it; and then it would be necessary to obtain the guidance of these collectors to its actual habitat; and this too must be exactly at the right times of the year to find it in flower and also in fruit.

It is not yet ascertained what genus and species produces the officinal Gamboge. There has been much discussion of this subject, and a great deal of argument in favor of different species of several genera; but the matter can never be decided with any certainty, till some botanist has visited the country in which it is collected, and has actually examined the tree which produces it, and this both in its flowering and fruit season.

We are still in the most profound ignorance of the source of the inspissated descending sap called Galbanum. There are several plants which produce something like it, but still not the identical article.

The source of the inspissated descending sap called Sagapenum is yet intirely unknown. Conjecture has selected several articles; but there would seem to be positive proof that the product of each of them differs essentially from the article in question.

The sources of the several sorts of Peruvian-bark to which the distinctive denomination Red-bark is applied are intirely unknown. In fact I do not think that any botanist can refer any sort of Peruvian-bark now found in the shops, with the least degree of certainty to the species which produces it. This ought to be so, in regard to such an important and unique article.

It is not yet certainly determined what species of *Smilax* produce the several sorts of *Sarsaparilla* found in the shops.

So far as I have information, no man can accurately and correctly refer the several sorts of Extract of *Aloë* to the respective species of *Aloë*, which severally produce them.

So far as I know, the several styptic extracts, found in the shops under the barbarous name of *Kino*, can not yet be referred with any certainty to the species and genera which produce them respectively.

There are several different sorts of *Oleiresin* called *Copaiva* in the shops; but can any man refer these respectively to the several species of *Copaïfera*, or any other genus, which produces them?

Till within a short time, the seeds known in the shops under the name of *Sabadilla*, were supposed to be produced by *Veratrum Sabadilla*, and the Alcaloid which they yield was accordingly named *Veratrina* or *Veratrine*. Now it is considered a determined fact that the seeds in question belong to *Asagraea officinalis*. But as appears to me, the subject will not be relieved of all difficulty and obscurity, till the seeds of *Veratrum Sabadilla* are examined and better known.

The officinal *Ipecacuanha* was first known to Europeans about the year 1649, and it came into use about the year 1686, and yet as late as 1805, its source was not well determined. In this last mentioned year Persoon's *Synopsis Plantarum* was published, in which it is referred to *Cephaëlis emetica* (Persoon) which he supposed to be identical with *Psychotria emetica* (Mutis) and *Callicocca Ipecacuanha* (Brotero.) I believe that Richard's description of the genuine article under the name *Cephaëlis Ipecacuanha* was not published till about 1818. As late as 1810 a London public instructor of botany and materia medica published a work upon these subjects in conjunction, in which he headed his article upon the officinal *Ipecacuanha* with the name *Viola Ipecacuanha*, I suppose, of Linnæus. Now Persoon was under a great error in regard to this matter, and the other author under still greater, even at the late periods at which they wrote.

I repeat the question, what botanist can afford so much time and toil and expense for ascertaining a single plant, unless he is employed, sent-out, supported and paid by some government, or

some scientific body with ample funds for just such purposes? The catalogue of articles whose source is either intirely unknown, or of whose origin we have only an imperfect knowledge might easily be very greatly extended. But the specifications that I have already made will suffice. It is not without probability that some articles are wrongly referred, of which at present we entertain neither doubt nor suspicion. In a word, our knowledge of the natural history of the materia medica is still very far from being complete or perfect in any single respect. It still requires diligent and unwearied investigation for errors that have hitherto escaped all our vigilance.

MATERIA MEDICA

OR

PHARMACOLOGY.

CLASSIFICATION.

I HAVE never happened to be acquainted with any person to whom express and formal classification in any department of human knowledge was not distasteful at least, and generally positively irksome. It can not be expected that every man should be able to know a Linnæus, a Jussieu, a Cuvier or a Lindley. When I first became a public instructor, I had numerous friends, the oldest of which were twenty or more years older than myself, and the youngest of which at least ten years older. These gentlemen had been my friends from my professional pupillage, and were ardently desirous that I should be not only useful, but also popular. Towards the latter object, they urgently advised me, without any solicitation on my part for advice, to dispense with all express and formal classification, on the ground of its unpopularity. It had not occurred to one of these gentlemen that calling articles antiphlogistics, narcotics, tonics etc. was in fact classification. Such being the fact, I have made repeated attempts to dispense with it, but always in vain. There is one sort of classification that is absolutely unavoidable in the materia medica viz. that founded upon the powers of the articles. We must mention the several agents of the materia medica, as antiphlogistics,

narcotics, tonics, styptics, diaphoretics, diuretics, emetics, cathartics etc.; or if we avoid the use of these terms, we must use their definitions instead, or some other periphrasis or circumlocution.

It is to be particularly observed of the classification founded upon the powers of the articles, that it can not possibly be avoided, whether we pretend to dispense with all classification, or to adopt any other of the several classifications that have been adopted by any writer upon the materia medica. Even Homeopathsists,* who appear, or affect to consider every medicinal agent as perfectly specific, and who do not seem to think that any two articles ought by any means to be brought together and associated into a group, and who seem to make the greatest efforts to avoid all the terms in common use expressive of power, are obliged to mention "Diarrhœa" as the effect of one agent; "frequent papescent stools" as the effect of an other agent; "purging stools" as the effect of an other; and "painful purging" as the effect of an other still. Now all of these articles might be called cathartics in one word. Among all the homeopathic writers on the materia medica, whose works I have ever had opportunity to examine, there has always been the same laborious effort to avoid all terms expressive of power, and consequently of classification founded

* 'Ομοιοπάθεια, ας, ή, or in the Roman alphabet, homœopatheia, means "likeness or conformity in feelings, affections or passions," and consequently it means "sympathy, connexion or agreement in nature, condition or properties." This term is also used as the name of "a figure in rhetoric by which an appeal is made to the sympathy of an other." (See *Donnegan's Greek and English Lexicon*.) 'Ομοιος or in Roman letters, homæus, by itself means "like; resembling; equal; the same;" while πάθεια, or in Roman letters patheia by itself denotes affection; passion; suffering; disease, etc. (*Donnegan*.) Now I never could discover the propriety or sense of the application of the term homœopatheia or homeopathy, that is the same disease, to the views, notions and practice of what is so called. 'Αλλοπάθεια, ας, ή, or in Roman letters allopatheia, means "the state of suffering from an other." (*Donnegan's Lexicon*.) 'Αλλος, or in Roman letters allos by itself means other, and hence allopatheia or allopathy, may be said to mean etymologically, other affection, passion, suffering, disease, etc. Now I never could discover the applicability of such a term to that practice of medicine, which (as far as our knowledge extends) is founded upon an acquaintance with pathological conditions of diseases, also with the powers, operations and effects of medicines, and an adaptation of the latter to the counteraction, relief and cure of the former. I therefore utterly repudiate this term for all those, to whom it is applied by those who call themselves homeopathsists. This sect have a right to call themselves what they please, but it is not the part of gentlemen to stigmatize others with such an absurd denomination.

on power, but so far as the writer has been intelligible, he has always expressed, periphrastically at least, what other authors mean by emetic, diuretic, diaphoretic, styptic, tonic, narcotic, etc.

It may perhaps be supposed that an instructor or writer on the *materia medica* may go on, and employ these terms in a sufficiently intelligible manner without definition or limitation, explanation or comment; but such is not the fact, as I very well know from repeated trials. There is much—very much that is required to be said of the several groups of articles designated by the preceding terms expressive of the powers of the *materia medica*, that does not belong to any single article of the group, but rather to the whole, which however can not be repeated with each individual, and will not answer in connexion with only one, but must be said by itself, and is really required preliminary to the consideration of all the articles of the group. As I became more conversant with the business of instruction, I found that it was not sufficient barely to specify, limit and define the classes of medicines which I felt myself constrained to adopt, but that it was necessary to defend them against opposing and incompatible classes. The truth can not always be established without a refutation of competing and established, or at least commonly received error. As I acquired experience in the business of instruction, I could not fail of being convinced that Cuvier was abundantly justified in saying that, “the greatest service, which can be rendered to science, is to clear the ground before we proceed to build; to begin by demolishing those phantastic structures, by which its avenues are choked, and which deter from such occupations, those who have happily become habituated, in the exact sciences, to yield only to evidence, or at least to class propositions according to their degrees of probability.” After I had found it impossible to dispense with the sort of classification just referred-to, I was very often admonished for bestowing too much time upon the subject of what is peculiar to myself—not only to its explanation and illustration, but to the reference of the several articles, of which I treat, to their places in the system, all of which is alleged to be a matter much more of curiosity than of utility. It will be exceedingly easy to show that this objection to my course is founded intirely in misapprehension and error, and in fact upon

absolute ignorance of the method in which I carry it out. As a matter of fact, I devote only an exceedingly short time and space to what is my own merely, in the subsequent classification. A less time and space would not serve to explain any system or method whatever, that has ever been devised by any author or practitioner within my knowledge. In fact the whole explanation of it summarily and independent of definitions consists in the statements that 1. I make just as many classes as there are different and distinct powers, operations and effects, in the *materia medica*. 2. These classes are arranged among themselves according to the affinities and analogies of the several different and distinct powers, operations and effects, upon which they are founded. 3. The individual agents under each class are arranged among themselves according to the number and identity of the powers which they happen to possess, and according to their general affinities and analogies. In reality, this constitutes the whole of the principles of my classification, and the exposition of these constitutes all the explanation, which they need, or which I have ever been in the habit of giving. As appears to me, the reader who considers this as fatiguing or burthensome, or as occupying too much time, must be very easily wearied, or exceedingly scrupulous about the employment of his time. After this statement in regard to my own classification. 4. I pass-on to definitions and descriptions of the several different and distinct powers, operations and effects of remedies, on which my classification is founded, but which in reality make no part of such classification, and which would be just as necessary, if I had no classification at all, or if I followed an alphabetical order, like that of the *Dispensatories*, or a natural history order, like that of *Linnaeus*, or of *John Andrew Murray*. My friends, who supposed that I spend too much time upon my own classification, must undoubtedly have considered the matter so slightly and so superficially, as to reckon this in fact a part of my own classification, which certainly it can have no sort of pretension to be. The truth is that the different and distinct powers, operations and effects of remedial agents, in fact constitute the whole of what is strictly and properly *materia medica*. The natural history and the composition of medicines belong to botany and chemistry; and their application to the treatment of disease belongs to therapeutics, as their best prepa-

ration for administration belongs to pharmacy. Those who consider the powers, operations and effects of remedies as belonging to my classification, in reality ascribe the whole of *materia medica* proper to it—what, I trust, all will acquit me of the arrogance of claiming, and what no body, who understands the subject, will ever be willing to concede.

In the most general terms, classification is an arrangement of subjects or objects according to their resemblances or affinities, and their diversities or dissimilarities; and consequently it is only a prominent and striking display of resemblances or affinities, and of diversities or dissimilarities. In more specific terms, classification is any distribution of the subjects or objects of science into groups, each of which has some character common to every individual of such group, and peculiar to the group as an aggregate. A strictly natural classification is called a system by the best authorities on the subject; while a purely artificial classification is called a method. By very general consent classification, in most branches of science, and particularly in natural history, consists, at the present day, of at least four grades, with the lowest of which, it is necessary to begin in the formation of a system. In the departments of natural history generally, the four grades in question are denominated species, genus, order and class. In some cases, subordinate to species, it is necessary to have sub-species, variety and sub-variety; subordinate to genus, sub-genus; subordinate to order, sub-order or tribe; and subordinate to class, sub-class or cohort. Species, the lowest of the four principal grades, to which I now refer, always constitute the proper basis of classification in all cases; and upon the accurate determination of these, it is rightly supposed that the perfection, the utility, and even the stability of a system of classification must, in a great measure depend.

Without classification of some sort, it is obvious that the subjects or objects of human knowledge would be vastly too numerous for the comprehension of most capacities, and altogether too various and diversified for most memories; and accordingly, wherever science has been cultivated, this aid has been deemed not only useful, but indispensably necessary. At all events, either an express or implied classification has always been employed in the *materia medica*; and in fact, I have no knowledge of any

writer, teacher or practitioner, who has ever dispensed with it. Even in Dispensatories, in contradistinction from Pharmacopœiæ, in which the only professed arrangement is merely an alphabetical order, there is always, and at every step, a perpetual recognition of even a refined classification, since all medicinal agents are constantly treated of as emetics, cathartics, diuretics, diaphoretics, expectorants, emmenagogues, tonics, astringents, narcotics, anthelmintics, etc. In reality, there is no possibility of treating of the *materia medica*, at least in an intelligible manner, without either a formal, or an implied classification. I shall therefore consider the utility, and even the necessity of classification in the *materia medica*, as established; and accordingly shall proceed to an examination of the several methods or systems that have been or may be employed.

As none of the preceding and just mentioned denominations of the divisions in classification are employed in the *materia medica*, except merely the term class, it is not necessary for me to define them in this place. It may however be well to remark here that the individual substances, articles or agents, which compose the *materia medica*, are the true species in this department of medicine; that the grouping of the individual articles of the *materia medica*, which is subordinate to what is always called class in this branch, seems to be equivalent to what is called sub-genus in other departments of natural history; that which is called class in *materia medica*, corresponds to what is called genus in the other departments of natural history; that the grouping of what are always called classes in this branch, for which there is only a very slight foundation, corresponds to what is called order, in the other branches of natural history; and that, in fact, there are no true classes in the *materia medica*, in the sense in which the term class is employed in natural history generally. So peculiar however is the *materia medica*, that I do not think it would be at all acceptable to the medical profession, or independent of this, at all convenient for practical purposes, to consider all articles possessing the same power, as constituting a genus, of which genera we should have only between twenty-one and twenty-three in the whole; nor could we very well group these twenty-one or twenty-three genera into orders, since at the most, we could only have between four and six such orders, and at least two of these contain-

ing only one genus. It really seems that the common sense of the medical profession has been decided, time immemorial, in reckoning all articles, that possess the same power, as belonging to one class, the name of which is always derived from such power. This seems to have been done by all medical writers and practitioners, and in a majority of cases, without any apparent consciousness that they were employing any classification or any system. This has been done even by authors who have disparaged, and as they seemed to think, discarded all classification and all system, and who have professedly treated of the subjects in alphabetical order merely.

As far as my knowledge extends only five methods of classification have ever been employed in the *materia medica*.

I. From the parts of the articles employed—a method now very generally exploded. According to this method, all the roots which are employed in medicine, would constitute one class; all the barks, another; all the leaves, another; all the fruits, another; all the exuded-inspissated saps, another; and perhaps even the expressed-inspissated juices, another. The former employment of such a classification contributes to explain a curious usage in the *materia medica*, viz. that the popular name of many medicines is often the genitive case of its name in Latin. I have seen many books, in which the classes were *radices*, *cortices*, *herbæ*, *folia*, *flores*, *semina*, *succi*, *extracta*, etc. followed by the names of the simples in their genitive cases. At the same time I have known very many physicians, who always called Rheum by its genitive case *Rheï*; Pulegium by its genitive case *Pulegii*; Marrubium by its genitive case *Marrubii*; Lupulus by its genitive case *Lupuli*; Sinapis by its genitive case *Sinapios*; Opium by its genitive case *Opii*; Aloë by its genitive case *Aloës*; etc. An other reason for this usage doubtless is the fact that the names of all medicines always stand in the genitive case, in all correct prescriptions. I need not pause to describe the merits (if there are any) or the demerits of this mode of classification, since they will at once be sufficiently obvious; and above all, since it is a method, that has now gone-out of use wholly and intirely, in all probability never to be revived.

II. From the proximate principles in which medicinal powers reside. According to this method, all the essential or ethereal

oils would constitute one class; all the resins-proper, an other; all the bitumens, an other; all the greasy oils, an other; all the salifying principles, an other; all the salifiable bases, an other; etc. This mode requires no more discussion than the preceding, and for the same reasons. In some instances it would coincide with the classification founded upon powers, but in general it would bring together articles the most heterogeneous in this respect. What possible group could be formed, that would vary more as respects powers than the Alcaloids. But no body uses this mode at the present time, though I have not infrequently heard it mentioned by various physicians, as a method which they considered as peculiarly eligible.

III. From chymical, mineralogical, botanical and zoölogical, or in one word, natural history affinities—a method which is still in great favor with mere naturalists, who are not practitioners of medicine, and even with many, who are practical physicians. According to this method, one or more of the natural orders of the vegetable kingdom, for example, would constitute what is called a class, and one or more natural groups of chymical substances would constitute an other. The popularity of this method, with a certain highly learned class of writers, entitles it to a more detailed consideration, than its true merits would otherwise deserve. But such a consideration has in fact been bestowed upon this very topic, in the introduction to this work, under the head of “Natural History Affinities as a Means of Determining the Medicinal Powers of New and Previously Unknown Articles.” Therefore I shall say nothing further upon it in this place.

IV. From the subordinate parts of the system, upon which remedies operate. A brief review of this method will clearly exhibit its imperfections and inconveniences, as a practical classification of the articles of the *materia medica*. Upon this plan, how many classes should we have in the whole *materia medica*? Those who adopt it I believe, have a greater or less number of the following classes, though perhaps not arranged in the order in which I shall name them. At all events I have known all of these supposed classes recognized, if not by any one individual author or practitioner, at least by different authors and practitioners.

1. Articles which act upon the nervous system. We will suppose that there is to be a class of remedial agents that produce

their primary sensible effects upon the nervous system. Such class, as appears to me, would comprehend what I call leantics (i. e. the demulcents and emollients of authors in conjunction) what I call neuragics (of which Lead is the type) narcotics, what I call erethistics (of which Strychnos Nux-vomica and Aconitum Napellus may be taken as types) and what I call euphrenics (of which Protoxyd of Nitrogen may be taken as the type.) As appears to me, the union of these several groups of remedial agents into one great class, would be of no sort of practical utility, and on the other hand, would interfere-with, and prevent, or at least contribute to prevent, wholesome discrimination in the selection of remedies for particular cases. Certainly these several groups of agents have little, if any thing, in common, except their operation upon the nervous system. Now I would greatly prefer subdividing some of these groups, to a union of any two or more of them. For example, the large group of agents commonly called narcotics, whose primary effects are manifested in the nervous system exclusively, always affect certain particular parts of the nervous system much more than they affect other parts, although they affect the whole in a greater or less degree. Shall those narcotics which affect the involuntary motor nerve of chymical action nutrition and reproduction more especially, and which destroy life by wholly suspending its functions, while the functions of the other subordinate parts of the nervous system continue to be carried-on for some little time more or less afterwards, be reckoned a class? Shall those narcotics which affect the involuntary motor nerves of expression more especially, and which destroy life by wholly suspending its functions, while the functions of the other subordinate parts of the nervous system continue to be carried-on for some little time more or less afterwards, be reckoned a class? A subdivision of the group of agents commonly called narcotics, according as they operate more especially upon one or an other subordinate part of the nervous system, would unquestionably contribute more or less to useful practical discrimination; while uniting and merging into one, several groups differing as much as those which I have mentioned as never the less producing all their effects upon the nervous system generally, could not fail of hindering sound practical discrimination, at least in a greater or less degree. Again, I doubt not that a grouping of the narcotics,

according as they produce clonic spasms, subtonic spasms and exquisitely tonic spasms, would be more or less useful practically. All my own observations, and all the testimony that I could ever obtain from friends, on whom I could place the greatest reliance, has uniformly contributed to prove that no individual narcotic ever produces more than one sort of spasms. I never could find the physician who has ever seen any but clonic spasms from *Cicutā maculata*, *Camphora officinarum*, *Myristica officinalis*, etc. and there are the best reasons for concluding that no individual narcotic produces but one sort of spasms. I have very many times happened to be called where some good woman had been wiser than physicians, and had administered *Nicotiana Tabacum* in the form of enema, to some child, for the destruction of intestinal worms, the existence of which no physician deemed sufficiently probable to justify an anthelmintic course. I always found the most intense clonic spasms; and I have received testimony to the same effect, from several physicians. I believe all articles of the *materia medica*, that ever produce spasms at all, whether they are narcotics or not, always produce the same sort. Who ever saw any but tonic spasms from *Ignatia amara*, *Strychnos Nux-vomica*, or any other of the *Strychni* with analogous powers? Now I doubt not that a knowledge of the sort of convulsions or spasms, which an article is capable of producing, when it produces any, ought in many cases to indicate or contra-indicate the use of such article. So far as practical utility is concerned, further subdivision of the received classes would undoubtedly be preferable to a union of any two or more of them, especially if the classes united had different, and much more, opposite powers.

2. Articles which act upon the sanguiferous system. I do not think there are any articles that operate exclusively upon the sanguiferous system. Some antiphlogistics operate primarily upon the sanguiferous system, and secondarily upon various other subordinate systems, and in the end, upon the whole. Some antiphlogistics operate primarily upon the organs of primary digestion, and secondarily upon various other subordinate systems, and in the end, upon the whole. Perhaps the group of agents that I am in the habit of calling antisbestics may be said always to operate primarily upon the sanguiferous system, and secondarily upon various other subordinate systems, and in the end, upon the whole.

It is equally certain that some tonics act primarily upon the organs of primary digestion, and secondarily upon the sanguiferous system, and in the end upon the whole system. There could be no propriety in considering such agents as these, as agents that operate merely upon the sanguiferous system. I do not think it would be expedient to associate into one class those antiphlogistics, antisbestics and tonics, which act primarily upon the sanguiferous system; and into an other class those antiphlogistics and tonics which act primarily upon the organs of primary digestion. This would be an association of articles widely different, and a separation of articles strongly alike.

3. Articles which act upon the alimentary canal, or in other words the chylopoiëtic system. Those tonics that primarily increase appetite and digestive power may be said to act upon the alimentary canal or the chylopoiëtic system, but they act upon other subordinate parts of the system in addition. Papaver certainly acts upon the alimentary canal or the chylopoiëtic system, since it certainly diminishes excessive secretion from the mucous follicles, and lessens downward peristaltic action, whether it is inordinate or natural, though it acts powerfully upon various other subordinate parts. Nausiatica or nauseants exert their primary influence upon the alimentary canal or the chylopoiëtic system; but they also affect other subordinate parts in addition. Both emetics and cathartics may be said to act primarily upon the alimentary canal or the chylopoiëtic system; but the action even of these is extended to several other subordinate parts in addition. Now I should not think of reckoning any of these articles as medicines that act upon the alimentary canal or chylopoiëtic system; but if not what shall we rank in such a class? I know of nothing else better deserving of such a place. Again if we are to rank these articles under such a class, what sort of a group should we have? We might as well rank a Horse, a Sparrow, a Snake and a Clam in the same Zoölogical class.

4. Articles which act upon the secernent and absorbent or glandular system. We will suppose that there is to be a class of remedial agents, that produce their primary and sensible effects either upon the whole, or a part of the secernent and absorbent or glandular system. In the classification that I am now considering, it must be recollected that the class depends intirely upon the

parts operated upon, and not at all upon the quality of the effects produced. Consequently in determining what the class shall comprise, the latter is not to be inquired after. If this were to be done, it would make it a classification founded on powers, operations and effects. That group of agents which I have been in the habit of calling adenagics, may be said (without any reference to the quality of their effects) to operate upon the whole, or a greater or less number of the parts of the secernent and absorbent or glandular system, though different individual articles operate with very different degrees of intensity upon different parts. I believe that the number of individual agents of this class is few, which do not fail of operating upon some particular part or parts of the glandular system, a greater or less number in different cases. On the other hand the true proper and mere diuretics, diaphoretics, blennagogues, and also the errhines and sialagogues, have their operation confined to a single individual part of the secernent and absorbent or glandular system. How many classes shall be made of these groups? They all operate upon the secernent and absorbent or glandular system, some upon only one part of it, some perhaps upon the whole of it, and some in all probability upon every possible proportion of it between these two extremes. If then we were to reduce to one class all those articles whose operation is upon a greater or less portion of the secernent and absorbent or glandular system, such class would certainly comprise all those agents which I call adenagics, diuretics, diaphoretics, blennagogues (if there are any) emenagogues (if there are any) errhines and sialagogues. Even the styptics operate in part upon the secernent and absorbent or glandular system, though I do not think sufficiently to require their association with the groups last specified, even upon the plan of classification that I am now considering. We may as well comprise in this class those agents that operate only upon a single part of the secernent and absorbent or glandular system, as those that operate upon half or two thirds of it. Now I really can not discover what practical utility there could possibly be in such a grouping of remedial agents.

5. Articles which act upon the dermatic or dermatine system so called, or in other words the skin. I do not think that there are any articles that operate exclusively upon the skin, though such an opinion is entertained by some, perhaps by many. The

true and proper diaphoretics act exclusively upon those excretories of the skin, whose function it is to carry off the effete heat of the system in a latent state, and in such a state of combination with water as to constitute vapor; and it is from this operation that we call them diaphoretics or hidrotagogues. But the diaphoretic or hidrotagogue excretories of the skin are a part of the secernent and absorbent or glandular system; and if we have a class of medicines operating upon this, the diaphoretics or hidrotagogues must be merged in it. Those agents which, by internal use, cure cutaneous diseases, are commonly said and believed to act upon no other parts? If I can trust my own observations and investigations, all of these articles act also upon the whole, or rather very nearly the whole of the secernent and absorbent or glandular system, and of course, under the method of classification that I am considering would fall into the great class that does this. But the operation of this last group is upon a very different subordinate part of the secernent and absorbent or glandular system, from that on which the diaphoretics or hidrotagogues operate; and the immediate operative effects, or in other words the manifestations of their operation are essentially different, and so of course must be the quality of their effects. Now I think that uniting the diaphoretics or hidrotagogues with the antipsorics or articles that by internal use cure cutaneous eruptions, would contribute to hinder, rather than to promote, proper and judicious discrimination in the application of remedies.

6. Articles which act upon the mucous membranes. I do not now certainly know of any articles that operate immediately and exclusively upon the mucous membranes generally, directly increasing the secretory activity of the mucous follicles; but I am strongly inclined to believe that there are such articles yet to be discovered. I esteem it quite certain that among the articles acting in the peculiar manner that I call adenagic, upon the secernent and absorbent or glandular system, there are some agents acting more in proportion upon the mucous follicles; and when this is the fact, it is commonly found that there are articles operating immediately and exclusively upon the excretory most affected by such adenagics. But admitting that there is such a class of agents as blennagogues, I should think that upon the plan of classification I am considering, it would be necessary to merge them

in the class that operates upon the secernents and absorbents or glandular system. Many articles act, as I suppose, indirectly upon the mucous membranes; but I conclude that such would not be put by any body into the supposed class, that I am considering. For example, I have often known *Blennorrhœa nasalis*, *Blennorrhœa bronchialis* and *Blennorrhœa vaginalis*, speedily and rapidly diminished by a sufficiently free internal use of Disulphate of Oxyd of Quinum, Protonitrate of Silver, *Cantharis vesicatoria*, *Capsicum annum*, etc. in various modes of conjunction. This is certainly an operation upon the mucous membranes, though as I have just said I believe it to be an indirect one.

7. Articles which act upon the hepatic system. I never yet met with any articles that appeared to act immediately and exclusively upon what is called the hepatic system, which, I suppose, means the liver and gall-bladder, directly increasing their secretory activity. One reason perhaps why there should be no true and proper cholagogues is what I suppose to be a fact, viz. that bile is a periodical secretion, i. e. it takes place only while food is undergoing chymification and chylication in the organs of primary digestion. Whether gall is a periodical secretion or not, I am unable to say, but I think it quite probable that it may not be excreted except during the digestion of food, whatever may be the fact as respects its secretion. Are there any agents that ever act immediately and exclusively to increase directly any periodical secretion? I can not now think of any. I believe however that most of that class of articles which I call adenagics do, under certain circumstances, directly increase the secretion both of bile and of gall; but in all cases and without exception, they do this as a part of a universal, or very nearly universal action of the same character upon the whole, or very nearly the whole of the secernent and absorbent or glandular system. So invariably has this been true under my observation, that I have long esteemed the fact that an article increases the secretions of the hepatic system, as the best evidence of its true and proper adenagic power. But were a group of articles to be well ascertained, that should act immediately and exclusively upon the liver and gall-bladder, directly increasing their secretory activity, such articles upon the plan of classification that I am considering, would require to be merged in the class operating upon the secernent and absorbent

or glandular system, since the liver and the gall-bladder are certainly parts of that system, and a class founded upon the system affected could not be broke-up and divided, according as a greater or less portion of the system was affected.

8. Articles which act upon the uterine system. If there were any articles that acted directly and immediately and exclusively to increase the activity of the catamenial excretories, I suppose they would be considered as acting upon the uterine system. I have no knowledge however of any true and proper emmenagogues. All the articles commonly so called, act upon the catamenial excretories only as a part of a general action upon the secernent and absorbent or glandular system. When I say this, I do not forget that *Chenopodium Vulvaria* and *Caroxylon foetidum* have been lately alleged to be true and proper emmenagogues. This is perhaps true, but I think it requires confirmation. I have always been inclined to believe that true and proper emmenagogues would yet be ascertained on thorough investigation. The catamenial excretion is more eminently periodical than any other in the whole animal economy; but how far this affects the probability of the future discovery of true and proper emmenagogues, I am not prepared to give an opinion. But even if there were true emmenagogues, they would require to be merged in the class that acts upon the secernent and absorbent or glandular system, since the catamenial excretories are a part of this system. I have very strong doubts whether any such articles are now known, but the subject requires further investigation. *Papaver* apparently acts upon the uterine system, since it appears to diminish uterine contractions or parturient efforts directly and immediately. I can not however persuade myself that a classification would be either useful or judicious, that should associate into one class true and proper emmenagogues, provided there are any, true and proper ecbolics, provided there are any, and the individual article *Papaver*.

9. Articles which act upon the uropoiëtic system. I have known this mentioned as a class of medicines; but I know not what it can comprise, except the true and proper diuretics or uragogues, unless it may be *Papaver*. The true and proper diuretics or uragogues act immediately and exclusively upon the kidneys, directly increasing their secretory activity; but as the kid-

neys are a part of the secernent and absorbent or glandular system, this would reduce such agents to the class which acts upon the secernent and absorbent or glandular system. Papaver if taken with sufficient freedom, directly diminishes excessive secretory activity of the kidneys, and even their natural and healthy activity to a greater or less extent. I do not know of any other articles, except those which act upon the whole of the secernent and absorbent or glandular system, that can be reckoned as acting upon what is called the uropoiëtic system. Now I can not discover any good foundation for considering these, or any portion of them, as a class of the character under consideration.

10. Articles which act upon the muscular system. I have no knowledge of any agents that can fairly be considered as acting upon the muscular system, unless it may be Ignatia amara, Strychnos Nux-vomica and the other Strychni of the same powers. But so far as I know, these articles are at present by almost universal consent reckoned as acting upon the nervous system. Which may be the most correct view of the matter I shall not here pretend to decide, but shall for the present leave it to the advocates of this method to have it whichever way, they can agree upon. I believe that some writers have considered the whole group of articles commonly called tonics, as acting upon the muscular system, while others have considered only a part of them as doing this; both, as appears to me, without just foundation for their opinion. Ignatia amara, Strychnos Nux-vomica and the other Strychni with similar powers, are indeed tonics; but it is by a different and distinct power, that they produce tonic or Tetanic spasms when they are pushed to a certain extent; and it is by virtue of this last power that I have always supposed they are reckoned by some as acting upon the muscular system. For myself I do not think there is the least good foundation for such a class of remedial agents as I am now considering.

11. Articles which act upon the reproductive system. Almost all writers upon the materia medica and therapeutics, and many writers on the principles and practice of medicine, occasionally mention aphrodisiacs and antaphrodisiacs. If there were any such agents, I suppose they might both be considered as acting upon the reproductive system; and I know of no other articles that can very properly be considered as doing this. But the plan of

classification I am now considering would equally comprise the two groups of opposite powers in one class, which would not contribute very greatly, to promote sound and judicious discrimination in the application of remedies, nor be much in conformity with the ordinary principles upon which classes are founded in other departments of knowledge. But I do not believe that there are any true and proper aphrodisiacs or antaphrodisiacs known, i. e. articles acting directly and immediately and exclusively upon the organs of reproduction, either to increase or to diminish venereal appetite and power. In health, what I call antisbestics (i. e. articles which produce a quickly diffused and transient increase of vital energy and strength of action etc.) and tonics, particularly that group of them of which *Cinchona* is the type, usually prove more or less aphrodisiac; while antiphlogistics and other exhausting agents usually prove more or less antaphrodisiac. It is evident however that these groups of articles produce these effects, not by any particular and specific operation upon the organs of reproduction, but by their general operation upon the whole, or very nearly the whole of the system at large. It is true that a few individual articles are supposed by some to be either aphrodisiac or antaphrodisiac, when the classes to which they belong do not produce any such effects. For example *Ignatia amara*, *Strychnos Nux-vomica* and the other *Strychni* with the same powers, are supposed to be specifically aphrodisiac, at least by some physicians and authors. The same is supposed to be the fact with good extract of *Conium maculatum*. An author says of *Mellocia tuberosa* "*radices a mulieribus comesæ mire fecundas reddere illas dicuntur.*" He does not say whether this is accomplished without any other means or not. On the other hand *Solanum Dulcamara*, and also *Solanum nigrum*, are supposed to be specifically antaphrodisiac. I have repeatedly been assured by physicians of high respectability that they felt assured that this is the fact, from multiplied observations, which they themselves had made. I have been often told by highly judicious physicians, that they were satisfied that *Papaver* is antaphrodisiac; and I have witnessed cases myself, in which such an effect uniformly resulted from the use of this article. When however I am asked to prescribe for *Agenesia* or *Aphoria*, I do not think of looking for articles that act especially upon the reproductive system; but

I investigate the general condition of the health of the patient, and if I find any thing wrong in it, I endeavor to remedy this wrong, as perfectly as I am able; and if I succede in this, the Agenesia or Aphoria, as the case may be, commonly disappears. It is true I some times meet with cases of Agenesia Impotentia, in which there seems to be no other deviation from health. In these, the use of certain oresthetic antisbestics (of which articles Phosphorum elementarium may be taken as a type) most commonly effects a cure. Some times pure natural Wine used long enough, will obviate the disease. I some times also meet with Aphoria unaccompanied by any other morbid condition of the system that I can detect. Such cases I never knew how to manage successfully, and I do not usually attempt it. All this contributes to show that there are in fact no articles, that operate immediately, directly and exclusively on the reproductive system; and that if there were, there must be opposite operations; and articles operating oppositely are never advantageously associated in one class.

12. Articles which act upon the contents of the alimentary canal. As appears to me, such a class as this must include what I call antoxyntics, but what are commonly called by the barbarous Greci-Latin term antacids; what are called antidotes; and what are called anthelmintics. The term antoxyntics may be employed in two intirely different senses, viz. 1. In application to those agents which obviate and prevent the secretion of acids into the alimentary canal; and 2. In application to those agents which combine with and neutralize such acids after they have been secreted. As will at once be perceived, the former is a true medicinal operation; while the latter is only a mere chiminal operation. It is only the latter that can be intended in the class of agents that I am now considering. As to the antidotes so called, this term may be employed 1. In application to articles which obviate the effects of a poison in the alimentary canal; 2. In application to agents which decompose and destroy, or combine with and neutralize poisons in the alimentary canal. 3. In application to agents which evacuate poisons from the alimentary canal. It is only the second and third, that the class which I am considering, would comprise. Articles that destroy intestinal entozoã are found in almost every active group of agents belonging to the

materia medica ; and they accomplish this purpose 1. By producing a perfectly healthy state of the alimentary canal, which is incompatible with the existence of entozoä in it ; 2. By destroying the life of the entozoön without evacuating it ; and 3. By evacuating it without contributing to its destruction in any other way. It will readily be perceived that only the articles which operate by the second and third method, would fall into the supposed class which I am considering. Such a group of agents, so heterogeneous in all respects, would not deserve to be called a class. If an equal number of articles were to be selected, on the ground that they differed in the greatest degree from each other possible in the materia medica, it would make an equally good class. As appears to me, a group made-up in the manner I am considering, would be a burlesque upon classification.

13. Articles which act upon renal and vesical calculi. If such a class of articles as is here specified exists, it is obvious that its effects must be purely chymical, and not at all medicinal. Such a class would coincide with the lithonthryptics of authors, often made a section of a class called antilithics. The other section of the antilithics would operate medicinally, since they are expected to obviate calculous diathesis, and so prevent the further formation of these concretions. That there are agents which are capable of acting-upon, and breaking-up both renal and vesical calculi when they are out of the body, is quite certain ; but as such calculi are of various composition, which can not be ascertained till they are voided ; and as the agents capable of acting upon them and breaking them up must be equally various ; and above all, as many of these agents can not be carried unchanged to the kidneys and the bladder in sufficient quantities, and in a sufficiently concentrated state, to affect calculi, I do not think there is any just foundation for such a class ; but as I propose to consider this subject more in detail hereafter, I shall say nothing more of it, in this place.

14. Upon this plan of classification, I should think it would be necessary to have a class that should comprise articles that operate upon the whole system. The whole of that large group of agents and processes, whose effects are to diminish vital energy and strength of action directly and immediately, seem to be remedies which operate upon the system universally. If pushed to a

certain extent, they affect the organs of primary digestion, the secernents and absorbents or the glandular system, the organs of ultimate assimilation, if these are distinct from the last, the sanguiferous system, the reproductive system, the involuntary muscular system of expression, the organs of common and special sensation, and the hemispheres of the cerebrum. The primary operation of this group of agents is not so much in the nervous system proper, as in certain organs, and the functions not commonly considered as strictly nervous. But shall all medicines that act with equal universality upon all the subordinate parts of the system, be associated with what are commonly recognized as exhausting agents, merely on account of such universality? Perhaps certain agents, recognized as having invigorating powers, are as universal in their operation upon all the subordinate parts of the system, as these exhausting agents. Do not what I call antiseptics operate either immediately or mediately, with about the same universality? Do not the tonics operate either immediately or mediately, with about the same universality? If both exhausting and invigorating remedies act upon the same subordinate parts, or if they act upon the whole of the system, such a fact would seem to show the ineligibility and inexpediency of the plan of classification, that I am now considering. In fact the existence of any considerable number of articles of opposing powers, that are thus universal in their operation and effects, must be fatal to such a plan of classification as that, which I am now considering. As appears to me, the only manner in which this method of classification can be rendered at all useful, is to make it the basis of a grouping of classes founded upon some other principle; but even for this purpose, I consider it as much less eligible, than some other plans. In fact, I am not sure that there are not insuperable difficulties in the way of employing it, even for this purpose.

V. From the operative effects which remedial articles produce. As this is the plan of classification which I intend to adopt, I need say but very little of it, in this place. Some pretend to doubt whether there is any just foundation even for this plan of classification in the *materia medica*, on the alleged ground that the operation and effects of every remedial agent is individually peculiar and specific, and absolutely unlike every other individ-

ual agent. I make no question but that every article of the *materia medica* (with the exception perhaps of a few small groups, such as Sulphate of Potassa, Sulphate of Soda, Sulphate of Magnesia, etc.) actually has some thing peculiar and specific in its operation and effects; but this affords no ground whatever for the rejection of classification founded on operative effects. An extract of *Aloë vera*, and of several other species, is a simple and pure cathartic, without any other power or powers in addition. An extract of *Polygala polygamum* is a simple and pure cathartic, without any other power or powers in addition, at least so far as I could ever discover by my own observations and experience. Of these two plants, the axis ascendens and its appendages, is employed for the preparation of the extract. An extract of *Convallaria Majalis* is a simple and pure cathartic, without any other power or powers in addition. Of this plant the whole of the axis descendens is the part employed for the preparation of the extract. The operation of these three extracts, when they are properly prepared, is said to be so near alike, so far as quality is concerned, that they can not be distinguished, though I believe the dose differs more or less. Why may not these three articles be very properly grouped together in one class? *Podophyllum peltatum* is a pure cathartic, without any other power or powers in addition. *Exogonium* (*Ipomœa*) *Purga* is also a pure cathartic, without any other power or powers in addition. *Leptandra* (*Veronica*) *Virginica* is believed to be a pure cathartic, without any other power or powers in addition. *Rheüm* (*officinale*) is very certainly a pure cathartic, without any other power or powers in addition. Of these four articles, the root is the part employed. Now though the quality of the operation of these several roots, as cathartics, differs very considerably, yet what is the reason why they may not be naturally and usefully grouped together in a class? And why may not these two groups be very properly arranged in one class founded on cathartic power? I venture to say that I might mention forty as natural groups as these, in the *materia medica*, which would fall as obviously under some of the classes founded upon powers, operations and effects.

It is not a little remarkable that such an objection should be adduced in opposition to a medicinal classification of the *materia*

medica, while the pathological classification of diseases is opposed on the ground that there is no such thing as specific diseases. But admitting that every individual remedy has its peculiarities, it is likewise equally the fact that the prominent and leading properties of individual remedies are always common to groups of articles. For example, there is a large group, whose most prominent and leading operation is a direct diminution of vital energy and strength of action. Some of these possess no other power in addition, while others possess one or more powers besides. There is another group, whose most prominent and leading operation is a direct increase of vital energy and strength of action. Most if not all of these articles possess one or more powers in addition. There is an other group, whose most prominent and leading operation is to produce diaphoresis directly, either of that grade called perspiration or of that grade called sweating. Some of these possess no other power besides, while others possess one or more powers in addition. There is an other group, whose most prominent and leading operation is to produce vomiting directly. Some of these possess no other power in addition, while others possess one or more powers besides. If this is correct, in what branch of natural science is there a better foundation for classification, than in *materia medica*? Indeed is there any reason to think that any remedies cure diseases, except through the instrumentality of powers, operations and effects common to a greater or less number of different individual agents? But there can be no further need of arguments to prove that there is as good a foundation for classification in the *materia medica*, as in any other department of knowledge; and that it is of just as much utility and importance in this branch, as in any other of no greater extent.

External sensible properties, i. e. taste and smell might afford as good a foundation for classification as the parts of the articles employed, or the proximate principles in which medicinal powers reside; but I know of no author who has ever made use of them for this purpose in any formal work on the *materia medica*. It is my belief that identity and close similarity of taste and smell afford much more certain and unequivocal indications of identity and also similarity of medicinal powers, operations and effects, than botanical or natural history affinities. I have some times exer-

cised myself in grouping articles according to their tastes and smells; and as far as I have pursued the subject, I have always found the coincidence between these sensible properties, if carefully and accurately observed, and medicinal powers, much more prominent than I should have supposed a priori. But this subject has been sufficiently discussed under the head of "Means of Ascertaining the Powers of New and Previously Unknown Articles" and therefore I shall not pursue it in this place.

It is easy to arrange the *materia medica* in natural medicinal groups; and such an arrangement has its advantages for purposes of instruction especially; but it has more value as an arrangement subordinate to classes founded upon powers, operations and effects, than as a primary and principal one. Upon this plan, the simple and pure bitter-tonics, whose primary operation is to increase appetite and digestive power, of which *Gentiana lutea* may be considered as the type, would make a group. The euphrenic bitter-tonics, of which *Humulus Lupulus* may be considered as the type, would make a group. The orethetic bitter-tonics, of which *Liriodendron Tulipifera* may be considered as the type, would make a group. The styptic bitter-tonics, of which *Cornus florida* may be considered as the type, would make a group. The bitter-tonics whose primary operation is to increase vital energy and strength of action in the sanguiferous system, of which *Cinchona* may be considered as the type, would make a group. This group is commonly known as febrifuges. I do not give these as by any means the whole of the groups of the tonics, but only as a specimen. There may be said not to be a single class in the *materia medica*, founded on powers, operations and effects, the articles of which do not admit of a similar grouping, the groups being in most cases more distinctly marked and characterized, than in the case of the tonics. I have already mentioned two groups of the cathartics, of one of which *Aloë vera* was the type, and of the other of which *Podophyllum peltatum* was the type. Of the latter the catalogue was by no means complete. I will only mention a few more groups selected promiscuously by way of specimens for illustration. Those articles of which *Narthex Assa-foetida* is the type constitute a very natural group. Those articles, of which *Zingiber officinale* may be reckoned as the type, constitute a large natural group. Those articles,

of which Cochlearia Armoracia may be considered as the type, constitute a very numerous, as well as very natural group. The Oleiresins constitute a very distinct and natural group, and a large one too. Many years ago I grouped the whole materia medica after this manner, arranging the groups among themselves, according to their affinities, in as natural an order as was possible, intending to follow such order in my instructions, without calling it a classification, or in any way alluding to it as such, or taking any notice that I had any order at all. I soon found however that I could not make it supersede a classification founded on powers, operations and effects, and that it furnished no place for such remarks, as were applicable to the last mentioned classes as aggregates; so that I was obliged to abandon it as my sole classification, and that too as an unacknowledged and informal one. Never the less, I found it convenient and highly useful to retain it, in subordination to classification founded upon powers, operations and effects, and did so accordingly.

Some however maintain that there is neither necessity for, nor utility in, any method of classification for the materia medica. It is alleged to be sufficient to know of any article that "it makes an impression upon the alimentary canal" (and thereby) "changes action, or produces a new condition" (in some part or parts of the system) "and thus has an effect upon disease" (and that) "if we know whether the system is above or below the range, within which it will be most likely to operate favorably, and become a counter-agent of disease, it is of little or no consequence by what name we call such operation, or what classification we make of it" (and that) "it is never serviceable to embarrass students particularly, with any discussions in regard to either of these points." It appears to me, that it is by no means sufficient to know merely that an article "makes an impression upon the alimentary canal" (and thereby) "changes action or produces a new condition, and thus has an effect upon disease." It appears to me that even the additional knowledge of "whether the system is above or below the range within which it will be most likely to operate favorably, and become a counteragent of disease," is not all that is necessary. All remedies, that are capable of producing any effect, make an impression upon the alimentary canal, or that part of the system to which they are immediately applied, and thereby

change action, and produce a new condition, and thus have an effect upon disease. Now surely every remedy, whose operation the system at a given time is neither above nor below, is not equally eligible, in the treatment of any single disease. Some further knowledge is certainly necessary for rational and judicious practice; and the more minutely we are acquainted with the peculiar operations of particular medicines, the better are we qualified for the most successful practice. It appears to me that there is no substitute for this sort of knowledge. Whenever it can be obtained it ought to be obtained.

A distinguished physician once observed to me, in a letter, that in his opinion "it is very often difficult, if not impossible, to tell upon what principles medicines act, even when they do good the most palpably." "In mild cases of disease" (said this gentleman) "as in Burns or Scalds, Sprains, Bruises, and even some slight febrile diseases, I have no doubt that almost any and every new medicinal action, which is not very intense, is better than the original diseased action, state or condition." This gentleman adds "it is very evident to me that three fourths of the patients of perfect routine practitioners, actually do much better than they would do, without any medication." "In most of their chronic, and in all of their malignant cases, such practitioners make sad work." Upon the opinion of this gentleman I must remark, that the difficulty of ascertaining the precise and exact operations of medicines, does not exonerate us from the obligation of ascertaining them, whenever it is actually practicable. It may be some palliation of our ignorance, but this is the most that can be said of it. But I do not agree with this gentleman, that it is so very often difficult to tell upon what principles medicines do in fact act. In order to learn the operations of medicines, they must be given with greater or less efficiency, and their effects must be carefully watched and observed, under which circumstances we can not fail of soon learning their operations and effects.

A professional friend once told me that "when *Spermœdia Clavus* was first brought into notice in recent times, a medical gentleman, who had Brunonian notions, gravely asked him whether this substance proved an ecboic or partus accelerator by a stimulant power, or the opposite?" This was mentioned by my friend as an example of a perfectly unimportant question—a question

whose answer could be of no practical utility. It is true that, in the spirit with which it seems to have been asked, it appears to evince rather narrow views in medicine generally, and rather a limited range of thought upon the operations and effects of remedial agents; and yet it is certainly a matter of considerable importance to know whether this article occasions uterine contractions or parturient efforts, by virtue of any operation, which is well known, and which constitutes the foundation of any previously existing class in the materia medica; or whether an ecbotic operation is an intirely distinct operation from any other, that is known and described.

It appears to me to be important, indeed absolutely necessary, for the judicious application of remedies, that we should know not only their peculiarities of operation in comparison with each other i. e. their dissimilarities, but also their similarities or points of agreement. These should be investigated and known just so far as they ought to influence prescription; and it will be obvious that they are of no importance any further. But without some classification of the various operations of remedies, our knowledge of peculiarities and similarities can not be communicated to any purpose, in the way of instruction—can not be recollected without the greatest difficulty, and in fact can not be very thoroughly or very perfectly understood. Besides, when ever we have occasion to refer to an article, we can not always enumerate all its specific effects in detail; we must have a name or names for such effects in the aggregate, by which we can briefly make reference to them in few words. It is however important that the same name should not be applied to aggregates of effects i. e. operations, so different, that when one is indicated, the other may be contraïndicated, or at least do no good. In this point of view, it appears to me that it is of the utmost importance to be just so definite and precise in classification, as may affect indications and contraïndications—just so accurate as respects the peculiarities and similarities of remedies, as these ought to affect practice, and no further. If we go to just this extent and no further, it is certainly and strictly true that we shall find a much greater number of facts, principles and rules to be absolutely necessary, than a mere learner can at once recollect, or perhaps much greater than he can at once understand, without great pains and labor, and with-

out great difficulty ; we shall find much less simplicity in the science of medicine, than is consistent with its supposed very easy attainment ; and in short, we shall find that there is “no royal road” to professional learning and skill ; but we are not on this account, to neglect and reject any part of it, which is capable of being of any practical utility—it must all be acquired and treasured-up ; and by due patience and perseverance, this may be accomplished, though it is to be feared that few do actually accomplish it.

If there is any thing in the *materia medica* that is more important to be known, or even as much so, as the precise operative effects of each article, and its peculiarities in comparison with other articles, and also what it has in common with other articles, then I am ignorant of what it is. The rejection of this sort of knowledge would reduce this department to the most complete empiricism. It appears to me that as soon as we begin to know definitely and accurately the precise operative effects of medicines, their peculiarities in comparison with each other, and their similarities, just so soon it becomes necessary to have names for these operations, by which we may make mention of them in one word, without tedious specifications of every particular. Now if these names of the operations of medicines have a definite and precise sense attached to them—a sense in which they are uniformly and invariably employed by a given writer or practitioner, then to ascertain the propriety of the application of one or more such terms, to a given article of medicine, is by far the most important part of *materia medica*—it is the part without which all the rest would be to a great extent useless. The knowledge in question is true science ; and it is almost the only scientific part of pure *materia medica* at least. If however the definitions of the terms designating the powers and operations of medicines are theoretical, or rather hypothetical, as is much too often the fact in all our systems of *materia medica*, and if their application is loose, vague and inaccurate, then indeed the employment of such terms does not involve any science, and is not only a very unimportant and trifling matter, but it is some thing worse, and is an actual hindrance to the progress of true knowledge in this branch of medicine.

It is to be particularly observed that this accuracy and precis-

ion of definition is absolutely necessary, in order to prevent the confounding of different and distinct operations; and by leading to a critical investigation of the effects of individual articles, to prevent also an oversight of any of their powers, and any of their peculiarities. Where mere general and vague definitions, and particularly definitions of a theoretical, or rather hypothetical character, are employed, as they are much too frequently and much too commonly, it will always be found to be the fact that many powers and operations are intirely overlooked and omitted; and powers and operations totally unlike, and some times even of an opposite character, are considered as essentially the same, and some times as perfectly identical. I venture to assert that no man can ever become truly learned and skilful in the *materia medica*, who does not study the powers, operations and effects of individual remedies quite minutely, and who does not limit and define every specific operation with accuracy and precision. I am aware that this requires diligent application, and careful and long study; but as I have so often inculcated, there is "no royal road" to the science of medicine—it must be acquired in this way, or it can not be acquired at all.

It is the opinion of some that individual remedies cure disease exclusively by producing a new disease, different from, and incompatible with the previously existing one. Those who entertain this opinion, generally believe also that every individual remedy is perfectly specific in its effects, and therefore they deny the utility, or even the propriety of attempting to reduce remedies to classes founded on a supposed community of operation in some prominent and important particular. The preparations of Mercury, Arsenic and Antimony, are supposed to afford a good illustration and proof of this doctrine. But as appears to me, this opinion is clearly and palpably fallacious. Indeed such a doctrine may be pronounced at once, to be extremely improbable, judging from phenomena which may be witnessed almost daily, at the bed-side of patients. In the first place, none but the more active agents of the *materia medica* seem capable, of themselves, of producing disease. A certain portion of them are certainly incapable of producing any morbid, noxious, deleterious or poisonous effects, however large the quantity, in which they may be taken, and however long their use may be continued; and though

these agents are never very efficient remedies, yet they often possess considerable medicinal value; and they are certainly capable of counteracting and curing a certain number of diseases. In the second place, so far as the force or power, even of the most active remedies, is spent in overcoming disease already existing, it seems incapable of producing a disease of its own; and it is only after the cure, or suspension of the previously existing disease, that the further i. e. the excessive use of the remedy in fact produces a disease of its own. In the third place, the production of a disease by the remedy, does not appear to be at all necessary to the cure of a previously existing one. The previous disease may generally, if not invariably, be cured by mere counteraction to a certain extent, without pushing the remedy so far as to produce any disease of its own, at least if it is truly appropriate to the case. As a general rule, the method of pushing active remedies to the production of disease of their own, in order to cure a previously existing one, is not only a clumsy method, but it is fraught with hazard to the patient. In a very few instances of peculiarly obstinate disease, it may possibly be necessary; but it is believed that this is very rarely the fact. Contrary to the opinion of some, these remarks appear to me to be perfectly applicable not only to the most highly active, but also to the least efficient articles of the *materia medica*. The whole appear to me to operate either upon known or easily ascertainable principles, and therefore are perfectly susceptible of classification.

It is my opinion that that classification of the *materia medica*, which is founded upon the medicinal powers, operations or effects of remedies, is so much preferable to every other hitherto employed or proposed, that no other, in comparison with such a one, is worthy of being retained, at least for practical purposes. It is also my opinion that according to this system, there should be just as many classes, as there are different, distinct and specific medicinal powers, operations and effects, or groups of effects, and no more; and of course, that an individual distinct and specific medicinal power, operation or effect, or group of effects, should always constitute the sole basis of every individual class in the *materia medica*. For such a method or rather system of classification, it must obviously be of the greatest importance to determine accurately the exact number of different, distinct and

specific medicinal powers, operations or effects, or groups of effects, which can be certainly ascertained; otherwise the execution of such a method or rather system, must inevitably be defective, erroneous and liable to mislead. For such a method or system of classification also, it must obviously be of the greatest importance to distinguish carefully the different grades of the operations or effects of a single individual medicinal power, from the operations or effects of distinct medicinal powers; otherwise also, the execution of such a method or system must be erroneous, defective and liable to mislead. For such a method or system of classification, it must likewise be necessary to avoid hypothetical, theoretical and vague or loose definitions; and the character of the class must obviously consist of a specification of all the operations and effects, which are common to every individual article possessing the power on which the class is founded, and no other, and which are peculiar to the class as such. In other words the definition ought in general to include every thing peculiar to the class as founded upon a single power; and it ought to exclude every thing common to it and other classes; and it should be so accurately descriptive, that every agent truly and properly belonging to such class, may be easily ascertained by it.

I am not apprised that any classifier of the *materia medica* hitherto, has ever had any such distinct and definite plan or scheme in view, in the execution of his method or system; and of course it can be no matter of surprise that a method or system in conformity with such a plan or scheme, has never yet been produced. At all events, it may be considered certain that in all the common methods or systems of classification (at least in the books in use in this country) a single individual power, operation or group of effects, is very often made the foundation of several classes; and on the contrary, several different and distinct powers, operations, and groups of effects, are very often made the foundation of a single individual class. A plan or scheme for a method or system so vague, indefinite and imperfect, as to admit of such an execution; or a correct and accurate plan or scheme, for a method or system so erroneously and incorrectly executed, must, on the whole, lead to as much, if not more error than truth; and therefore must retard, rather than promote the advancement of such a branch of knowledge as the *materia medica*.

In order to render such a classification, as I propose, as perfect and as useful as possible, the medicinal classes so constituted, should be arranged in reference to each other, according to their mutual relations or affinities, and their dissimilarities or discrepancies. In such an arrangement too, the classes may also be usefully distributed into several nexus alliances or groups, each nexus alliance or group founded upon some prominent characters common to each class in the nexus alliance or group, and peculiar to the nexus alliance or group as an aggregate; but this is of much less importance than the accurate determination of the classes. After these remarks and this statement, it will be almost superfluous to say that the method or system of classification of the *materia medica*, which I purpose to employ, and of which I am now about to give a synopsis, is formed as nearly in conformity with these principles, as appears to me to be at present practicable. It would be doing injustice to my readers as well as to myself, and to the method or system in question, were I to omit the statement that the observations, experiments, and facts, on which it is founded, have been a constant subject of investigation and research, ever since the summer of 1810; so that they have not been adopted hastily and precipitately. Whether the principles by which I regulate the doses of medicines, their periods of repetition and the time of their continuance are favorable, or unfavorable for the acquisition of that sort of knowledge, and for the attainment of sufficient accuracy of information for the execution of the plan or scheme, for the method or system of classification attempted, all who have heard these principles can judge for themselves. For myself I must say that they appear to me to be the only principles, with which I am acquainted, at all well calculated for this purpose. If any one has any very strong prejudices against classification in the *materia medica*, he is desired to observe in this place, that the classification here adopted is intended to be not only mainly but exclusively a specification, definition and naming of all the various and different powers operations or effects or groups of effects, of all the remedial agents employed in the practice of medicine—a kind of knowledge, which (as will at once be obvious) must be absolutely necessary, so far as it is attainable, to every rational practitioner of the healing art.

It is to be observed that in making just as many classes in the *materia medica*, as there are different, distinct and specific powers, and neither more nor less, it may in some cases be necessary to merge two or more of the classes of authors into one; and in other cases, to divide the classes of authors into two or more. It is actually the fact that in one of the classes of authors, four unequivocally different, distinct and specific powers have been confounded, as I trust I shall be able to show satisfactorily to every one, who will investigate the subject, or even attentively consider the facts which I shall adduce. Now both the union of two or more of the classes of authors into one, and the separation of one of the classes of authors into two or more, involves the employment of some new terms. Therefore in naming and defining the different, distinct and specific powers, operations and effects, or groups of effects, on which my classes are founded, I have been obliged to employ several terms not hitherto employed by any other author or practitioner of medicine. This I have been constrained to do, in part from absolute necessity, and in part from the obvious propriety of having all the names of the classes Latinized-Greek, as nearly all have been, time immemorial. I must here be allowed to urge, what I have found it difficult to make many understand, that the necessity here referred-to originates from the fact that I discriminate different, distinct and specific powers, operations and effects, or group of effects, which have hitherto been neglected or confounded with other powers, operations and effects, by all the classifiers of the *materia medica*, of whom I have any knowledge. When I first made such unions in some cases, and such separations in others, I avoided as long as possible, the adoption of any new name, but spoke of such classes periphrastically or circumlocutorily. This often proved a very awkward course, and one extremely difficult to be made easily intelligible. At last it got to be a matter of course for my pupils to send a request, every year, that I would either find some old terms, that would be sufficiently appropriate for my new classes, or devise some new ones. This I postponed as long as possible, on the grounds that it might perhaps be difficult to perform what was desired in a perfectly satisfactory manner; and that it was vehemently objected-to by some of my particular friends who had always taken the strongest interest in my useful-

ness as an instructor, and whom I had previously suffered to dictate my course in nearly all respects. At last, when I complied with the desire of my pupils, the best terms which I could adopt, were regularly laughed-at, ridiculed, and sneered-at; and though I can safely say (in the words of Mr. Jefferson once before quoted) that this treatment did not "break my leg" yet I can not so truly affirm that it did not "pick my pocket." It is obvious that under such circumstances, either old terms must be employed in new senses, and of course, in double senses, and therefore be perpetually liable to misunderstanding, misapprehension and mistake, or the definition must be used in all cases as a substitute for a term (a method at once tedious and perplexing) or the plan of coining new terms (to which I gave the preference) must be adopted. To be intelligible is my first object, and subordinate to this I consider it important to be easily intelligible. It appears to me that the new terms which I employ are necessary to both these purposes. However, if any one, after obtaining an accurate knowledge of my views, can express and communicate them clearly and easily, without any new terms, he will have attained to what I have been unable to accomplish; and I certainly shall not object to his doing it in such a manner.

It will be observed that, time immemorial, the names employed by nearly all authors on the *materia medica*, for their classes, have been Latinized-Greek. The only British writer on this subject, at least of any distinction, within my present recollection, who has deviated from this rule, is Darwin; but he has only seven classes, even including the *nutrientia*; and his whole treatise on the *materia medica* is comprised in eighty octavo pages of comparatively large type. It is true that we occasionally find a few pure Latin terms employed for the purpose under consideration, such as *refrigerantia*, *demulcentia*, *emollientia*, *astringentia*, etc. but these are exceptions to the general rule. In the terms which I have been obliged to devise, I have followed the regular canon, and employed Latinized-Greek. In the whole I have had occasion to adopt eleven new names for powers in the *materia medica*. Two of these I found ready made for my use, and nine I coined myself, as I believe, according to the strictest laws of Greek and Latin philology. With six of the eleven new terms that I have adopted, I am well satisfied. None more appropriate,

more elegant or more normal, exist in the *materia medica*. With five of these eleven terms I am not satisfied; but they are the best I could devise, even with the assistance of a native Greek distinguished for scholarship, though not a physician. If any body will devise better terms for me, I will immediately adopt them. In the mean time I trust that those which I employ will answer the purpose for which they were intended. I will only add in this place that the parts of the words which I have coined are all familiar to every tolerable Greek scholar, and can be found as readily in any Lexicon as any words in the whole Greek language.

Occasional intimations have been received by the publisher of this work that some of the terms employed hitherto, to express power operation and effect of medicines, have not been deemed sufficiently intelligible. This has been a matter of surprise to the author, since all of the terms that appear to have been intended in this objection could not fail of being understood generally, by every man of a classical education, being regular ancient Greek, like all the terms that have been used for the same purpose, ever since the time of Hippocrates. Assuredly the term *leäntic* is as intelligible generally as the term *antiphlogistic*, and who does not understand the latter? The term *neuragic* is certainly as intelligible generally as the term *narcotic*. The terms *euphrenic*, *oresthetic* and *antisbestic* are as intelligible generally, as the terms *tonic* and *styptic*. The term *blennagogue* is as intelligible generally, as the terms *emmenagogue*, *sialagogue*, *hidrotagogue* and *uragogue*. All these terms belong to the same language; and so do *emetic*, *cathartic* and all our other technical terms. A science must either be confined to a single nation, or its terms must be derived from a language or languages studied by the whole civilized world. Latin and Greek are such languages; and every physician who is not a graduate in arts at some of our colleges or universities, is expected and required to have studied these languages preparatory to his entering upon the study of medicine proper. Proper names and technical terms can not be translated from one language to an other; they must be transferred bodily. Who ever thought of objecting to such names as *Tartar emetic* and *Calomel* on the ground of their being Greek? But beside this whenever I have mentioned any of the terms expressive of power,

operation or effect, that are said to have been complained-of, I have given an explanatory illustration. Thus when I have mentioned leāntics, I have added the clause "which consists of the demulcents and emollients of authors, in the aggregate." When I have mentioned neuragics I have added the clause "of which Lead is the type." When I have mentioned erethistics I have added the clause "of which Strychnos Nux-vomica is the type." When I have mentioned euphrenics, I have added the clause "of which Protoxyd of Nitrogen is the type." When I have mentioned oresthetics, I have added the clause "of which Capsicum annuum is the type." Such a term as blennagogue did not seem to need any explanatory illustration, since every school-boy in Greek knows that βλέννα (blenna) means mucus; and the termination of the word blennagogue can not possibly fail of being understood by all those who understand emmenagogue, sialagogue, etc.

When I was a young practitioner of my profession, I recollect distinctly that it was by no means uncommon for the elderly physicians, with whom I associated, even though not graduates in arts, to keep-up the custom of reading Hippocrates, Galen, Celsus, etc. in their original languages; and at the least, they always retained Greek enough to have the etymology of all technical terms at once brought into their minds, when ever they either heard or used such terms; and surely this can not but be the fact, under all the advancement and improvement of medical education in recent times. The complaint of the unintelligibility of some of my terms (if there has really been any) can not therefore have been in serious earnest. Every author's own peculiar limitations and restrictions of the import even of the most common terms, which he employs, may indeed be different in some respects from the limitations and restrictions of every body else; but this is of no importance to the general understanding of them, which is required for my introduction to this work. My own limitations and restrictions will be given hereafter in their proper place and as I trust in sufficient time, not only for my own purposes, but also for the purposes of my readers, if I shall happen to have any. I am perfectly apprised that it is often difficult to treat of one part of a subject, without some knowledge whose natural place of explanation falls into an other part. Thus the several topics of my introduction necessarily and inevitably involve references

to matters, that could be properly treated of only in the subsequent part of the work. Indeed it will be utterly impossible to treat of the antiphlogistics (my first class of medicines) without reference to other classes, that must come after it, in my regular order; and the same would have been equally true, under any method or system that I could have adopted. In view of these and other considerations, I shall give a synopsis of the classification, which I propose to adopt, and which indeed I can not possibly avoid, before I come to the consideration of each class in the aggregate. But in addition even to this, I shall accompany such synopsis with brief definitions, since the same classes mainly have been employed by others with very different limitations and restrictions, so that the synopsis alone would hardly afford any correct notion of my method or plan. These brief definitions can not however be made to supersede the fuller ones hereafter, so that as the greater must necessarily comprehend the less, I shall be obliged so far to have a repetition. This synopsis therefore must not be expected to comprise as full and as satisfactory a definition of each class, as every one will doubtless desire, and consequently it must not be forgot that a much more complete definition and account is soon to follow. It would be no matter of surprise if after the synopsis merely, there should be doubts of the soundness of the foundation of some of the classes. Under such circumstances, I can only ask the favor of a suspension of the final judgment, till all I may have to say upon such classes shall have been given; and if I do not finally establish it satisfactorily, I desire that it may be rejected. Not that even finally I can expect to make the subject as clear to those who have not previously studied it at all, as it may be to myself who have studied it for years, but still I should hope that it may be sufficiently clear to enable any one to form a generally correct judgment upon it.

It appears to me to be perfectly and absolutely certain that there are at least nineteen different, distinct and specific medicinal powers, operations and effects, in the *materia medica*, while it is probable that there are between one and four more. At all events I shall make twenty-three classes, though I am well satisfied that this number of different, distinct and specific medicinal powers has not yet been proved to exist; but I am not without

expectation that three beyond the nineteen will yet be found to have a good foundation, when they have been properly searched-for. When so much is certain, it will not be considered as at all surprising that some thing should be doubtful, some thing uncertain. But I do not consider it as at all essential to the value of any method or system, that it should be perfect at once, or when it is first proposed. It is my intention to treat as thoroughly of the doubtful classes, as of the certain ones. By way of appendix I shall even treat of three spurious classes, and perhaps of four, in addition to the nineteen true, and the four doubtful ones—classes for which I am absolutely certain that there is no foundation whatever upon my plan or scheme—classes which are not based upon any one individual power, operation or effect; but on the contrary, produce their effects by virtue of at least half the powers and operations on which the preceding classes rest. I am induced to consider and treat of these three or four spurious classes because they are universally received by almost all writers on *materia medica*, and on the principles and practice of medicine; and because some thing in regard to them was uniformly called-for, by all the classes of young men that ever came under my instruction. Students of medicine always desired the reasons why they were not admitted as legitimate classes, and needed information in regard to the numerous, various and some times even opposite modes, in which the articles commonly referred to the classes in question actually do operate.

There appears to me to be a sufficiently good foundation for a distribution of these nineteen or twenty-three medicinal classes into five sufficiently natural nexus alliances or groups, which are capable of definition and description. I do not however attach very much importance to this grouping of the classes, and it would certainly be a great mistake to consider it as well founded and as natural as the classes themselves. It must be kept in mind that when a subordinate part of the system is specified, in the diagnostic characters of each class, upon which a particular agent, or a particular class of agents is said to exert its especial operation, that part is always intended, in which its sensible effects are first manifested, without any reference to other parts, through the medium of which, the influence must have been conveyed to the parts or organs, that exhibit the first manifestations.

SYNOPSIS OF THE CLASSIFICATION.

The first nexus, alliance or group of classes may be considered as characterized by the circumstance that the operations, upon which the classes are founded, are not evacuant, at least essentially, and except in one, viz. the last class, never even accidentally or occasionally so. The character of this group of classes, as will be seen, is rigidly correct with respect to each class of the group, except the last, viz. the adenagics; and even of this, the most important, the essential part of its operation, viz. its resolvent, antipsoric and discutient effects are wholly unattended by any evacuation at all; but as the excretories constitute a part of the secernent and absorbent or glandular system, upon which this class exerts its peculiar influence, preternatural evacuations of some sort or other, must, not infrequently, be the result of the operation of the adenagics. Such evacuation however is of no medicinal value.

NEXUS PRIMUS.

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|---------------------|------------------|
| 1. ANTIPHLOGISTICA, | 7. EUPHRÆNICA, |
| 2. NAUSIATICA, | 8. ORÆSTHETICA, |
| 3. LEÄNTICA, | 9. ANTISBESTICA, |
| 4. NEURAGICA, | 10. TONICA, |
| 5. NARCOTICA, | 11. STYPTICA, |
| 6. ERETHISTICA, | 12. ADENAGICA. |

The classes belonging to this nexus alliance or group, are arranged among themselves in the order of their affinities to each other.

ANTIPHLOGISTICA.

The term Antiphlogistica is composed of a Greek preposition signifying against, and a derivative of a Greek* verb signifying to inflame. It has been in use in the *materia medica* time imme-

* My briefs and notes contain the full etymology of the names of the classes; but I am obliged here to omit giving the etyma because our printer has no suitable Greek type. This I regret exceedingly as I know that some of my readers are desirous of having what I am compelled to withhold. Greek can not be intelligibly written in Roman letters, since the Roman alphabet does not contain equivalents of all the Greek letters. What I shall give will perhaps be better than nothing. I think it will enable every moderately good scholar in Greek to find easily all the words referred-to, even if he could not do this just as well without.

morial, and the class of agents to which it is applied has been recognized for an equal length of time, either under this name, or some other deemed equivalent, whether truly or not I will not here pretend to decide, but shall leave this to be discussed in an other and more appropriate place.

Definition. Antiphlogistics are articles which, in a peculiar manner and in a definite degree, directly diminish vital energy and strength of action in the sanguiferous or circulating system, and perhaps indirectly lessen vigor generally, thereby diminishing and obviating entonic, sthenic or phlogistic diathesis when it exists, and (except in the case of depletion of blood and catharsis by the antiphlogistic salts) independent of any evacuation, at least as a necessary part of their operation.

The only thing peculiar in regard to this class, to which I make the least pretension, is a more strict limitation, and a more accurate and precise definition.

NAUSIATICA.

The term Nausiatica is believed to be a regularly formed one from ancient Greek words signifying to be sick at stomach and sea-sickness. It must be noticed that the vowel of the second syllable in Greek is not the same as in Latin, and therefore in the name of the class it should be the letter *i*, because such name is Greek, while in the name of the sensation it should be the letter *e*, because this word is Latin. The term Nausiatica, as I must confess, does not please me very well. It seems however to be correct and appropriate in signification and legitimately formed; and I have been unable to devise any thing better.

Definition. Nausiatics are articles which produce a distressing variety of common sensation called nausea, sea-sickness or sickness at stomach, having its seat in the stomach, always attended with a loathing of all food and a tendency to vomiting, and always aggravated by motion or exertion. It is often produced by the motion of a ship in a high wind; by riding in a carriage so hung upon its springs, that its motions resemble those of a ship in a high wind; by swinging; by whirling, etc. It is often but not always a precursor of vomiting; often but not always a symptom of pregnancy; and it occurs very frequently as a symptom of various diseases, as for example, of all the species of Gastritis; of all the species

of Enteritis; often of all the species of Diarrhœa, if there is more than one species; etc.

The continuance of nausea for any material length of time, especially if it is at all intense, is productive directly of a greater or less degree of positive exhaustion of all the parts dependent upon the nerve of chymical action nutrition and reproduction, commonly called the great sympathetic nerve. Nausea has been long known to be capable of proving remedial or medicinal; but I do not now recollect that *nausiatica* or *nauseants* have ever before been formally ranked as a class in the *materia medica*. I am not prepared to maintain that giving them such formal rank is worthy of being claimed as any thing original; but this and the peculiarities of the definition, are certainly all that can be considered as in any degree novel in relation to this subject.

LEÄNTICA.

The term *Leäntica* is an ancient Greek word, which signifies alleviating; assuaging; calming; smoothing; etc. As appears to me, the term *Leäntica* is perfectly unobjectionable. It is ancient and classical, strictly appropriate in signification, and decidedly euphonous. I do not think that a better can possibly be devised.

Definition. *Leäntics* are articles which, independent of any antiphlogistic, neuragic, narcotic or euphrenic powers, but by virtue merely of a peculiar impression upon the mucous membrane of the alimentary canal, or the dermatic membrane of the surface of the body, made by a mucilaginous, amylaceous, saccharine, oleaginous or gelatinous principle, or by heat and moisture conjoined, or some combination of these, allay irritation in various parts of the system, alleviate phlogotic soreness, swelling and pain, and contribute either to a resolution or a supuration according to the stage and other circumstances of the case.

This class is composed of the *Demulcents* and the *Emollients* of authors—two supposed classes founded upon a single individual and specific power. There is therefore nothing new about it, except its reduction to one class, its name, and perhaps its definition. As to the propriety of its reduction to one class, I should think there can be neither doubt nor question. Neither the term *Demulcent* nor the term *Emollient* can be employed as its name, because both are used in different acceptations, and because they are not Latinized-Greek, but pure Latin, contrary to the rule for

the names of the classes. As to the definition, I trust there will be no objection to it, at least after it has been given in full in the proëm to the class, whatever may be thought of it in the abridged form in which it is here given.

NEURAGICA.

The term Neuragica is derived from a Greek substantive signifying a nerve, and a Greek verb signifying to affect; to influence; to control; to rule; to lead; to conduct; etc. Etymologically the term signifies an article that affects, influences, or controls the nerves. But an etymological signification of the name of a class, can never be a definition of such class.

Definition. Neuragics are articles which, without either euphrenic, narcotic, leäntic or antiphlogistic powers, operate primarily upon the nervous system generally; some of them, *in the first degree of their operation*, efficiently allaying, in a manner peculiar to themselves, morbid susceptibility, morbid sensation and irritative action or motion, without any vertigo or general tremors in conjunction; while others produce more or less vertigo and general tremors; both at the same time obviating or contributing to obviate spastic or convulsive diathesis; *in the second degree of their operation*, they occasion, in addition to the preceding effects, more or less languor, lassitude and prostration, more or less neuralgic pain, more or less palpitation of the heart and arteries, more or less torpor as respects peristaltic action of the intestines, more or less stupor or numbness in some parts of the extremities, sensations of constriction in the feet and hands, peculiar weakness in the lower extremities, many symptoms and phenomena resembling Chorea, morbid wakefulness, etc. *in the third degree of their operation*, they produce violent neuralgic pains in various parts of the body and limbs, great anxiety, general agitation, spastic or convulsive affections, obtuseness of the special senses or positive abolition of some of them, dilatation of the pupils of the eyes, emaciation, Paralysis of the extremities both as respects voluntary motion and common sensation, delirium, general exhaustion, and some times, even not infrequently death.

The medicinal class Neuragica is much easier defined by its ultimate effects, i. e. those which transcend or go beyond its medicinal operation, than by its proper remedial effects. But it will

at once be obvious, that a class in the *materia medica* can never be properly founded upon non-medicinal operations. And yet whatever may be the obscurity of the diagnosis of this class of agents, by the abridged definition or character, there is none at all, by the full one.

The power, upon which this class is founded, has been recognized time immemorial, as belonging to the compounds of Lead; but I do not now recollect that it has been known to be possessed by any other articles, and much less by a large group; and I think that such power has never been made the foundation of a class of remedial agents, by any writer upon the *materia medica*. This class then is new, as may be said to be the fact that the power on which it is founded belongs to very numerous articles; and under such circumstances, it will be obvious that the name must be new. With the latter I admit that I am not satisfied, on the ground of its generality of signification; and if any one will furnish me with a better and more appropriate appellation, I will readily and cheerfully adopt it. Till such a thing is accomplished however, the term will answer provisionally, as the writers on natural history some times say.

NARCOTICA.

The term *Narcotica* is ancient and classical, but not very appropriate in its signification. It signifies "having the property of stiffening, benumbing, or rendering insensible." Now I do not know that the narcotics ever stiffen, unless they fix the muscles of voluntary motion in a convulsion; I do not know that a pure and mere narcotic ever benumbs; nor do I know that the narcotics ever produce insensibility, unless it is in consequence of occasioning profound coma, which would be an indirect operation. But even if we admit these as effects of the narcotics, they must certainly be ultimate effects, i. e. effects which transcend those which are medicinal, and therefore effects which can never be the foundation of a class in the *materia medica*. I make these strictures merely to show that ancient and classical names are no more correct than my own.

Definition. Narcotics are articles which, *in the first degree of their operation*, directly allay morbid irritability and irritation, and irritative actions generally, morbid sensibility and sensation,

morbid mobility, restlessness, jactitation and watchfulness, when they are connected with a non-phlogistic, or a positively atonic condition of the system; *in the second degree of their operation* they directly relieve pain; *in the third degree of their operation*, they directly produce more or less somnolency, or even positive sleep;—*in the fourth degree of their operation*, they produce vertigo, headache, faintness, dimness of sight, the sensation of a cloud before the eyes, or some imperfection of vision, either with great dilatation or extreme contraction or an immovably fixed but otherwise natural state of the pupils, nausea and retching, with epigastric uneasiness, especially when the head is raised or otherwise much moved, accompanied with small and irregular pulse, cold extremities, cold clammy and slippery sweats, delirium, convulsions either clonic, subtonic or exquisitely tonic, succeeded by coma, and some times death; and when the narcotic has no other medicinal power conjoined, without any other accompanying operations.

This class as well as its name is old; but the definition is new, i. e. unlike any that I have ever seen in any work on the materia medica. Perhaps it may be improved, but some thing very much like it, must in my opinion be the only correct definition of this class, which appears to me to be as well founded as any other class in the materia medica.

ERETHISTICA.

The term Erethistica is ancient classical Greek; and it signifies provoking; irritating; exciting; etc. (*Donnegan.*) The Lexicon of this author contains not less than six different and distinct terms relating to this subject. But in application to this class, the denomination Erethistica is not to be understood in its loose etymological sense any more than Narcotica.

Definition. Erethistics are articles which produce a preternatural degree of activity and an augmented exertion of the powers and energies, by which any function is discharged. A mere and pure Erethistic agent may indeed be correctly compared to the whip and the spur, which do not give any new and additional power, or energy, but only bring into greater activity that which already exists—which was not previously exerted to the same extent—and which perhaps was latent or prostrated

in a greater or less degree. This as a bare definition is applicable only to mere and pure Erethistic articles. It is not by any means true of Antisbestics and Tonics as is so often alleged. The preceding I consider as more appropriately the diagnostic definition of this class of medicinal agents; but yet, all of the most unequivocal, and of the most active of the Erethistics, when pushed to a certain extent, produce likewise what is called stupor in some part of the system. According to a deservedly distinguished medical author "stupor est singularis et molesta sensatio cum obscuritate tactus, quam excitat contusio olecranii, aliarumque partium per quas nervi majusculi sub cute transeunt; quam quisque experitur cum olecranii aut nervi fortis pressio facta est; aut cum diutius horis pomeridianis brachio incubuimus; quam experiuntur artus diu compressi vel proprio vel alieno pondere et situ non mutato." "Dolor sit qualis a millenis Formicis seu spiculis partem stupidam pungentibus." (*Pg. 24 ta Col. 1 ma. Tom. 2do Nosol. Method. Francisc. Boissier. Sauvages. Venetiis, 1773.*) Stupor or numbness is a peculiar and annoying sensation, attended with imperfection of the sense of feeling, such as is produced by a blow upon the lower and inner side of the olecranon, or upon other parts, through which rather large nerves of common sensation pass-along just beneath the skin; or such as is produced when strong pressure is made upon such nerves; or when in the night, we lie too long upon an arm; or such as we experience when a limb, without change of posture, is long compressed, either by its own weight or by some thing else. The pain of the benumbed part is like that which might be produced by a thousand small Ants, or by the pricking of a multitude of minute spicules.

This class is intirely new, as is the term Erethistica as its name, though as I have already said, the word merely is ancient and classical. My particular friends who objected to all formal classification, and particularly to all innovations in relation to this subject (though they never informed me what individual author they would have me follow, which was quite an omission on their parts, since no two authors agree) had an especial antipathy to this proposed class, and remonstrated so vehemently against it, that as a public instructor I always omitted to mention it, enumerating the articles which it comprises in the class narcotics. Some times a student would enquire of me in private, how I

could possibly reckon *Ignatia amara*, *Strychnos Nux-vomica*, etc. as narcotics, when all their medicinal operative effects are so widely different and even opposite. In these circumstances I was accustomed to mention my own private views of the matter, but did not enforce and illustrate them in detail. In fact I was so much bored and harrassed about my classification, that I very often regretted that I had ever pretended to have any; and yet when I omitted it wholly, there was always greater dissatisfaction than ever. I therefore often omitted as much of it as I could (and this class invariably) without giving rise to complaints; but as a general rule there was dissatisfaction, whatever course I took in relation to this subject. No classification whatever could be found that suited. It is any thing but desirable to be an instructor in a public institution, over which so large a host consider themselves as constituted watchmen, whose duty it is to guard against the introduction of all dangerous heresy and schism.

EUPHRÆNICA.

The term *Euphrænica* is not ancient Greek and classical, but is derived from a Greek word signifying to gladden; to delight; to cheer; to amuse; to render gay; to exhilarate. There is an ancient Greek classical term of the same import as *Euphrænica*, derived from an other part of the same verb viz. *Euphrantica*; but I did not like this as well as the term which I have chosen. Hitherto I have been in the habit of employing the ancient Greek term *Euphrasia*, in English *euphrasy*, which signifies cheerfulness; joy; exhilaration; etc. to denote the effects of a *Euphrenic*, though this term is not immediately from the same verb, that gave origin to *Euphrenic*. It has always been my opinion however that *Euphrænia* or *Euphreny* would be a better and more appropriate term.

Definition. The *Euphrenics*, *in the first grade of their operation*, obviate languor or lassitude when it exists; *in the second grade of their operation*, produce a peculiar calm, placid and pleasant state or condition; *in the third grade of their operation*, occasion a peculiar and rather agreeable preternatural, but not exhausting wakefulness; *in the fourth grade of their operation*, produce more or less positive exhilaration, which, by some articles, is increased to such a degree that the actions of the

subject are not under the control of his will, and perhaps amount to actual delirium; *in the fifth grade of their operation*, occasion a complete anæsthesia or destitution of common sensation, or a state of insensibility to pain, some times with, and some times without loss of consciousness.

If the Euphrenics are capable of destroying life, the aggregate of symptoms immediately preceding death, would constitute the *sixth grade of their operation*; but the manner in which a pure Euphrenic power destroys life is wholly and intirely unknown, so far as I have information. None of the numerous cases of death, that have been reported as resulting from what is absurdly called Chloroform, appear to have been in any way connected with its Euphrenic power. It is my full conviction that Chloroform (so called) is just as capable of destroying life, when it has not produced a single grade of a Euphrenic operation, as when it has produced every grade, except the *sixth*, with which I am wholly unacquainted.

The power on which the class of Euphrenics is founded has been known imperfectly ever since the *Ætheres* have been known; but in them it is conjoined with several intirely different and distinct powers, the metes and bounds of which do not appear to have been accurately discriminated. This power exists pure, and also variously conjoined with other powers, in numerous vegetables, and likewise in a small number of animal substances; and it is a power which produces important medicinal effects, so that it ought to be recognized in the *materia medica*. It exists in Protoxyd of Nitrogen perfectly pure, and intirely unconjoined with any different and distinct power, so that it has been better understood since the discovery of this substance. All that is new in relation to the Euphrenics is the making of this power the foundation of a class in the *materia medica* and the definition and the name. The latter two were a necessary and inevitable consequence of the first.

ORÆSTHETICA.

The term Oræsthetica is compounded of a Greek verb signifying "to arouse; to stir-up; to awaken; to excite;" etc. (*Donnegan*) and a Greek attribute signifying "susceptible; sensible;" etc. The term Oræsthesis, which I use for the effects of an Oresthetic,

as a compound of the same Greek verb ; and a Greek name signifying "susceptibility ; sensibility ;" etc.

Definition. Oresthetics are articles which, when taken internally, act primarily and immediately upon the mucous membrane of the alimentary canal, and by textural sympathy, upon all the other mucous membranes viz. the Schneiderian, the oral, the faucial, the bronchial, the uterine, the vaginal, the vesical, the urethral, etc. *in the first stage of their operation* changing action and condition in a peculiar manner, in all these membranes ; on the one hand, obviating torpor and insensibility and deficient activity i. e. in a word, prostration ; or on the other hand, obviating morbid irritability and morbid sensibility i. e. in other words, morbid susceptibility, and also irritative activity ; relieving laxity and spongy turgescence ; increasing deficient secretions, diminishing excessive ones, and favorably changing vitiated ones, when connected with prostration and atony, and not with phlogistic diathesis ; disposing aphthous affections and other ulcerations to heal ; and producing a positive augmentation of healthy susceptibility ; if pushed still farther, *in the second stage of their operation* occasioning a preternatural and sub-morbid degree of irritability and sensibility i. e. susceptibility ; if pushed still farther, *in the third stage of their operation* kindling into action a Phlogosis or Inflammation of an Erythematic sort ; if pushed still farther, *in the fourth stage of their operation* occasioning ulceration and supuration ; if pushed still farther, *in the fifth stage of their operation* extinguishing vitality i. e. producing Gangrene ; and when no other different and distinct powers are conjoined, without any other different and distinct effects.

When applied externally the Oresthetics produce the same effects upon the skin, which they produce upon the mucous membranes when taken internally. It will be observed that the *third, fourth and fifth stages of an oresthetic operation*, constitute what are commonly called Epispastic effects. It must not be supposed however that the operations and effects of the Oresthetics are confined to the mucous membranes and the skin, to which they are immediately applied. So far from it, they actually affect the whole system in a greater or less degree, as they affect the parts to which they are applied. This appears to be accomplished by

means of the several sorts of sympathy heretofore specified and defined.

This power has been confounded, time immemorial, with at least three and some times four other powers, under the most vague term in the whole *materia medica*, viz. Stimulant or Excitant. And yet the gentlemen, with whom I associated during my professional pupillage, and the early part of my professional practice, knew very well that the group of articles which I now call Oresthetics, never increased vital energy and strength of action in any part of the system; and they always kept this fact in view when they prescribed them. In most of the books of *materia medica*, the Oresthetics, when administered internally, are called stimulants, though John Murray puts Capsicum the type of the Oresthetics into the class of Tonics. When applied externally the effects of the Oresthetics are always called Epispastic; but an Epispastic operation is commonly said to be a Stimulant operation. Two classes in the *materia medica* are then confessedly founded upon one single individual power. As a public instructor I was overruled by particular friends (viz. those so much opposed to my employing any formal classification, or if I adopted one, to any variation from some commonly received author) to retain the Epispastics as a distinct class from the Oresthetics, entirely contrary to my own judgment, and my own principles. Upon this plan, the only differences that I could possibly specify between the two, were the confining of the term Oresthetic to the internal use of the articles, and the reckoning of only *the first two grades of operation*, as medicinal, the other *three* being considered as morbid; while the term Epispastic was restricted to the external application of the articles, and the reckoning of the *third and fourth grades of their operation* as medicinal, the *fifth* only being considered as morbid. From what has been said, it will be obvious that this class is new in the *materia medica*, that the name is new, and so is the definition.

ANTISBESTICA.

The term Antisbestica is made-up of a Greek preposition signifying against, and a derivative from a Greek verb signifying "to extinguish; to exhaust; to quench;" etc. Antisbesis, i. e. the operation of an Antisbestic, is compounded of the same preposi-

tion and a noun signifying "extinction; exhaustion; quenching;" etc. (*Donnegan.*) Although these compounds are not ancient and classical, yet the parts of them are so; and certainly no one can deny that they are legitimately combined.

Definition. Antisbestics are articles, which directly produce a quickly diffused and transient increase of vital energy and strength of action of a peculiar sort, primarily at least, if not mainly in the sanguiferous system, but probably in a greater or less degree, in all the parts dependent upon the nerve of chemical action nutrition and reproduction commonly called the great sympathetic nerve; thereby obviating or contributing to obviate atony, exhaustion or debility in the parts specified, when it exists.

Notwithstanding this definition we are not to suppose that the whole of the effects of the Antisbestics, under all modes of management, are transient. It is quite certain that by a continuous and suitably frequent repetition of well adapted doses of the Antisbestics, we get the real permanence of effect, which belongs to the Tonics. It is a law of the operation of the Antisbestics (just as it is of the Euphrenics) that a frequent repetition of the production of their effects, gradually renders them more and more protracted and considerable. The truth is that the real cessation of the effect of a dose, does not coincide with the apparent cessation, the former without doubt being much more protracted than the latter. Now by the mode of administering the doses above specified, there is really a prosthesis of the effects of the several doses, which some how or other (I know not in what manner) causes them to be much more enduring and permanent. On first entering upon the use of the Antisbestics, it frequently appears, for a considerable time, as if nothing valuable is likely to be accomplished by them; but when there has been a sufficiently continuous repetition of suitably frequent doses, there begins to be not only a considerable, but also a permanent increase of vital energy and strength of action in the heart and arteries. This is a mere statement of a fact, not an explanation of it.

The power on which this class is founded has undoubtedly been known for an indefinite time past; but by all authors and practitioners of medicine, that I have any knowledge of, it has always been confounded with at least three other different and distinct powers, and very often with more. At all events I can not find

that any author has ever founded a class upon this power specifically. I think therefore that I may say with certainty that the class, the name and the definition are new. The name has been much cavilled-at; but as I trust, it is legitimately formed, and also applied to that which has never had a previous name; and therefore I hope I shall be permitted to use my own term, in my own way.

It is worthy of addition in this place that as respects conveniently available articles, I consider this class as the most meager in the whole materia medica. Unlike other medicinal classes, it does not contain a single article which has no other power in addition to that, on which the class is founded, though all the powers that are ever associated with an Antisbestic power occur not only unaccompanied with any degree of Antisbestic power, but often intirely by themselves or unaccompanied by any other power whatever.

TONICA.

The term Tonica is ancient classical Greek, derived from a Greek word signifying "tension; firmness; strength; vigor; force; etc. or from a Greek verb signifying to give tension; firmness; strength; vigor; force;" etc. (*Donnegan.*) The term Tone which is ancient classical Greek, is used in medicine, to denote a healthy degree of vital energy and strength of action, in all of the subordinate parts of the human animal system, of which these qualities or attributes can be predicated. The term Entony is used in medicine to denote a preternatural and morbid degree of vital energy and strength of action in the sanguiferous system. The term Atony denotes a morbid diminution of vital energy and strength of action as respects the vital functions merely i. e. the functions discharged by the nerve of chimical action nutrition and reproduction, or in other words the great sympathetic nerve so called.

Definition. Tonics are agents which directly produce a slow gradual and permanent but moderate increase of vital energy and strength of action, of a peculiar character, primarily in a part, and secondarily in the whole of the human animal system.

This power has been long known and recognized, and a class in the materia medica has long been founded upon it; but I believe that

the name *Tonica* is not as old as the class, or the knowledge and recognition of the power. The definitions commonly found in books are more or less faulty, as I trust I shall be able to show satisfactorily in my proëm to the class.

STYPTICA.

The term *Styptica* is ancient and classical Greek, signifying astringent; styptic; etc. It is derived from a Greek verb signifying to constrict or constrict; to contract; to condense; to thicken; etc. *Stypsis* is an ancient Greek name signifying constriction; contraction; condensation; etc. and in short the effects of a *Styptic*. (*Donnegan.*)

Definition. Styptics are articles which produce a vital contraction and condensation of the soft solids, together with increased absorption and diminished excretion, particularly in diseased parts.

A pure Styptic power is commonly reckoned as intrinsically an invigorating power. This I consider to be a mistake. The sole Styptic principle of all vegetables, the Tannic (*Scytodepsic*, *Scytodephic*, *Byrsodepsic*, *Byrsodephic* or *Stryphnic*) Acid I have determined by repeated trials to be moderately exhausting. When however it is conjoined with an active Tonic principle, as in *Cinchona*, and various other vegetable articles, its exhausting power is more than counteracted. I know of no Styptic principle of chymical inorganic origin. All the Styptics from this source possess other powers in addition, and much oftener exhausting ones than any other.

The power on which this class is founded, appears to have been known from the earliest records of medicine; and the name by which I call it, would seem to be older still. From the ordinary definitions and the accompanying remarks of authors, I should judge that most, if not all of them, consider a Styptic operation as either mechanical or chymical or both in conjunction. My views differ widely from this. If they are not intelligible from the brief accompanying definition, they doubtless will be so, from my proëm to this class.

ADENAGICA.

The term *Adenagica* is derived from a Greek noun signifying a gland, and a Greek verb signifying to affect; to influence; to

control ; to lead ; to conduct ; to rule ; etc. Etymologically the term signifies articles which influence the glands, or the glandular system.

Definition. Adenagics are articles which exert a direct, an especial, a peculiar and a specific operation upon the secernents and absorbents or the glandular system generally, by which a greater or less change of action and condition is produced—a change manifested by a direct resolution of certain chronic? sub-acute and acute atonic Phlogoses or Inflammations ; certain Parabysmata and glandular enlargements ; by the obviation of certain Dysthetica ; by the improvement of certain vitiated Ulcers ; by the relief or cure of certain cutaneous diseases ; by the obviation of torpor and inactivity of all the secretories and excretories ; and as is commonly said, but I think incorrectly, more obviously the secretory apparatus of the liver ; and by a consequent increase of the secretions and excretions, as is commonly but erroneously believed, more especially of the biliary than of any other ; and also by a diminution and improvement of the secretions and excretions when they are excessive and vitiated ; the whole independent of any change in the degree of the vital energies, or the strength of the action of the sanguiferous system, and not caused by any evacuation which may happen to be produced.

It is to be observed that this class is not founded upon the mere power of acting upon the secernent and absorbent or glandular system, but on the power of acting upon it in a peculiar manner, which is explained generally in the definition, but which will be fully illustrated in the proëm to the class. But without reference to the quality of the effects, the true Adenagics do in fact operate either upon the whole, or at least upon the greatest portion of the secernent and absorbent or glandular system, though different individual articles operate with very different degrees of intensity upon different parts. It must be understood however, that the number of individual agents of this class, which do not fail of operating upon some particular part or parts, is probably not great.

The recognition of this power as a unity, and the founding of one single class upon it, appear to me to be new. Authors seem to have considered it as “legion,” judging from the number of classes that they have founded upon it. I have now before me a list of twenty, and doubtless with proper research I might find more.

But beside this, they have referred every individual agent, properly belonging to the class, as I consider it to be rightly constituted, to some other class legitimately founded upon some different and distinct power; but this I shall specify and treat-of in my proëm to this class. The name which I employ, and the definition which I give, are as new as the recognition of the power as a unity, and the foundation of a class upon it. As the Adenagics are incidentally, though not essentially, attended with an evacuation, they may perhaps be considered as a connecting link between the first nexus alliance or group of classes and the second.

The second nexus alliance or group of classes, may be considered as characterized by the circumstance that their essential operation consists in a supposed remedial evacuation from excretories remote from the parts on which the agents producing them make their primary and principal, if not their sole impression, as I think, never coming into contact, and at all events not immediately, with the excretory more especially affected. It is however to be particularly remarked that a distinction between this group of classes, and the next, does not depend merely upon the establishment of the doctrine of the production of the remedial effects of medicines previous to, or without their reception into the mass of the circulating fluids. The distinction remains equally obvious, in whichever way any individual may choose to consider this point as decided.

NEXUS SECUNDUS.

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| 13. DIURETICA vel URAGOGA, | 16. ? EMMENAGOGA, |
| 14. DIAPHORETICA vel HIDROTAGOGA, | 17. ? ECBOLICA. |
| 15. ? BLENNAGOGA, | |

DIURETICA.

The term Diuretica is regularly derived from an ancient and classical Greek verb signifying "to discharge urine" and this is made-up of a Greek preposition signifying "through" or "by" and a Greek name signifying "urine." The term Uragoga is derived from an ancient and classical Greek name signifying "urine" and an ancient and classical Greek attribute, which signifies "alluring; leading; guiding;" etc. (*Donnegan.*) Diuresis is a regularly formed name, from the ancient and classical Greek verb above alluded-to, which signifies "to discharge urine."

This term of course signifies "a discharge of urine," and consequently the effect of a diuretic.

Definition. Diuretics or Uragogues are articles, which by a direct, immediate and exclusive operation upon the renal glands or kidneys, restore the excretion of all those effete elements of the system, which pass-off as salts held in solution by water, when such excretion is suspended; augment it, when such excretion is diminished; and increase preternaturally the proportion of water in this excretion, when it was previously in the natural quantity; and all this, without any influence upon any other part or parts of the secernent and absorbent or glandular system, and without either increase or diminution of vital energy and strength of action, but merely by an increase of excretory activity.

Diuretics or Uragogues do not appear to change the quality of the urine in any way, except to augment the proportion of water which it contains, when this excretion is increased preternaturally by them. In the proëm to this class which is soon to follow, I shall endeavor to explain the renal function with reference to the animal economy, without a knowledge of which, the true uses of Diuresis, as a remedial process, can not possibly be adequately understood. This class, I believe, is as old as the *materia medica*, and so I suppose are the names. The limitation and restriction of the class, and the definition which I adopt, are new.

DIAPHORETICA.

The term Diaphoretica is ancient classical Greek, signifying articles which promote perspiration and sweating. The term Diaphoresis is likewise ancient and classical Greek, signifying perspiration and sweating. The term Hidrotagoga is from an ancient and classical Greek noun, signifying any thing that oozes-out, drop by drop, and hence sweating, and an ancient and classical Greek attribute signifying "alluring; leading; guiding;" etc. Hidrosis is an ancient and classical Greek noun, signifying "perspiration; sweating;" etc. (*Donnegan.*)

Definition. Diaphoretics or Hidrotagogues are articles, which by a direct, immediate and exclusive operation upon the cutaneous hydrothermal excretories, restore the elimination of the effete heat of the system in a latent state in combination with water so as to constitute vapor; augment it when it is diminished; and

produce an excretion of mere liquid water, when the cutaneous hydrothermal discharge was previously in a normal state; and all this without any influence upon any other part or parts of the secernent and absorbent or glandular system, and without either increase or diminution of vital energy and strength of action, but merely by an increase of excretory activity.

Diaphoretics or Hidrotagogues do not appear to change the quality of the diaphoresis or hidrosis, except to cause the excretion of mere water not converted into vapor by heat in a latent or combined state. In the proëm to this class, I shall endeavor to explain the function of the hydrothermal excretories of the skin with reference to the animal economy, without a knowledge of which, the true uses of diaphoresis or hidrosis, as a remedial process, can not possibly be adequately understood.

This class of remedies has been recognized in the materia medica from the remotest times of medicine, and the names of it are equally old. The limitation and restriction of the class, and the definition, which I adopt, I believe are new.

BLENNAGOGA.

The term Blennagoga is from an ancient classical Greek noun signifying "mucus," and an ancient classical Greek attribute signifying "alluring; leading; guiding;" etc. (*Donnegan*.) The term myxagoga is from an ancient classical Greek noun signifying "mucus; phlegm;" etc. and the above mentioned and explained attribute. The term Phlegmagoga is from an ancient classical Greek name, signifying (in Hippocrates) "mucus; phlegm;" etc. and the attribute above mentioned and explained. (*Donnegan*.)

Definition. Blennagogues, Myxagogues or Phlegmagogues (if there are in fact any such agents) are articles which directly, immediately and exclusively increase excretory activity in the mucous follicles of all the mucous membranes, without affecting any other part of the secernent and absorbent or glandular system, and without either increase or diminution of vital energy and strength of action.

There is certainly no such class of agents in existence as true and proper Chremptica or Expectorantia, i. e. articles which directly, immediately and exclusively increase excretory activity

in the mucous follicles of the bronchial membrane, without affecting any other mucous membrane, or any other part of the secernent and absorbent or glandular system. Neither do I think that there is any such class of agents yet known, as true and proper Blennagogues, Myxagogues or Phlegmagogues affecting no other part of the secernent and absorbent or glandular system except the mucous membranes merely. There are Adenagics however, that very decidedly increase the excretory activity of the mucous follicles of the mucous membranes as a part of a general operation upon the secernent and absorbent or glandular system. But as all writers upon the *materia medica*, and on the practice of medicine, invariably recognize Chremptics or Expectorants (as appears to me an utterly impossible class) the most that we can do, is to retain a class of Blennagogues (not an impossible one) as a candidate for future discovery, and as a peg upon which to hang many statements, that seem to be necessary in a treatise on *materia medica*.

This possible class is new, and so is its name; which however (as I trust) is formed legitimately, from ancient and classical Greek terms. The limitations and restrictions of the definition are exactly such as distinguish true and proper Diuretics or Uragogues, and true and proper Diaphoretics or Hidrotagogues, from efficient adenagics affecting these excretories the most powerfully.

EMMENAGOGA.

The term Emmenagoga is made-up of an ancient classical Greek word signifying the monthly uterine excretion of women between certain ages, and an ancient classical Greek attribute that has already been mentioned and explained several times.

Definition. True and proper Emmenagogues (if there are any such agents) are articles, which by a direct, immediate and exclusive operation upon the catamenial excretories of the uterus, produce the catamenia, when they are retained or suppressed; and increase them, when they are deficient in quantity; and this without either augmentation or diminution of vital energy and strength of action, but merely by a production of increase of excretory activity.

I have no knowledge of any true and proper Emmenagogues, and the same is admitted by most authors on the *materia medica*.

Swediaur says "menagogum specificum nullum novi præter terrorem subitum." (*F. Swed. Mat. Med. Par. Ann.* 8 vo. p. xii. *conspectus*.) Stephenson and Churchill think that two true and proper Emmenagogues have been recently ascertained viz. *Che-nopodium Vulvaria* (*Linn.*) and *Caroxylon foetidum* (*De Cand.*) The public require confirmation of this. I very well know a considerable number of adenagics that act with sufficient efficiency upon the catamenial excretories, as a part of a general operation upon the secernent and absorbent or glandular system, to be often capable of restoring this excretion, when idiopathically suppressed; and I also know various articles belonging to five other classes at least, that are often capable of restoring this excretion when symptomatically suppressed; but I need not say that none of these are true and proper Emmenagogues; and yet none of the classes of medical students, that ever came under my instruction, were ever satisfied without one or two lectures upon the subject of Emmenagogues. How it may be with my readers, I am unable to conjecture. The admission of a class of Emmenagogues, even though accompanied with the admission that there is no just foundation for it, is old; and so is the name. A definition, or a specification of what alone would constitute true and proper Emmenagogues is new. I have long been in the habit of supposing it as on the whole probable that articles will yet be discovered, which will be found capable of acting directly, immediately and exclusively, and without affecting any other part or parts of the secernent and absorbent or glandular system, upon every individual excretory and secretory, that is ever affected by the adenagics, to increase their activity, and at least render them more dilute if it did not change their quality in any other manner. The actual facts however seem to favor the opinion that this is to be expected only in reference to the four great emunctories of the system viz. the lower extremity of the alvine canal; the renal glands or kidneys; the hydrothermal excretories of the dermatic membrane or skin; and the carboniferous excretories of the bronchial membrane. We are already acquainted with agents, that act directly, immediately and exclusively to increase the activity of three of these emunctories, though we are ignorant of any that act in this manner upon the fourth.

ECBOLICA.

The term Ecbolica is a regular derivative from an ancient and classical Greek verb, signifying to throw-out; to cast-off; to reject; to expel; to produce abortion; etc. The term Ecblexis is a regularly derived noun from the ancient classical verb above referred-to, signifying "abortion; a throwing-out; a casting-off; a rejection; an expulsion;" etc. I think that this noun is in regular use, in modern Greek. The term Ecbole is an ancient classical Greek noun having the same signification as Ecblexis. The term Ectitrotica is regularly and legitimately formed from an ancient and classical Greek verb signifying "to cause abortion or miscarriage; to produce parturition; etc. The term Oxytocica is a regularly and legitimately formed one from two ancient and classical Greek words, whose joint import cause it to signify "sharpeners of parturition; promoters of parturition; etc. Oxytocia is an ancient classical Greek term having exactly the same import. The term Ecbolica is not (as far as I know) ancient and classical, and yet I suspect that it is old. I suspect that the same is true of the term Ectitrotica; and so, most likely, it is of the term Oxytocica. But the term Oxytocia, I believe is the ancient one, that was employed in the same sense as Ecbolica, Ectitrotica and Oxytocica. The fact that there are two regular nouns connected with Ecbolica, viz. Ecblexis and Ecbole, both denoting the effect of an Ecbotic, and none connected with Ectitrotica or Oxytocica, would give Ecbolica the preference, even if it did not deserve it in all other respects.

Definition. True and proper Ecbotics (if there any such agents) are articles, which act directly, immediately and exclusively upon the gravid uterus, contributing to produce, or actually producing parturient contractions when they did not previously exist, and increasing them when already begun, and this without increase or diminution of vital energy and strength of action, and without being preceded and occasioned by any other operation.

All this (as appears to me) is necessary to constitute a true and proper Ecbotic. But are there any such agents? I never could actually satisfy myself that there are. In fact all positive evidence seems to me to be against it. If there is such a class of agents, I should think that its proper place in my system would be in the first nexus alliance or group, immediately after the Erethetics, or

between them and the Euphrenics. If it were absolutely certain that this class is spurious, it would require to be thrown into the nexus alliance or group of spurious classes, which never the less require consideration and discussion, in order to show that they are spurious, since they are almost universally received by authors and practitioners. Considering the class however as in some measure doubtful, or at least, as a fair candidate for discovery, when proper investigation has been made, I have been in the habit of locating it immediately after Emmenagogues and Blennagogues, classes that I have supposed might yet be discovered, though perhaps the character of the nexus alliance or group may not be strictly applicable to the Ecboles. An explanation of the manner in which *Spermœdia Clavus*, ? *Sclerotium Maydis* or perhaps ? *Uredo Maydis*; *Botrophis Actæoides*, and several of the species of *Cimicifuga* and *Actæa*, etc. seem to prove indirectly ecboles would involve too much for this synopsis, so that it must be deferred to the proëm of this class. This class seems to be an old one, which never attracted so much attention as it deserved, which fell into unmerited neglect for a long period, but has been revived in recent times. So far as I have ever seen any definitions of this class of agents, they have always been so loose and vague as to comprise all articles, whether operating either indirectly and mediately or directly and immediately. A definition limited and restricted like the preceding I think must be new.

The third nexus alliance or group of classes may be considered as characterized by the circumstance that their essential operation consists in a supposed remedial irritation and consequent evacuation from excretories, to the outer extremities of which the agents are immediately applied, and upon which they are commonly supposed to make their sole useful impression. I do not think that the evacuation produced ever contributes to the remedial efficacy of the processes, at least in any material degree; but a different opinion is entertained, as I believe, by most physicians. This character is strictly true of the first two classes of this nexus alliance or group, but I doubt whether it is as perfectly so, of the last two. I have already said, I do not attach much importance to this grouping of the classes, or to the characters which separate the groups, and therefore it is not worth while to multiply the groups. If it were worth while to increase the number of

groups, the last two of the third nexus might very well be separated from the first two.

NEXUS TERTIUS.

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|------------------|-----------------|
| 18. ERRHINA, | 20. EMETICA, |
| 19. ESSTOMATICA, | 21. CATHARTICA. |

ERRHINA.

The term Errhina is ancient classical Greek. It is compounded of a preposition signifying *in*, and a noun signifying *nose*. It is therefore not new. I know of no technical term denoting the whole, or even the essential effects of an Errhine.

Definition. Errhines are articles, which, by direct topical application to the Schneiderian membrane, produce a peculiar irritation in that texture, some times occasioning sneezing, and always increasing secretory activity in the mucous follicles, and causing them to effuse a larger quantity of fluid.

Acrimony is not necessary to an Errhine power, since there are many non-acrid as well as acrid Errhines known and employed. The discharge produced by an Errhine, will be merely a thin sero-mucous fluid, when the agent employed is non-acrid and mild, and of a peculiar character; it will be merely serous, when the agent is moderately acrid and more active, and of such a character as to occasion such a secretion; and I have seen it mucipurulent, or even purulent and of long duration, when the agent used was very acrid, or of a character favorable for the production of such a secretion. The class Errhina is probably as old as the name, but my definition is more precise and specific than any which I now recollect in any of the ordinary books.

ESSTOMATICA.

The term Esstomatica is formed after the model of Errhina, from an ancient Greek preposition signifying *in*, and an ancient Greek noun signifying the *mouth*. It is not ancient nor am I at all satisfied with it, but it is in all respects as good as its model. I should have been glad of a better term, but could not well devise one. There is no name for the effects of an Esstomatic or a Sialagogue immediately connected with either of these terms; but we have a Latinized-Greek term viz. Ptyalismus, and a pure Latin one viz. Salivatio, having this signification, both of which are

ancient and classical. We might have had the term Ptyalagoga as well as Sialagoga, and Sialismus is as ancient and as classical as Ptyalismus. It is therefore to be regretted that cognate terms were not selected as names for the agents, and for their effects. I do not consider that it is ever too late to make amendments, but in this particular case I leave it to somebody else beside myself to take the lead, since I deem this correction of less importance than those which I have made.

Definition. Esstomatics are articles, which, by direct topical application to the mouth, increase the activity of the salivary glands, and often also of the mucous follicles, to whose excretory ducts they are immediately applied.

It will be observed that authors on materia medica, include in this class, not only articles that operate locally and by mere topical application, but also articles that operate by being taken into the stomach, and that affect the salivary glands as a part of a general operation upon the secernent and absorbent or glandular system at large. These are certainly quite different operations, and as I think should by no means be confounded. It will be perceived therefore that this definition excludes the Mercurials, when they operate by being taken into the stomach, from this class, and also all the other Adenagics, which occasionally produce a greater effect upon the salivary glands, than upon the other subordinate parts of the secernent and absorbent or glandular system. The propriety of this, as I trust, will be evident, from the consideration that the ptyalism of the Mercurials when taken into the stomach, and that of the other Adenagics, which produce the same effect, is of no remedial benefit whatever, being never of the least service in the cure of disease, but at most only a test that the secernent and absorbent or glandular system is under the influence of the article which produces the salivation, a test however, which is never necessary for the production of all its desirable effects. Assuredly medicines are not to be classified by effects not at all medicinal. Besides, the *modus operandi* of the Mercurials, and the other salivating Adenagics, are certainly as different from my Esstomatics in their effects upon disease, as in the manner in which their effects are produced. When therapeutic applications and *modus operandi* are so diverse, it is certainly an absurdity to associate such articles in one class. As I

have limited and defined this class I think the term Sialagoga is inadmissible in application to it, since all the other terms formed upon this model are applied to agents which when taken into the stomach, act directly and immediately to increase the activity of certain excretories, either with or without affecting any other part of the secernent and absorbent or glandular system. Thus Uragoga and Hidrotagoga are applied to articles, which, when taken into the stomach, directly, immediately and exclusively increase activity in certain excretories; while Blennagoga and Emmenagoga are applied to articles, which when taken into the stomach, increase activity in certain excretories, as a part of a general operation upon the secernent and absorbent or glandular system. Now we need the term Sialagoga to denote the increased activity of the salivary glands, which a considerable number of articles produce, when taken into the stomach, as a part of a general operation upon the secernent and absorbent or glandular system. From the statements here made it will be obvious that the name and the definition of this class are new, at least as far as I have knowledge.

EMETICA.

The term Emetica is ancient classical Greek. There are no less than fifteen different terms in the common Greek Lexicons, all connected with, and either directly or indirectly derived from a Greek verb signifying to vomit, I believe, a greater number than is contained in the language, in relation to any other medicinal operation, from which I think we may infer that Emetics were deemed highly important, and were much employed by the ancient Greek physicians.

Definition. Emetics are articles, which, when taken into the stomach, directly and immediately produce, independent of odor taste or bulk, *first* and essentially upward peristaltic action of the stomach and œsophagus; *second*, usually but not always, a violent and perhaps spastic action of all the muscles of expression but more especially those of the trunk, which is commonly known by the name of retching, by means of both of which, but mainly by the former, there is a more or less perfect rejection of the contents of the stomach by the mouth. Associated with these, there is frequently but not always, *third*, increased secretory activity of

the mucous follicles of the stomach, by means of which a thinner and more abundant effusion from these, takes place; and if the vomiting is violent and protracted, the secretions from the pancreas in all probability, and from the liver certainly, are also augmented. In most cases there is still in addition to this, *fourth* more or less nausea, either preceding, accompanying or succeeding, or the whole together, the act of vomiting.

It must be observed that there is no such thing as upward peristaltic action of the upper and smaller intestines. Whenever any of their contents are rejected by vomiting, they are exantlated or pumped-up, by means of the vacuum produced by rapid and more or less protracted upward peristaltic action of the stomach and œsophagus.

This class of agents is, of course, as old as its name. My definition may perhaps be considered new, since it involves more or less of the physiology of vomiting, though by no means the whole of it. This will be further explained in the proëm to the class.

CATHARTICA.

The term Cathartica is ancient classical Greek, derived from a verb signifying to purge. Catharsis denoting the operation and effects of a Cathartic is likewise ancient classical Greek.

Definition. Cathartics are articles, which, when taken into the stomach, directly and immediately produce, *first*, a secretion of more or less air into the upper and smaller intestines; *second*, a secretion of more or less serum or water into the same cavity, both secretions being from the mucous follicles; *third*, some times but not always, an increased secretion from the pancreas in all probability, and from the liver certainly; *fourth*, preternaturally increased downward peristaltic action of the whole intestinal canal, beginning in the upper and smaller intestines, and extending to the lower and larger; by means of which there are preternaturally frequent, as well as preternaturally liquid alvine discharges.

Pure and simple Catharsis and pure and uncomplicated Diarrhœa do not differ in any material respect from each other, except as respects their cause, and the circumstance that Catharsis is usually suspended when the Cathartic agent has passed-off, while Diarrhœa continues for an indefinite period, longer or shorter in

different cases. There are as many varieties of Catharsis as of Diarrhœa. Catharsis then is merely a factitious Diarrhœa. These facts will greatly assist us in judging of the wisdom of employing Cathartics in Diarrhœa, a piece of homeopathic (so called) practice, in all respects, except the size of the cathartic doses, which has been common, and almost universal, time immemorial.

The class Cathartica has been recognized from the earliest records of medicine. My definition of Cathartics will perhaps be admitted to be more suggestive of the physiology of the process than the ordinary definitions of authors.

The fourth nexus alliance or group (or rather section) of classes, is characterized by the fact that the operation of all the agents, which it comprehends, is purely and exclusively chemical. Their effects are indeed commonly considered as indirectly medicinal; though I think that this may very well be doubted. This point however will be discussed in the proëm to the class.

NEXUS QUARTUS.

22. ANTIOXYNTICA.

This term is new, coined by myself (I trust legitimately) as a substitute for the barbarous one *Antacida*, which may be said to be "denominatio damnanda," a mongrel half Greek and half Latin, and formed contrary to all correct rules. Perhaps *Antoxyca* might have been a correct term; but of this I was doubtful, and am still so. At any rate I think it would have been as good as *Oxydum*—a term that has been adopted into the nomenclature of chemistry.

Definition. Antoxyntics are articles which combine with and neutralize free acids in the alimentary canal.

I doubt whether this can be properly considered as a true medicinal effect. The presence of a little of some acid in the stomach does not constitute disease. It is rather the act of the morbid secretion of such acid, or perhaps better the morbid condition of the stomach, which gives rise to this secretion, which constitutes the disease. The mere neutralization of such acid does not contribute in the least to remedy such disease; but on the contrary, the agents employed for the neutralization of the acid, (I believe) invariably aggravate the true disease. With what pro-

priety this is reckoned a class of medicinal agents, my readers can therefore well judge. It is my belief that, if it were struck from the *materia medica*, the department would suffer no real loss. That patients take *Antoxyntics*, and get well after it, I do not at all pretend to deny, but only the "post hoc ergo propter hoc."

This is an old class, though not as ancient as most of the classes that are generally received by physicians of the present times.

The *fifth* nexus alliance or group (or rather section) of classes, comprehends those remedial processes, whose operation is merely and exclusively mechanical. Their remedial effects are therefore indirect. They may all be fairly reduced to a single class.

NEXUS QUINTUS.

23. ERGASTICA, [*Ascetica, Gymnastica, Tribica, Tribaca.*]

I have found the greatest difficulty in the selection of a perfectly appropriate, and at the same time, wholly unobjectionable term, to denote or designate those remedial processes, which mechanically agitate and exercise the whole body, in such a manner as to contribute to the restoration of health, under certain circumstances.

Ascetica is an ancient and classical Greek term, signifying "pertaining to exercise, gymnastic contests, the active or ascetic life." (*Donnegan*)

Ascesis is an ancient and classical Greek term denoting "exercise; practice; application; the mode of life and exercises of Athletes;" etc. (*Donnegan.*)

Both these terms, and several others relating to the same subject, are derived from an ancient Greek verb, signifying (among other things) "to exercise; to train; to form by exercises;" etc. (*Donnegan.*) The only objection to this term is its confinement in common English to its ecclesiastical sense of "devotional exercises," (*Donnegan*) such as flagellation, and other acts of penance, employed by fanatical recluses, not only among the heathen, but in the papal or Romish church, so that all rigid and austere persons, who are severe in self-denial and mortification, have long been called *ascetics*.

Gymnastica is an ancient and classical Greek term denoting those athletic exercises which were performed naked, in a gymna-

sium by the gymnastæ or professed athletes. *Gymnasia* denotes that exercise or training, which is performed or received naked. *Gymnasis* has precisely the same signification, and is the more elegant and preferable term. All of these terms, and many others, are immediately connected with an ancient Greek verb signifying "to exercise naked in a palæstrum or circus; to exercise or train as a preparation for athletic exercises;" etc. (*Donnegan*.) The theme of the whole is an ancient Greek attribute signifying naked. If it were not for the essential and absolute nakedness implied by all these terms, they might answer in application to this class of processes or agencies.

Tribica is an ancient and classical Greek term often found in composition, as *Pædotribica*, and doubtless existing in a separate form though not to be found in any Greek lexicon, that I happen to have at hand just at present. It has the signification of pertaining to exercise, drilling, training by practice; etc. *Tribaca* is found in Greek lexicons, and in signification it has reference to being practised, trained, habituated or hackneyed in any exercise, labor, etc. These and numerous other terms, are all connected with an ancient Greek verb, signifying, among other things, to exercise; to practice assiduously; to drill; etc. It signifies also to rub.

Triptica is a derivative from an other part of the same verb; but it is employed exclusively in the sense of rubbing, and the numerous other processes comprehended in what is often called *Shampooing*. *Tripsis* is an ancient and classical Greek term, signifying rubbing, and every thing which constitutes *Shampooing*. *Tripsimon* (I believe) is the modern Greek term for *Shampooing*. If *Triptica* and *Tripsis* were not confined exclusively to *Shampooing*, and if these two were not so near *Tribica* and *Tribaca* in origin and import, one or the other of the last two terms would be appropriate as a name of this class of processes or agencies.

Ergastica is an ancient Greek term having a reference to work, labor, etc. *Ergatica* is likewise an ancient Greek term of just about the same signification. *Ergasia* is an ancient Greek term signifying work, labor, toil, etc. (*Donnegan*.)

These and numerous other cognate terms are all connected with an ancient Greek verb signifying to work; toil, or labor, etc. All of these terms seem to have reference to the work, toil or

labor of a mechanical or agricultural employment, rather than to the processes or agencies comprehended in this class; and as the common work, toil or labor of a mechanical or agricultural employment may be made to subserve all the remedial purposes of all the processes or agencies, that I shall hereafter mention as falling appropriately into this class, the terms that I am now considering are not as inappropriate, as at first view they may seem. However I must admit that I am not at all satisfied with them, nor am I with any other that I have mentioned.

Definition. Ergastics are processes or agencies, which mechanically agitate and exercise the whole body, in such a manner as to contribute to the restoration of health, under certain circumstances.

The group of processes or agencies constituting this class, I have never known to be brought together as medicinal processes or agencies, and consequently they have no established name as a class in the *materia medica*. They are commonly reckoned as a part of what is usually called regimen, and as such, are rarely spoken-of, or treated-of, in the aggregate. So far as this is ever done, I believe that they are oftener called *Gymnastica* or *Gymnastics* than any thing else. To this name as I have elsewhere said, I have a very great repugnance. In its intrinsic etymological import (as I also state elsewhere) it is wholly inappropriate; and as this is so obvious to every one who has even a smattering of Greek, it greatly augments the objections to it. It is true, there are a greater or less number of objections to all the names, that have occurred to me, as being at all suitable for the designation of this group of processes or agencies.

The foregoing classes comprehend all the remedial powers and operations of medicines with which I am acquainted, or of which I have any knowledge. But nearly all the young men that have ever been under my instruction, have felt as if their course was incomplete, without a consideration of what are commonly called *Anthelmintica*, *Antilithica*, *Antidota* and *Specifica*. It seemed to be in vain to say that the agents, to which these general terms are applied, did not operate respectively by virtue of so many distinct individual powers, that could be properly made the foundation of so many classes, and that had not already been made the foundation of classes, but that each operated by virtue of sev-

eral different and distinct powers already the foundation of so many classes. They would have all this definitely pointed-out and shown. Perhaps some of my readers may have the same feelings. I shall therefore submit some considerations in relation to these four spurious classes, more especially as they are still recognized as legitimate, by very many medical writers.

NEXUS NOTHUS.

24. ANTHELMINTICA,

26. ANTIDOTA,

25. ANTILITHICA,

27. SPECIFICA.

ANTHELMINTICA.

The term Anthelmintica is made-up of an ancient Greek preposition signifying against, and an ancient Greek noun signifying what are commonly called intestinal worms.

Definition. Anthelmintics are articles, which are supposed to have a peculiar power, *first*, to obviate that morbid condition of the alimentary canal, which is essential to the development of intestinal entozoä; *second*, to destroy them, when they are developed; *third*, to evacuate them either living or dead. The powers, by which the development of intestinal entozoä is prevented, and by which they are destroyed, or evacuated, are numerous, and are already the foundation of other classes in the materia medica, as I trust I shall show satisfactorily, in my proëm to this spurious class. Accordingly upon my plan there is no foundation for a class of Anthelmintics. In the proëm to this supposed class, I shall endeavor to point out how many and what powers are capable of operating as Anthelmintics. The term and the class are both old, but are none the more entitled to be retained on that account.

ANTILITHICA.

The term Antilithica is compounded of a Greek preposition signifying *against*, and a Greek noun signifying *a calculus* or *stone*.

Definition. Antilithics are articles, which are supposed to have a peculiar power, *first*, to obviate the morbid condition of the system, which constitutes what is called calculous diathesis—a condition essential to the formation of calculous concretions within the animal system; or which, *second*, breaks-down and

destroys such concretions when they are already formed. Articles producing this latter effect are called Lithonthryptics, and are by many considered as constituting a distinct class from the articles, which produce the former effect.

The term Antilithica is modern, as I believe; and it has been said to have originated with Dr. Benjamin Smith Barton of Philadelphia; but this I think must be an error, since it is too obvious a word to have escaped employment till his time. Many terms have been in use time immemorial, without ever having got into dictionaries. However I have never made any search to ascertain whether this term may not be in books of a much older date than Dr. Barton's time; and as I reject the class, I shall not now be at the trouble of doing it. The term Lithonthryptica is made-up of a Greek noun signifying a calculus or stone and a derivative from a Greek verb signifying to break in pieces; to crumble; to grind down; etc. (*Donnegan.*) The term Lithonthryptic is not as comprehensive in its signification as Antilithic. I believe that it is a much older term, from which it follows that the class of which Lithonthryptic is the name, is much older than the more comprehensive class called Antilithics. Hereafter, in my proëm to the spurious class Antilithica, I shall endeavor to show that there is no single individual power in the materia medica, to which either the term Antilithic or the term Lithonthryptic can be applied with the least shadow of propriety; and that the articles or agents, to which these terms are applied, operate by virtue of various, different and distinct powers, all of which are the foundation of legitimate classes.

ANTIDOTA.

Antidota is an ancient and classical Greek term, signifying "given in return, as a recompense, as a compensation, or instead of; given for; given as a remedy; a remedy against poison." This term is made-up of a Greek preposition signifying against, and a Greek attribute signifying "given; granted;" etc. (*Donnegan.*) So far as its etymological and intrinsic signification is concerned, this term is just as appropriate to what are called Anthelmintics, Antilithics, Antoxyntics, Antisbestics, etc. as to the supposed class, to which it is actually applied. In fact, as it merely means something given against something, it is equally

applicable to every class in the *materia medica*. And yet faulty and exceptionable as this term really is, it is almost universally received by the medical profession of the present times. As appears to me those who receive such a term as this have no right to be fastidious or critical about any terms whatever. As I shall show hereafter, if this class were to be admitted, it would be equally comprehensive with the name.

Definition. Antidotes are articles, which are supposed to have a peculiar power, *first*, either to decompose or destroy; *second*, to neutralize chimically all material poisons; and *third*, to counteract pathologically all their morbid effects.

The class of Antidotes has been received time immemorial. The ancient Greeks recognized it, as do the physicians of the present time; and yet the very idea of such a class carries absurdity upon its face, since all that is meant by a poison is so much of any thing, as will kill a person. Now there must be just as many modes of poisoning, as there are different and distinct powers, by which life may be destroyed, which would require as great a diversity of chymical habitudes and relations to decompose and destroy or neutralize all the various poisons, as there is diversity of poisons. There is scarcely an article capable of destroying life, which does not produce several grades of effect quite diverse from each other and requiring equal diversity of treatment. According to the rapidity and the manner, in which there is exposure to the influence of Lead, for example, is the number and diversity of pathological conditions which it produces. I will not pause here to reckon them up, but there can not be less than half a dozen, and probably there are more; and the same is equally true of numerous other poisons. Such being the fact, the catalogue of the Antidotes would be coëxtensive with the *materia medica*. But all this will be considered in the proëm to this supposed class, and is out of place in this synopsis.

SPECIFICA.

The term Specifica is probably as old as the Latin language as it has come down to us in books. It means intrinsically "special, specific, particular," etc. It is in reality much too general a term, as a name for a class of remedies. Besides it is pure Latin, and the medical canon requires that all names of classes in the mate-

ria medica should be Latinized Greek. But as I do not admit any such class as Specifics, I need not here discuss the merits and demerits of this name.

Definition. Specifics are articles, which are supposed to have a peculiar power, *first*, to cure infallibly all the diseases, to which they are truly appropriate; or *second*, to cure them upon a principle or principles wholly and intirely peculiar to themselves, and not common to any other agents.

There are a few, but not many works, upon the materia medica that admit a class of Specifics; and yet, I scarcely ever conversed with a physician who had not a full belief in the existence of very numerous articles of this character. Who is there that does not consider that Mercurials act as perfect and complete Specifics in the cure of Lues Syphilis? Who does not consider that Cinchona acts as a perfect and complete Specific in the cure of Intermittents? Who does not consider that Sulphurum acts as a perfect and complete Specific in the cure of Ecpyesis Scabies? But I shall discuss all essential points in relation to this subject in the proëm to this class, and therefore sufficient has been said in this place.

Some thing too has always been required by my pupils on the subject of Dietetics for the sick. In many cases this is as important as medical treatment; indeed in some cases it is more so. After the proëms to my classes, I shall therefore offer a few considerations upon this subject. I can discover no good reason why it may not be as interesting to my readers as to medical students. Let it be distinctly understood that it is no part of my plan to treat of Dietetics for those in health but only for the sick; though I may have occasion to refer to some perfectly evident principles on the subject of Dietetics for the healthy, for the purpose of explanation and illustration.

I have given this synopsis in this place, because it was deemed important by some of my friends to have an express and formal explanation and definition of my terms and classes, before this could be regularly arrived-at, in the proëms to the several classes which I adopt. I can not say that I am as well satisfied of the necessity of this measure, as some of my friends appear to be, since my terms are all very plain and obvious Latinized Greek, and since the understanding of my system of classification must be

exceedingly imperfect without a catalogue, and a proper arrangement and grouping of the several articles belonging to each class. For this however, my readers must be contented to wait patiently, till the publication of the proëms to the several classes. Should this work ever come to a second edition, this synopsis will of course be cancelled, as being no longer necessary or in any degree useful, because all that it contains will be found in the proëms, all of which will be printed at once, and not periodically like the present edition.

I take this occasion to call the attention of my readers to the fact that some individual or individuals (I know not whom) have, I believe more than once, caused to be printed what has been said to be a synopsis of my classification. I know not how such synopses have been made-out, though I suppose from recollection or notes, by some one who had attended upon my instructions. Lest it should be supposed that I have had several schemes or plans of classification, each materially different from that here so imperfectly and so incompletely given, I feel constrained to say that what has been published as my classification, is as different from what it ever was, or is now, as my classification is from that of any other person, who has ever had any system or method at all. What I have here given is exactly what my system has always been, ever since I have had one. It is the fact however, that in my public instructions, I have always omitted to treat of the Nausiatica as a class, merely alluding to them as justly constituting a class. I always took the same course with the Erethistica (as I have already said) at the earnest desire of particular friends much older than myself, who were particularly and thoroughly opposed to all classification, and much more especially to any innovation upon anything, that had become hackneyed or common. I likewise treated of the Epispastica as a distinct class from the Oræsthetica, arranging it as a kind of appendix to my third nexus alliance or group, immediately after Cathartica, just as I arranged the doubtful class Ecbolica, as a kind of appendix to the second nexus alliance or group, though if well founded, it would undoubtedly fall into my first nexus alliance or group, immediately after the Erethistica. All this, I repeat, I did in deference to the opinions of friends, though never without giving my own opinion briefly, as to what would have been a more correct

course. As to the names of the classes that are new in the *materia medica*, I did not at first use those, which I have here given. For several years, I tried to dispense with any and all names for them; but I found this impracticable. I do not consider it as a merit in any one, never to have changed his opinion upon any particular topic; but I had bestowed so much, and such protracted attention upon the topic of the classification of the *materia medica*, before I committed any thing to writing upon the subject, that without the detection of some error as respects supposed facts, I have had no occasion to change. The determination of some hitherto doubtful points, may hereafter create a necessity for variations in my present system, as may the discovery of new and additional facts; but I repeat that since I first committed my system to writing, and employed it in instruction, I have made no material change—no change worthy of being specified; so that whatever may have been published to the contrary has been error. I make no complaint on this score, since I am perfectly apprised of the difficulty of always understanding correctly a rapidly uttered lecture, and especially where of necessity there is always a great deal of abridgement, and a great deal of omission.

PROËM TO THE CLASS ANTIPHLOGISTICA.

The term *Antiphlogistica* is compounded of the Greek preposition ἀντι, *against*, and φλογιστικός, an attribute derived from the verb φλογίζω, *to burn*. So far as I know, the term ἀντιφλογιστικά did not exist in ancient Greek; but it is as regularly formed as it would have been, had it actually been ancient. The term *phlogistic*, from the Greek φλογιστικός, an attribute from the Greek verb φλογίζω, *to burn*, must of course have been in use before the term *antiphlogistic*. The attribute φλογιστικός, I believe did not exist in ancient Greek; but it is as regularly formed as if it had been done by Hippocrates himself.

The attribute *phlogistic* is perfectly equivalent in import to the attributes *sthenic* and *entonic*. Each of these terms denotes a morbid preternatural increase of vital energy and strength of action in the heart and arteries, and doubtless also in the rest of the sanguiferous system. An *Antiphlogistic* therefore must necessarily be a remedy for a *phlogistic*, *sthenic* or *entonic* state, condition or diathesis. Now a remedy for such a state, condition or

diathesis must necessarily be a direct exhausting agent, and that to a sufficient extent to counteract and obviate the morbid preternatural increase of vital energy and strength of action in the heart and arteries, and doubtless also in the rest of the sanguiferous system, which constitutes a phlogistic, sthenic or entonic state, condition or diathesis.

But there are some other terms in constant use in medicine, cognate with phlogistic and antiphlogistic, that have a materially different signification. Phlogosis is ancient Greek φλόγωσις, signifying *inflammation; burning; blazing; setting on fire*; etc. from φλόγω, *to inflame; to set on fire*. In its modern acceptance, Phlogosis is the name of a genus in nosology, which comprises several as perfect and distinct species, as exist in nosology. It does not by any means imply an entonic, sthenic or phlogistic state, condition or diathesis, since one of its species only is some times entonic sthenic or phlogistic, and some times atonic or asthenic, while all the rest are either positively and decidedly atonic or asthenic, or at all events neither entonic nor atonic. It has been but too common a mistake to suppose that every Phlogosis must necessarily be entonic, sthenic or phlogistic. Such however is very far from being the fact, and yet, in consultation, I have very often met with physicians, who were greatly shocked at the opinion that every Phlogosis did not necessarily require antiphlogistication. Phlogotica is the Greek φλογότιζα from, φλόγω *to inflame; to set on fire*; etc. Phlogotica, I believe, is not ancient Greek, but it is regularly and legitimately formed from φλόγω. It no more implies an entonic, sthenic or phlogistic state, condition or diathesis, than Phlogosis. This term is appropriate to all diseases, of which a Phlogosis or Inflammation constitutes an essential part. The term Phlogotic in the singular number, and as a mere attribute, is in constant use to denote consisting in Phlogosis or Inflammation, as thus, we say a Phlogotic disease i. e. one which consists essentially of a Phlogosis or Inflammation. It is to be remarked that the terms Phlogistic and Antiphlogotic are derived immediately from φλογίζω, while Phlogosis, Phlogistic, and Phlogotica are from φλόγω, the whole derived from φλέγω.

Phlegm, is ancient Greek φλέγμα, *flame; burning; inflammation*; in Hippocrates, *mucus; phlegm; pituita*; derived from φλέγω, *to burn; to blaze; to diffuse light*; etc. In

modern times it is employed exclusively in its Hippocratic sense, notwithstanding its etymology. Phlegmatic, that is φλεγματικός, *pertaining to phlegm or mucus; abounding in phlegm or mucus; suffering from phlegm or mucus; etc.* from the ancient Greek φλέγμα, *flame, burning; inflammation*; in Hippocrates, *mucus, or phlegm, or pituita*. This is an ancient Greek term now used in application to one of the four fundamental temperaments. The phlegmatic temperament stands in relation to the sanguine temperament, just as the melancholic temperament stands to the choleric temperament. Again the phlegmatic temperament stands in the same relation to the melancholic temperament, that the sanguine temperament stands to the choleric temperament. In short it is the light complexioned insusceptible temperament, just as the melancholic temperament is the dark complexioned insusceptible temperament. As will be perceived, this term is very far from implying any thing like entonic, sthenic or phlogistic diathesis. The vernacular or popular English use of this term has a loose and vague reference to the phlegmatic temperament. Phlegmasia, is ancient Greek φλεγμασία, meaning *inflammation*, etc. The term Phlegmasia in the singular number, is used in the same sense as Phlogosis; but Phlegmasiæ in the plural number, is used in the same sense as Phlogotica. It no more implies entonic, sthenic or phlogistic diathesis, than the terms Phlogosis and Phlogotica. Phlegmon, in ancient Greek φλεγμονή, *inflammation; burning; a Phlegmon*; etc. from φλέγω, *flame; burning; inflammation*; in Hippocrates, *mucus; phlegm; pituita*; etc. In modern medicine Phlegmone or Phlegmon is the proper name of a genus in nosology. It is a Phlogosis, or in an other word, a Phlogoticum, and it is taken as the type of one of the species of Phlogosis, and hence that species of Phlogosis, which consists of the same identical Inflammation, is called Phlogosis Phlegmonea. Phlegmonous is a mere English attribute, immediately derived from the term Phlegmon and it implies relation to Phlegmon. I suppose that φλέγω, *to blaze; to diffuse light; to burn*; etc. is the theme of all these terms, and a large number besides, notwithstanding their diversity of signification. The received import of a term ought not to be in opposition to its etymological import; but the etymological import can never be expected to constitute the definition of a technical term in any science or art.

It is to be particularly observed that though a phlogistic, entonic or sthenic condition is often called Inflammation, yet that Inflammation is quite a different condition. Phlogosis or Inflammation denotes a genus of diseases, of which there are several distinct species; while a phlogistic, entonic or sthenic state or diathesis (as the common language is) denotes a mere pathological condition, which never occurs by itself, but always makes a part of some distinct and specific disease. Now of all the species of Phlogosis or Inflammation, only one is ever truly phlogistic, and even this species is very often atonic or asthenic. All the rest are always atonic or asthenic in some grade or degree. Hence the great practical mischief of confounding a phlogistic, entonic or sthenic condition with the genus of diseases Phlogosis or Inflammation. We should therefore never call entonic, sthenic or phlogistic diathesis, which is a mere pathological condition, by the denomination of Inflammation, since this would wholly confound widely and totally different things.

Definition. Antiphlogistics are articles, which, in a peculiar manner, and in a definite degree, directly diminish vital energy and strength of action in the sanguiferous or circulating system, and (perhaps indirectly) lessen vigor generally, thereby diminishing and obviating entonic, sthenic or phlogistic diathesis when it exists, and (except in the case of Depletion of Blood, and Catharsis by the Antiphlogistic salts) independent of any evacuation, at least as an essential part of their operation. Perhaps the following would constitute as correct a definition as the preceding, viz. Antiphlogistics are articles that produce a direct reduction or diminution of vital energy and strength of action, and this in a sufficient degree, and of such a quality, as to mitigate, relieve or overcome a phlogistic sthenic or entonic diathesis. These definitions seem to require a fuller and more complete definition of entony, or what is more commonly called entonic, sthenic or phlogistic diathesis. The entonic, sthenic or phlogistic diathesis is indicated essentially by a morbid preternatural increase in the strength of the action of the heart and arteries, a state that is manifested and distinguished with absolute certainty only by a regular, strong, tense, some times full, and some times small pulse; and its degree can only be measured accurately by the degree of these symptoms. Fordyce says that a hard pulse alone indi-

cates entonic, sthenic or phlogistic diathesis; but it is to be observed that when he mentions hardness merely, it may be considered as quite certain that he always means preternatural strength, a conclusion which may be inferred indubitably from his context; but when he mentions strength and hardness in conjunction, the latter is always used in the sense of tenseness. All writers, that I have examined, appear to use the term hard, in relation to the pulse, just as Fordyce does. This term therefore should be avoided in relation to the pulse, on account of this double sense in which it is so generally employed. Though hardness of pulse, in the sense of preternatural strength, may be diagnostic of entonic, sthenic or phlogistic diathesis, yet hardness of pulse, in the sense of tenseness, certainly is not so. Tenseness of pulse is indicative only of irritation. Tenseness in conjunction with preternatural strength undoubtedly heightens the indication of entonic, sthenic or phlogistic diathesis, while tenseness in conjunction with preternatural weakness of pulse, which is much more common at the present time, heightens the indication of atonic or asthenic diathesis. A loose inaccurate and variable use of terms is always productive of mistake or error, and is often productive of much mischief. But there are other symptoms always accompanying preternatural strength of pulse, which are almost equally diagnostic of entonic, sthenic or phlogistic diathesis. These are general tumefaction and florid or scarlet redness of the surface and a sensation of unusual distension, or in other words, congestion of the capillaries of the skin; firmness of the coagulum and concavity of the surface of blood abstracted, together with a coriaceous or buffy coat; a close white fur upon the tongue extending to its very edges, these edges being florid or scarlet red; and very considerable pure augmentation of temperature, without any of the acrid stinging sensation erroneously called *calor mordax*; the whole of these accompanied with great dryness, always attend the entonic, sthenic or phlogistic diathesis. In order to appreciate, in fact to understand entonic, sthenic or phlogistic diathesis, it must be compared, not with health, but with its opposite, the atonic or asthenic diathesis; and in like manner, in order to appreciate, in fact to understand atonic or asthenic diathesis, it must be compared, not with health, but with its opposite, the entonic, sthenic or phlogistic diathesis. I have always found that those

medical gentlemen who have commenced the practice of their profession, since the disappearance of the entonic, sthenic or phlogistic diathesis, have very inadequate, in fact very incorrect notions of its real and true character.

A peculiar quality of exhausting power is in fact essential to a true and proper Antiphlogistic, as much as any other circumstance. There are certainly very many articles, that are efficiently exhausting, not only in positively atonic conditions of the system, but also in non-atonic, and at the same time non-phlogistic conditions, which I do not consider to be at all well adapted to the treatment of phlogistic, entonic or sthenic diseases. For example, *Colchicum autumnale* is a very active and efficient exhausting agent, and yet, it does not appear to be adapted to the treatment of phlogistic, entonic or sthenic diseases. I very greatly doubt whether it is capable of producing any beneficial effect in such diseases, though it is capable of producing a large amount of exhaustion in a condition which is neither phlogistic nor atonic, and much more in a positively atonic disease. I have too often seen a fatal exhaustion produced by it, in such cases, and this previous to, and independent of any evacuation. It is true I have never witnessed the use of *Colchicum autumnale* in a phlogistic, entonic or sthenic disease; but from my knowledge of its operation in a healthy degree of the vital energies and of the strength of action of the sanguiferous system, I think I have very strong reasons for doubting whether it is an exhausting agent, that is at all well adapted to remove phlogistic, entonic or sthenic diathesis. I very well know that there are numerous other articles belonging to the same group of medicinal agents, that will produce similar effects, though perhaps not in as powerful a degree. I have often seen a very troublesome and inconvenient degree of exhaustion produced by a free and continuous use of *Urginea Squilla* in non-atonic and non-phlogistic diseases, and much more especially in positively atonic diseases; and yet, I am not apprised that any practitioner of medicine ever supposed that it is by any means adapted to the purpose of obviating true and proper phlogistic, entonic or sthenic diathesis. The same may be said of *Polygala Senega* and *Polygala grandiflora* that has just been said of *Urginea Squilla*.

For a long time I entertained the opinion that the only difference between exhausting agents that are not Antiphlogistic, and

true and proper Antiphlogistics, consisted in the degree of their exhausting power, and the nature and character of their accompanying powers; but I have finally been compelled to think that the quality of their exhausting power must be taken into the account. The differences between articles that are exhausting, but not Antiphlogistic, and those which are truly Antiphlogistic are very important in practice. They differ 1. In the quality of their exhausting power; 2. In the degree of their exhausting power; and 3. In the nature and character of their accompanying powers. Doubtless very many more medicinal agents are really and truly exhausting, than is commonly supposed by the medical profession. I have myself ascertained that a very considerable number of articles generally reckoned as Tonics, or generally believed to be Stimulants (as the term is) are in fact truly exhausting. The Tritochlorid of Formicigen (commonly called Chloroform) is a direct and efficient exhausting agent, a power intirely independent of that, which produces its anæsthesia, and yet I do not believe it would be well adapted to abate and obviate phlogistic, entonic or sthenic diathesis. I never saw it tried however. The Protoxyd of Etherogen (commonly called Sulphuric Ether) is a direct exhausting agent, though less efficiently so, than Tritochlorid of Formicigen. Its exhausting power is distinct from, and independent of the power by which it produces anæsthesia. I do not believe that this article would be well adapted to abate and obviate phlogistic, entonic or sthenic diathesis, but I never witnessed a trial of it. I have actually witnessed the employment of Hyponitrite of Oxyd of Etherogen (commonly called sweet Spirit of Niter) in moderately phlogistic, entonic or sthenic diseases; and it appeared to answer very well. But this article though possessing all the powers possessed by Tritochlorid of Formicigen, and Protoxyd of Etherogen, yet possesses them in very different comparative degrees. Though the Hyponitrite of Oxyd of Etherogen is less exhausting than the other two agents which I have mentioned, yet it has a much less comparative degree of all its other powers, so that it will not lead to correct conclusions to reason from this agent, to the other two.

Beside the articles here specified, I might perhaps mention fifty, or seventy five, or possibly a hundred more, which are directly exhausting in a natural state of the vital energies, and of

the strength of action of the circulating system, and much more in an atonic or asthenic disease, without being Antiphlogistic i. e. without being capable of diminishing or obviating phlogistic, entonic or sthenic diathesis. In view of the general fact that there are so many exhausting agents which are not antiphlogistic, I once contemplated establishing a medicinal class under the denomination of *Analcica*, which was to comprise two great groups of articles the first including the Antiphlogistica true and proper, and the second including all those articles which are directly exhausting, in a natural state of the vital energies and of the strength of the action of the sanguiferous system, as well as in atonic or asthenic diseases. *Analcica* is from α privative, and ἀλγή, robur ἀνάληξις, ἰδος, ὁ, ἡ, impotens, ἄλκιμος, ου, ὁ, ἡ, robustus, fortis, ἀνάλκεια, or ἀνάλκεια, debilitas, impotentia. There are no other terms than the foregoing formed from α privative and ἀλγή, robur. *Analcima*, from α privative and ἄλκιμος, might be a sufficiently correct term for a name of such a class; and yet, it would not correspond in form with the names of all the other classes, and therefore I should not like it. I abandoned this plan however, on the ground that that degree and quality of exhausting power, which is incapable of palliating, diminishing or obviating phlogistic, entonic or sthenic diathesis, can be of no service in medicine, but must rather be reckoned as a morbid or noxious effect, and therefore requires no classification in the materia medica, except as a morbid or noxious effect. However, the operation in question ought undoubtedly to be well understood and will frequently be referred-to hereafter.

Although a direct reduction of vital energy and of strength of action is the essence of the operation of the Antiphlogistics, yet it must not be forgotten that there are articles capable of producing this effect in a certain degree, which can not be made to operate at all as Antiphlogistics. There are articles, which will produce a direct reduction of vital energy and strength of action in a very atonic disease, which do not possess this power in a sufficient degree, or of the right quality, to produce this effect even in a moderately atonic disease, and which, of course, are incapable of producing it in any other case. There are articles which will produce a direct reduction of vital energy and strength of action in any positively atonic disease, but which do not possess a sufficient degree of this power, at least of the right quality, to

operate in this manner in any other case. There are articles which will produce a direct reduction of vital energy and strength of action in any positively atonic disease, in any disease which is neither positively atonic nor entonic, and in health, but which are nevertheless incapable of producing this effect in a positively entonic or phlogistic case. In order that an article should be truly Antiphlogistic, it must possess the power of directly reducing vital energy and strength of action to a certain extent or degree, and of a certain quality. I am not certain that there is not something just as specific in the quality of the action and condition constituting phlogistic diathesis (independent of the mere degree of vital energy and strength of action) as in the quality of Strumous diathesis, or Phthisical diathesis, or Arthritic diathesis, etc. but of this I am not quite certain.

That degree of direct exhausting power, not sufficient to prove Antiphlogistic, but only to reduce in health; that degree not sufficient to prove Antiphlogistic, nor to reduce in health, but only in atonic disease; and that degree not sufficient to prove Antiphlogistic, nor to reduce in health, nor even in moderately atonic disease, but only in very atonic disease, when it is produced by an agent of chymical origin, is in all probability exactly the same power in quality, as a true and proper Antiphlogistic power. But as there are no known vegetable Antiphlogistics (except certain vegetable acids, and the plants which contain them in sufficient abundance) it is much the most likely that the reducing power of *Polygala Senega*, *Urginea Squilla*, *Colchicum autumnale*, etc. is a power different and distinct from a true and proper Antiphlogistic power. On the whole, I feel warranted to believe that the exhausting power of a true and proper Antiphlogistic is something peculiar and specific in comparison with other exhausting powers. I feel warranted to believe that all exhausting powers, though they should happen to be about equal in degree, are not equally adapted to abate and obviate true entonic, sthenic or phlogistic diathesis. I feel warranted to believe that different agents may be found, which are equally exhausting in health, and yet not equally exhausting in an entonic, sthenic or phlogistic disease. Whether two agents differing in this respect would be equally exhausting in an atonic or asthenic disease I know not. It is quite certain that a given degree of nausea produced and

kept-up by Tartrate of Antimonia and Potassa, is much more exhausting than the same degree of nausea produced and kept-up by Cephaëlis Ipecacuanha. This however may perhaps be wholly accounted-for, by the fact that Tartrate of Antimonia and Potassa is efficiently exhausting in doses which fall-short of producing nausea, while in such doses Cephaëlis Ipecacuanha is not commonly admitted to be exhausting at all, and at all events, if it is so, its exhausting power must be much less in degree, and as I think, of a specifically different quality. I do not believe that Cephaëlis Ipecacuanha possesses any power whatever, in a sufficient degree to be available in medicine for such power, except a Nauseating, an Emetic, and a Sub-cathartic power; and I do not believe that any degree and continuance of nausea producible by Cephaëlis Ipecacuanha, would be capable of abating and obviating even a moderate degree of true entonic, sthenic or phlogistic diathesis. I feel quite confident however that Cephaëlis Ipecacuanha would not aggravate or increase a true entonic, sthenic or phlogistic diathesis in any degree whatever, and I am equally confident that it would exhaust to a greater or less extent, in an atonic or asthenic disease, and I believe also in health. But what would be the fact from nausea produced by Protosulphate of Zinc and Protosulphate of Copper? It may perhaps be said that these articles possess a Tonic power, by which a true entonic, sthenic or phlogistic diathesis would be aggravated, more than it could be relieved by any nausea, which these articles might produce. I am however well satisfied that neither of these articles does possess any degree of Tonic power, the evidence of which I shall endeavor to exhibit, in an other and more appropriate place. Both these articles however possess what I call a Neuragic power, which is not capable of aggravating a true entonic, sthenic or phlogistic diathesis; both possess a moderate degree of what I call Oresthetic power, which might aggravate a true entonic, sthenic or phlogistic diathesis, but which would not be, materially exerted by nauseating doses; and both possess Emetic and Sub-cathartic powers, which as Emetic and Sub-cathartic powers merely and simply, could not aggravate true and proper entonic, sthenic or phlogistic diathesis; and yet, not withstanding all this, it is my belief, from such observations as I have had opportunity to make, that both of these articles would not

only fail of at all relieving true entonic, sthenic or phlogistic diathesis, but would doubtless aggravate it. And yet, both in nauseating doses would unquestionably exhaust, in a true atonic or asthenic disease, and would also exhaust in health. However, I shall speak of the effects of nausea protracta ab emeticis antiphlogisticis in an other place.

As Antiphlogistication consists essentially in a direct exhaustion of vital energy and strength of action, primarily in the circulating system, and secondarily in other parts of the system, it is obvious that there may be every possible gradation of this power, from the most intense degree of it known, to the very smallest degree that is capable of being distinctively appreciated; and it is equally obvious that the very smallest degree of it, that is capable of being distinctively appreciated, might be of no importance to the treatment of an acute entonic, sthenic or phlogistic disease, though it might be capable of doing injury in a very atonic or asthenic disease, and even in a non-atonic and at the same time non-phlogistic one. Hence the reason why many articles must be mentioned as having a greater or less degree of exhausting power associated with some other power, although they are not exhausting enough to be employed for the obviation of entonic, sthenic or phlogistic diathesis, and consequently not enough so to deserve specification in the medicinal class of Antiphlogistics. However if they possess a sufficient degree of exhausting power to be capable of doing mischief by means of it, I think it as important to be mentioned somewhere, as if they possessed enough of it to render them capable of doing good by means of it. This will explain why I shall hereafter mention numerous articles, as possessing a certain degree of exhausting power, though not sufficient in degree, or appropriateness of quality, to render them worthy of being marshalled regularly in the class of Antiphlogistics.

In pure phlogistic, sthenic or entonic diseases, the Antiphlogistics directly and powerfully abate the morbid heat and dryness of the skin, the morbid irritability and irritation, the morbid sensibility and sensation, and morbid restlessness and jactitation of this diathesis. In pure atonic or asthenic, typhoid and typhous diseases, unless given in inordinate and extreme doses and quantities in the twenty-four hours, the Antiphlogistics commonly either increase the morbid heat and dryness when they are already

preternatural, or the morbid coldness and sweating, when these symptoms previously exist. If efficiently given in a state of health, they always occasion a greater or less reduction both of strength and temperature. The Antiphlogistics are highly efficient for the relief of that general superficial capillary congestion of blood, which always makes a part of a phlogistic diathesis. This effect they produce, by weakening the action of the heart, and thereby restoring the due balance between its action and the pressure of the atmosphere upon the surface. In this way they produce a determination of the blood from the surface to the viscera. But they do not by any means relieve all sorts of congestion of blood. On the contrary they inevitably augment all congestions, that are connected with, and dependent upon an entonic diathesis, whether they are visceral or superficial.

The ultimate and morbid effects of the Antiphlogistics are atony or exhaustion in the parts dependent upon the nerve of chymical action, nutrition and reproduction; Leucophlegmasia; and Porphyræ. Their operation is in reality primarily exerted upon the nerve of chymical action, nutrition and reproduction; but the primary manifestations of their operation are 1. In the sanguiferous system, as from Depletion of Blood; 2. In the alimentary canal as from Nitrate of Potassa; 3. In the secernent and absorbent or glandular system; 4. In the reproductive system; 5. And secondarily, and some what remotely, in every other subordinate part of the animal economy. The essence of their operation consists in a diminution of power and strength of action. They destroy life by suspending or interrupting the function of the nerve of chymical action, nutrition and reproduction.

It must be observed that Antiphlogistic and Antisbestic and Tonic powers are the very opposite of each other; that they are never found associated in the same agent; and that they are never indicated at one and the same time in any disease. This statement I make here as the result of observation and experience; but it may be shown to be true by reasoning i. e. as a logical deduction from established premises. I have often heard it alleged that particular agents are Antisbestic in moderate doses and quantities, and Antiphlogistic in large ones. I have likewise heard it alleged that certain other agents are Antiphlogistic in moderate doses and quantities, and Antisbestic in large ones. The term

Antisbestic was not used in either of these statements, but the term stimulant; yet on being called upon for a definition, those who employed the term, claimed that they attached, in a loose way, very much the same ideas to it, which I attach to the term Antisbestic. An experimentum crucis upon both points, was however obtained by a specification of the articles supposed to operate in these modes. Hyponitrite of Protoxyd of Etherogen, commonly known by the name of Sweet Spirit of Niter, was mentioned as an article that is stimulant in certain doses and quautities, and in certain doses and quantities, and in certain cases, while it is Antiphlogistic, in certain other cases. In short I believe it is no uncommon thing to find this article recommended and employed as a stimulant in some cases, and as an Antiphlogistic in others. I doubt not I have seen this agent employed much more than a hundred times as a stimulant; and yet in the sense of Antisbestic, I do not think that it possesses a particle of stimulant power. I never met with any physician, who could ever give any better reason for considering this agent as stimulant, than the facts that it is pungent in the mouth and the stomach, which must of course indicate stimulant power; that it is an *Æther* and all the *Æthers* are of course stimulants; and that it has always been reckoned a stimulant by all medical authors and practitioners. Now I consider pungency as no sort of evidence of a stimulant power meaning thereby an Antisbestic power. Capsicum is very pungent, and yet I must deny it a particle of stimulant power, in the sense of Antisbestic power. Ammid of Hydrogen, or in an other word Ammonia, is also very pungent, and yet I must likewise deny it a particle of stimulant power in the sense of Antisbestic power. It is no more a fact that the *Æthers* are stimulants in the sense of Antisbestics, than it is a fact that all acids or pungent articles are such. Out of the great catalogue of *Æthers*, only a single solitary individual, and that not commonly reckoned an *Æther* (I mean Unihydrite of Protoxyd of Etherogen, or Alcohol) possesses any degree of Antisbestic power, so far as is known. As to the evidence of the Stimulant power of the Hyponitrite of Oxyd of Etherogen, which is afforded by tradition, I have no sort of respect for it. I perfectly agree with Mr. Nicholas Culpepper that "tradition is a monster," and I always endeavor to "avoid being led by the nose by it,"

It may truly be said to be "the blind's being led by the blind." As a matter of observation and experience, I must deny to Hyponitrite of Oxyd of Etherogen or Sweet Spirit of Niter in any degree, however small, of Antisbestic power. But is the Hyponitrite of Oxyd of Etherogen, or Sweet Spirit of Niter in any degree Antiphlogistic? I commenced the practice of medicine before the intire disappearance of entonic, sthenic or phlogistic diseases, and I often saw this agent administered by old practitioners, in such diseases, either after depletion of blood, or catharsis by the Antiphlogistic salts, and administered under the notion that it is truly Antiphlogistic, for at that time this was a generally received opinion with the physicians with whom I associated. Now I do not think I can say that it certainly operated as an Antiphlogistic, because I can not say exactly how much was due to preceding measures, or how much exacerbation there would have been, if this article had not been taken, but I am confident that it did not aggravate the disease, which I think it would certainly have done, had it possessed any Antisbestic power, and which even the use of Opium would have done, as I know from actual trial in similar cases. At all events, I know from multiplied observations, that this article is exhausting in an atonic disease; and I think that it is so in health. I once knew a person who, from a relish for its taste, was in the habit of using it diluted with water and a little sweetened, as a common beverage, in the summer. To all appearance, it impaired appetite and digestive power, produced a general paleness, and occasioned more or less weakness of the whole system, so that it was deemed necessary to consult a physician. The physician consulted, who was a man of considerable reputation and eminence, pronounced all the symptoms of the case to have been caused by the Hyponitrite of Oxyd of Etherogen or Sweet Spirit of Niter, and I doubt not correctly. This case came under my observation before I had studied medicine at all; but the facts were so simple and plain, that there could be no mistake in regard to them. But from the use, that I have seen made of this article, by elderly physicians, in truly phlogistic diseases, I can not doubt that it is more or less Antiphlogistic, so much so as to be capable always of relieving moderate phlogistic diathesis. Most of my observations with regard to this agent were made before it was superseded in the shops by a mere mixture of Alcohol and Nitric Acid. How the

substance, which is now commonly sold among us for Hyponitrite of Oxyd of Etherogen or Sweet Spirit of Niter, would operate, I can not pretend to say, for I have never prescribed enough of it, to afford me any opportunity of learning. I am inclined to think that the Alcohol is too much changed by the Nitric Acid to operate as Alcohol; but what it is changed into, I am unable to say. It appears to me to be some thing different from what it ought to be, for it is certainly a good deal unlike genuine Hyponitrite of Oxyd of Etherogen or Sweet Spirit of Niter.

As a matter of observation, I consider that Oxyd of Etherogen or common Æther, which is commonly called Stimulant, is actually more exhausting than Hyponitrite of Oxyd of Etherogen or Sweet Spirit of Niter; but it is at the same time much more Oresthetic, and much more Euphrenic, so that, as I think, it can not possibly be made to operate as an Antiphlogistic, because its Oresthetic and Euphrenic effects would be likely to do more injury than its exhausting effects would do good in an entonic, sthenic or phlogistic disease. I believe that this agent has never been deemed Antiphlogistic, even by those who employed Hyponitrite of Oxyd of Etherogen or Sweet Spirit of Niter as such. I have often heard Oxyd of Etherogen mentioned as one of the most powerful Stimulants in the whole materia medica. When I was a young practitioner, I recollect asking an elderly one, whom I heard make this remark, why it was not employed instead of Alcohol, in diseases of extreme exhaustion, i. e. in uniform doses, at regular and short intervals, and continued for a week or fortnight, as Alcohol is sometimes used. The reply was that it is so powerfully Stimulant, that it produces indirect debility very speedily, when so administered. This explanation was just about as satisfactory, and just about as reasonable, as explanations in chimistry founded on the doctrine of phlogiston. However it recognized the fact that, in sufficient quantities, this agent is certainly exhausting, which ought to have led to the inquiry whether it is ever truly invigorating; and common sense ought to have immediately answered the last in the negative.

I have often also heard the Water of Ammid of Hydrogen (Ammonia) mentioued as one of the most powerful Stimulants in the materia medica, while at the same time, it was admitted that if used in uniform doses, at regular and short intervals, and con-

tinued for any considerable time (the very way to get the greatest effect from a true Antisbestic) it invariably produces a very considerable degree of exhaustion, which is alleged to be indirect debility, the result of excess of Stimulation. The real truth is that this agent is never in any degree Stimulant, in the sense of Antisbestic; but it is Oresthetic and in addition to this very decidedly exhausting. Now its Oresthetic power never opposes, counteracts, or diminishes its exhausting power, though it might irritate in a phlogistic, sthenic or entonic disease, and prevent any benefit in such cases, from its exhausting effects. Such is probably the fact in all the cases of supposed indirect debility from excess of Stimulation.

As I have already had occasion to state, I believe that the Tritochlorid of Formicigen (Chloroform) is universally called Stimulant; while there are few articles in the materia medica that are so potently exhausting, and that in such a direct and immediate manner. I have never heard the exhausting effects of this agent called indirect debility; but I know of no reason why they should not be so called, as well as the exhausting effects of Oxyd of Etherogen (common Æther), of Hyponitrite of Oxyd of Etherogen (Sweet Spirit of Niter) or of Water of Ammid of Hydrogen (Ammonia) etc. None of these agents possess a particle of Antisbestic power, or the least degree of invigorant power of any sort or kind. Throughout my whole professional life, I have constantly been accustomed to hear the three Carbonates of Oxyd of Ammonium called Stimulants, and to see them employed as such, while it is certainly recognized in books that if they are taken for any considerable time, they produce a prominent and intense exhaustion. I have always heard this explained upon the absurd hypothesis of indirect debility from excess of Stimulation; but I never could find any body who had ever witnessed this supposed excess of Stimulation, either from these salts, or any of the articles, which I have before mentioned; and yet I never heard it questioned that they are all Stimulants. I can not but agree with Culpepper heretofore quoted, that "many authors have invented whimsies, and when they have done, set them down to posterity for truth."

I have often heard the Protosulphate of Zinc pronounced to be Stimulant and Tonic in small quantities, and exhausting in large

quantities. According however to the best observations that I am able to make, this salt is wholly destitute of any degree either of Antisbestic or Tonic power; but is Oresthetic, Styptic, Nauseating, Emetic, Sub-Cathartic, Neuragic and more or less exhausting when given alone with any efficiency, and for any length of time. The exhausting effects of a small quantity may however be frequently counteracted and transcended by the conjunction with it of pure vegetable Oresthetics, Antisbestics and Tonics.

I have often heard Nitrate of Potassa alleged to be Antiphlogistic in small quantities, and Stimulant in large ones. There can be neither doubt nor question that this salt in certain doses, and quantities in the twenty-four hours, is directly and immediately exhausting, and that of such a degree and quality, as to be truly and actively Antiphlogistic. This therefore can need no discussion in this place. But if by accident or mistake, an ounce (Troy weight) more or less, should be taken and retained in the alimentary canal, it will most commonly produce irritation and Erythematic Phlogosis of the lining mucous membrane. This is considered as Stimulation, by very many physicians, on the ground that whatever irritates must be a Stimulant; that whatever produces disease must be a Stimulant; and above all, whatever produces a Phlogosis must be intensely Stimulant. But other facts are to be observed in this case, viz. that at the very time, when this irritation, and this Erythematic Phlogosis begins, as well as after it has taken place, there is a positive, decided and often a great diminution of vital energy and strength of action in the sanguiferous system, and as would seem also in all the subordinate parts dependent upon the nerve of chymical action, nutrition and reproduction, which soon extends to all other parts, attended with extinction of appetite and digestive power, great diminution of the natural temperature, great pallor of skin, etc. This is assuredly the very antipodes of Antisbestic. The truth is that neither irritation nor Phlogosis afford the least indication of Stimulation, and both not only may be, but often are attended with extreme exhaustion, so that their production by Nitrate of Potassa does not afford the slightest reason for considering it a Stimulant, certainly in the sense of Antisbestic.

I have even heard it maintained that Tartrate of Antimonia and Potassa is Tonic in doses and quantities that fall short of Nau

seating in any degree; that in large doses and quantities, it is Antiphlogistic; and that in larger doses still, and especially in very large doses and quantities, it is Stimulant. I never could ascertain that there is any ground for considering doses and quantities of this agent, that fall short of Nauseating in any degree, as Tonic, except the assumed, and as I think, the unfounded principle, that all preparations of the metals, in the doses and quantities specified, are always tonic. When I have asked for evidence of the truth of this supposed principle, I have always been told that its truth rests upon observation and experience. But it so happens that all my observations and experience contribute to prove the contrary. The sole evidence of the Stimulant operation of very large doses and quantities of this salt, so far as I have knowledge, is the fact that they often irritate and produce an Erythematic Phlogosis; but upon this point I have given my own views in connexion with Nitrate of Potassa. I never yet saw the Tartrate of Antimonia and Potassa administered long enough to produce any decided effect, without its producing more or less exhaustion, either in the organs of primary digestion, or some other subordinate part or parts of the system. If there is any exception to this, it is in the use of this salt as an Emetic; but this is by no means always an exception.

Digitalis purpurea is claimed by some, to be not only exhausting, but Antiphlogistic; while it is alleged by others, to be Stimulant. Others still (and so far as I can judge, the greatest number) consider it as Stimulant in small quantities, and exhausting in large quantities. Now I have bestowed much time upon the study of this article, and I could not possibly avoid the conclusion that it is utterly destitute of any exhausting, and much more of any Antiphlogistic power, when given in any medicinal doses and quantities. The conclusion was also equally unavoidable that this agent is wholly destitute of any Antisbestic power. I have seen it used in exquisitely phlogistic diseases, without any mitigation or relief of the entonic, sthenic or phlogistic diathesis, and in intensely atonic diseases, without any increase of the exhaustion, provided it produced no nausea. In short this article appears to me to be Narcotic, Adenagic and Sub-Nausiatic and nothing else.

As appears to me, it is altogether contrary to fact, to suppose that any article in the *materia medica* can ever possess opposite

powers, that are exerted at one and the same time ; altogether contrary to fact to suppose that any article can prove Antisbestic or Tonic in moderate doses and quantities ; Antiphlogistic in larger doses and quantities ; and Antisbestic again in still larger doses and quantities ; though I have often heard such views very earnestly maintained, and my own views as earnestly combated and condemned. As a matter of fact, some of the most active of the Antiphlogistics are at the same time decided Oresthetics when they are given in larger doses than is necessary for their ordinary Antiphlogistic operation. This is certainly the fact with Nitrate of Potassa, Sesquicarbonate of Potassa, Sesquicarbonate of Soda, and probably with Chlorite of Potassa (viz. $K^1 O^1 + Cl^1 O^5$) In any quantity whatever, these agents are exhausting, but in very large quantities, they exhaust in a highly morbid degree, and at the very same time, they irritate the mucous membrane of the alimentary canal, and produce Erythematic Phlogosis, which is one of the grades of what I call ultimate Oræsthesis. I doubt whether Bicarbonate of Potassa, and Bicarbonate of Soda are Oresthetic at all, since they are wholly and intirely destitute of any Alcaline taste. Their insipidity however is by no means conclusive on this point, since many active Oresthetics are intirely destitute of acrimony to the taste. I think that the two salts above mentioned have considerably less taste than any of the Antiphlogistic Cathartic salts. Again, some of the most active of the Oresthetics are at the same time decidedly exhausting, so that they can not be administered for any length of time, even for their Oresthetic effect, without producing at least an undesirable, if not a positively injurious degree of exhaustion. This is certainly the fact with Water of Ammid of Hydrogen (Ammonia) with Carbonate and Sesquicarbonate of Oxyd of Ammonium ; and I believe with Bicarbonate of Oxyd of Ammonium. I presume also that it is the fact with the Carbonates of Ammid of Hydrogen (Ammonia) but I am not sufficiently acquainted with them to be able to say what is the true fact with regard to them. Now though an Oresthetic operation would doubtless aggravate a phlogistic disease, yet it would not in the least hinder an exhausting or even an Antiphlogistic effect. Intense Oræsthesis and powerful exhaustion may certainly be produced simultaneously, each even to a dangerous degree. Such a fact shows that there

is a very great difference between Oræsthesi and Antisbēsi—between the effects of Oresthetics and Antisbestic, notwithstanding they are so generally confounded. Even those active Antiphlogistics, which possess other powers in conjunction, and perhaps even when these powers are of an evacuant character, not infrequently produce the same Oresthetic effects, when they happen to be taken in inordinate doses. I think it quite possible, perhaps probable, that the Antiphlogistic cathartic salts might also irritate and produce Erythematic Phlogosis, in sufficiently large doses, could they be retained in the alimentary canal for a sufficient length of time, without the aid of Papaver, which is such an effectual Narcotic, Antirritant and Antisbestic, and therefore so efficient for the prevention and obviation both of irritation and exhaustion.

All the Antiphlogistic vegetable-organic, or chymical-inorganic acids that are capable of being obtained in a concrete state, and many of them even in their liquid state, are actively Oresthetic, and are capable of producing topical irritation and Erythematic Phlogosis, at the same time that they exert their exhausting power even in the highest degree of which they are capable. This is notoriously true of Hypocarbonic (Oxalic) Acid, of Citric (Hypotartaric) Acid, and of Tartaric Acid; and yet I have known great pains taken by a public instructor to inculcate upon medical students that the first is poisonous, in consequence of possessing this very power, that I call Oresthetic, and therefore could not be used internally with safety; while the last was alleged not to possess this power at all, and therefore might be used internally with the most perfect safety. This was deemed a highly important fact. Nothing was said of the second in this immediate connexion. The truth is that as medicines and poisons, the three agree in all respects, except degree of power, and consequently, medicinal and poisonous dose. Even Carbonic Acid the weakest of all Antiphlogistics if used in the form of Aqua Gassis Acidi Carbonici, and in that shape destitute of all Oresthetic power, becomes, in its concrete or solid state, one of the most active Oresthetics known, unless it is a white-hot Iron.

The chymical-inorganic Acids called Sulphuric, Nitric, Chlorohydric, Phosphoric, etc. even in their liquid state, are intensely Oresthetic, and are therefore reckoned Stimulant and Tonic by

virtue of this power. I once knew a public instructor, who took great pains to teach his pupils that these Acids were Antiphlogistic, and at the same time Stimulant and Tonic. He always inquired about this point, at the examination of every student for a degree; and I do not know that he would have let a young man pass, who did not confess his faith in this doctrine. An individual candidate, notoriously a good scholar in his profession, once determined not to admit this; and he accordingly refused such admission for a considerable time; but his examination was so much protracted in consequence, that at last, he became willing to admit any thing, that it might be ended. Such are some of the ridiculous absurdities that the confounding of Antisbestic, Oresthetic, Euphrenic and Erethistic powers under the all comprehending term Stimulant, very often occasions.

The topically rubefacient effect of many of the Antiphlogistics, i. e. their power of producing Erythematic Phlogosis, of which I have just been treating, is to the best of my knowledge, universally considered and called a Stimulant effect, both by authors and practitioners of medicine. But can that be Stimulation (at least in the sense of Antisbesis) which is essentially attended with a prominent and considerable diminution of the vital energies, and of the strength of action? It is assuredly a very great mistake to suppose that every agent which is capable of producing a topical irritation, that passes into Erythematic Phlogosis, is a Stimulant in the sense which I attach to Antisbestic. Mere Antisbesis is wholly incapable of producing a topical Phlogosis of any species, or in any degree; and when a topical Phlogosis is actually produced by any article, which is truly Antisbestic, this effect is never occasioned by the Antisbestic power, but by another and a different one, viz., what I call an Oresthetic power, which will always be found associated with the Antisbestic power, when such a result happens; and the Phlogosis is always atonic or asthenic, whether it is produced by a pure Oresthetic, or by an Oresthetic having an Antisbestic power associated with it. For example, the Phlogosis which is produced by *Cantharis vesicatoria* is as purely Erythematic, and as decidedly atonic or asthenic, as the Phlogosis produced by *Capsicum annum*, though the former has an Antisbestic power, of which the latter is altogether destitute. Such facts as these, which I believe are very numer-

ous, would seem to show that, in relation to the Phlogosis, the Antisbestic power is perfectly neutral, exerting no influence in any way or manner. If this is a well ascertained fact, it throws no light upon the subject to say either that it is what we should, or what we should not expect, since "no argument like matter of fact is." It is of no avail against a fact established by a multiplicity of observations, that it contravenes or coïncides with a priori expectations; and yet these are often objected to my conclusions.

It is a very great error to suppose that Phlogosis or Inflammation supervenes only upon an augmented state of the vital energies, and that it is always accompanied with such a state, and also with augmented strength of action. On the contrary, Phlogosis or Inflammation very often indeed supervenes upon an extremely impaired state of the vital energies, and it is very often indeed accompanied with extremely weak action, or in one word exhaustion; and it is in fact much more liable to occur in such a condition of the system. The very frequent deficiency of knowledge of true facts in reference to this point, with which we so often meet, and the prevailing doctrine of the identity and unity of character of all Phlogoses or Inflammations, together with the consequent notion that there must be an equal identity and unity of treatment in all cases, has undoubtedly, been attended with very mischeivous consequences in the practice of medicine.

Irritation and Antisbesis are demonstrably different. Irritation may accompany Antisbesis without making any part of it, just as it may accompany Antiphlogistication without making any part of that effect. Whenever Irritation and Phlogosis or Inflammation are the result of excessive or inordinate quantities, either of any Antisbestic or any Antiphlogistic, it may be considered certain, that in either case such Irritation or such Phlogosis or Inflammation is not occasioned by mere Antisbestic power on the one hand, nor by mere Antiphlogistic power on the other, but by another intirely different and distinct power, which the articles employed happened to possess, viz., what I am in the habit of calling an Oresthetic power. It is an irrefutable proof that Antisbestic and Antiphlogistic powers are both perfectly distinct from that power possessed both by certain Antisbestics and certain Antiphlogistics, which produce irritation and Phlogosis or Inflammation, that there are very many articles which possess it in a

most eminent degree, without possessing at the same time a particle either of Antisbestic or Antiphlogistic power. It is indeed very often the fact that irritation and Antisbesis are confounded; but it should never be supposed that an Antiphlogistic can possibly be Antisbestic because it happens to be irritant, in the manner which I call Oresthetic. It appears to me to be almost, if not quite an absurdity to suppose that any article can possess, at one and the same time, the power of directly and immediately diminishing vital energy and strength of action, and the power of directly and immediately increasing, in a quickly diffused and transient manner, both vital energy and strength of action. As exhausting principles, and invigorating principles do exist separately, it is possible for them to be associated in one crude article; but I have no knowledge of such a case, nor did I ever hear of one.

There is one important fact, that deserves to be stated here, viz. that no inordinate dose, however large it may be, of any one of the Oresthetic Antiphlogistics, seems to be capable of irritating to such an extent as to produce an Erythematic Phlogosis, so long as any appreciable degree of entonic, sthenic or phlogistic diathesis exists; but after all this pathological condition is intirely obviated, then there is susceptibility to the production of the peculiar Phlogosis which results from ultimate Oræsthesis.

If the whole of the foregoing statement is correct (and possibly there may be doubt of it) it is quite possible, perhaps probable, that there is no absolutely pure Antiphlogistic agent known (though certain processes may be exceptions) by which I mean, that there may be no single Antiphlogistic article which is wholly incapable of exerting any other medicinal operation in any degree; and yet, for all practical purposes, I suppose that Nitrate of Potassa, Chlorite of Potassa ($K^1 O^1 + Cl^1 O^2$) Chlorate of Potassa ($K^1 O^1 + Cl^1 O^3$) and doubtless Bicarbonate of Potassa and Bicarbonate of Soda, together with all the vegetable aids of compound radicals of $H. C. + O.$ that are sour to the taste, may be reckoned as such, since they can be employed to produce any desirable degree of their Antiphlogistic operation, without endangering the production of any degree of ultimate Oræsthesis. It is remarkable, if such is really the fact, that the Antiphlogistics and the Antisbestic should be the only classes in the materia medica, of which there should be no individual articles, that possess the power on

which the class is founded, intirely alone, and unconjoined with any other power. As with the Antiphlogistics, so it is with the Antisbestics, as respects articles, which, for all practical purposes, possess only one single power.

As there is no such thing as a topical entonic, sthenic or phlogistic diathesis, so there can be no such thing as topical Antiphlogistication. No limited and comparatively small part is ever in a state of preternaturally increased strength of action, without being attended with the same condition of the system at large. The thing seems to be absolutely impossible, and consequently there can never be need of, or occasion for, a limited and truly local Antiphlogistication. Many however, who fully admit the statement that I have made, believe that there may be a true topical atonic diathesis, a preternatural diminution of vital energy and strength of action, in a limited and comparatively small part, without involving the system at large. Provided that such topical atony is produced by some mechanical violence, or some powerfully exhausting agent locally applied, we may admit this proposition in the abstract; but what then? How extensive can such topical atony be, without involving the system at large; and if it does not involve the system at large, can such local atony be of any material importance? It may give rise to a congestion of blood in the part under atony; but then, I have no sort of faith in the fashionable, and consequently prevalent notions, that congestions of blood perform such an important part in pathology, as many at the present day seem to suppose. I never could find any reasons for believing that congestions of blood either kill or cure any body; and they certainly exist in all diseases that involve the constitution, certainly in all that are in any degree either positively entonic or positively atonic. If Dr. Armstrong had not thought proper to remove to London at a certain time, and then had not found it necessary *to write* himself into business, I believe that we should have heard very little of congestion in these days. The truth is, that congestions of blood are mere effects of far more important pathological conditions, and as effects, they serve to indicate these conditions, but in themselves, they are no more important than bile and wind etc. commonly so called, are.

Is there any such thing as constitutional Antiphlogistication from the application of Antiphlogistics topically to the skin?

From the nature, character and degree of this power, as possessed by the most active articles of this class of agents, they seem to be incapable, as a general rule, of operating through such a dense membrane as the skin, in its sound and healthy state—incapable of operating when applied to such a small extent of surface, as that to which we are able to apply them, with any sort of convenience—incapable of operating on account of a deficiency of power adequate to produce such an effect, under such circumstances. These remarks are intended to be applicable to the production, not only of a constitutional, but also to that of a local Antiphlogistic effect, by mere local applications. Perhaps, indeed I know, that there is one single exception, I should think there is certainly not more than one, to the incapability of producing an Antiphlogistic effect, by topical applications; but even in the case of this exception, there are accompanying operations, which render it unavailable in therapeutics. In these remarks, I have reference to the Tartrate of Antimonia and Potassa, which, by a sufficiently continuous application to a moderate extent of surface, may undoubtedly be made to produce a direct reduction of vital energy and strength of action, not only topically, but throughout the system. This I have witnessed too often to have any doubt upon the subject. But the Antiphlogistic effect thus produced, is much too slow and gradual, to be of any service in the treatment of any single individual phlogistic disease; and besides, is always, and even necessarily, attended with such a degree of Oresthetic or Epispastic effect, as to cut us off from its employment for this purpose. I have seen the Nitrate of Potassa tried as a topical Antiphlogistic of this sort, and without the least evidence of its producing any such effect. It is a prevalent opinion, that the local application of an aqueous solution of the Protacetate of Lead operates as a topical Antiphlogistic; but this is certainly a very great error. The Protacetate of Lead certainly possesses no Antiphlogistic power in any grade or degree. Who employs this agent internally in any single phlogistic disease? If this article is intirely destitute of any Antiphlogistic power, if it is incapable of producing this effect by internal use, it certainly can not produce it by external topical application.

The production of cold locally is very commonly considered as a topical Antiphlogistic; and hence cold lotions are very fre-

quently recommended and employed for this purpose; but it is remarkable that this same process is likewise commonly considered as Tonic, and hence, the same lotions are employed to produce local invigoration. How often do we know cold lotions employed to relieve what is supposed to be a phlogistic or entonic Phlogosis; and again, how often are they recommended and sedulously employed to restore tone, or invigorate a greatly weakened joint, the result of a bad wrench, strain or sprain. But it is equally erroneous to consider cold as either Antiphlogistic or Tonic. Cold, unless pushed almost to the freezing point, does not produce any direct reduction of vital energy and strength of action, on the one hand, nor on the other, does it produce a slow, gradual and permanent increase of vital energy and strength of action. Cold is merely the abstraction of heat, and of course, is not a positive agent. Refrigeration or cooling, the only effect of cold within certain limits, may be useful either in an entonic or atonic disease (and it is actually as often useful in the one as in the other) without operating either as an Antiphlogistic or a Tonic.

From what I have now said of this class of agents, I trust the appropriateness of the name that I have selected for it, will be obvious. The etymological import and the technical signification of the term Antiphlogistica, perfectly coincide, which I think is not the fact with any other term employed, at all events, to the same extent. From this circumstance, there is very little hazard of a mistake as to its meaning, which can hardly be said of any other term in use. The whole of the following terms have been employed by different authors and practitioners, as names of this class, viz. *Analcica*; *Debilitantia*; *Infirmantia*; *Reducentia*; *Asthenica*; *Atonica*; *Temperantia*; *Sedantia*; *Sedativa*; *Refrigerantia*. As I have already stated, I once contemplated employing the term *Analcica*, which implies exhausting agents and, of course, is equivalent to *Debilitantia*, *Infirmantia*, *Reducentia*, and also to *Asthenica* and *Atonica*, when employed as the name of a class of medicinal agents. All of these terms imply too much, since, as I have already explained, there are numerous *Analcic*, *Debilitant*, *Infirmant*, *Reducent*, and in this application, *Asthenic* and *Atonic* remedies that are by no means Antiphlogistic. In short all these terms have a more comprehensive, and a more extensive signification than Antiphlogistic. If

all remedies that exhaust, debilitate, prove weakening or reductent, or that occasion atony or astheny in any degree, are not Antiphlogistics, assuredly none of these terms can be eligible as a name of this class, and if employed as such, must be more or less liable to mislead. As I never knew a case where a term is technically employed in a sense at variance with a plain and obvious etymological import, which did not give rise to much confusion and error in its application, I always consider such discrepancy or variation as a very serious objection to the adoption of such term. But these terms may be said to be needed for their etymological, more comprehensive or more extensive signification, and therefore can not be spared for the more limited and restricted use. Provided we were to attempt to confine their application to the Antiphlogistics, we should certainly fail in it. They would be employed in their etymological sense, and the result would be a perfect confusion of the two. But *Debilitantia*, *Infirmantia* and *Reducentia*, being pure Latin, are excluded by the rule that requires all the names of the classes to be from the same language, and that language Latinized Greek. At all events it would not appear at all well to have names of classes from several different languages, so that if a part of them are pure Latin, the whole should obviously be the same, which would involve a great change in the terms at present received.

The terms *Asthenica* and *Atonica* are indeed Latinized Greek; but these terms are in constant use, in a widely different acceptance, viz. in application to that pathological condition, which consists in a preternatural and considerable deficiency of vital energy, and strength of action, in the sanguiferous system, and probably in all the subordinate parts dependent upon the nerve of chymical action, nutrition and reproduction. I have heard these last two terms defended as names of this class, on the ground that they are applied ordinarily to designate a pathological condition, producible by this class of agents. I can not perceive the expediency however of applying the same name both to the cause, and to its effects.

The name *Temperantia*, is not infrequently applied to this class of agents. The Latin verb *tempero*, of which *temperantia* is a derivative, signifies *to temper*; *to mingle*; *to rule*; *to govern*; *to restrain*; *to make supple or soft*; etc. (*Littleton's Dictionary*.)

Now I can discover nothing in the signification of the term *Temperantia*, that renders it in any respect an appropriate name for this group of medicines. As appears to me, any word taken at hap-hazard from the Latin language generally, would be just about as proper. To those who can see no unfitness in this term, on the ground of its signification, it will be hardly worth while to mention the circumstance, that it is pure Latin, and consequently incongruous with the names of the other classes.

The terms *Sedantia* and *Sedativa* are sometimes employed as names of this class. It is true that *Antiphlogistics* are sedative of entonic, sthenic or phlogistic diathesis, but of nothing else. The sedative or antirritant operation of the *Antiphlogistics* is however only a relative effect, not capable of taking place in all conditions of the system, but only in phlogistic diathesis, since they allay no morbid irritability and irritation, no morbid sensibility and sensation, no morbid restlessness and jactitation, except those connected with a phlogistic diathesis. It appears to me quite certain that the sedative operation of the *Antiphlogistics* is intirely the effect of the obviation of the morbid vital energy and morbid strength of action in the sanguiferous system, and probably in all the other subordinate parts dependent upon the nerve of chymical action, nutrition and reproduction, which constitutes the very essence of phlogistic diathesis, and on which doubtless depend the morbid irritability and irritation, the morbid sensibility and sensation, the morbid restlessness and jactitation, the morbid heat dryness and pain of this pathological condition. If all this is true, it will at once be plain why the *Antiphlogistics* are so utterly powerless as sedatives or antirritants in non-phlogistic, and much more especially atonic or asthenic diseases, and also why they so greatly aggravate the aggregate of symptoms called by the same names which I have just specified, that is found in many positively atonic or asthenic diseases, since in such cases, the aggregate in question is inseparably connected with, and completely dependent upon, the existing atonic diathesis which is augmented by *Antiphlogistics*. But *Leantics* are as truly *Sedatives* as *Antiphlogistics*, and so are *Neuragics*; and *Narcotics* are far more so. The last two of these classes are therefore much better entitled to this name, and the first is about equally entitled to it. *Antiphlogistics*, *Leantics*, *Neuragics* and *Narcotics* are all direct sedatives

of certain pathological conditions; and several other classes are indirect sedatives. As appears to me therefore, this name is utterly inadmissible in application to the Antiphlogistics. If all the remedies that ever allay morbid irritability and irritation, morbid sensibility and sensation, morbid mobility restlessness and jactitation of any character, and in any case, are not to be brought together into one class, then the terms Sedantia and Sedativa can not be appropriate names for the Antiphlogistics; and when applied to them, must be ever liable to mislead. But I believe that the terms Sedantia and Sedativa are commonly used, both by authors and practitioners, with so much latitude as to comprehend not only the Antiphlogistics, the Leantics, the Neuragics and the Narcotics, but also the Euphrenics, the Oresthetics, the Antisbetics and the Tonics, whenever these last four classes produce antirritant effects indirectly.

Among terms that were considered as proper for this class of agents (strange to be told) I have heard Antaphrodisiaca mentioned—I have even heard it mentioned that an Antaphrodisiac operation is a characterizing and essential effect of this class of agents; but I do not consider this as dependent upon any peculiar and specific power. It is merely the effect of exhaustion in those subordinate parts of the system which depend upon the nerve of chymical action, nutrition and reproduction.

Most British authors (as I think unfortunately) have preferred the term Refrigerant. John Murray, whose work on the materia medica was more read and studied, for a considerable number of years in our country, than any other; and also Cullen, whose work on this subject preceded that of Murray in general favor, employed the term Refrigerant, and were constantly misled by the etymological signification of the name of their choice. Murray says that "Refrigerants are defined to be such medicines as diminish the force of the circulation and reduce the heat of the body without occasioning any diminution of sensibility or nervous energy." (*John Murr. Syst. Mat. Med. and Pharm. Beck's Edit. N. Y. 1828, Pg. 621.*) Now this definition, as a whole, does not appear to me to be applicable to any agent at present known. The first part of it can be applicable to nothing but the Antiphlogistics, and the catalogue of articles marshaled under it, meager as it is, clearly shows that the Antiphlogistics are intended; and

yet the last part of the definition is intirely inappropriate to this class. The Antiphlogistics always diminish the morbid sensibility and sensation of the phlogistic sthenic or entonic diathesis; if they did not they could not be considered as Antiphlogistics, and would be just about worthless in the treatment of phlogistic, sthenic or entonic diseases. The truth seems to be that Murray, misled by his own name Refrigerants, i. e. coolers, employs this group of agents as much in the treatment of atonic or asthenic diseases, as in the treatment of entonic, sthenic or phlogistic diseases, because the former are attended with preternatural heat as well as the latter. In the former however, they are actually incapable of diminishing the morbid sensibility and sensation which so frequently exist; and thus the definition would seem to be taken from their effects in cases to which they are altogether inappropriate. The Antiphlogistics certainly diminish nervous energy; if they did not, they would not be Antiphlogistics. All sensation depends upon nerves of sensation, and all motion or action depends upon nerves of motion or action. Now an increase of nervous energy, and a consequent increase in the strength of sensation and the strength of motion or action, in certain subordinate parts of the system, is essential to phlogistic, sthenic or entonic diathesis, and must of course disappear when that is obviated. All the Antiphlogistics with which I am acquainted, invariably "diminish" morbid "sensibility and nervous energy," such as exist in phlogistic, sthenic or entonic diathesis, in precisely the same proportion that they "diminish the force of the circulation, and reduce the heat of the body," a fact which as I have already said, renders the last clause of the definition given by Murray completely inappropriate to any article of the materia medica, of which the first clause may be predicated. Here again Murray seems to have derived his notions of the operation of the Antiphlogistics from other diseases than true phlogistic, sthenic or entonic ones.

Most physicians seem to forget that the morbid irritability and irritation, the morbid sensibility and sensation, the morbid mobility, restlessness and jactitation, the pain, the dryness and heat of the skin, and the thirst, are all of different specific characters in the two diatheses under consideration; and that these conditions in the phlogistic, sthenic or entonic diathesis are inseparably connected with the preternatural increase of vital energy and

strength of action in the circulating system, and will not be relieved, except by means that will relieve them; and that the symptoms which take the same name in the atonic or asthenic diathesis, are equally connected with the preternatural deficiency of vital energy and strength of action in the sanguiferous system, and will not be relieved, except by means that will relieve them. Some atonic diseases are attended with great morbid increase of heat. Such morbid increase of heat is almost invariably aggravated by the Antiphlogistics. In very excessive and inordinate doses and quantities, such as it can never be proper to employ, the active and efficient Antiphlogistics may diminish the heat of the system in all cases; yet I repeat, they can not be made to accomplish this in intensely atonic cases, in any proper and safe doses and quantities, and under any proper and safe management. Some atonic diseases are attended with great morbid diminution of heat. Such morbid diminution of heat is almost invariably aggravated by the Antiphlogistics. Some atonic diseases are attended with great morbid dryness of the skin. This is almost invariably aggravated by the Antiphlogistics. Some atonic diseases are attended with great morbid sweating. This also is almost invariably aggravated by the Antiphlogistics. But it may be asked, are not these contrary or opposite effects? I answer by no means. The essence of the atonic or asthenic diathesis is a certain amount, and perhaps a certain quality of exhaustion in the sanguiferous system, and probably in all the subordinate parts dependent upon the involuntary motor nerve of chymical action, nutrition and reproduction. All the accompanying symptoms and conditions by whatever name they may be called, are of a quality congruous with the exhaustion, and therefore, whatever aggravates the exhaustion will necessarily aggravate all the accompanying symptoms and conditions; and vice versa, whatever diminishes the exhaustion will necessarily mitigate all the accompanying symptoms and conditions.

Cullen says that Refrigerants—by which he evidently intends Antiphlogistics, since every one of his Refrigerants is an unequivocal and decided Antiphlogistic—are “medicines supposed, as their name implies, to diminish the heat of the living body.” (*Cull. Mat. Med. B. S. Bart. Edit. Phil. 1812, Vol. II. Pg. 227. Chap. VII.*) Here cooling would seem to be considered the

most essential part of an Antiphlogistic operation; a very great mistake according to my views. Cullen says "as the heat of the body, whether from internal or external causes, is never increased beyond its ordinary degree, but with an increased action of the sanguiferous system, so the Refrigerants, as they diminish this increased action, are justly put under the general title of Sedantia; but being substances of quality and operation very different from the Sedantia we have already considered, they are here to be treated-of separately." (*Ibidem.*) But what does Cullen intend by "an increased action of the sanguiferous system"? In true and exquisite phlogistic, sthenic or entonic diathesis, there is certainly preternaturally increased vital energy and increased strength of action in the sanguiferous system, and in conjunction with greatly increased heat. Every intelligent physician will of course admit this to be "an increased action of the sanguiferous system." That by "an increased action of the sanguiferous system" Cullen intended true phlogistic, sthenic or entonic diathesis, I do not think there can be any reasonable doubt. But is there no such thing as preternatural heat, in fact greatly augmented thermometrical temperature, in an exquisitely atonic or asthenic disease? In one of the varieties of intense and very malignant Typhus putridus, and in one set of the cases of all those Phlogotica, that are attended with Typhus putridus as the constitutional febrile affection, and in which the topical Phlogosis is an Erythema Gangrænosum, there is very greatly increased temperature, sometimes the greatest amount of heat that I ever witnessed in any disease, in conjunction with extreme reduction of the vital energies, and of the strength of the action of the sanguiferous system, as indicated by very frequent, quick, small, weak and unequal pulse, great disturbance of the functions of the hemispheres of the cerebrum, and of the nervous system generally, and all the other indications of exhaustion. One of the varieties of Isthmitis Typhodes-Erythematica (by which I intend an Erythema Gangrænosum of the fauces, with an exquisitely hot Typhus putridus as the constitutional febrile affection) may be taken as an example of the sort of cases to which I allude. By the disease here particularized, I do not intend any variety of Rosalia or Scarlet-Fever (which, by the bye, is never scarlet but always crimson) but a perfectly distinct specific disease. Now is there here "an increased action of the sanguiferous system?"

There is certainly great augmentation of temperature, as indicated by the thermometer; and though there is much of the acrid stinging sensation which is incorrectly called "calor mordax," but which may exist where there is no preternatural heat; yet there is besides, a great deal of unequivocal heat. In such a case will any of the agents, called Refrigerants by Cullen and Murray, at all abate the preternatural heat, as they will actually abate the heat of phlogistic, sthenic or entonic diathesis? If not, there is some fault in the views and definitions of these gentlemen. The dogma of Cullen that there is never preternaturally increased heat, without phlogistic, sthenic or entonic diathesis, and that the amount of the preternatural heat is an exact measure of the degree or intensity of such diathesis, has just about as much foundation in fact, as John Mason Good's dogma that there is never preternatural frequency of the pulse without phlogistic, sthenic or entonic diathesis, and that the degree of the frequency of the pulse is an exact measure of the degree or intensity of such diathesis.

The term Refrigerant, as used by every author within my knowledge, appears to comprise only those agents which I greatly prefer to call Antiphlogistics; but as the term Refrigerant obviously means cooler, and nothing else, the etymological import of the term has been taken for the definition of the class of agents to which it has been applied. This evil almost always results from the use of a term inappropriate in its signification to the thing or things to which it is applied. But what is Refrigerant i. e. cooling in entonic, sthenic or phlogistic diathesis, is by no means Refrigerant or cooling in atonic or asthenic diathesis; and vice versa, what is Refrigerant or i. e. cooling, in atonic or asthenic diathesis, is by no means Refrigerant or cooling in entonic, sthenic or phlogistic diathesis; and hence the great evil in the practice of medicine of confounding Antiphlogistics with Refrigerants or coolers. There is an intense heat, which is a part of an atonic or asthenic diathesis, which is never obviated by Antiphlogistics, but is usually much aggravated by them, and which, on the contrary, is relieved and obviated by the Euphrenic and Antisbestic-Narcotics, and often by the pure Antisbetics and pure Tonics of a character similar to Cinchona. Assuredly every medicine that is capable, under any circumstances, of abating any kind of febrile heat, is not

to be reckoned a Refrigerant, in the sense of Antiphlogistic, the sense in which this term is always used, at least so far as I know, in application to a specific class of remedies, because in an acceptation so comprehensive, the term would include equally Depletion of Blood and Alcohol; Nitrate of Potassa and Opium; Tartrate of Antimonia and Potassa and Cinchona; since under certain circumstances, and in certain states and conditions of the system, each one of these agents may be quite efficient for abating morbid heat, or in other words, for proving Refrigerant or cooling.

But Cullen says—"in many trials made on purpose, it has not appeared to me that the supposed Refrigerants diminish the temperature of the body, which is the ordinary temperature of health; and therefore I am disposed to define the Refrigerants to be such medicines as diminish the temperature of the body when " (it is) "preternaturally increased." (*Ibidem.*) This is altogether at variance with my observations and experience. I have seen the effect of Nitrate of Potassa when employed quite efficiently, and for a long time, in a state of perfect health; and it certainly did greatly diminish the natural heat of health. In order to render the preceding statement credible it may be necessary to explain how a patient in perfect health should have happened to take this article efficiently and for a long time. The subject was healthy and vigorous, but had what is commonly called Stricture of the Urethra, the sequel of a neglected and protracted Urethritis Pyoblennorrhœica, which latter had been long cured when the Nitrate of Potassa was administered. The physician, consulted in the case, would have it that the disease was Gravel, and his remedy for Gravel was Nitrate of Potassa. Without going into any particulars, it will be sufficient to say in this place, that this agent did very greatly lessen the natural heat, the heat of health. I was once concerned in a series of experiments with the Chlorite of Potassa ($K^1 O^1 + Cl^1 O^5$) and found that this salt decidedly diminished the heat of health, as much in proportion to its Antiphlogistic power, as Nitrate of Potassa. At a still earlier period, I was concerned in a series of experiments with Bicarbonate of Potassa (then called Essential Salt of Tartar) and also with Bicarbonate of Soda (which might as well have been called Essential Salt of Soda, though I never heard it so called) and I found that both

of these Salts diminished the natural heat of health. To prevent any mistake in regard to the articles here intended, I must state that where I have resided for about twenty-five years past, the Sesquicarbonates of Potassa and of Soda have been called indiscriminately Supercarbonates and Bicarbonates, as well as *Salia aërata*; but I do not mean these; I mean the true and genuine Bicarbonates, which are intirely destitute of even the least Alcaline taste and action. I have repeatedly witnessed a diminution of the natural heat of health from a continuous use of Tartrate of Antimonia and Potassa, of Sulphuric and of Nitric Acids, and of Tartaric Acid. I have likewise seen a diminution of the natural heat of health produced by the Hyponitrite of Oxyd of Etherogen (Sweet Spirit of Niter) and much more prominently by Tritochlorid of Formicigen (Chloroform.) In short I believe that a diminution of the natural heat of health is as regular an effect of the continuous use of Antiphlogistics, as any other of their operations; and I can account for Cullen's ignorance of the fact only by supposing that he had never witnessed such use. In Cullen's day it was not necessary to use highly exhausting beverages, in order to prove one's temperance; nor did people think it important to inflate their unfermented and unbaked dough with the gas extricated from the mutual action of Sesquicarbonate of Soda and Bitartrate of Potassa, and to swallow along with it when baked the resulting Rochelle Salt (one of the most active of the Antiphlogistic purging Salts) to avoid the possibility of the bread's retaining a particle of the Alcohol, which is said to be generated during the process of the fermentation of the dough. In Cullen's time a man might eat a Biscuit or a piece of Cake without taking a cathartic in it; and Dyspepsia, Leucophlegmasia and Chronic Diarrhoea, produced by the Antiphlogistics and other exhausting agents eaten and drank, were not the prevalent chronic diseases. Now if a person is not more or less pale and leucophlegmatic or anhematous, and if his appetite is not fastidious, to say the least, he is considered as having "a terrible rush of blood to the head," and being in danger of dieing from over eating, excess of food or gluttony. I have been purged many a time, and many a time had Diarrhoea from Bread, Biscuit and Cake containing Antiphlogistic purging Salts, which I have felt constrained to eat, where no other was used; and I have known many other

persons affected in the same manner. Although phlogistic, sthenic or entonic diseases have disappeared from among us at the present time, yet as will be perceived, there is no lack of opportunity to study the effects of the Antiphlogistics, if we choose to avail ourselves of it.

The cooling operation of the Antiphlogistics is certainly a relative operation only, since they do not prove cooling in all cases of preternatural increased heat. It is only when heat makes a part of a general phlogistic, sthenic or entonic diathesis, that they prove cooling. Their operation to relieve dryness of the skin, thirst, pain, Coprostasis, deficiency of urine, watchfulness, etc. is as relative as their operation of cooling. In other conditions of the system, articles with powers quite opposite to, and incompatible with an Antiphlogistic may prove efficiently cooling, while an Antiphlogistic might actually augment the heat which is already preternatural. But though Cullen does not admit that what he calls Refrigerants—in reality Antiphlogistics—will diminish the heat of health, yet he appears to consider them as just about equally adapted to entonic and atonic cases, provided there is only preternatural heat to be cooled. There is then no peculiar and specific power, by which any article or articles are capable of proving positively cooling in all cases; but this effect is produced in various and different cases, by various and different powers, which are the foundation of various and different classes of agents. If all the articles that ever prove cooling under any circumstances were to be brought together, it would make a group very nearly as heterogeneous as one composed of a single article from every true and legitimate medicinal class.

Unless the term Antiphlogistic (and also Refrigerant when used as a synonym of Antiphlogistic) is restricted to those agents which abate or diminish phlogistic, sthenic or entonic diathesis, it will fail of comprising any group of articles, that have any thing like a community of operation, or that are at all adapted to obviate similar pathological conditions of the system; and if we do not retain the true and proper Antiphlogistics, in substantially the sense in which I receive them, as a class in the *materia medica*, then no place will remain in our systems of classification for several of the most important articles of the *materia medica*. I do not know how Cullen's "trials made on purpose" were con-

ducted; but as appears to me, nothing is better established than the power of Nitrate of Potassa for example, to diminish even the heat of health. The same that is true of Nitrate of Potassa is equally true of the concentrated vegetable Acids, provided they are taken in operative quantities, and for a sufficient length of time.

Edwards and Vavasseur say that the term Refrigerant is applied to "substances which moderate too great activity of the organs, and act more especially, by diminishing the rapidity of the circulation and the production of animal heat." These authors add that Refrigerants are also called Antiphlogistics, but they prefer the term Temperant to either of these terms. But how is "too great activity" of the sanguiferous system to be measured? I should think by the frequency with which the whole mass of blood goes the whole round of the circulation. I should think that our authors would assent to this, from another clause of their definition to the effect that the agents defined "diminish the rapidity of the circulation." But again the question recurs, how is this to be ascertained? To this I am not at all prepared to give an answer. I think that the answer should have been given by our authors; and in fact that they should have made all the explanations that their definition so much requires. It is commonly supposed, at least by many, that there is the greatest activity of the sanguiferous system, when there is the greatest frequency of the pulse; and at first view this seems plausible; and yet in some instances, when a patient is in articulo mortis, I have known the pulse as frequent as two hundred in a minute. I have seen this as often when there has been no inanition, as when there has been the greatest degree of it. In some exquisitely malignant cases, where the patient dies within six, and some times within even two hours, from what seemed to be perfect health, and where there were no evacuations at all, I have known the pulse as frequent as two hundred in a minute, and at the same time exceedingly weak. I can not think that, in such a case, the whole mass of the blood of health goes the whole round of the circulation with the greatest frequency when the pulse is the most frequent; but perhaps it does. If this is the fact "too great activity" of the sanguiferous system belongs only in a moderate degree to the phlogistic diathesis, and is eminently a quality of the atonic or asthenic diathesis. As a matter of fact, too great activity is very often reck-

oned as a part of the atonic or asthenic diathesis, in consequence of the great frequency of the pulse, which is so often found in that diathesis. This however I can not but consider an error.

From the context it is evident that, by their definition, our authors intend remedies for phlogistic, sthenic or entonic diathesis; for they expressly tell us that the class of agents which they are defining is called Antiphlogistics, though they object to this denomination for it. Preternatural vital energy and preternatural strength of action in the sanguiferous system are the essential pathological conditions of this diathesis; and certainly neither of them necessarily involves "too great activity;" and as a matter of fact I doubt whether such activity exists to any very prominent degree. I know of no reason to conclude that there is "too great activity of any organ or organs," unless it may be the sanguiferous system, in phlogistic, sthenic or entonic diathesis. Assuredly no other organ or organs can be considered as truly having "too great activity," at least as I think. It is true that this class of agents "diminishes the production of animal heat" in phlogistic, sthenic or entonic diathesis; though it fails of doing this, and sometimes even increases its production, in many cases of atonic diathesis.

In regard to the operation of the Antiphlogistics or Refrigerants, Edwards and Vavasseur say, "their local action upon the tissues, and especially on the mucous membranes, induces a contraction of the capillary vessels, paleness of the parts, etc." "When taken into the circulation" (say these gentlemen) "their proximate action is scarcely appreciable in a healthy state; but is more marked when the circulation is more active, the animal heat increased, and in a word, when the functions are in a state of morbid excitement." "If administered then, in a proper manner, they lessen the force and frequency of the pulse, moderate the animal heat, quench the thirst, increase the cutaneous perspiration, and the secretion of the urine; and in a word, they diminish all the febrile symptoms." By "local action," in this place, our authors must intend local effects produced by the internal use of these agents; and effects which result from a change in the condition of other parts of the system, beside those which they here specify. The Antiphlogistics when taken internally with proper efficiency, do indeed produce paleness of certain parts of the skin,

and I suppose of the mucous membranes also; but they do not accomplish this by producing directly a contraction of the capillaries of these parts, but by lessening the vis a tergo. The engorgement of the capillaries generally, in entonic, sthenic or phlogistic diathesis, is due to the preternatural vital energy and strength of action in the heart, etc. and the obviation of this engorgement, and the production of the paleness mentioned by our authors, results from the obviation of the preternatural vital energy and strength of action in the heart, etc. and the paleness particularly, is due to a reduction below the natural standard. When the capillaries are not filled, they contract of course to a greater or less extent. It is certainly an error to suppose, in consequence of this paleness of parts, that the Antiphlogistics directly produce a contraction of the capillaries. This is no speculation, but the principles on which it is founded, are capable of absolute proof. Entonic and atonic engorgements depend upon definite and precise principles, which may easily be made not only intelligible but clear. I have been in the habit of discussing this subject, as a part of the true and proper Institutes of Medicine (which I do not consider as mere physiology) in connection with regular instruction on the principles and practice of medicine, and some years ago, I published in the Boston Medical and Surgical Journal, a brief abstract of my lectures on Congestion, to which I must refer, since I can not go into the subject in this place. I do not think that in health, any material amount of any of the Antiphlogistics is capable of being taken into the mass of the circulating fluids, which would seem to be a sufficient reason why no great effects are produced by them when thus absorbed. But this is evidently not what our authors really intended. They clearly mean to say that the Antiphlogistics are not capable of producing much effect in health. This I positively deny. The effects of all the Antiphlogistics are as great in health as in entonic, sthenic or phlogistic diseases; and these effects are exhausting effects, though they are not Antiphlogistic effects, since there can be no Antiphlogistic effects where there is no phlogistic diathesis.

Since atonic diseases have been the prevalent acute diseases, I have very often heard the opinion that Antiphlogistics are articles of but little power. They are indeed of but little power for beneficial effects in such cases; but they have great power for bad ef-

fects, which I doubt not is sufficiently obvious to prevent any judicious man from pushing them to any considerable extent. It is rather difficult to conceive what motive there can possibly be for administering any known medicines in health, and especially exhausting medicines, as all Antiphlogistics are of course, unless it may be for fashion's sake merely; and since it has been the fashion to swallow exhausting and even Antiphlogistic salts in common beverages, and to take them in our Bread, Biscuit and Cake, there have been ample reasons for admitting their activity even in health. It would seem probable that the Antiphlogistics can not be used in France as they are in the U. S. A. that is, both as food and drink, or our authors would not have considered their action as scarcely appreciable in health.

When administered in a proper manner, in phlogistic diseases, the Antiphlogistics (as our authors say) do indeed "lessen the force and frequency of the pulse, moderate the animal heat, and allay the thirst;" but I do not think that they directly "increase the cutaneous transpiration at all, or to any material amount, the excretion of the urine," though they undoubtedly "diminish all the febrile symptoms." Phlogistic diathesis is attended essentially with a diminution of these two excretions, and obviating this diathesis allows them to be restored to their natural state, or something like it. I do not think that any Antiphlogistic is a direct diaphoretic, to a sufficient degree, to be of any remedial value for this operation; though a considerable number are entitled to be mentioned as sub-diuretic, and that merely.

"Taken in large doses" (say Edwards and Vavasseur) "they may irritate the digestive organs, and occasion alvine evacuations." "When the stomach is the seat of a slight irritation, the use of the Refrigerants may subdue it; but if any ulcerations, or other serious organic lesions, have taken place, these remedies produce a bad effect on the state of the patient." Edwards and Vavasseur add "it has been observed that when their employment is too long continued, they are apt to produce debility of the digestive organs, paleness of the skin, and general emaciation." Many of the Saline Antiphlogistics undoubtedly possess a true and proper Cathartic power, in addition to their Antiphlogistic power. In appropriate cases and in appropriate doses and quantities, no Cathartics operate with less morbid irritation either of

the intestinal canal, or the system at large. It is true I have heretofore suggested that if these very articles could be retained in the alimentary canal in an inordinate dose or quantity, without the aid of any powerful antirritant narcotic, like *Papaver* for example, I thought that, like Nitrate of Potassa, they would be capable of irritating the lining mucous membrane, and of producing a *Phlogosis Erythematica*. I came to this conclusion from observation of the fact that these salts when applied to exceedingly tender parts of the surface, do actually irritate in a moderate degree, giving unequivocal indications that they actually possess more or less true Oresthetic power; still, in consequence of their Cathartic power, they occasion such a large watery secretion from the mucous follicles, and augment the peristaltic action of the intestines to such an extent, that they are washed off, as it were, before there is time for them to irritate. In other words, they fail of irritating because they are hydragogue Cathartic—it is not true that they purge because they irritate. As I do not know what is intended by the stomach's being the seat of a "slight irritation," I can not judge whether, according to the results of my observation and experience, Antiphlogistics are likely to relieve such a condition or not; but I can say that I have never witnessed any material benefit from them, except in truly phlogistic diseases. The term irritation is applied to almost every pathological condition that can be mentioned, so that without details, nothing definite can be understood by it; but I would not advise Antiphlogistics for any irritation of any part not attended with a decided phlogistic diathesis. They are assuredly very ill adapted to relieve any mere irritation. As in a state of ulceration, there is never a phlogistic diathesis, Antiphlogistics can not be indicated in ulcerations of any part of the mucous membrane of the alimentary canal; and if used, it is quite likely they may aggravate the ulceration, though I never saw them employed in such a case. As to other organic lesions, it must depend upon their specific nature and character, whether they are likely or not to be injured by them. I doubt whether they would injure a stricture in the intestines dependent upon a contraction of the mucous membrane. The Antiphlogistic Cathartics always occasion a copious secretion of air into the intestinal canal, which may indirectly produce a great deal of trouble, in such a disease as stricture of the intes-

tines; and in such cases it is seldom desirable to adopt any measure, which tends to produce even a slight degree of general exhaustion. Otherwise I can not discover what injury an Antiphlogistic cathartic would be likely to do in stricture of the intestines. However I do not mean to express even a doubt that there are organic lesions of the intestines in which Antiphlogistics may be injurious. I only think that the generalization of our authors is too exclusive, much too loose, and much too vague.

It is quite certain that the essence of the operation of an Antiphlogistic is directly to diminish vital energy and strength of action, etc. and that, not only in atonic diseases, and also in health, but even in phlogistic, sthenic or entonic diseases. Such being the fact, I imagine it will not be deemed very remarkable that they debilitate or exhaust, particularly the organs of primary digestion, and that they weaken the action of the heart so that it is incapable of filling the capillaries of the skin, both of which result in emaciation and paleness. How is it possible that a practical physician can fail of discovering at once that it is this very operation that constitutes an Antiphlogistic.

Cullen says—"in what manner the Refrigerants produce their effects is not well ascertained; and whether they act by diminishing the temperature of the body, as cold bodies, or those of a temperature lower than that of the body itself do, or if they operate only by removing the cause of heat, has been a question" (*Cull. Mat. Med. B. S. Bart. Edit. Phil. 1812, Vol. II. Pg. 227. Chap. VII.*) It may be considered as absolutely certain that the Antiphlogistics do not diminish the heat of the body in the manner in which it is diminished by exposure to very cold air, or by the application of cold water, or in short by any substance much colder than the human body, and at the same time a perfect conductor of heat. Assuredly common observation and common sense in conjunction teach this most forcibly and clearly. If Nitrate of Potassa, Chlorite of Potassa ($K^1 O^1 + Cl^1 O^5$) Chlorate of Potassa ($K^1 O^1 + Cl^1 O^7$) Bicarbonate of Potassa, Bicarbonate of Soda, and even Tartrate of Antimonia and Potassa were to be administered in warm solution, it would not lessen their Antiphlogistic and exhausting operation in any degree whatever. This is conclusive in regard to the last point. There is just as much reason for asking—in what manner do the Nausiatics produce their

effects? In what manner do the Neuragics produce their effects? In what manner do the Narcotics produce their effects? This question might be asked with equal propriety of any other class. But then the question recurs—in what manner do the Refrigerants, or far better, the Antiphlogistics produce their effects? Cullen however does not hesitate to grapple with this question, and he appears to have explained it, to his own satisfaction at least, if to no body's else. He says that—"for this purpose" (viz. explaining "in what manner the Refrigerants produce their effects") "I am disposed to admit a doctrine delivered by the late ingenious Tuberville Needham, which seems to me to have been too little attended to, in the physiology and pathology of the human body." "We assume from him what we think he has demonstrated in fact, that there is every where in nature an expansive force and a resisting force; and that particularly under a certain degree of heat, the expansive power appears in all the parts of organized bodies, in consequence of which they show a singular vegetating power; while at the same time, in other bodies, there is a power resisting and preventing the action of this vegetating power, and at least of diminishing its force." "This resisting power he actually found in those saline bodies, which are commonly supposed to be Refrigerant powers with respect to the living body; and we hope that this doctrine may be applied to our purpose in the following manner." "As heat is the great support of expansive power, so we suppose that every increase of heat is no other than an increase of the expansive force in the heated parts; and from this we conceive it may be understood how resisting powers may diminish any preternatural expansive force and heat in our bodies." "We thus endeavor to account for the Refrigerant power of saline bodies; and the doctrine seems to be illustrated and further confirmed by this, that beside organized bodies, there seems to be an expansive force in all bodies disposed to any fermentation." "This seems always to begin by an expansion of air from a fixed to an elastic state; and it is very certain in fact, that by the contiguity of a sufficient quantity of saline substances, that is of resisting power, the beginning of every fermentation is prevented." "Such resisting powers have been often taken notice-of, as antiseptics; but there is hardly any doubt that the more general term antizymic may be fairly applied to them." (*Ibidem* Pg. 228-9.)

This supposed explanation is worthy of preservation as a physiological, and perhaps as a chemical curiosity, if for nothing else. Whether mere cooling, or a direct reduction of vital energy and strength of action, is the effect of this class of remedies, how is it possible that Cullen could even expect to be able to ascertain "in what manner these agents produce their effects"? How is it possible he could suppose that what he has said by way of explanation of this subject, is any explanation at all, or is capable of being even understood by any body? But in connexion with Murray's speculations upon this very subject, I shall have something to say upon it hereafter, not by way of analysis or refutation of Cullen's views, but by way of showing the utter impracticability of ever arriving at what they so readily undertake to ascertain, and therefore I shall make no further remarks upon it in this place.

Cullen says "the operation of Refrigerant powers, though we have supposed it different, seems in some respects to be analogous to the operation of actual cold applied." "Its operation upon the body is attended with this peculiar circumstance, that when applied in a moderate degree, and with no long continuance, it always increases the heat of the part, to which it is applied, and from the redness which it at the same time produces, it is pretty certain that both effects are produced by its increasing the action of the blood-vessels of the parts." "Its effects as a stimulant are upon no occasion more remarkable than when any substance is taken into the stomach, of such a temperature as to feel cold there" (when) "it commonly produces a sense of heat on the surface of the body; and a disposition to sweat is easily promoted, if at the same time the cold of the external air is avoided by" (suitable) "coverings." (*Ibidem.*) The whole of this statement appears to me to be a singular tissue of errors, having no real connexion with the operation of the Antiphlogistics or Refrigerants, as Cullen calls this class of agents. It is not true that a moderate negation of heat of no long continuance always increases the heat of the part to which it is applied; nor does it increase the action of the blood-vessels of the part; but if it were true, it could have no possible connexion with the operation of the Antiphlogistics. It is not true that a substance so cold as to feel cold in the stomach stimulates the stomach, or produces a sense of heat on the surface; and if it were true, it could have no possible connexion with the operation of the

Antiphlogistics. All that is true in this statement of Cullen may perhaps be better stated as follows. If the capillaries are the agents which propel the blood through the veins (as I entertain no doubt that they actually are) it will be obvious that any power that merely produces a suspension of their function for a short time, i. e. a temporary and transient Paresis, or an incomplete and imperfect Paralysis, will by virtue of such operation, cause them to be engorged with blood, or as the more fashionable and almost cant phrase of the day is, congested; and of course will cause them to become red. The irritation produced by this engorged or congested state will some times undoubtedly give rise to preternatural heat; and thus there may be both preternatural heat and preternatural redness, without "increased action." Now does not cold, or in other words, sudden and very considerable reduction of temperature produce redness and heat in this manner, rather than in the manner supposed by Cullen? Cullen continues, "quite analogous to this is the action of (medicinal) Refrigerants when taken into the stomach; for though we have denied their producing any actual cold there, they always produce a determination to the surface of the body, and a disposition to sweat, which from the analogy mentioned we are ready to ascribe to a Refrigerant power, or if the expression may be allowed to a potential cold which they can produce." Cullen says "how this is to be reconciled to the Refrigerant power which they are supposed to exert with respect to the whole system, is not to be easily explained." (*Ibidem.*) Leaving all discussion whether Refrigerants, or better Antiphlogistics, produce actual or potential cold in the stomach, or whether (which last is probably the fact) I procede to state that, by "determination to the surface of the body" I here understand the sending of a disproportionate quantity of blood to this part. If this phrase means any thing else, I am ignorant of what it is, and so far as I am concerned it needs explanation. It is certainly not true that Antiphlogistics when taken into the stomach "always produce a determination to the surface, and a disposition to sweat." It is not true that "determination to the surface of the body" is necessary, or at all favorable to the production of sweating; and therefore we may omit the trouble of attempting to reconcile what Cullen says "is not to be easily explained," and what, if it were reconciled and explained would probably be nothing

at all to the purpose. Cullen adds "to our present purpose however, it may perhaps be enough to say, that the Stimulant operation of actual cold, which sometimes occurs, will not be sufficient to make us deny its power, when longer continued, or frequently repeated, of diminishing the temperature of the body; so the Stimulant power which our Refrigerants frequently exert in the stomach, will not be sufficient to make us doubt of their Refrigerant power with respect to the whole system, which the experience of all ages has certainly established." (*Ibidem* Pg. 230.)

All this in reality has no sort of connexion with the Antiphlogistics, which we know by Cullen's catalogue are intended by his term Refrigerants. Mere coolers are never Antiphlogistics, though Antiphlogistics are often but not always coolers. A better case could not be selected to show how we are misled by words. Cullen and others call a class of remedies Refrigerants, which means coolers, and therefore the agents so called are mere coolers. If this class of agents had been named Abracadabra, or some thing else equally unmeaning, the name would have had no obvious etymological signification to mislead, and the real powers, operations and effects of the articles which it comprises, would doubtless have been investigated and arrived-at; but calling them coolers was an assumption that they are such, and such merely; and so their powers, operations and effects were summarily settled, and that for so long a time as they are called coolers.

Cullen says, it is proper to observe, that "the substances we suppose to be Refrigerant, are such as act not only by the potential cold, we have alleged them to be endowed with, but at the same time, by other operations that may be supposed to contribute to their general effects of diminishing the action of the sanguiferous system." "These operations" (he says) "are their being laxative in the intestines, and diuretic in the kidneys;—and we are disposed to judge, their relaxing a febrile spasm on the surface of the body, to be another means of their concurring to their general effect." (*Ibidem* Pg. 230.) If it had not been for the name Refrigerant, we should have been spared all speculation as to the operation of this class of agents by actual cold or potential cold, and their exhausting operation would never have been ascribed to either. How obvious it must be, that mere cooling can never obviate phlogistic diathesis. How much mere cooling would be

necessary to cure an exquisite Cephalitis Caumatodes-Phlegmon-
ea, that is, an entonic Phlegmonous Phlogosis of all the textures
of the brain in its widest acceptation, with a Cauma as a consti-
tutional febrile affection? How much mere cooling would be ne-
cessary to cure an exquisite Pneumonitis Caumatodes-Phlegmonea,
that is, an entonic Phlegmonous Phlogosis of all the textures of the
lungs, with a Cauma as a constitutional febrile affection? Is it
possible that any practical physician can ever have believed that
mere cooling is equivalent to Depletion of Blood; or vice versa,
that Depletion of Blood even relieves phlogistic diathesis by means
of cooling merely?

Except in its production of inanition, the operation of Deple-
tion of Blood, and a sufficiently free use of Nitrate of Potassa
agree perfectly in their effects upon phlogistic diathesis, upon a
state of health, and upon atonic diathesis. No doubt a positive
cathartic operation is more or less exhausting; but as produced
by every article, it is not Antiphlogistic. A mere laxative effect
is never Antiphlogistic, unless produced by an article that is An-
tiphlogistic independent of its laxative effect; and yet a laxative
effect however produced may be exhausting in very many cases.
A laxative, or even a full purgative effect produced by an infusion
of Cinchona, can never be Antiphlogistic in any case whatever,
and yet it would be exhausting in very many cases. Such exam-
ples show the importance of the distinction between true and
proper Antphlogistics, and articles or processes which, without be-
ing Antiphlogistic, are nevertheless more or less exhausting.

I am not aware that Diuresis is ever an Antiphlogistic process;
nor do I know that simple, pure or mere Diuresis is ever ex-
hausting without being Antiphlogistic. Many of the chiminal
Antiphlogistics are sub-duretic, by which I intend that they mod-
erately increase the quantity of water excreted by the Kidneys,
but not sufficiently to be of any service for the removal of the ef-
fused fluid of Dropsy. I am not apprised that the sub-diuretic
Antiphlogistics, are any more Antiphlogistic, or any more exhaust-
ing in any manner or degree, for the slight Diuretic power which
they happen to possess. At the present time we need not inquire
whether the Antiphlogistics are capable of relaxing "a febrile
spasm on the surface of the body" or not, since that singular spec-
ulation once deemed so ingenious, and once received by so many

as sound pathology, went to the tomb of the Capulets long ago.

John Murray says—"The theory of the operation of the Refrigerants is unsatisfactory and obscure; nor are the facts adduced to establish their operation altogether precise." (*Jn. Murr. Mat. Med. J. B. Beck's Edit. N. York, 1828. Pg. 621.*) This is almost exactly tantamount to Cullen's remark that—"in what manner the Refrigerants produce their effects is not well ascertained" etc. a remark heretofore quoted. But does not the same difficulty exist, and in an equal degree, in regard to the operation of every other class of agents, and every individual agent? The former of Murray's remarks in regard to this class of remedies is undoubtedly true, but the latter appears to me to be clearly incorrect. The effects of the Antiphlogistics are as unequivocal, as uniform, as decided and as prominent, and likewise as easy to be observed and recorded, as the effects of any other agents in the materia medica, and they have been as often observed and recorded. As to "the theory of their operation," as John Murray's language is, or "in what manner they produce their effects" as Cullen's words are, do we not know just as much about it, as we know of the theory of any other class or group of agents? The Antiphlogistics, as I have already so often said, produce a direct diminution of vital energy and strength of action; while the Antisbestics produce a direct increase of vital energy and strength of action; but how this is accomplished in either case, we are equally ignorant, certainly not more so in the former, than in the latter. The Antisbestics produce an increase of vital energy and strength of action in a quickly diffused and transient manner, while the Tonics produce an increase of vital energy and strength of action in a slow, gradual and permanent manner. Who can say how the first operate in the former manner, and how the second operate in the latter manner? The Euphrenics directly obviate languor and lassitude when they exist; produce a peculiar calm placid and pleasant sensation; occasion a peculiar but rather agreeable preternatural wakefulness; produce a greater or less degree of positive exhilaration; and occasion anæsthesia or insensibility to pain; etc. and yet who has ever pretended, or will ever pretend to explain how, or in what manner they produce all these effects? The Narcotics directly allay non-phlogistic irritability and irritation and irritative actions generally, non-phlogistic morbid sensibility and sen-

sation and pain generally. They also directly produce sleep in non-entonic conditions of the system; beside an aggregate of symptoms that are beyond their medicinal grades of operation, and which I am in the habit of calling ultimate Narcosis. I do not consider it possible for any person to explain how all these effects are produced. The Emetics produce upward peristaltic action of the stomach and œsophagus, and commonly (but not always) along with this, almost a convulsive action of the external muscles of expression called retching; but how or in what manner they do this, no man can ever pretend to tell. The Cathartics directly increase the secretion of air and serum or water into the intestinal canal, while at the same time, they augment the downward peristaltic action; but how they do this it is impossible to tell. Cicero said—"quid Scammoniæ radix ad purgandum, quid Aristolochiæ ad morsus Serpentum possit video; quod satis est; cur possit nescio"—which is just as true at the present day, as it was in the days of Cicero; and doubtless it will remain true to the end of time. As well might we expect to ascertain the peculiar quality of the action of the salivary glands which causes them to secrete the several varieties of saliva; of the mammary glands which causes them to secrete the several varieties of milk; of the testicular glands which causes them to secrete semen; etc. If we are in no greater ignorance of "the theory of the operation" of the Antiphlogistics, than of "the theory of the operation" of every other class of agents in the materia medica, why should this circumstance throw doubt upon "the facts adduced to establish their operation," or what ground does it afford for saying that these facts are not "altogether precise." I think that we should be satisfied with the knowledge of all the operations of the Antiphlogistics, and of the circumstances under which they are capable of taking place; but whether we are satisfied or not, it is all we ever have known, and as appears to me, all that we ever shall know. In such circumstances no explanation is possible, except a mere and pure hypothesis; and such hypotheses are much more than worthless—they always mislead in some way or other. It was (I believe) an old Roman maxim "*Fingere quæ non visa potest*," "*hunc tu Medice caveto*;" which is as judicious at the present period, as it was anciently; and yet the existence of such a maxim in former times shows

that the medical profession has ever been prone to fiction.

A brief review of a very few of the hypotheses for the explanation of the operation of the Antiphlogistics, it is believed, will abundantly justify my opinions and conclusions. The most simple of these, that, within my knowledge, has ever been advanced, by any physician or author of high eminence, is that first detailed by Cullen, but objected to, and not adopted by him. This author says, it has been supposed that "they act" (merely) "by diminishing the temperature of the body, as cold bodies, or those of a temperature lower than that of the body itself do." Cullen says—"as the neutral salts" (the Antiphlogistic salts are by no means all neutral, as for example, Bicarbonate of Potassa and Bicarbonate of Soda, Bitartrate of Potassa, Sesquicarbonate of Potassa and Sesquicarbonate of Soda, etc.) "which are the Refrigerants" (or Antiphlogistics) "chiefly employed, do, upon being dissolved in water, generate a considerable degree of cold; so it has been supposed that they may in like manner generate cold in our bodies, and therefore produce their effects as by an actual cold applied." (*Cull. Mat. Med. B. S. Bart. Edit. Phil. 1812, Vol. II. Pg. 227-8-9-10. Chap. VII.*) This doctrine Cullen himself very thoroughly refutes. He says, "this conclusion will readily appear to be mistaken, when it is considered that the cooling power of the" (Refrigerant) "neutral salts in water appears only during the time of their solution." When taken indeed undissolved, they may generate cold in the stomach, and from thence have particular effects; but as after solution they produce no permanent cold; so when taken in a dissolved state, as they commonly are, their Refrigerant" (Antiphlogistic) "powers can not be ascribed to any actual cold applied." Cullen adds "the conclusion drawn from their solution in water further appears to be very erroneous, from this, that acids, which are as powerfully Refrigerant" (Antiphlogistic) "in the human body, as the neutrals" (not merely neutrals) "do, however upon being mixed with water always generate heat;" (the mineral i. e. chimical-inorganic acids are weak Refrigerants or Antiphlogistics, and the vegetable acids do not, to my knowledge produce heat when mixed with water) "and even the neutral salts when any how deprived of the water necessary to their crystalline state, do, upon that water's being restored to them, always generate heat." "It is not therefore any

thing in the nature of the saline matter that has a power of generating heat or cold, in water or other bodies, but that the appearance of such a power depends intirely upon the circumstances of solution and mixture, and appears no longer than these circumstances exist." Hence Dr. Cullen very reasonably concludes that "it is not therefore by any actual cold applied, that the" (Antiphlogistics or) "Refrigerants diminish the heat of the living body." (*Ibidem.*) The simple fact that the saline Antiphlogistics are just as effectual when taken in tepid solution, as when taken in water of the ordinary temperature of wells in general, is conclusive against the opinion that they prove Antiphlogistic in consequence of being of a lower temperature than the body of the patient, or in consequence of producing a reduction of temperature by their solution.

As I shall hereafter inculcate, mere cooling is by no means Antiphlogistication; though true and proper Antiphlogistication is always cooling in genuine phlogistic diathesis. But though Cullen himself did not believe this explanation, yet I have often met with physicians who did believe it—indeed I know many such at the present time—otherwise I should not deem it worthy of comment—should not consider it as requiring what I have already said upon it. As to the supposed explanation of the manner in which the Antiphlogistics produce their effects, which Cullen actually adopted, it is too unintelligible and absurd to require refutation—too much so, to be capable of it; and beside this, I have already said all that is necessary in regard to it. It is but a few pages previons to this, that I have quoted it in detail, and it can not be necessary to repeat the quotation. John Murray says of it that "it is scarcely possible to understand it," and I think he would have been more correct, if he had said, it is utterly impossible to do so. John Murray adds that this and all the previous theories of the operation of the Refrigerants or Antiphlogistics are "extravagant and improbable." For myself I do not think that any of them are worthy of the name of a theory, in the legitimate sense of the term, i. e. a system of inferences or conclusions logically deduced from previously established premises, whose truth rests on independent evidence. They are not even true hypotheses, i. e. systems of assumptions, received only because they serve to connect and explain certain phenomena or

facts, assumptions of whose truth there is no other evidence than that they serve to connect and explain the phenomena or facts in question. They are at best nothing but speculations, of whose truth there is no sort of evidence. They do not even perform any of the functions of a hypothesis, but are visionary, useless, and in fact utterly impossible. I am perfectly apprised that in medicine the term theory is almost always employed in the very lowest sense of the term speculation; and as a member of the medical profession, I am heartily ashamed that such should be the fact—a fact so notorious, that it can neither be denied nor concealed. And yet John Murray is ready to add just such an other, to the list of those already exploded. John Murray says—“The discoveries of modern chemistry furnish some facts which may perhaps be applicable to this subject; and indeed it is only to those discoveries, which establish the source of animal temperature, that we are to look for an explanation of the changes to which it is subject.” (*Murr. Mat. Med. J. B. Beck's Edit. N. Y. 1828. Pg. 261.*) Now though John Murray speaks of “discoveries which establish the source of animal heat,” I have never met with the least account of any such, nor do I know of the least reason to conclude that any such have been made. So far as actual and positive knowledge is concerned, it is my belief that we are now as ignorant upon this point as Hippocrates was; and yet, with our present knowledge of chemistry, I think that the hypothesis that animal heat is previously latent heat rendered free, or in other words, heat extricated from a strict chemical combination, is highly probable. Latent heat or heat thus combined is insensible in all liquids, and in all gasses; and such latent or combined heat becomes free and sensible when a gas is reduced to a liquid, or a liquid to a solid. Animal heat seems to be extricated at every point of the system. Now I know of no gas in any part of the system, except intestinal flatus, which is never reduced to a liquid while in the intestines; and certainly I know of no gas which pervades every part of the system. Neither do I know of any liquid, except water, that pervades every part of the system, and this is never reduced to a solid within the system, i. e. never assimilated to any of the solid parts of the body, never digested, i. e. never decomposed within the system, and its elements united to any of the solids. Upon the hypothesis that animal heat is the

latent or chimically combined heat of some gas rendered liquid, or some liquid rendered solid, and its latent or combined heat set free, or extricated from its combination, we are most profoundly ignorant what the gas, or what the liquid may be, from which it is extricated. Admitting that animal heat is latent or combined heat thus rendered free, or extricated from strict chimal combination, the next enquiry must be as to the power by which such chimal decomposition is effected.

It has long been a matter of observation with me that the amount and quality of the animal heat seems to depend upon the condition of what is commonly called the great sympathetic nerve. From facts which it would be out of place to give here in detail, I have long been satisfied that all the chimal actions of the system depend upon this nerve; and from other and additional facts, I am equally satisfied that the production of animal heat depends upon it. This, if correct, affords strong evidence that the production of animal heat is a chimal process although we should be unable to designate the gas, or much more probably the liquid from which it is extricated. I can not doubt that, if animal heat is the product of chimal action, such action must consist in the liquefaction of a gas, or the solidification of a liquid. It was once attempted to be accounted-for on the supposition that the new products of digestion and assimilation have a much less capacity for free heat or heat of temperature; but I know of no evidence of any such thing; and even were such the established fact, I doubt whether it would be adequate to account for one quarter of the heat that is generated every day in health. This is a mere opinion however, since I have never made any accurate researches in regard to this matter.

Dr. Murray continues—"it is established by experiment that the consumption of Oxygen in the lungs is materially influenced by the nature of the ingesta received into the stomach." "Where these are composed of substances which contain a small proportion of Oxygen, the consumption of Oxygen is increased, and this in a short time after the aliment has been received." (*Ibidem.*) As this is intended, I do not think that such a statement is true. It must be recollected that all "the consumption of Oxygen in the lungs," that ever takes place, is merely its combination with the effete Carbon of the system, presented to it in a nascent state, un-

der which circumstances the two elements readily unite and form Carbonic Acid, all the inspired Oxygen, that disappears in the lungs, existing in the expired Carbonic Acid. The amount of Oxygen that disappears or (as Murray incorrectly says) is consumed in the lungs, is therefore only a test of the amount of Carbon excreted from the system. I believe that if either of these elements in a nascent state is presented to the other in a perfectly separate state, they are capable of entering into strict chymical combination with each other.

As appears to me, there is just as much, and as conclusive evidence that the lungs (except as organs of expression) are merely an emunctory for the effete Carbon of the system, as there is that the skin is merely an emunctory for the effete heat of the system, the kidneys merely an emunctory for those effete elements, which are capable of passing-off in a saline form, in solution in water, and the lower intestines merely an emunctory for the refuse of food. Again, all the Oxygen, that enters into the composition either of the soft or of the hard solids of the human body, is certainly taken into the system through the stomach, this being the only avenue for nutrition. All the nutriment of the system then is undoubtedly received through the stomach, the lungs being no more an organ of supply than any other emunctory. As well might we expect to receive nutriment through the skin or even the kidneys. It is undoubtedly true that during primary digestion i. e. chymification and chylication, there is a more active excretion of effete Carbon from the lungs, than when no food has been taken, and the more nutritious the food the greater is this excretion. Animal food contains more nutriment than vegetable, and therefore it occasions a greater excretion of Carbon. It may be true, for aught I know, that those articles of food, which contain the largest proportion of Oxygen in proportion to their other elements, are the least nutritious, and therefore require the least excretion by way of preparation for the reception of all the nutriment that can be elaborated from them. This is a matter which can be definitely ascertained only by long and careful observation. Murray says—"thus Mr. Spalding the celebrated diver observed that when he used a diet of animal food, or drank Spirituous Liquors, he consumed in a much shorter time the Oxygen of the air in his diving-bell, and therefore he had learned from experience to confine

himself to a vegetable diet, and water for drink, when following his profession." Murray continues—"during digestion too, it was well established by the experiments of Lavoisier and Seguin, that a larger proportion of Oxygen than usual is consumed." (*Ibidem.*) Such facts as these contribute only to prove that there is much more nutriment in animal food and Alcoholic liquors than in vegetable food and water; and consequently that there must be or at least may be more excretion of the effete Carbon of the system, by way of preparation for the ultimate assimilation of the nutriment, which they afford, than is required for vegetable food or water. Water being wholly indigestible, and incapable of assimilation to the solids, and performing only a mechanical part in the system, occasions no increase of the excretion of Carbon from the lungs; while Alcohol (at least the base of the salt which constitutes it) is digestible and nutritious, and of course increases the excretion of Carbon. The plain and obvious reason why animal food occasions the consumption (as it is termed) of a greater quantity of Oxygen in respiration than vegetable food, appears to me to be the fact that it furnishes a greater quantity of chyle, and a greater quantity of aliment; and perhaps for the time being requires greater exertion of the system to elaborate it, i. e. to digest and assimilate it. Again, the plain and obvious reason why Alcohol occasions the consumption (as it is termed) of a greater quantity of Oxygen in respiration than water, appears to me to be the fact that it is a powerful Antisbestic, and as such increases the vital energy and the strength of action, by which the functions of the nerve of chymical action nutrition and reproduction are discharged—to say nothing of the nutriment which it affords by digestion.

It must be recollected that the essential part of the respiratory function belongs to the secernent and absorbent system, and depends upon the nerve of chymical action nutrition and reproduction, a branch of which is sent to the lungs for this purpose. It is true that Dr. Good in his Nosology and in his Study of Medicine recognizes the respiratory function as a distinct function; but there is no sort of ground for such recognition. The true and proper respiratory function is assuredly only a part of the secernent and absorbent function. I do not think it is the fact that those substances which contain the least Oxygen are, like Alcohol, Antisbestic. Neither do I think it is the fact that those articles

which contain the most Oxygen are Antiphlogistic. The final cause for the increase of the excretion of Carbon from the lungs, during the digestion of a new supply of food, seems to be preparation for matter soon to be assimilated, for though without nutriment excretion goes on in a gradually diminished degree, yet whenever food is taken it is always more or less augmented.

Murray goes on—"the animal temperature is derived from the consumption of Oxygen-gas by respiration; and an increase of that consumption will occasion a greater evolution of caloric in the system, and consequently an increase of temperature, while a diminution of the consumption of Oxygen will have an opposite effect." "If then when the temperature of the body is morbidly increased, we introduce into the stomach substances containing a large proportion of Oxygen, especially in a loose state of combination, we may succeed in reducing the morbid heat." "This we accomplish in part by a vegetable diet, but still more effectually by the use of acids." "The vegetable acids in particular, which by experience are found to be the best Refrigerants" (Antiphlogistics) "are acted on by the digestive powers, and assimilated with the food." "As the Oxygen they contain is in a concrete state, little sensible heat can be produced by the combination of that element with the other principles of the food." "The nutritious matter conveyed to the blood containing thus a larger proportion of Oxygen than usual, will be disposed to abstract less of it from the air in the lungs, and consequently less caloric will be evolved." "The temperature of the body will be reduced, and this operating as a reduction of stimulus will lessen the number and force of the contractions of the heart." (*Ibidem* Pg. 261-2.)

Some items of this long speculation may possibly be true; though as a whole I think it would be difficult to find a more visionary and groundless attempt at physiology, unless it may be in some of the late writings of Dr. Justus Liebig. A physician distinguished for not only a philosophical but a practical knowledge of medicine, once observed to me, in a letter, that "Liebig's vital and pathological chemistry" (commonly so called) "seems to me infinitely more mischievous than the good old humors and vital spirits." "These seem never to have done much more than to cause the prescription of inert articles which were ultimately swept-out of use by observation and experience of facts, far more

easily than might have been expected, certainly far more easily than the Leeching, bleeding, purging and starvation of modern fashion." "Men believed in the humors and spirits just as they believed in the crystalline spheres of astronomy, but with no hindrance from them, went-on making inductions in regard to diseases and remedies, just as they went on accumulating valuable observations in regard to the heavenly bodies." If Murray intends that "the animal temperature is produced or sustained by heat disengaged in the bronchial tubes and the air cells of the lungs, in consequence of the combination of Oxygen with the effete Carbon of the system," he could not possibly have fallen into a greater error, since the latent or combined heat necessary to convert Carbonic Acid into a gas, is as great, as that necessary to sustain Oxygen in the state of a gas, so that no heat is extricated in the bronchial tubes or the air cells of the lungs. The lungs are certainly no warmer than any other part or organ of the trunk, as they ought assuredly to be, if they were the very *fire-place* of the whole system. If Murray intends that "the animal temperature" is produced or sustained by heat disengaged within the substance of the bronchial membrane, from Oxygen gas absorbed and combined there, with the effete Carbon of the system, either being liquefied or solidified by such combination, he is certainly in a very great error. There is not the least reason to believe that a particle of Oxygen gas is ever absorbed by the bronchial membrane; and even if it were so absorbed and liquefied or solidified within the substance of this membrane, all the heat that could possibly be extricated, would be immediately required to convert the Carbonic acid into a gas, which according to this hypothesis ought to be a liquid or a solid before it is excreted into the bronchial cells and air tubes of the lungs. And yet I have heard both these hypotheses or speculations, with all their absurdities and impossibilities, stoutly maintained by several chimists, though never by a physician who had been a practitioner long enough to have his medical knowledge corrected and extended by long observation at the bed-side of the sick.

It is probably true that in health, the more nutriment is assimilated, the more animal heat is generated; though I do not know certainly that this is the fact; and if it is so, I am unable to specify the exact connexion between the two facts. It is probably

true that the more nutriment is assimilated, the more effete Carbon is extricated, and consequently the more Oxygen disappears in the lungs. It is probably true that a diminution in the quantity of nutriment assimilated, diminishes the production of animal heat, though in what manner I am unable to point-out. It is probably true that a diminution in the quantity of nutriment assimilated, diminishes the quantity of effete Carbon excreted, and consequently diminishes the quantity of Oxygen which disappears in the lungs. All this is the very opposite of what John Murray seems to suppose. For myself I do not think that animal heat has any direct and immediate connexion whatever with the disappearance of Oxygen gas in the lungs, in consequence of its combining with the effete Carbon of the system, and thus forming Carbonic Acid, which is immediately expired. The consideration of the facts, as thus stated, renders the hypothesis or speculation under consideration sufficiently absurd.

I do not think that the fact that less Oxygen disappears in the lungs, or in other words that less effete Carbon is excreted, during the digestion of vegetable food than during the digestion of animal food is at all due to the circumstance that vegetable food contains more Oxygen than animal food, but rather to the circumstance that vegetable food affords much less nutriment, and is much less rapidly digested and assimilated. It certainly is not the fact that those articles which contain Oxygen in the loosest state of combination, are the most efficient and the best Antiphlogistics. The Deut-oxyd of Hydrogen may be mentioned as one of these. It is not the fact that those Antiphlogistics which are digested, i. e. decomposed and recomposed into a new form in the alimentary canal, continue to prove Antiphlogistic after their digestion; or in other words, after their decomposition and recombination. The vegetable organic acids are certainly not the most active and the best Antiphlogistics. The vegetable organic acids certainly do not contain Oxygen in the loosest state of combination, as Murray supposes. It is not true that the vegetable organic acids as such, and without decomposition, are ever assimilated to the living solid along with other nutriment, as Murray supposes, and thus prove Antiphlogistic. It is not true that the assimilation of the elements of the vegetable organic acids, after such acids have been perfectly decomposed in the organs of primary digestion, proves either exhausting or Anti-

phlogistic. For aught I know Oxygen may be an exhausting and an Antiphlogistic agent per se, in which case it would impart exhausting and Antiphlogistic powers to its compounds. I have no knowledge however that it is such. Elementary Hydrogen and Elementary Carbonum I have always considered as medicinally inert, and therefore incapable of imparting any medicinal power to their compounds. I do not now recollect that any of the compound radicals of H. C. with which we are familiar in an insulated state, are exhausting or Antiphlogistic. Oxygen may then be the active element of those vegetable organic acids, that are used as Antiphlogistics. But I have never made any investigation in this direction, having always considered the vegetable organic acids as feeble Antiphlogistics in comparison with Tartrate of Antimonia and Potassa, Bitartrate of Potassa, Nitrate of Potassa, Chlorite of Potassa ($K^1 O^1 + Cl^1 O^5$) Chlorate of Potassa ($K^1 O^1 + Cl^1 O^7$) Bicarbonate of Potassa, Bicarbonate of Soda, etc. Now the first of these articles contains two exhausting and Antiphlogistic elements, exclusive of Oxygen, and all the rest contain one exhausting and Antiphlogistic element exclusive of any other doubtful one. From these articles therefore, it is impossible to form even a probable judgement as to whether Oxygen is exhausting or Antiphlogistic or not. But if Oxygen is the exhausting or Antiphlogistic principle of the vegetable organic acids, why is not water exhausting and Antiphlogistic, which contains eight parts by weight of Oxygen to one part by weight of its other element, and that an inert element? Assuredly if those articles that contain the most Oxygen are the most exhausting or Antiphlogistic, a substance that is eight ninths Oxygen, and the other element utterly inert, ought to be one of our most efficient exhausting or Antiphlogistic agents. But water certainly is not Antiphlogistic or even exhausting in any degree however small.

Although John Murray does not inculcate it, totidem verbis, yet he seems all along to be possessed with the notion that Oxygen gas is received into the blood through the lungs, and as a gas is carried the round of the circulation, being assimilated and of course solidified at every point of the system, and giving out its latent or combined heat, and that this is the source of animal heat. He very evidently supposes that the system may receive all its Oxygen in a solid state from aliment received into the stomach, and

that in this case no new animal heat is generated. Now there is not the least evidence that a single particle of Oxygen gas is ever received into the system through the lungs. Except as an organ of expression, the lungs are a mere emunctory for the excretion of the effete Carbonum of the system, which must, from its intrinsic nature, pass-off either in the form of a solid, or of a gas. Now no emunctory seems capable of excreting a solid; and none but the lungs is capable of excreting a gas. The lungs are as much an emunctory of the system as the skin, or the kidneys, and are no more an avenue for the reception of aliment. Every thing that nourishes, supports or builds-up the system, must pass through the organs of primary digestion and be digested; that is, be several times decomposed and recomposed into a new form. In the stomach, it is converted into chyme; in the upper and smaller intestines, it is converted into chyle; in the mesenteric glands, it undergoes another change; and quite likely two or three more previous to ultimate assimilation.

There is no sort of evidence that either gaseous or solid Oxygen has any instrumentality whatever either in producing or increasing animal heat on the one hand, or diminishing it on the other. John Murray very evidently supposed that the animal heat always operates as a stimulus; that this stimulus produces increased frequency of the pulse, and all the other phenomena of Fever, whether entonic or phlogistic, or atonic; and that diminution of animal heat lessens stimulus, and consequently lessens all the symptoms and phenomena of fever. Elsewhere he gives indications of other opinions; but in this connexion this seems to be the most, if not the whole, that he recognizes. Although he expressly tells us that by Refrigeration he intends Antiphlogistication, the two processes, in his opinion being a unity; yet such is most assuredly not the fact; and his speculations very obviously have reference to cooling merely, and not at all to Antiphlogistication, or even to exhaustion in a less degree. Let it be borne in mind that John Murray's theory (as he calls it) is a theory of cooling merely, and not at all of Antiphlogistication; and that it is certain that mere cooling and Antiphlogistication are quite different things. It is Antiphlogistication and not cooling, that I am considering, while Murray is considering cooling, which I consider as comparatively a very unimportant process, not valuleless how-

ever by any means. John Murray's theory (so called) is palpably as defective as the theories of his predecessors. It would be difficult to devise a more absurd and unfounded speculation—one so utterly destitute of all proof, and one so capable of complete disproof. It is plainly based upon the utterly groundless hypothesis that Oxygen gas is absorbed by the lungs into the blood, carried the round of the circulation, and finally assimilated to the living solids, and of course solidified when it is assimilated, extricating its latent or chimically combined heat at every point of the system, thus producing what is called animal heat. But Murray's grand mistake is the opinion that the Antiphlogistics act chimically instead of vitally.

Murray says that upon his theory, "it might be supposed that any effect of this kind" (i. e. any refrigerant or Antiphlogistic effect) "must be trivial," and therefore like other such theorists, he maintains that "it is actually so;" and he says "we find in practice that Refrigerants" (or Antiphlogistics), "produce no sudden or great change," that "they operate insensibly and have little other effect than moderating the morbid heat." Of late it is very frequently said and inculcated, that this class is made up of very feeble remedies—agents of weak action upon the system and of small medicinal powers. Now this is absolutely and totally at variance with all my observations and all my experience. I never could find any such thing in actual practice. I find no difficulty in employing the Antiphlogistics so inefficiently, and in such a manner, as to obtain little obvious and decided immediate effects from them. I can say the same of Cyanid of Hydrogen; of Benzhylid of Hydrogen, of Arsenous Acid, etc. But such a statement proves nothing. Are there not some doses and quantities, and these within the range of convenience, in which they would prove operative and even active? So far as books contain specifications upon this subject, and so far as I have had opportunity to make observations upon the practice of those who follow the common routine of the books, in which Antiphlogistics and articles of an opposite character are mingled in practice, I am satisfied that this erroneous notion originated in the first place from inefficient doses and quantities of these agents; and in the second place, from employing them in atonic and consequently inappropriate diseases. It is true that efficiently operative doses

of the Antiphlogistics are not as small as efficiently operative doses of the Narcotics for example ; but they are not therefore to be pronounced feeble agents. The bark of the officinal species of Cinchona, I should not be inclined to consider a feeble agent ; and yet it must be administered in large doses and large quantities in comparison with the Narcotics, in order to have it prove efficiently operative. Now, if the Antiphlogistics are employed in doses of one quarter the size of those of Cinchona, I doubt not that they will be found sufficiently active. Tartrate of Antimony and Potassa, which has already been repeatedly mentioned as an Antiphlogistic, is certainly not a feeble remedy, even in such doses and quantities, as those in which we employ Narcotics.—Very nearly the same may be said of several saline Antiphlogistics that I have already mentioned many times. I can not therefore assent to John Murray's statement that the effects of the Antiphlogistics must be trivial.

If Murray's is the best theory of the operation of the Antiphlogistics, that can be given, well might he say that it is "unsatisfactory and obscure," and that "the facts adduced to establish their operation" (in such a way) "are not altogether precise." I am perfectly satisfied however that such hypotheses only embarrass the subject with difficulties altogether factitious. And yet upon this theory (so called) this "baseless fabric of a vision," Dr. Murray, in his classification of the *materia medica*, has transferred the Antiphlogistics from the vital to the chymical remedies ; though there is just as strong evidence that they operate upon the vital powers of the system, and therefore upon true medicinal principles, in contradistinction from either chymical or mechanical principles, as that the Narcotics, or the Euphrenics, or the Antisbestics operate in this manner. To those who are enamored with chymical speculations in physiology and pathology, I would recommend a careful consideration of the judicious remarks upon this topic, of a distinguished American medical author. The author referred-to declares that "chymistry in general, as applied to the functions of living" (organized) "matter, should receive no countenance from physiologists" (and pathologists). "It neither performs any of them, nor aids in the performance." "Within its proper sphere, that science is delightful and important." "None can be more so." "But it is concerned exclusively with dead mat-

ter." "With life and all its attributes, it is at war." "It is the great antagonist of life, and life of it." "It is no more suited to explain a single function of living matter, than the laws of life are to explain the formation of Carbonate of Magnesia, or Sulphate of Soda." "When an attempt is made to expound by it a vital phenomenon, it is dislocated and misapplied, and that dislocation" (and misapplication) "like every other, proves a source of mischief." "Chimical causes can produce only chimical effects; and vital causes, vital effects." "They are not transmutable in themselves nor" (is) "their action." "Physiologists" (and pathologists) "would escape an infinity of trouble and the" (medical) "profession no less confusion and error, were chimists to confine themselves to their proper laboratories, and to dead matter."—"The living body of man is as completely without their sphere as its structure and economy are beyond the imitation of the manufacturer of chess-playing automata, and rope-dancing harlequins." "It is in vain for Broussais and other animal chimists" (so called), "to endeavor to explain away the error they propagate, and the mischief they do, by the terms they employ," "To tell us that by 'animal and vegetable chimistry,' they mean the mutual action in the form of decomposition and recomposition of the 'radical molecules of organized matter, under the control of the vital principle' is of no avail as to the object they profess to have in view." "Chimistry is a technical word possessed of a definite meaning." "For centuries, it has been the representative of certain changes in the composition and qualities of matter, produced by affinity and repulsion, under the influence of given laws."—"Nor is there between those changes, and the phenomena of life, the slightest similarity." "On the contrary they are the very antipodes of each other." "Dissimilitudes stronger than those which exist between them can scarcely be imagined." "Yet, when the changes in living matter are said to be produced by 'animal chemistry,' nine persons out of ten—I might say ninety-nine out of a hundred—attribute them to the agency of the common chimical affinities—I mean the chimical affinities of the laboratory." "They consider respiration, digestion, and other vital functions, as belonging to the same class of processes with the combustion of charcoal, the decomposition of water and the formation of salts." "Thus the error is propagated by an improp-

er use of words." "Nor does there exist for that use the slightest necessity." "The expressions, animal action—vegetable action—or the more general one vital action, would be much better than animal, vegetable or vital chimistry." "The former, although not explanatory of anything, do not mislead; whereas the latter do." "I need scarcely add that every phrase which propagates error, ought to be erased from the language of science." (*Caldwell on Malaria, Amer. Journ. Med. Sci., No. xvi. Aug. 1831. Pgs. 288-9.*)

There is another theory (so called) in regard to the operation of the Antiphlogistics that I will just mention in this place, viz., that "they operate only by removing the cause of heat in the animal system." As a substitute for this, and one which will explain the matter full as well, and be much less liable to objections, I would propose the theory that Antiphlogistics operate by producing Antiphlogistication. If this view of the subject be universally received it would save a vast deal of fruitless discussion, and prevent much racking of the brain to imagine something new upon this topic by future writers on materia medica.

As Cullen's views and opinions mainly produced the present British and Angli-American practice with the Antiphlogistics, though the views and opinions themselves are now but little known, I shall not only give a catalogue of his Antiphlogistics, but also a few words by way of explanation of the views and opinions above alluded-to. It must be observed that Cullen calls this class of remedies Refrigerants while I call them Antiphlogistics. They are the same group of agents, however, by whichever name they may be called. John Murray, Edwards and Vavasseur and others, expressly tell us that they consider these terms as synonymous in application to a class in the materia medica; and though Cullen does not say the same, totidem verbis, yet the agents, of which the class is composed, say it as plainly and as positively as it can be said. Except in a verbatim quotation, I shall hereafter employ the term Antiphlogistic instead of Refrigerant, because it is correct in its signification, whereas Refrigerant is not, and because it will not be liable to mislead by an inappropriate signification.

CULLEN'S CATALOGUE OF ANTIPHLOGISTICS.

ACIDUM VITRIOLICUM.

Cullen says that this article is Stimulant and Tonic, as well as Antiphlogistic.

ACIDUM NITRICUM,

Aqua fortis, vel *Acidum Nitri*. Cullen thinks that the medicinal powers of Spiritus Nitri dulcis are due to the Acid of Niter which it contains, though he adds, if properly prepared, it should contain no Acid. He says that this substance is Diuretic as well as Antiphlogistic.

ACIDUM MURIATICUM,

Acidum Marinum, vel *Spiritus Salis rectificatus*. Dr. Cullen seems to consider this as Tonic, though he does not use the term, as well as Antiphlogistic, but only says it is useful in improving appetite. Cullen mentions Spiritus salis dulcis, and he says that he considers its use as an employment of this Acid.

ACIDA VEGETABILIA.

Cullen says there are three kinds of these, viz. the native, the distilled and the fermented. He thinks them the most effectual Antiphlogistics that we possess, and says that they are never caustic or stimulant, except so far as the weaker and more dilute Acids are Stimulant. He considers them as antiputrescent, as occasioning appetite and promoting digestion, and as increasing the urinary secretion. He adds that they are liable to undergo fermentation in the stomach, which is attended with flatulency, a more powerful acidity, and all the other symptoms which we term Dyspeptic; but that all this does not do much harm, nor affect their Antiphlogistic power. Dr. Cullen considers that all the virtues of Tar-water (so called) which, beside being Antiphlogistic, he says strengthens the stomach, increases appetite, promotes digestion and cures all the symptoms of Dyspepsia, are due to what he calls a distilled Acid; and he combats the opinions of those who think otherwise.

ACETUM.

This is Cullen's principal fermented Acid. He considers it as very Antiphlogistic and at the same time Stimulant, Antiseptic, Astringent, Diaphoretic even to a Sudorific degree, and Diuretic. He says it cures obesity.

ACIDUM LACTIS.

Cullen considers what he calls Acid of Milk as vegetable, because Kine are herbivorous; and he considers it likewise as a fermented Acid.

ACIDUM TARTARI.

Cullen says that Acid of Tartar is a fermented Acid, but he merges its consideration in that of Neutral Salts.

Cullen says that these are the principal Acids now used in medicine, but that there are many others, that have been sometimes employed, and that deserve to be inquired-after. He however mentions only one more, and that not vegetable, viz.:

ACIDUM BORACIS.

This Acid Cullen is inclined to consider inert. At least he says that according to his own experience, large doses have no effect upon the human body.

SALIA NEUTRA.

Neutral Salts are the next Antiphlogistics mentioned by Cullen, which, he says, with the Acids, are the Antiphlogistic remedies on which we chiefly depend in practice. Cullen says that an Antiphlogistic power seems to belong to every Neutral Salt except those formed "of Muriatic Acid and Fossil Alkali, and perhaps some other Acids, which carry into the composition of Neutrals some other matters of an acrid kind; but these are not well ascertained." He adds—"we take it for granted that it is of the nature of a Neutral Salt, composed of an Acid and an Alkali, with the exception mentioned, to give a Refrigerant substance." Cullen considers what he calls Neutral Salts as being antizymic, antiseptic and sedative. The Neutral Salts formed of Muriatic Acid and Fossil Alkali, he considers as Stimulant also, and in addition still, Sudorific. He says that the prejudices of physicians are in favor of the Neutral formed of the native Acid of vegetables with the fixed vegetable Alkali; but Cullen says that the Muriatic or even the Vitriolic Acid is just as good. He adds that the original Antemetetic draft of Riverius was made with Vitriolic Acid.

The individual salts reckoned as Neutral by Cullen, which he particularizes, are the following, viz.:

TARTARUM VITRIOLATUM.

This he says is Diaphoretic, on which account it enters into the composition of Dover's Power.

SAL MIRABILE.

This Cullen says is employed as a purgative; but that it has Antiphlogistic powers appears from the intestines' being left in a lax and flatulent condition after its operation.

SAL AMMONIACUM SECRETUM.

He says that this Salt is nearly of the same nature as common Sal Ammoniacum. I suppose he means medicinally, and not as respects composition.

SAL NITRI.

Cullen says that Niter has been esteemed the most powerful Antiphlogistic; and that from Dr. Smith's experiments, as well from those of Mr. Alexander, it appears to be so. He says that all Antiphlogistics produce a determination to the surface of the body, and thereby increase the force of the circulation, and after this operation they prove directly Stimulant to the stomach and alimentary canal; and in this way Niter is as remarkable as any other, and it is therefore, in large doses, very often uneasy and painful to the stomach. He considers Niter as also Sudorific.

NITRUM CUBICUM.

Of this article Cullen professes to know but little practically, though he seems to consider it as possessing the same powers as Sal Nitri.

SAL COMMUNE.

Cullen mentions Common Salt among his Antiphlogistics, but only to tell us that when "applied to the nerves or other irritable parts" (it) "shows a strongly stimulant power, and is therefore to be thrown out of our list of Refrigerants." "Its Stimulant power seems in part to be owing to the Fossil Alkali in its composition; for this Alkali joined with the Nitrous or Vegetable Acids, does also, in the first application to the nerves, in Dr. Smith's experiments, show somewhat of a Stimulant power, which however soon passes away, and they afterwards prove manifestly sedative." But Cullen says that those Neutrals consisting of the fixed Vegetable or Volatile Alkali, though formed by the Muriatic Acid, may be taken into our list of Refrigerants; and their common employment as Sudorifics for preventing the recurrence of Intermittent Fevers is only to be explained upon this ground."

SAL AMMONIACUM.

Though Cullen enumerates this Salt among his Antiphlogistics, yet he says "what are its peculiarly useful powers I dare not determine." He says "its resolving powers by attenuating or dissolving the fluids I do not admit-of; but that, like other saline matters, in passing by the excretions, they are suited to promote these, may be readily allowed." As appears to me this is tantamount to admitting what he combats. Cullen contests many other prevalent opinions in regard to this Salt, but he does not attempt to establish any thing new in regard to it.

Cullen says "the Neutrals composed of the Vegetable Acids must be different according to the species of Acid employed; but they are all Refrigerant and Diaphoretic."

MISTURA SALINA.

This is the next Antiphlogistic which Cullen mentions. He says that this is composed of the Native Acid and the fixed Vegetable Alkali. He says that the Acid commonly employed is that of Lemons, but only because it is most easily obtained. He says that he has frequently employed the Acid of other fruits, as Apples for example, with equal advantage.

SAL ABSINTHII ALKALINUM.

The Alkaline Salt of Wormwood, says Cullen, is equally Antiphlogistic with the other Salts, which he calls Neutrals. He considers it as Sudorific, Diuretic and Purgative besides. On the whole he seems to prefer it to Niter, because he says it does not produce as much uneasiness in the stomach. He says it is commonly given in too small doses, and at too great intervals.

ACIDUM AËREUM.

Cullen considers this as a moderate but useful auxiliary Antiphlogistic; as no doubt it is.

SPIRITUS MINDERERI.

He says that this is famous in the practice of Britain; but that it is so weak a preparation as to accomplish little or nothing. He has given eight ounces within a short time without any sensible effect.

SALES TERRESTRES.

He should have said *Salia Terrestria*. He considers them as Antiphlogistics, but says they are no better than what he calls Neutrals, i. e. Salts with an Alkali as a base.

Cullen just mentions

SALIA METALLICA,

for the purpose of saying they are Acrid and Stimulant, and that none of them can be considered as Antiphlogistic and sedative, with the single exception of

SAL PLUMBI.

I repeat that I have been thus particular and full in regard to Cullen's list of Antiphlogistics and his views of their powers, operations and effects, because all the use I have ever seen made of them (except by a small body of physicians in Connecticut, at least half a century in advance of their cotemporaries, all of whom are now dead) has evidently and palpably been founded on these views, though many of the physicians so practicing had never read Cullen, but had derived their practice, either from their private instructors in medicine, or from their professional neighbors and associates. It is truly surprising that a man who advanced medicine so much should have fallen into so many, and such extraordinary errors in regard to this class of agents. I shall not pause here to discuss, refute or correct any of these errors. What I conceive to be true of this class, has already been stated, which, when it differs from Cullen's views, implies what I conceive to be a correction of them. What pertains to individual articles will be corrected according to my notions, when I come to treat particularly of such articles.

I shall now proceed to give the catalogue of the Antiphlogistics of two more British authors, and one French author on materia medica, that have been extensively read and have had great influence in the U. S. A. In selecting these I do not mean to decide that they are the best British and French authors. I do it because they have been more widely circulated, and have had a far greater influence than any other foreign authors.

JOHN MURRAY'S CATALOGUE OF ANTIPHLOGISTICS.

CITRUS MEDICA,

evidently meaning

CITRUS LIMONUM.

CITRUS AURANTIUM,

evidently including

CITRUS BIGARADIA.

TAMARINDUS INDICA.

OXALIS ACETOSELLA.

ACETUM.

SUPERTARTRAS POTASSÆ.

NITRAS POTASSÆ.

SUBBORAS SODÆ,

evidently meaning

BIBORAS SODÆ.

Murray says that all Acids are Antiphlogistic, though the Vegetable Acids are to be preferred, "a superiority founded on their being more easy of assimilation, and of being acted-on by the chymical process of the living system," etc. He says that "the Neutral Salts are less effectual than Acids, and" (that) "their Refrigerant power is even problematical, except in so far as they operate on a principle different from that which has been pointed out" (viz.) "the power they have of producing in the stomach a sensation of cold." If such is the fact, why are many physicians so solicitous that patients with Typhus, and Typhus with extreme atony, should swallow as large a quantity of Acids as possible. I venture to say that many more than a hundred times, I have heard it enjoined upon the non-medical attendants, by the physicians, that the patient should be induced to swallow as much of some Acid or other, as can be done with any convenience.—Indeed I have very rarely seen such a case of Typhus treated without Acids, except by that small body of physicians in Connecticut to which I have just referred. It is rare that I visit in consultation a patient affected with Typhus who is not well furnished with Lemons and other sour fruits; and such patients are not only furnished with them but they use them, and that too with freedom. Murray expressly inculcates the propriety of the use of Antiphlogistics, in phlogistic and in atonic diseases—in Cauma and in Typhus.

Is it not the fact that in Cauma and the Caumatoid Phlogotica, there is a preternatural increase of vital energy and strength of action in the sanguiferous system; and does not the danger from these diseases result from this condition mainly if not wholly? Is it not the fact that in Typhus and the Typhoid Phlogotica there is a greater or less degree of deficiency of vital energy and strength of action in the sanguiferous system, and probably in all

those subordinate parts, which depend, for their action, upon the great sympathetic nerve (commonly so called) or better the nerve of chymical action, nutrition and reproduction; and does not the danger in these diseases result from an increase of this deficiency to a certain extent? Is it not the fact that all the true and proper Antiphlogistics if used with sufficient freedom, always abate the preternatural increase of vital energy and strength of action in the sanguiferous system, which constitutes phlogistic diathesis, the most important pathological condition of Cauma and the Caumatoid Phlogotica; and is it not for this purpose that they ought always to be employed? How unimportant is the single symptom of preternatural heat in comparison with preternatural increase of vital energy and strength of action in the sanguiferous system, much more especially when we note the facts that the former cannot be relieved without the obviation of the latter; while the obviation of the latter causes the immediate disappearance of the former. Is it not the fact that all the true and proper Antiphlogistics, if used continuously and freely in health, always produce a greater or less degree of general debility, but more especially in those subordinate parts dependent for their action upon the nerve of chymical action, nutrition and reproduction? At all events, I have been called to prescribe for patients with greatly impaired tone of the stomach, indicated by loss of appetite and lesion of digestive power, with intolerance of many articles of food, that the healthy are enabled to eat and digest without inconvenience, these symptoms being accompanied with considerable constitutional debility, the whole produced by the frequent use of Lemonade, preparations of Tartaric Acid, sold under the name of Syrup of Lemons, sweetened solutions of Bitartrate of Potassa, etc. used as common beverages. If persons must employ exhausting or debilitating agents in this manner, I have no doubt they are less injurious with the addition of a small quantity of Alcohol in some form, constituting what is commonly called Punch; but I do not believe that accompanying exhausting agents improperly used, with invigorating agents, ever perfectly prevents the ill effects of the former.

In connection with this statement, I must repeat that I do not by any means consider the Acids as the most active of the Antiphlogistics. I have already mentioned the injury to the tone of

the stomach and of the system at large, which I have so often seen produced by the frequent use of the Metallic Alkalies.—These I regard as considerably more hurtful than the Acids of vegetable organic origin, and I am inclined to think more hurtful than the Acids of chymical inorganic origin; but of this I am not quite sure. I have known persons not affected by any disease to swallow the Metallic Alkalies under the notion that they were salutary and healthful, till they had produced serious and permanent disorder of the organs of primary digestion.

I repeat, is it not the fact that all the true and proper Antiphlogistics still further increase the deficiency of vital energy and strength of action in the sanguiferous system, and probably in all those subordinate parts dependent for their action upon the nerve of chymical action, nutrition and reproduction, which constitutes the atonic diathesis, and is the most important pathological condition of Typhus and the Typhoid Phlogotica? Of what importance is the preternatural heat of Typhus and the Typhoid Phlogotica, in comparison with the deficiency of vital energy and strength of action in the sanguiferous system; but here also the former is usually removed by the obviation of the latter, and not usually without it, in a greater or less degree, except by a fatal sinking of the case at a critical period.

I have always been so situated as to have opportunity among my professional acquaintance, to see Typhus and the Typhoid Phlogotica treated, almost every year, with Antiphlogistics, with a mixture of Antiphlogistic and Non-Antiphlogistic agents, and exclusively with Non-Antiphlogistic remedies; and if there is any correct judgment to be formed from comparative observations, the last method has an immense superiority over the preceding two. Between the first and second methods I never could discover any material preference in non-malignant cases; but in malignant ones the second method has a decided advantage. Of all the Antiphlogistics, I think that Antimonials leave the stomach particularly, and also the system at large, in the very worst condition, when the patient does not happen to die. Within the sphere of my observations, the Antiphlogistics have commonly been employed in Typhus and the Typhoid Phlogotica, not under their proper name of Antiphlogistics, nor under that of Refrigerants, but as Febrifuges—a very different application of this term from the most

common one, viz. to Cinchona and other articles supposed to possess analogous powers for the cure of Intermittent, Remittent, etc.

PEARSON'S CATALOGUE OF ANTIPHLOGISTICS.

a. From the Vegetable Kingdom.

CITRUS MEDICA,

clearly meaning

CITRUS LIMONUM.

OXALIS ACETOSELLA.

ACETUM.

KALI ACETATUM.

KALI NITRATUM.

TARTARI CRYSTALLI.

b. From the Mineral Kingdom.

AQUA FRIGIDA.

ACIDUM MURIATICUM.

ACIDUM VITRIOLICUM.

PLUMBI PRÆPARATA.

ZINCUM VITRIOLATUM.

EDWARDS'S AND VAVASSEUR'S CATALOGUE OF ANTIPHLOGISTICS.

ACIDUM BORACICUM.

POTASSÆ NITRAS.

ACETUM VINI.

ACIDUM TARTARICUM.

CITRUS MEDICA,

clearly meaning

CITRUS LIMONUM.

CITRUS AURANTIUM,

apparently including

CITRUS BIGARADIA.

ACIDUM CITRICUM.

RIBES RUBRUM.

MORUS NIGRA.

RUMEX ACETOSA.

ACIDUM OXALICUM.

POTASSÆ SUPEROXALAS.

RUMEX ACETOSELLA.

OXALIS ACETOSELLA.

FRAGARIA VESCA.

RUBUS IDÆUS.

CERASUS VULGARIS.

PYRUS MALUS.

BERBERIS VULGARIS.

PUNICA GRANATUM.

Edwards and Vavasseur say "the Refrigerant" (Antiphlogistic) "remedies are indebted for their virtues to the presence of a weak Acid, such as the Citric, Malic, Tartaric, Acetic, Oxalic, etc., edulcorated* with a large proportion of water." "Most of these are of a vegetable nature; their component elements, if we except their Acid, are nearly the same, and their action on the economy is very similar, so that they may be employed almost indefinitely." (*Togno's and Durand's Transl. of Edwards's and Vavasseur's Man. Mat. Med. Philad. 1829. Pg. 417, Temperants.*)

Edwards and Vavasseur say that "all remedies belonging to this class possess an acid taste more or less marked." (*Ibidem.*) This is a specimen of French notions in regard to the Antiphlogistics. Now it is true that those Acids which are sour (in contra distinction from a larger number of compounds, which, as respects their chymical habitudes and relations, are Acids, without being sour) are probably always Antiphlogistics; but they are by no means all the Antiphlogistics known and used, and by no means the most active and efficient which we possess. It is true that most of the Antiphlogistics are of vegetable origin; but there is a considerable number of chymical and inorganic origin; and these are in fact the most active as Antiphlogistics, though their efficient use for this purpose is very greatly hindered by their intense Oresthetic power. What Edwards and Vavasseur mean by "their compound elements, if we except their Acid," I am not quite sure that I comprehend, indeed I doubt whether I understand it at all. What the "component elements" of an Acid can be, beside those that constitute the Acid, I can not conjecture; nor how

* Edulcorated signifies sweetened, while Edulcorants as a group of medicines are defined to be such articles as "purify the fluids by depriving them of their acrimony." (Hoop. Lex. Med.) But this cannot be the import of the word in this place. Here it would seem to mean diluted—a new and far-fetched and probably incorrect use of the word. Whether this peculiár usage is in the original work, or is found only in the translation, I know not.

these can be spoken of without speaking of the Acid, I know not. It is true that the "component elements" of almost all of the vegetable Acids are, not nearly but absolutely the same. The number of vegetable Acids used in medicine as Antiphlogistics, that are not composed of a compound radical of H. C. acidified by O. does not exceed two, so far as I can recollect just at the present moment; and one of these can hardly be said to be sufficiently employed for this purpose to be worthy of being mentioned in this place. That which immediately precedes, with what I have heretofore quoted, contains all the information to be derived from Edwards and Vavasseur, in regard to the Antiphlogistics as a class. So far as I can judge by what is said of individual Antiphlogistics, these gentlemen seem to consider them as equally adapted to the treatment of diseases of exhaustion, and of phlogistic or entonic diseases.

NOTE.—So far as the articles belonging to this class are of chymical-inorganic origin, I shall in connection with this proëm, designate them by the names, which, according to the laws of the chymical nomenclature of Guiton de Morveau, Lavoisier, Bertholet and Fourcroy, the most accurately express their composition, notwithstanding this nomenclature has been so much disregarded of late, in naming new compounds. It will be obvious that in my proëms, I can not give any synonymy, though subsequently when I come to treat of individual articles, I intend to bestow particular attention upon that subject, so that every one may find a name to suit his fancy. I am perfectly aware that it is customary to speak of the use of the names, which I propose to employ as mere anile pedantry; but it never appeared to me that those who entertain such a view, can have well considered the subject. But there are very many articles, that have been introduced into the *materia medica*, within the last half century, which have no other names. What shall we do with these? Shall we devise names for them in conformity with some antiquated and exploded nomenclature of chemistry or not? If we must choose such a name, what particular nomenclature shall we conform to? I think every one will decide that they shall remain with their present names. If so, why not make the nomenclature of all other articles of chymical-inorganic origin conform to that of these articles? All previous nomenclatures had certainly become inadequate to the exigencies of a department of knowledge so rapidly progressive as chemistry; and shall we retain in medicine, a chymical language, that has been wholly abandoned by all scientific chemists? There was certainly no deficiency of talent and ingenuity exercised in the formation of the nomenclature in question, as has been subsequently evinced by the perfection with which it has answered its purpose whenever and wherever it has been employed. From the time that I

first became a public instructor in medicine, my attention was particularly turned to the subject of the best nomenclature of the materia medica; and I endeavored to ascertain what one was in the most common use. The result of my inquiries was, that no author followed any one nomenclature whatever, unless it was this very nomenclature of chemistry, and that of natural history; but their language was made-up of parts of half a dozen different nomenclatures. As to the language of practitioners of medicine, I never met with one whose nomenclature was not a perfect hotch-potch of bad Latin, bad French and bad English, being attempts at names from every nomenclature of materia medica, that has ever existed, at least with Britons and U. S. men. I have often been highly amused at the attempts to use French nomenclature, often without any knowledge that it was an attempt at French.

It seems to be a prevalent opinion among physicians that unless they employ the medley of names, that I have described, they can not possibly be intelligible; and I have repeatedly known men who had studied chemistry thoroughly before they studied medicine, and at a time when the regular nomenclature was in universal use, bestow far more labor upon the acquisition of this jargon, than a thorough knowledge of the regular nomenclature had ever cost them; and after all, they found that the jargon in question was not the same, in any two different places. I have repeatedly known medical students refuse to study the regular chymical nomenclature at all, because they believed they should not be understood if they employed it; and instead of such study, I have known them to attend an apothecary's shop for six, nine or twelve months, to become acquainted with the labels merely. After getting through with this apprenticeship, they knew what appearance *Fol. Anthos* had to the eye, but they had no knowledge that this substance consisted of the leaves of *Rosmarinus officinalis*; they knew how *Rad. Saffron* appeared to the eye, but they had no notion whatever that this substance is the unpulverized root of *Cephaëlis Ipecacuanha*; they knew the appearance of *Ol. Coni*, but were ignorant that it was the Terebinthinate Essential Oil of *Abies Canadensis*; they knew the appearance of *Ol. Pyrolæ*, but they had no knowledge that this substance is a true Saline Æther, the Spirhylate or Oxy-spirhylate of Protoxyd of Methygen existing naturally in the plant *Gaultheria procumbens*; they learned that what is called *Precip. Alb.* is a white powder, but never discovered that four different powders of quite different composition are called by this name, etc. Such being the fact I shall be perfectly contented, and receive it as a compliment besides, to be called a pedant, for using a nomenclature of the chymical inorganic articles, in conformity with the principles of Guiton de Morveau, Lavoisier, Bertholet and Fourcroy; and for using the latest and most improved nomenclature of natural history for vegetable and animal-organic articles. If any one educated to medicine can not understand or find-out this, as appears to me, the less he says about it the better for his own reputation.

I do not purpose, in this place, to explain the laws of chymical nomenclature any further than is barely necessary for the illustration of the names which I shall have occasion to use; and I do not do this because I have any

new laws to give, but because the study of the long established laws has been so much neglected of late, and because so many names have been formed in direct contravention of these laws, when a name in conformity with them would have been so much preferable. It is my intention that the names which I employ, shall be in conformity with these laws. My explanation of the laws in question will be more brief, and perhaps less plain, in consequence of many omissions, than if I were writing exclusively upon the subject of the nomenclature of chemistry.

In my catalogue of articles belonging to each class, I shall use a name for all the elements and compound radicals terminating in *um* or *ium*, as euphony may require, which is according to the law of the nomenclature upon this point. Oxygenium, Sulphurum, Phosphorum, Hydrargyrum, Argentum, Aurum, may be taken as examples of the names of elements in conformity with the law of the nomenclature of chemistry. There are three inorganic compound radicals that consist of only two elements which not only have names terminating in *um* or *ium*, but more than this, terminating in *genium* in Latin and *gen* merely in English. The first of these is $C^2 N^1$ which is called Cyanogenium in Latin and Cyanogen in English, i. e. the blue-producer. Cyanogen is both a basifying and acidifying compound radical. Its bases are therefore called Cyanids, and its acids Cyanacids. Cyanogen is also basifiable and acidifiable. Cyanid of Potassium, Cyanid of Sodium, Cyanid of Magnesium, etc. are examples of basic compounds produced by Cyanogen. In what is popularly called Prussian Blue, the Sesquicyanid of Iron performs the functions of a salifiable base, while the Sesquoxyd of Iron performs the functions of an Acid, this salt being a Diferrite or Disoxyferrite of Sesquicyanid of Iron, there being two equivalents of base to one of Acid in this Salt, so that the original Prussic Acid was Sesquoxyd of Iron. Cyano-ferric Acid, Cyanoferrous Acid, Cyanohydrargyric Acid, etc., are examples of Acid or Salifying compounds produced by Cyanogen. I have no knowledge of any case in which the Cyanid of Hydrogen performs the function of an Acid. There is a compound of one equivalent of Protocyanid of Iron $(Fe^1 Cy^1)$ with two equivalents of Protocyanid of Hydrogen $2(H^1 Cy^1)$ and this has commonly been supposed to be a Bicyanohydrate of Protocyanid of Iron. But Protocyanid of Iron is electro-negative in comparison with Protocyanid of Hydrogen, and consequently the former is the Acid and the latter is the Base of this Salt. Thus all the supposed cases, within my knowledge, of Protocyanid of Hydrogen's performing the functions of an Acid vanish under examination.

The second inorganic compound radical consisting of only two elements, and having a name terminating not only in *um* but in *genium* Latin, and *gen* in English is $H^2 N^1$ and is named Ammidogenium or Ammidogen.—Ammidogen means the Ammid producer. In analogy with this, Cyanogen should have been Cyanidogen, the Cyanid producer. Oxygen should have been Oxydogen, the Oxyd producer, etc. But if Oxygen the Acid producer, Cyanogen the Blue producer, etc. are correct, Ammidogen should have been Ammonigen the Ammonia or rather the Ammonium producer, since one

equivalent of Ammidogen united to one equivalent of Hydrogen constitutes Ammonia (incorrectly so called) or Ammid of Hydrogen as the nomenclature of chemistry now requires it to be called. The term Ammonia, I repeat, is very incorrectly applied to Ammid of Hydrogen ($H^1 + H^2 N^1$). It really belongs to Oxyd of Ammonium ($H^4 N^1 + O^1$). The termination *a* or *ia* denotes basification by Oxygen as Potassium Potassa; Sodium Soda; Lithium Lithia; Barytum Baryta; Strontium Strontia; Calcium Calcia; Magnesium Magnesia; Aluminum Alumina, etc., in all of which cases the word terminating in *a* or *ia* denotes a basic Oxyd of the element whose name immediately precedes and terminates in *um* or *ium*. Ammidogen is ordinarily a basifying compound radical, though sometimes it performs the functions of a salifiable base. In the last case I think it is not to be considered as Ammidogen, but as Hydrogen basified by Nitrogen. I do not now recollect any case, in which Ammidogen is an acidifying compound radical, though I have a vague impression that I have met with one instance at least, which has now escaped from my memory, but might be found by a little pains. If there is in fact one instance, there are doubtless more, and probably a considerable number. I think there are a few examples in which Ammidogen is not only basifiable, but acidifiable, but having omitted to record them they have escaped my recollection. In the following case it is either basifiable or acidifiable and most probably both, since similar compounds are usually such. Thus $H^2 N^1 + H^5 C^14 O^2$ is true Benzhylid of Ammidogen, and not an Ammid of Benzhyde, as is commonly supposed, because the Ammidogen is certainly the electro-positive proximate principle of the compound.

There is another inorganic compound radical cognate with Ammidogen, and composed of the same elements, though in different proportions, and having analogies with the compound radicals of H. C. N. O. which bears an abnormal name, and of course requires express specification in this place. I here refer to $H^4 N^1$ commonly called Ammonium. Ammonium is ordinarily a basifiable compound radical; but it is sometimes acidifiable. In the salt $Mg^1 Cl^1 + H^4 N^1 Cl^1$ the Chlorid of Ammonium is certainly the electro-negative proximate principle, and consequently the Acid, the Salt being a Chlorammoniate of Chlorid of Magnesium. Magnesium is very certainly electro-positive not only in regard to Nitrogen, but also in regard to Hydrogen, and therefore Chlorid of Magnesium must be electro-positive in regard to Chlorid of Ammonium.

I doubt not that Bromid, Iodid, Sulphid, etc. of Ammonium, would all be electro-negative in comparison with the Chlorids, Bromids, Iodids, Sulphids, etc. of Potassium, Sodium, Lithium, Barytum, Strontium, Calcium, Magnesium, etc. so that we should have Chlorammonic, Bromammonic, Iodammonic, Sulphammonic, etc. Acids and doubtless a number of others. We may well say therefore that Ammonium is as much acidifiable as basifiable. The facts that Ammonium is both acidifiable and basifiable, and that it may be transferred without decomposition from one compound to another, sufficiently proves that it is a true compound radical, and not a Disammid of Hydrogen, so far as respects its chemical habitudes and relations. The compound radical Ammo

nium has considerable analogy with the compound radicals of H. C. N. O.

1. It differs in composition from the Alkali, or (as I prefer to call it) Alcaloid Ammonia, only in containing one more equivalent of Hydrogen, just as the compound radicals of H. C. N. O. differ from the Alcaloids, with which they are cognate.
2. The Alkali or Alcaloid with which it is cognate, contains only a single equivalent of Nitrogen, and this seems to be essential to its Alcalinity; and certainly the compound radical Ammonium contains only one equivalent of Nitrogen. This is equally true of the vegetable-organic Alcaloids and the compound radicals of H. C. N. O. which are cognate with them.
3. The Alkali, or Alcaloid with which it is cognate, is not a salifiable base, except rarely in the capacity of Ammid of Hydrogen (or rather Hydrogen basified by Nitrogen) which is equally the fact with the vegetable-organic Alcaloids.
4. It is both basifiable and acidifiable. The respects in which Ammonium differs from the compound radicals of H. C. N. O. are:
 1. It is constituted of only two elements instead of four.
 2. It is not always a vegetable-organic product.* There should certainly have been some peculiarity in the termination of the name of H! N! to distinguish it from a mere element, from other genera of compound radicals, and to mark it as a peculiar genus of itself. There should have been some peculiarity also in the body of the name, as in the cases of other compound radicals. If Ammonium were always a regular ingredient of some living vegetable i. e. a proximate principle, a name might have been formed from the name of such plant, with an appropriate and peculiar termination. Perhaps the name *Chenopodium* or *Caroxylum* might have formed a good basis for a more appropriate appellation for this compound radical; and from its analogy with the compound radicals of H. C. N. O. perhaps the termination *inum* might have been as much to be tolerated as the termination *genium* or *gen* for compound radicals of C. N. and H. N. and H. C. But it is not now probable that any change will ever be made, and we can certainly do very well without any. But for the vegetable-organic compound radicals of two genera at least, something more is necessary to the name than a termination in *um* or *ium*. According to the laws of the nomenclature of chemistry, the names of compound radicals of H. C. must be made-up either of the generic or the trivial name of the plant producing the compound radical named, with its termination changed to *genium* in Latin or *gen* in English. Unless the trivial name of the plant happens to be a substantive however, it can not enter into the composition of the name of a compound radical. Sometimes the generic or trivial name of the plant is more or less abridged in the name of the compound radical; but for such abridgment there is no rule, and in my opinion it is always best not to

* *Chenopodium Vulvaria* (Linnæus) and *Caroxylum fœtidum* (De Candolle) in their living and healthy state are said to exhale gaseous Ammid of Hydrogen commonly called Ammonia. Does this exist within the plant, or is it formed by the decomposition and recomposition of other compounds that are excreted simultaneously? Is it formed by the union of excreted elements presented to each other in a nascent state?

make it. The first compound radical of its genus that was named according to this law was $H^8 C^{10}$. This exists naturally in *Camphora officinarum* (Nees) or *Laurus Camphora* (Linnæus). The regular name would have been *Camphorogenium* (Latin) or *Camphorogen* (English); but it was needlessly abridged to *Camphogenium* or *Camphogen*. In popular language this term has been still further abridged. In consequence of writing *Camphⁿ* instead of *Camphogen*, the term was first read into *Camphen* and then lengthened into *Camphene*, in order to conform it to *Morphene*, *Strychnene*, *Kenene*, the affected pronunciation of those who endeavor to conform Latin and English to French, and that in utter ignorance of genuine and proper French. *Camphogenium* or *Camphogen* is then the proper chymical name of the compound radical $H^8 C^{10}$ which, in common language, is *Terebinthinate Essential Oil of Turpentine*; and all other liquid compound radicals (and perhaps gaseous and solid ones) of $H. C.$ should be named in analogy with this. I think that the term *Essential Oil* should be applied only to liquid compound radicals of $H. C.$ but *Essential Oil* is a name not belonging to the nomenclature of chemistry. Examples of other names according to this law are *Formicigenium* or *Formicigen* for $H^1 C^2$ commonly called *Essential Oil of Storax*. Perhaps, however, the compound radical called *Formicigenium* or *Formicigen* differs from the compound radical called *Styracigenium* or *Styracigen*, notwithstanding they consist of the same elements, in the same proportion, and even in the same number of equivalents. If they differ, it will doubtless be found that they have different volumes, for with difference of volume, there are different external sensible properties, e.g. *Limonigenium* or *Limonigen* for $H^4 C^5$ commonly called *Essential Oil of Lemon* and *Elemigenium* or *Elemigen* for $H^4 C^5$ commonly called *Essential Oil of Elemi*. It will be observed that this compound radical consists not only of the same elements, in the same proportion, but even of the same number of equivalents as the *Essential Oil of Lemon*. If there is no mistake about the composition, and if in fact they are not identical substances, they probably have different volumes; for a particular volume is always essential to these compounds. Other examples are *Asarogenium* or *Asarogen* for $H^4 ?^5 ?^1 C^1$ commonly called *Essential Oil of Asarum*, *Menthagenium* or *Menthagen* for $H^{10} C^{10}$ commonly called *Essential Oil of Peppermint*, *Caryophyllogenum* or *Caryophyllogen* for $H^{18} C^{20}$ commonly called *Essential Oil of Cloves*, and *Cubebogenium* or *Cubebogen* for $H^{14} C^{16}$ commonly called *Essential Oil of Cubebs*. It is therefore the easiest thing in the world to name a new compound radical of $H. C.$ according to the law of the nomenclature of chemistry, since such name should always consist of the generic name of the plant producing it, or some times the trivial name, under certain circumstances, with its termination changed to *genium* Latin, and *gen* English. It is somewhat of an objection to the termination *genium* or *gen* for the designation of this genus of compound radicals, that there are three elements whose names have the same termination; but if the rest of the names are considered, this objection is very nearly obviated, if not quite so. Again, it is somewhat of an objection to the termination *genium* or *gen* for the designation of this genus

of compound radicals, that there are two inorganic compound radicals, of essentially different composition, whose names have the same termination ; but here again, if the names are considered, the objection is likewise very nearly obviated. The compound radicals of H. C. are never basifying and acidifying, but probably always basifiable and acidifiable. The number of compound radicals of H. C. is very great ; and probably the whole of them combine with the basifying and acidifying elements or compound radicals forming both salifiable bases and salifying compounds, which it would be out of place for me to treat of, in this connexion. In a full account of the nomenclature of chimistery, much more would be required upon this subject ; but what I have just said will be sufficient for my present purpose.

The compound term Essential Oil is used in chimistery and pharmacy with almost as much latitude as the terms stimulant and excitant in the *materia medica*. The liquid compound radicals of H. C. are called Essential Oils. The first group of *Ætheres*, viz., those which are composed of compound radicals of H. C. united with such a small number of equivalents of one of the basifying and acidifying elements, that they are not at all sour to the taste, whether they happen to be salifying or not, are commonly called Essential Oils, though with exceptions. Many of the *Camphoræ* are called Concrete Essential Oils. Many anomalous compounds not belonging to any group of analogous substances are called Essential Oils ; but these cannot be specified in this place. As I shall have occasion very often to mention Essential Oils, and perhaps to treat of several quite particularly, I think it may be well to say in this place that I shall not use this term with all its vagueness, looseness and latitude. In my opinion, this compound appellation, if indeed it is to be retained in chimistery, and is to be considered as a part of the chymical nomenclature, should be limited and confined to liquid compound radicals of H. C. But as so few of this class of bodies in a state of purity have ever been analyzed, and as the impurities always contain Oxygen, it is impossible in the present state of our knowledge to determine when the presence of this element is essential, and when it is accidental. Besides, most, if not all the liquid compound radicals of H. C. have a strong affinity for Oxygen, and when exposed to the influence of the atmosphere slowly and gradually absorb and combine with it, forming *Camphoræ* or *Ætheres*, or Acids containing still more Oxygen. For example *Camphogenium* or Oil of Turpentine ($H^8 C^{10}$) by exposure to the influence of the atmosphere, soon absorbs and combines with Oxygen, forming a *Camphora*, Oxyd of *Camphogen* or *Camphogenia* ($H^8 C^{10} + O^1$) which is common Camphor, a compound that is ordinarily basic in relation to strong Acids, though I venture to say that it is salifying in relation to certain bases. By still further exposure to the same influence, it absorbs still more Oxygen, and forms what is called *Camphoric Acid 2* ($H^8 C^{10} + O^2$) so that unless measures are adopted immediately before analysis, to free Camphogen or Oil of Turpentine from the Camphor and the Camphoric Acid which it contains, Oxygen will always be found in it. The same seems to be equally true of all the other Essential Oils, that are compound radicals of H. C. so that the find-

ing of Oxygen in an Essential Oil (so called) affords scarcely any evidence that Oxygen is essential to such Oil, but it rather indicates that it is contaminated with a Camphor or an Acid of its compound radical H. C. or that it is a compound analogous to Benzhylid of Hydrogen, Spirhylid of Hydrogen, Cinnamomhylid of Hydrogen, etc. I doubt whether any Oxydized compound radical of H. C. can fairly and properly be considered as an Essential Oil in contradistinction from *Ætheres* and *Camphoræ*; and if we call all liquid compound radicals of H. C. all *Ætheres* of the first group, i. e. all liquid Oxydized compound radicals of H. C. all *Camphoræ*, i. e. solid Oxydized compound radicals when they appear to be held in solution by a liquid compound radical of H. C. all compounds of Hydrogen with a compound radical of H. C. O. Essential Oils, we make this term so comprehensive, that it scarcely answers the purpose of a name. I can not discover why each of these groups of compounds does not require a distinguishing appellation as much as any other group of compounds that are the subjects of the science of chemistry. What I have just said requires a definition of an *Æther* and of a *Camphor*. An *Æther* generically is a more or less volatile, odorous, pungent and inflammable liquid, consisting of any compound radical of H. C. united with so small a number of equivalents of any one of the eleven basifying and acidifying elements, that it is not an Acid; or, it is any liquid compound of any one of the *Ætheres* above described with any Acid. A *Camphor* generically, is any more or less volatile, odorous, pungent and inflammable solid, made-up of a compound radical of H. C. united with such a number of equivalents of any one of the eleven basifying and acidifying elements that it is not an Acid; or, it is any solid compound of any one of the *Camphors* above described with any Acid. The only difference between one group of the *Ætheres* as a genus, and the *Camphoræ* as a genus, is the fact that the *Ætheres* referred to are liquid, while the *Camphoræ* are solid. The same definition therefore is applicable to both, with the variations which liquidity and solidity necessarily require. Mutato mutando either of the preceding is a definition of the other. All the known liquid compound radicals of H. C. beside many that have never been insulated, form different specific *Ætheres* or *Camphoræ*, so that *Camphora* is as much a genus as *Æther*, though there can hardly be said to be more than one group of *Camphoræ*, while there are several distinct groups of *Ætheres*.

According to the laws of the nomenclature of chemistry, the names of compound radicals of H. C. O. must be made-up of the generic or trivial name (or at least the root of such name) of the plant producing the compound radical, with the suffix or termination *hylum* in Latin or *hyle* in English. Unless the trivial name however is a substantive, it can not enter into the name of the compound radical. It is not a little remarkable that there should have been so much diversity among chimists, as respects the Roman letters into which the Greek $\upsilon\lambda\eta$ has been translated. I never could see how it is possible to consider any other Roman letters as equivalents of the Greek letters of which the word is made-up than *hyle*; and yet I have now books before me in which it is written *oyl*, *oyle*, *oil*, *oile*, *ul*, *ule* and *yle*. Now such

gross carelessness is certainly very censurable; and if it were as great in regard to terms in general would reduce language to a perfect chaos. The first compound radical named according to this law was $H^5 C^{14} O^2$. This was obtained from the resin of *Styrax Benzoin* (Dryander). It was called *Benzhylium* in Latin and *Benzhyle* in English. Only a moderate number of compound radicals of $H. C. O.$ are now known at all; and the number is very small in comparison with the number of compound radicals of $H. C.$ I am not apprized that more than about half a dozen are well known. It seems however to be the tendency of the chymical authors of the present time to apply this set of names to compound radicals of $H. C.$ as well as to those consisting of $H. C. O.$ but this is a great error. It is certainly very convenient to have terms that will distinguish two such different groups of compounds; and so long as the laws of the nomenclature have made provision for such terms, they should certainly be applied with discrimination and correctness. In this connexion, it is proper for me to say barely, that this genus of compound radicals is not only basifiable and acidifiable, but also basifying and acidifying. Many of the compounds of compound radicals of $H. C. O.$ with the basifying and acidifying elements, are basic in relation to certain Acids, and salifying or Acid in relation to certain bases. Compounds of $H. + H. C. O.$ I believe, are always both basic and Acid—basic in relation to certain Acids, and salifying or Acid in relation to certain bases; and yet, notwithstanding this, these compounds are always reckoned and called Essential Oils.

Benzhylid of Hydrogen is supposed to be a highly active substance; and it is considered as being active in a manner quite different from any other compound of *Benzhyle*. Is this equally true of *Spirhylid* of Hydrogen, *Cinnamomhylid* of Hydrogen, and all other analogous compounds? This question I am unable to answer. Other examples beside that already given, of compound radicals of $H. C. O.$ are *Spirhylium* or *Spirhyle*, consisting of $H^5 C^{14} O^4$. This is sometimes called *Salighylium* or *Salighyle*, but with no degree of propriety. The compound radical of $H^5 C^{14} O^4$ was first detected in the flowers of *Spiræa Ulmaria* (Linnæus) and therefore, according to the law of chymical nomenclature for compound radicals of this genus, it was called by an appellation made-up of the root of the natural history name of the plant affording it, and the suffix *hylum* from $\nu\lambda\eta$ (hyle) the material of any thing. In the flowers of *Spiræa Ulmaria*, *Spirhyle* is found in combination with Hydrogen, constituting *Spirhylid* of Hydrogen or *Spirhylohydric Acid*; for this substance is both a salifiable base and a salifying compound. *Spirhylium* or *Spirhyle* was therefore the first name imposed on this compound radical. As this substance was first discovered in *Spiræa Ulmaria*, it was rightly named from this plant. Again, as this substance exists in a more simple state of combination in this plant, than in *Gaultheria procumbens*, it is more proper to name it after *Spiræa*, than after *Gaultheria*, on this ground. Subsequently *Spirhyle* was detected in *Gaultheria procumbens* (Linnæus) in more complex combinations, than in *Spiræa Ulmaria*. In *Gaultheria procumbens* the *Spirhyle* is first combined with Oxygen and thereby

converted into Spirhylic or Oxyspirhylic Acid, and this is combined with Protoxyd of Methygen, so as to constitute a Spirhylate of Protoxyd of Methygen, which is commonly but erroneously called Oil of Gaultheria. But Gaultherylium or Gaultherhyle would have been a very appropriate and less unexceptionable name, if a more appropriate and less exceptionable one had not been previously imposed. The mere fact that the substance, very incorrectly called Salicina or Salicine may be factitiously decomposed and resolved into several new substances, one of which is Spirhyle, and none of which have any analogy with the so called Salicine, either in composition, external sensible properties, or in occult and medicinal properties, neither affords any good reason for changing a good name for a bad one, nor for giving it a name derived from *Salix*, a Willow from which the term Salicine was formed, though incorrectly, for the name of the substance to which it is applied.

Cinnamomhylium or Cinnamomhyle, consisting of $H^7 C^{18} O^2$ is another example of one of these compound radicals, which is perfectly analogous to Benzhyle and Spirhyle. By some authors Kinhylium or Kinhyle $H^2 C^3 O^8$ is mentioned; but with this I am not acquainted. By others Quinhylium or Quinhyle $H^2 C^{12} O^4$ is mentioned. Neither am I acquainted with this. By others still, Camphorhylium or Camphorhyle $H^{14} C^{20} O^1$ is mentioned. This I know nothing of. Cyanhylium or Cyanhyle is sometimes mentioned; but I do not recollect what is given as its composition, nor can I at present refer to any author who treats of it. I might specify several other compounds of this sort; but what I have already given are sufficient for examples. I consider it as altogether probable that many of the supposed Essential Oils, to whose composition Oxygen is considered as necessary, are compounds analogous to Benzhylid of Hydrogen, Spirhylid of Hydrogen, Cinnamomhylid of Hydrogen, etc. or in other terms Benzhylohydric Acid, Spirhylohydric Acid, Cinnamomhylohydric Acid, etc. for all these compounds perform the functions both of salifiable bases, and salifying principles or Acids.

There are a great number of compound radicals of H. C. N. though the proportion that has been insulated is small. So far as I know, the laws of the nomenclature of chemistry have made no provision for regular distinctive names for this genus of compound radicals. So far as I know, the first that was recognized in an insulated state was $H^5 C^{15} N^1$. As this, in combination with O^2 produced the Alcaloid Indigotina, and as it existed in several species of Indigofera, it seems to have been named Indigogenium in Latin, and Indigogen in English, without regard to any other employment of the termination *genium* or *gen*. It would doubtless have been better to have named it Indigothylum, or Indigothyle, since I think that there are certainly compound radicals of H. C. with each of the eleven basifying and acidifying elements; and if so, they may all with great propriety be named upon one common plan, or according to the same method. As there is only a single name to be changed, I hope that this will yet be done, and that the analogous unnamed bodies will have names imposed in conformity with this suggestion. I have a small list of this sort of compound radicals, that have

been insulated now before me, and it strikes me as not a little singular that the quantity of Oxygen necessary to convert each of them into an Alcaloid, has been ascertained only in the single instance, which I have just specified. Nobody seems to have been particularly interested in this subject. There is well known to be a large number, that have never been insulated, and which are known only as Alcaloids, of which the quantity of Oxygen necessary for their Alcalization is perfectly ascertained.

Alcaloids being once supposed to be basic compounds (though this is now considered to be an error) are called by a name made-up of either the generic or trivial name of the plant producing them, with its termination changed to *ina* often injudiciously abridged in English to *ine*. This termination is required for two purposes; as first to distinguish the name of the Alcaloid from the plant which produces it, as Sanguinarina from Sanguinaria, and second to distinguish Alcaloids from basic compounds that contain no Nitrogen, but consist intirely of H. C. + O. not being compound radicals of H. C. O. Such bases have their names terminating in *a* or *ia* simply. The Alcaloids always consist of a compound radical of H. C. N. + O. or in other words H. C. N. Alcalified or Alcalized by O. It has recently been the fashion with some to leave out the letter *n* in the termination of the names of the Alcaloids, and to name them exactly like the basic compounds consisting of H. C. + O. but I think it is quite important that those two classes of bodies should be distinguished by their names, since their composition differs so essentially, and their functions still more, the Alcaloids not being salifiable bases, while the other class of compounds is always essentially such, from the very terms of their definition. Surely salifiable bases, of H. C. + O. should be distinguished in name from compounds of H. C. N. + O. which are never salifiable bases. But omitting the *n* in the termination of the name of the Alcaloid, very often renders the name of such Alkaloid identical with the name of the plant from which it is derived, a thing certainly not to be tolerated in nomenclature, when the Alcaloids are so much more active than the crude plants affording them. Esenbeckina from Esenbeckia; Sanguinarina from Sanguinaria; are examples where sinking the *n* from the name of the Alcaloid, makes it identical with the name of the plant affording it. This catalogue might easily be much extended, but two examples are as good for illustration as two hundred. Again, there are a great number of cases in which the omission of the *n* from the name of the Alcaloid, though it does not make the name of the Alcaloid perfectly identical with that of the plant affording the Alcaloid, yet it makes it so nearly such, as to render such omission of the *n* perfectly inadmissable. The following are examples of this sort, viz. Daturina from Datura; Atropina from Atropa; Cinchonina from Cinchona; Sabadillina from Sabadilla; Violina from Viola; Vomica from Vomica; Brucina from Brucea; Quina from Quina; Currarina from Curra-
ra; Thetna from Thea; Hurina from Hura; etc. Now Daturia is much too near Datura; Atropia much too near Atropa; Cinchonia much too near Cinchona; Sabadillia much too near Sabadilla; Viola much too near Viola; Vomicia much too near Vomica; Brucia much too near Brucea; Quinia

much too near Quina; Curraria much too near Currara; Thera much too near Thea; Huria much too near Hura; etc. Indeed the great body both of physicians and apothecaries, in reading such names, would never notice the difference between the names of the Alcaloids and the names of the plants from which they are derived. I have heard the names to which I am objecting, when used, misread ten times to where I have heard them correctly read once. In fact scarcely any apothecaries, and very few physicians know the difference in the import of Potassium and Potassa; Sodium and Soda; Barytum and Baryta; Calcium and Calcia; Magnesium and Magnesia; Aluminum and Alumina; etc. Every day of my life I hear these confounded and used one for the other, not only by apothecaries but by physicians. Medical gentlemen have sometimes remarked to me that I appeared to employ the terms Cinchoninum and Cinchonina; Quininum and Quinina; Morphinum and Morphina; etc. as if there was a difference between them, and have seriously inquired if such was the fact. I have often been troubled to convince such persons that there could possibly be any difference between Chlorid, Bromid, Iodid, Sulphid, etc. of Cinchoninum, Quininum, Aricinum, etc. and Chlorid, Bromid, Iodid, Sulphid, etc. of Cinchonina, Quinina, Aricina etc. even if the latter compounds existed or were capable of existence.—Such being the fact, I think it highly important that very different compounds should not be called by the same sort of names; and that distinctive names should not be changed so as to resemble each other so nearly as to be likely to be confounded. I do not think that the utterance of the *n* in the name of the Alcaloids will be at all likely to shorten any man's life, or the omission of it to lengthen it. In view of a part only of these reasons, Dr. Thomas Thomson has decided to retain the intire termination *ina* in the names of the Alcaloids, instead of Anglifying them by a change to *ine*; and accordingly, in his great work on chimistery, he writes Cinchonina, Quinina Aricina, Morphina, Strychnina, Vomicina or Brucina, Veratrina, etc. instead of Cinchotine Quinine, Aricine Morphine, Strychnine, Vomicine or Brucine, Veratrine, etc. and in this, I think he ought to be followed, instead of changing these names so that they can scarcely be distinguished from the names of the plants which produce them.

According to the laws of the nomenclature of chimistery, the names of compound radicals of H. C. N. O. take the name of the Alcaloid with which they are cognate, with the change of the termination *ina* in the name of the Alcaloid to *inum* in the compound radical. It is true that the names of about half a dozen of the elements terminate in *inum*; but this is scarcely an objection to this termination for the distinguishing names of this genus of compound radicals, when we consider that it does not constitute the sole peculiarities of the names, and that the remainder of the names is full as distinctive, as the termination. This set of compound radicals is both basifiable and acidifiable. All the supposed salts of the Alcaloids, are in fact salts of the Oxyds of these compound radicals. I have no actual knowledge that the Oxyds of these compound radicals ever perform the functions of Acids, though I think it not improbable that they may do so, in conjunction

with the Oxyds of the elements near the electro-positive extremity of the scale. The Chlorids, the Bromids, the Iodids, the Sulphids, etc. of these compound radicals, are certainly capable of combining with the Chlorids, the Bromids, the Iodids, the Sulphids, etc. of a considerable number of the elements near the electro positive extremity of the scale, and thus forming Salts with them, of which the Chlorids, Bromids, Iodids, Sulphids, etc. of these compound radicals are certainly the electro-negative proximate principle, and consequently the Acid. I have employed in medicine the Iodoquininate of Iodid of Potassium, and it would be a peculiarly valuable medicine were it not for the exhausting effect of the Potassium in the compound.

For binary compounds of the eleven basifying and acidifying elements or compound radicals, the termination of the names of the former will be changed into *ydum* or *idum* in Latin, and *yd* or *id* in English, and the names of the latter in the genitive case will precede in Latin, and succede in English preceded by the preposition *of*. Oxygen always requires *ydum* or *yd*; but all the other basifying and acidifying elements require *idum* or *id*. The non-acid compounds of the eleven basifying and acidifying elements are therefore, in English Oxyds, Fluorids, Chlorids, Bromids, Iodids, Sulphids, Selenids, Telurids, Nitrogids, Phosphids and Arsenids. The names of the basifying compound radicals are changed in their termination, in the same manner, in the names of the salifiable bases which they form. I have already had occasion to mention Ammids, Cyanids, Benzhylyds, Spirhylyds, Cinnamomhylyds, etc. which are salifiable bases, the electro-positive element or compound radical of which is rendered basic by combination with Ammidogen, Cyanogen, Benzhylye, Spirhylye, Cinnamomhylye, etc.

The number of equivalents of a basifying and acidifying element or compound radical in a basic compound, when equal with, or predominating over those of the basifiable element, or compound radical, is indicated by prefixing to the name of the former an abbreviation of one of the Greek numerals, as *prot.* before a vowel and *proto.* before a consonant, *deut.* or *deuto.*—*trit.* or *trito.*—*tetart.* or *te arto.*—*pempt.* or *pempto.*—*hect.* or *hecto.*—*hebd.* or *hebd.*—*ogd.* or *ogdo.*—*ennat.* or *ennato.*—*decat.* or *decat.*—etc. When this is to be expressed in the name of a salt, these same numerals are to be prefixed to that part of the name which is derived from the salifying principle or Acid. When, in a non-acid compound of a basifying and acidifying element or compound radical with a basifiable or acidifiable element or compound radical, there is no Greek ordinal numeral prefixed, it is to be understood either that the number of equivalents which enter into its composition is unascertained or unknown by the writer or speaker; or that it is constituted of one equivalent of each element or compound radical. In strict propriety, the numeral should never be omitted when the number is known, that we may distinguish the cases in which it is unknown, from those in which it is a unity. If this had not been the plan and intention of the formers of the nomenclature, doubtless they would not have employed the numeral *protos* at all.

Sulphurum is one of the eleven basifying and acidifying elements; and it is most probable that all its binary compounds, either with other elements, or with compound radicals, are either Acids (not necessarily sour by any means) or Bases. Now there are five mere binary compounds of Sulphurum with Kalium or Potassium, and all most likely basic compounds. According therefore to the laws of the nomenclature of chemistry, the names of these compounds must be made-up respectively of the name of their electro-positive proximate principle in the genitive case, followed by the name of the electro-negative proximate principle with its termination changed to *idum*, and with an abbreviation of one of the Greek ordinal numerals prefixed, expressive of the number of equivalents of Sulphurum in the compound. This is converted into English by abbreviating the name of the electro-negative ingredient from the termination *idum* to *id*, and having this followed by the name of the electro-positive ingredient in the nominative case but preceded by the preposition *of*. According to these principles, the names of these five compounds should stand as follows, viz.

1. POTASSII PROTOSULPHIDUM;
2. POTASSII DEUTOSULPHIDUM;
3. POTASSII TRITOSULPHIDUM;
4. POTASSII TETARTOSULPHIDUM;
5. POTASSII PEMPTOSULPHIDUM;

or in the English of the nomenclature,

1. PROTOSULPHID OF POTASSIUM;
2. DEUTOSULPHID OF POTASSIUM;
3. TRITOSULPHID OF POTASSIUM;
4. TETARTOSULPHID OF POTASSIUM;
5. PEMPTOSULPHID OF POTASSIUM;

And yet the names of these five compounds stand as here follow, in a book now before me, prepared for the text-book of a public teacher of chemistry in a distinguished institution, viz.

- PROTOSULPHURET OF POTASSIUM;
 BISULPHURET OF POTASSIUM;
 TERSULPHURET OF POTASSIUM;
 QUADROSULPHURET OF POTASSIUM;
 QUINQUESULPHURET OF POTASSIUM;

Now the termination *uretum* Latin, *uret* English, belongs only to a non-basifying and non-acidifying electro-negative ingredient of a binary compound, as Ferri Carburetum or Carburet of Iron, etc. The prefix *bini*, or its abbreviation *bi*, is not one of the Greek ordinals, which are required here, but is a Latin distributive. If the numerals intended to indicate the number of the equivalents of the electro-negative ingredient of a basifying compound, are confounded with those intended to denote the number of equivalents of the Acid in a Salt, both sets of numerals become useless; but if their proper applications are carefully discriminated and accurately

observed, they can be never misunderstood, but will always convey precise, definite and important information. The prefix *ter* is neither a Greek ordinal, nor a Latin distributive, but belongs to a different set of numerals, viz. the Latin multiplicatives. It is therefore intirely out of place here.—Indeed, this set of numerals is not at all employed in chimical nomenclature. In the next prefix, viz. *quadro*, there are two errors or mistakes. The Latin word *quadrus* is not truly a numeral, but an attribute properly implying square; and besides this, *o* is not the combining termination in Latin. We find *i*, so far as I know, in all cases, except about two, the combining termination in this language, as *barbiger*, *armiger*, *securiger*, etc. It will be observed that the former part of these three words belong to three declensions. *Fructifer* belongs to still another declension, and if I were to search for them, I doubt not I should find examples from the only other declension. *Aqueductus* is one of the exceptions to the general rule; but the other I cannot think-of, at present. *O* is the combining termination in Greek, but not in Latin. *Quinque* the last prefix in the example quoted, instead of being a Greek ordinal, as it should be, is a Latin cardinal, a set of numbers not employed in the nomenclature of chimistry. I can not discover how there could have been more than one other mistake, since there is only one prefix that is now correct, viz. the first.

When a basic compound of a basifying and acidifying element or compound radical, with a basifiable element or compound radical, is in the proportion of three equivalents of the former to two equivalents of the latter, this proportion is indicated by the prefix *sesq.* before a vowel, and *sesqui* before a consonant, to the modified name of the basifying and acidifying element or compound radical, this prefix should have been *duplisesq.* or *duplisesqui*; and the reason why it is not, will be explained hereafter, when I mention the application of this same prefix to a different case. When, in a basic compound, the number of equivalents of the basifiable element or compound radical predominates over those of the basifying and acidifying element or compound radical, this is indicated by prefixing to the name of the latter one of the Greek multiplicative numerals, as *dis* before a vowel, and *di* before a consonant, *tris* or *tri.*—*tetrac.* or *tetraci.*—*pentac.* or *pentaci.*—*hexac.* or *hexaci.*—*heptac.* or *heptaci.*—*octac.* or *octaci.*—*enneac.* or *enneaci.*—*decac.* or *decaci.* etc. In the name of a Salt these numerals keep this same place, because they are employed also to indicate the number of equivalents of the electro-positive proximate principle or base of the Salt, when that predominates over the electro-negative proximate principle or acid; and there are no means of distinction between these two uses, except the position of the numeral. In the latter case it must be prefixed to that part of the name of the Salt, which is derived from its salifying proximate principle or acid. Thus for example, Nitrate of Disoxyd of Mercury denotes $\text{Hg}^2 \text{O}^1 + \text{N}^1 \text{O}^5$ while Dinitrate of Mercury denotes $2 (\text{Hg}^1 \text{O}^1) + 1 (\text{N}^1 \text{O}^5)$ while still Dinitrate of Disoxyd of Mercury denotes $2 (\text{Hg}^2 \text{O}^1) + 1 (\text{N}^1 \text{O}^5)$. I may as well mention in this connexion, though it is somewhat out of place, that these Greek multiplicative numerals are also used to express the number of equivalents of the base

of a Salt, when they happen to predominate over those of the Acid, as well as the number of equivalents of the electro-positive element or compound radical in a salifiable base, likewise when they predominate over those of the electro-negative element or compound radical. From these two different applications of this set of numerals, there is great liability to confusion and error; and it is always unfortunate that one set of numerals should be employed in two such cases; but by a proper position of the numeral, a writer or speaker may always be correctly intelligible. In the following names, we know that *dis* or *di*, *-tris* or *tri*, *-tetraci* or *tetrac*, *-pentaci*, or *pentac*, *-hexaci*, of *hexac*, etc. have reference to the number of equivalents of the base of the Salt, because we find the ordinal *proto*, which never has reference to anything but the number of equivalents of the basifying element or compound radical in the electro-positive proximate principle of the Salt. $2 (\text{Pb}^{\text{I}} \text{O}^{\text{I}}) + 1 (\text{N}^{\text{I}} \text{O}^{\text{V}})$ is a Diprotonitrate of Lead. $3 (\text{Pb}^{\text{I}} \text{O}^{\text{I}}) + 1 (\text{N}^{\text{I}} \text{O}^{\text{V}})$ is a Triprotonitrate of Lead. $4 (\text{Pb}^{\text{I}} \text{O}^{\text{I}}) + 1 (\text{N}^{\text{I}} \text{O}^{\text{V}})$ is a Tetraciprotonitrate of Lead. $5 (\text{Pb}^{\text{I}} \text{O}^{\text{I}}) + 1 (\text{N}^{\text{I}} \text{O}^{\text{V}})$ is a Pentaciprotonitrate of Lead. $6 (\text{Pb}^{\text{I}} \text{O}^{\text{I}}) + 1 (\text{N}^{\text{I}} \text{O}^{\text{V}}) + 1 (\text{H}^{\text{I}} \text{O}^{\text{I}})$ is a Unihydrous Hexaciprotonitrate of Lead. But much higher numbers are often required, as $20 (\text{Hg}^{\text{I}} \text{Cl}^{\text{I}}) + 1 (\text{I}^{\text{I}})$ which is an Eicosacioidid of Protochlorid of Mercury. Turner calls this compound Ioduretted Bichlorid of Mercury. If he had said Iodidized Deutochlorid of Mercury his phraseology would have been less exceptionable for what he desired to say; though, as a matter of fact, it is the Protochlorid of Mercury that enters into the composition of this compound. Even if the Greek ordinal were omitted, we should presume that the Greek multiplicative had reference to the number of equivalents of the base of the Salt, because when it has reference to the number of equivalents of the basifying element or compound radical in the electro-positive proximate principle of the Salt, we should give it a different position, as in an example given, viz. Nitrate of Disoxyd of Mercury. But it is a great inconvenience to have but one set of numerals for two different and distinct purposes; and happily the Greek multiplicatives are the only examples of this sort, though if inaccurate writers could have their way, the Greek ordinals and the Latin distributives would be confounded together.

When a basic compound of a basifying and acidifying element or compound radical, with a basifiable element or compound radical, is in the proportion of two equivalents of the former to three equivalents of the latter, this is denoted by prefixing to the modified name of the basifying and acidifying element, the abridged term *hemiol*, before a vowel, and *hemio*, before a consonant. This prefix should have been *duplihemiol*, or *duplihemio*, and the reason why it is not, will be explained hereafter, when I mention the application of this same prefix to a different case. In the name of a Salt, basification by Oxygen, in its electro-positive proximate principle will be expressed by a change in the termination of the name of the electro-positive element into *a* or *ia* as euphony may require; and this name thus changed will, in Latin, precede, in the genitive case, the name of the electro-negative proximate principle of the Salt, and in English succede it, preceded by the preposition *of*. Potassæ Sulphas or Sulphate of Potassa; Sodæ Nitras or

Nitrate of Soda; *Calciæ Carbonas* or Carbonate of Calcia; *Barytæ Nitras* or Nitrate of Baryta; *Antimoniz et Potassæ Tartras* or Tartrate of Antimonia and Potassa; etc. are examples which will sufficiently illustrate this law of chymical nomenclature. Though a change of the termination of the name of an element to *a* or *ia* denotes that this element is combined with the basifying element Oxygen, yet it is not confined to any particular Oxyd. As a Protoxyd is much more commonly a salifiable base than any other Oxyd, this termination is most commonly applied to this Oxyd; but as Disoxyds, Sesquoxyds, sometimes Deutoxyds, and occasionally even Tritoxyds are basic, this termination may denote either of these; and yet, when a salifiable base is any thing but a Protoxyd, a numeral should be employed in its proper place to denote the specific Oxyd. We may say, if we choose, Sulphate of Protoxyd of Iron, Sulphate of Sesquoxyd of Iron, and if there were a Deutoxyd or a Tritoxyd that was basic, we might say Sulphate of Deutoxyd of Iron, and Sulphate of Tritoxyd of Iron. But the same thing may be expressed with equal definiteness and certainty by saying Protosulphate of Iron, Deutosulphate of Iron, Tritosulphate of Iron, etc. and this is the most brief and the most common way. The second example, however, cannot be expressed in this manner, because *Sesqui* is equally applicable to the electro-negative element or compound radical of the base of the Salt, and to the electro negative compound proximate principle of the Salt. Sesquisulphate of Iron might mean $1 (\text{Fe}^2 \text{O}^3) + 1 (\text{S}^1 \text{O}^3)$ or it might mean $2 (\text{Fe}^1 \text{O}^1) + 3 (\text{S}^1 \text{O}^3)$ or it might mean $2 (\text{Fe}^2 \text{O}^3) + 3 (\text{S}^1 \text{O}^3)$. These three Salts can be named unequivocally as follows, viz. Sulphate of Sesquoxyd of Iron, Sesquisulphate of Protoxyd of Iron or Sesquiprotosulphate of Iron, and Sesquisulphate of Sesquoxyd of Iron. When a Salt has any other than an Oxygen base, the name of such base must always be expressed in full, as Chlorohydrargyrate of Chlorid of Potassium, Chloraurate of Chlorid of Sodium, Hemioloxyhydrargyrate of Cyanid of Mercury or Hemioloxyhydrargyrate of Cyanid of Mercury.

When there is but one Acid of a given acidifiable element or compound radical, its name is made-up of an abbreviation of the name of the acidifying element or compound radical prefixed to the name of its acidifiable element or compound radical, the latter being converted into an attribute by changing its termination into *icum* in Latin or *ic* in English, preceded in Latin by the term *Acidum*, and succeeded in English by the term *Acid*. Acidum Fluohydricum or Fluohydric Acid; Acidum Chlorohydricum or Chlorohydric Acid; Acidum Bromohydricum or Bromohydric Acid; Acidum Iodohydricum or Iodohydric Acid, Acidum Sulphicarbonicum or Sulphicarbonic Acid; Acidum Cyanoferricum or Cyanoferric Acid, will serve as examples of this law. When Oxygen is the acidifying element of any Acid, the abbreviation of its name as a prefix to the name of the acidifiable element or compound radical, is usually omitted; but when any other acidifying element or compound radical enters into the constitution of an Acid, its abbreviated name must always be retained as a prefix to the name of the acidifiable element or compound radical, etc. In my opinion, it would have

been better if an abbreviation of Oxygen had always been retained; but such is not the usage. Chimists write Acidum Chloricum or Chloric Acid, and not Acidum Oxychloricum or Oxychloric Acid, Acidum Bromicum or Bromic Acid, and not Acidum Oxybromicum or Oxybromic Acid, Acidum Iodicum or Iodic Acid, and not Acidum Oxiodicum or Oxiodic Acid, Acidum Sulphuricum or Sulphuric Acid and not Acidum Oxysulphuricum or Oxysulphuric Acid.

When there are two Acids, and only two, of a given acidifiable element or compound radical, with an individual acidifying element or compound radical, that which contains the greatest quantity of the acidifying element or compound radical, is named as just specified, and that which contains the smallest quantity of the acidifying element or compound radical, has the name of its acidifiable element or compound radical converted into an attribute, by changing its termination into *osus* in Latin and *ous* in English, the name being in all other respects exactly like the preceding. Acidum Selenicum or Selenic Acid; and Acidum Selenosum or Selenous Acid, will illustrate this law of the chymical nomenclature. This example will likewise illustrate another law of the nomenclature. What is now called Selenous Acid ($\text{Se}^1 + \text{O}^2$) was the first, and for some time, the only Oxygen Acid of Selenium known. According to the law of the nomenclature, it was then called Acidum Selenicum or Selenic Acid. After a time, another Oxygen-Acid of Selenium was discovered, viz. $\text{Se}^1 + \text{O}^3$. As this contained more Oxygen in proportion to the Selenium than the Acid previously known, the law of the nomenclature required that it should bear the name of Selenic Acid, and that the other should take the name of Selenous Acid, and the change was made accordingly. Other cases of the same sort have occurred, and a similar change of names has been made. Now why is not this law always observed? When there was only one Acid of Chlorine and Oxygen known, viz. $\text{Cl}^1 + \text{O}^2$ it was very properly called Chloric Acid; but when $\text{Cl}^1 + \text{O}^1$ was discovered, that compound ought to have taken the name of Chloric Acid, and $\text{Cl}^1 + \text{O}^2$ ought to have been called Chlorous Acid. The same is true in regard to the Oxygen Acids of Bromine and Iodine.

When there are three Acids, and only three, of a given acidifiable element or compound radical, that which contains the greatest quantity of the acidifying element or compound radical, is named as if these were but one Acid, while that which happens to have been first known of the remaining two, is named like the second of only two Acids, and the third is called by one of these two names with the prefix *hypo* according as it contains a greater or less proportion of the acidifying element or compound radical than is contained in the second named Acid. For example $\text{N}^1 + \text{O}^2$ and $\text{N}^1 + \text{O}^1$ were long known before any other Acid of these two elements, and they were therefore very properly named Acidum Nitricum or Nitric Acid, and Acidum Nitrosum or Nitrous Acid. When $\text{N}^1 + \text{O}^3$ became known, it was very properly called Acidum Hyponitrosum or Hyponitrous Acid. But had $\text{N}^1 + \text{O}^2$ and $\text{N}^1 + \text{O}^3$ been first known, the former would still have been called Acidum Nitricum or Nitric Acid, the latter would have been called Acidum

Nitrosum or Nitrous Acid, and $N^1 + O^4$ would have been called Acidum Hyponitricum or Hyponitric Acid.

If there are four Acids of a given acidifiable and acidifying element or compound radical, the name of that Acid which contains the greatest quantity of the acidifying element or compound radical is as above specified; that Acid which contains the next greatest quantity of the acidifying element or compound radical, has the same name with the prefix *hypo*; that Acid which contains the greatest quantity of the acidifying element or compound radical, has the name terminating in *osum* or *ous*; and that Acid which contains the least quantity of the acidifying element or compound radical, has the same name with the prefix *hypo*. At present I know of no acidifiable element or compound radical that forms just four Acids with any acidifying element or compound radical; though it is but a short time since Sulphurum was known to form a greater number with Oxygen. This was the fact when Turner, Graham and Kane wrote. As a specification of the constitution and the legitimate nomenclature of these four Acids will illustrate the principles of the law in relation to just this number, I shall here make it.

1. $S^1 + O^3$ Acidum Sulphuricum or *Sulphuric Acid*.
2. $S^2 + O^5$ Acidum Hyposulphuricum or *Hyposulphuric Acid*.
3. $S^1 + O^2$ Acidum Sulphurosum or *Sulphurous Acid*.
4. $S^2 + O^4$ Acidum Hyposulphurosum or *Hyposulphurous Acid*.

If there are five Acids of a given acidifiable and acidifying element or compound radical, four of them would bear the same names that are employed when there are only four Acids, and either the third Acid in the series will have the termination of the name of its acidifiable element or compound radical changed to *icum* or *ic*, with the prefix *subhypo*; or the fifth Acid in the series will have the termination of the name of its acidifiable element or compound radical changed to *osum* or *ous* with the prefix *subhypo*. Whether the third or the fifth Acid in the series takes the new name depends upon the relative times of the discovery of the several Acids. If the third in the order of the amount of the acidifying element or compound radical, which it contains, should be discovered last, that would take the new name; but if the fifth in the order of the amount of the acidifying element or compound radical, which it contains, should be discovered last, that would take the new name. If any other in the series should be discovered last, the names would be so applied, as would require the least change in the names of the previously known Acids. For a time, only four Acids of Sulphurum and Oxygen were known, but soon a fifth was discovered. The nomenclature for these will illustrate the law for the naming of just this number of Acids.

1. $S^1 + O^3$ Acidum Sulphuricum or *Sulphuric Acid*.
2. $S^2 + O^5$ Acidum Hyposulphuricum or *Hyposulphuric Acid*.
3. $S^1 + O^2$ Acidum Sulphurosum or *Sulphurous Acid*.
4. $S^3 + O^5$? Acidum Hyposulphurosum or *Hyposulphurous Acid*.
5. $S^2 + O^4$ Acidum Subhyposulphurosum or *Subhyposulphurous Acid*.

The accommodation of the nomenclature to these five Acids, instead of four, it will be perceived, required only one name to be changed, viz $S^2 + O^2$ was required to be called Subhyposulphuric Acid, instead of Hyposulphuric Acid.

At present, it is well known that both Protoxyd and Deutoxyd of Nitrogen some times perform the functions of salifiable bases, at least with a few Acids, and some times perform the functions of salifying compounds or Acids, at least with certain bases, as Potassa and Soda. These compounds therefore require names as Acids, in order that their Salts may have names. This involves a revision of the nomenclature of the compounds of Nitrogen and Oxygen, and an application of names, as adapted to five Acids. This would be as follows, viz.

1. $N^1 + O^5$ Acidum Nitricum or *Nitric Acid*.
2. $N^1 + O^4$ Acidum Hyponitricum or *Hyponitric Acid*.
3. $N^1 + O^3$ Acidum Nitrosum or *Nitrous Acid*.
4. $N^1 + O^2$ Acidum Hyponitrosum or *Hyponitrous Acid*.
5. $N^1 + O^1$ Acidum Subhyponitrosum or *Subhyponitrous Acid*.

At present, likewise, six Acids of Sulphurum and Oxygen are recognized, and the legitimate names for this number will illustrate the law for the nomenclature of Acids, and will exhibit the changes, which the progress of discovery requires, under the existing system.

1. $S^1 + O^3$ Acidum Sulphuricum or *Sulphuric Acid*.
2. $S^2 + O^5$ Acidum Hyposulphuricum or *Hyposulphuric Acid*.
3. $S^1 + O^2$ Acidum Subhyposulphuricum or *Subhyposulphuric Acid*.
4. $S^3 + O^5$ Acidum Sulphurosum or *Sulphurous Acid*.
5. $S^4 + O^5$ Acidum Hyposulphurosum or *Hyposulphurous Acid*.
6. $S^2 + O^2$ Acidum Subhyposulphurosum or *Subhyposulphurous Acid*.

This involves a change in the name of only two Acids.

The laws of the nomenclature make no provision for naming more than six Acids of one acidifiable element or compound radical; and yet there is one instance of seven such Acids, and only one, so far as I have knowledge. In the case referred-to, the Acids are composed of Carbonum and Oxygen, and are as follows, viz.

1. $C^1 + O^2$ Acidum Carbonicum or *Carbonic Acid*. Aer Fixus or *Fixed Air*.
2. $C^2 + O^3$ Acidum Hypocarbonicum or *Hypocarbonic Acid*. Acidum Oxalicum or *Oxalic Acid*.
3. $C^1 + O^1$ Acidum Subhypocarbonicum or *Subhypocarbonic Acid*. Acidum Rhodizonicum or *Rhodizonic Acid*.
4. $C^2 + O^2$ Acidum Carbonosum or *Carbonous Acid*. The Acid contained in what is called Oxalammid.
5. $C^1 + O^1$ Acidum Hypocarbonosum or *Hypocarbonous Acid*. The Acid of what might, by some, be called Carbonamid ($H^2 N^1 + C^1 O^1$). Carboni Protoxydum or *Protoxyd of Carbon*. Oxydum Carbonicum or *Carbonic Oxyd*.
6. $C^5 + O^4$ Acidum Subhypocarbonosum or *Subhypocarbonous Acid*. Acidum Croconicum or *Croconic Acid*.

7. $\text{Cl} \pm \text{O}^3$ There is no provision among the laws of the nomenclature of chemistry for a seventh Acid. Acidum Meliticum or *Melitic Acid* from Melilite or Melite, in which it is contained naturally.

It will be observed that the Acids are arranged according to the proportion of the acidifying element or compound radical in comparison with the acidifiable element or compound radical, that which contains the greatest quantity being placed highest on the scale, the rest following according to the same principle. It will be observed too, that the names have a relative position adapted to this order.

In the nomenclature of Salts in Latin, the name of the electro-positive proximate principle or base, precedes in the genitive case, and the name of the electro-positive element or compound radical of the Acid with its termination changed from *icum* to *as*, or from *osum* to *is* succeeds in the nominative case of course. In English the name of the Salt is made-up of the name of the electro-positive element or compound radical of the Acid, with its termination changed from *ic* to *ate*, or from *ous* to *ite*, succeeded by the name of the electro positive proximate principle or base preceded by the preposition *of*. Some times the name of the electro-positive element or compound radical of the Acid, besides having its termination changed in the name of the Salt, is abridged, as Sulphate instead of Sulphurate; Sulphite instead of Sulphurite; Phosphate instead of Phosphate; Phosphite instead of Phosphorite; etc. For such abridgements there is neither principle nor rule, but they are dependent upon the mere caprice of an author. It would have been much better if they had never been made. In Carbonate and Carbonite, there is no abridgement, and these terms appear to me to be preferable to Carbate and Carbite, which would have been an abridgement in analogy with Sulphate and Sulphite, Phosphate and Phosphite. By way of illustration of the nomenclature of the Salts in Latin, Oxydum Calcii and Acidum Carbonicum make *Calciæ Carbonas*; the same salifiable base with Acidum Hypocarbonicum makes *Calciæ Hypocarbonas*; the same base with Acidum Subhypocarbonicum makes *Calciæ Subhypocarbonas*; the same base with Acidum Carbonosum makes *Calciæ Carbonis*; the same base with Acidum Hypocarbonosum makes *Calciæ Hypocarbonis*; the same base with Acidum Subhypocarbonosum makes *Calciæ Subhypocarbonis*; etc. But though these names are in strict accordance with the laws of the nomenclature, and in perfect analogy with other names of Salts, for which there is absolutely no substitute, yet it is very rare that any of them, except the first, is ever used. The second Acid is commonly called Rhodizonic, and its Salts Rhodizonates; the fourth (so far as I know) has no other name as an Acid, and its Salt with Ammidogen is called Oxalammid; the fifth has no other name as an Acid; but as it occurs in a regular series of compounds which are confessedly Acids, not only those which contain more Oxygen in proportion to their Carbonum, but those which contain less, being unequivocal Acids, this compound is presumed to be such. But there is supposed to be one compound of it with Ammidogen, one with Potassa or Protoxyd of

Potassium, and if there is no mistake about this, there are doubtless more, which would render its claim to be considered an Acid beyond controversy. The sixth Acid is commonly called Croconic, and its Salts Croconates; while the seventh has no other name than Melitic, and its Salts are always called Melitates, or sometimes by an unfortunate abridgement Melates.

When in a Salt, the number of equivalents of the salifying proximate principle or Acid is equal with, or predominates over the number of equivalents of the base, this is indicated by prefixing to that part of the name derived from the Acid, an abbreviation of one of the Latin distributive numerals as *un.* before a vowel and *uni* before a consonant, *bin.* or *bi.*; *tern.* or *terni*; *quatern.* or *quaterni*; *quin.* or *quini*; *sen.* or *seni*; *septen.* or *septeni*; *octon.* or *octoni*; *noven.* or *noveni*; *den.* or *deni*; etc. If a Greek ordinal numeral is required at the same time, to indicate the number of basifying elements in the base of the Salt, such Greek ordinal numeral must however come between the Latin distributive numeral, and that part of the name of the Salt derived from its Acid. The application of the Greek ordinals and the Latin distributives should never be confounded, as is done by many, since this frustrates the very end and object of having distinct numerals for distinct purposes. So long as the correct application of these two sets of numerals is carefully regarded, we always may know that the Greek ordinals have reference to the number of equivalents of the basifying element or compound radical in the base of a Salt, and the Latin distributives have reference to the number of equivalents of the salifying principle or Acid in the Salt; but whenever an author begins to talk about *Binoxyds* and *Bichlorids*, we never can tell whether the prefix *bin.* or *bi.* has relation to the number of equivalents of the basifying element or compound radical in the base, or of Acid in the Salt.

When in a Salt, there are three equivalents of the salifying principle or Acid, to two equivalents of base, this is denoted by prefixing *Sesq.* or *Sesqui* (according as the word to which it is prefixed begins with a vowel or a consonant) to that part of the name of the Salt, which is derived from the Acid. When this term was first chosen, it was supposed that an equivalent of the Salt consisted of one equivalent and a half of Acid, to one equivalent of base. When this error was corrected, the name should have been corrected into the prefix *duplisesqui*; but this has never been done, and in all probability never will be. When in a Salt, there are three equivalents of the base to two equivalents of the salifying principle or Acid, this is denoted by prefixing *hemiolo.* before a consonant, *hemiol.* before a vowel, to that part of the name which is derived from the Acid. This should properly be *duplihemio.* but it is not so, and probably never will be.

The prefix *Super* to the name of a Salt denotes merely that there is a greater number of equivalents of the Acid than of the base. This prefix should only be used generically. Thus, we may say correctly that there are two Superhypocarbonates or Superoxalates of Potassa, viz. the Binhypocarbonate or Binoxalate, and the Quaternhypocarbonate or Quaternoxalate. We may also very properly speak of one of the Superhypocarbonates or

Superoxalates of Potassa. When, however we have occasion to speak of one of these Salts specifically and definitely, the proper numeral denoting which of these Salts we intend, should always be employed. It is never worth while to write or speak of Supercarbonate of Potassa or Supercarbonate of Soda, because this language leaves it doubtful whether we intend the Sesquicarbonate or the Bicarbonate. It is always better to write and speak of Bitartrate of Potassa than of Supertartrate of Potassa, though there is but one Supertartrate of Potassa in common use, or kept in the shops. The prefix *Sub* to the name of a Salt, denotes merely that there is a greater number of equivalents of the base than of the Acid. This prefix also should only be used generically. Thus we may say correctly that there are three Subacetates of Lead in common use, viz. the Disacetate, the Trisacetate and the Hexacetate. We may also very properly speak of one of the Subacetates of Lead. When however we have occasion to speak of one of these Salts specifically and definitely, the proper numeral denoting which of these Salts we intend, should always be employed. In short, the remarks that I have just made in regard to the use of the prefix *Super*, are equally applicable to the use of the prefix *Sub*. Chymical writers sometimes mention Superoxyds, Superchlorids, etc. and Suboxyds, Subchlorids, etc. In these cases also *Super* denotes the predominance of the electro-negative element or compound radical, while *Sub* denotes the predominance of the electro-positive element or compound radical. This application of these prefixes is still more objectionable than their application to Salts. The proper numeral as a prefix, is always very greatly to be preferred, as it gives definite and precise information, while the prefixes *Super* and *Sub* often leave room for the question which of the *Super* Salts or which of the *Sub* Salts. If there is but one, it is far better to use the proper and precise numeral, which gives all the information needed or desired.

We frequently meet with the prefix *Per* to Oxyds, Chlorids, Bromids, Iodids, Sulphids, etc. As I never use it, I might well be exonerated from explaining it; but I will just state that it is used to imply that Oxyd, which contains the greatest proportion of Oxygen in comparison with the other element or compound radical; and so with the Chlorids, Bromids, Iodids, Sulphids, etc. Now a Peroxyd may be a Disoxyd, a Protoxyd, a Sesquoxyd, a Deutoxyd, a Tritoxyd, a Tetartoxyd, a Pemptoxyd, etc. and so of the Chlorids, Bromids, Iodids, Sulphids, etc. Persulphid of Potassium is a Pemptosulphid. Now I cannot perceive that such a term is of any service whatever, and I think that the proper numeral ought always to supersede its use. The numeral gives definite and valuable information.

I believe that I have now explained the nomenclature of chemistry so far as I shall have occasion to employ it, though what explanation I have made constitutes but a small proportion of the whole. An explanation of the whole would be out of place in this work, even if what I have already given is not. This subject ought to have preceded all consideration of any individual class of medicines; but the requests for it did not reach me in such a shape as to lead me to think of complying with them, till the printing of the proëm

to the Antiphlogistics was begun. So far as its utility is concerned, it is immaterial whether it precedes this proëm, or is printed as a note to it, immediately preceding the first catalogue of chimical names.

The Antiphlogistics may very properly be distributed into four *turmæ* or groups, thus :

TURMA PRIMA,
ANTIPHLOGISTICA DEPLETORIA.

TURMA SECUNDA.
ANTIPHLOGISTICA ACIDA.

TURMA TERTIA,
ANTIPHLOGISTICA SALINA.

TURMA QUARTA,
ANTIPHLOGISTICA ÆTHEREA.

1. ANTIPHLOGISTICA DEPLETORIA.

SANGUINIS DEPLETIO.

CATHARSIS A SALIBUS ANTIPHLOGISTICIS.

Depletion of Blood though it cannot be called an article of medicine, is nevertheless a most important remedial process, and one whose principal, indeed sole effect (with the exception of the shock, or strong impression upon the system which it may be made to produce) is that of a powerful Antiphlogistic. It is therefore not only proper, but necessary that it should be treated-of in a work on the *materia medica*, since in fact there is no other appropriate place for it. As a process of the *materia medica* it should stand at the very head of the Antiphlogistics.

Catharsis by the Antiphlogistic Salts very much augments the effects of these agents ; but catharsis by Non-Antiphlogistic articles can not be made Antiphlogistic, though it is usually more or less exhausting ; but this topic belongs else where, and will be treated-of in its proper place.

Emesis which, by some, has been considered as an Antiphlogistic process, is certainly not truly such. Neither are Ptyalism or Salivation, Chrempsis or Expectoration, Blennagogia or Blennagogy, Diuresis, Diaphoresis, etc. Antiphlogistic processes, though sometimes they are erroneously considered as such. It may be considered as a general rule that all those vegetable Acids, which are often found in considerable quantities in a free state, or in the form of *bin.* or *quatern.* Salts of Potassa, possess decided Antiphlogistic power. It is likewise a general rule that all the Salts

of Potassa, Soda, Magnesia and Calcia, that are soluble in water, and do not contain Acids possessing other powers, which are incompatible with, or transcend an Antiphlogistic power, are decidedly Antiphlogistic. The same is equally true of the Salts of Oxyd of Ammonium, only the Carbonates possess such a degree of Oresthetic power, as to prevent them from being used with sufficient freedom within a short time to be of any service in an acute phlogistic disease. These Salts however, actually possess a sufficient degree of exhausting power to render them efficient Antiphlogistics provided their Oresthetic operation did not prevent their employment in phlogistic diathesis. What I state, not only may be, but actually has been abundantly proved by the exhausting effects of a protracted use of these Salts in non-phlogistic diseases. I have witnessed such cases myself, and there are such upon the records of medicine. Exhaustion so produced is commonly said to be occasioned by excess of stimulus, and it is called indirect debility. But in this sense, I deny that there is any such thing as indirect debility; and I insist that these Salts are as directly exhausting as Nitrate of Potassa, Catharsis by Sulphate of Soda, or even Depletion of Blood; and I deny that these Salts are any more Antisbestic than the agents and processes just mentioned.

Most, and probably all of the saline Cathartics, if administered in moderate or small and uniform doses, at regular and sufficiently short intervals, but in a quantity short of that, which will produce immediate purging, operate efficiently as Antiphlogistics; and if they are pushed so far as to keep the intestines in a continuously lax state, even though they should not produce active evacuations, the Antiphlogistic effects will be thereby materially increased. Nitrate and Chlorate of Potassa may be considered as the best types of the pure saline Antiphlogistics, which in their ordinary doses and quantities in the twenty-four hours, are practically pure Antiphlogistics. In consequence of the preponderance of this over their other powers, we never perceive any effects from the latter in ordinary practice. Oræsthesia is never indicated along with Antiphlogistication; and indeed the former may be considered as medicinally incompatible with the latter. Euphrasy is never indicated along with Antiphlogistication, and in part, it may be considered as incompatible with the latter. All the *Ætheres* of

the several sorts, with only a single exception (viz. Unihydrite of Protoxyd of Etherogen or Alcohol,) are more or less exhausting and would doubtless be good Antiphlogistics if they did not possess such a degree of Euphrenic and Oresthetic power, as to interfere with their employment in phlogistic diathesis for their exhausting or Antiphlogistic effects. But one Æther is actually employed in practice as an Antiphlogistic (viz. Hyponitrite of Protoxyd of Etherogen, formerly called Spiritus Nitri dulcis, or Sweet Spirit of Niter) and this possesses only a very moderate degree of Euphrenic or Oresthetic power, so moderate that no effect is perceived from either of these powers, when it is used with the greatest freedom necessary for its Antiphlogistic operation. It is however only a feeble Antiphlogistic. I have not the least doubt that Tritochlorid of Formicigen or Chloroform (so called) might be used as an Antiphlogistic. It is true that it appears to be both more Euphrenic and more Oresthetic than Hyponitrite of Protoxyd of Etherogen, but then it seems to be considerably more exhausting in proportion. However, I have never known it employed as an Antiphlogistic, but from the very considerable amount of debility that I have known it produce, when taken as a Stimulant, and that within a short time, I can not doubt that it would be a better Antiphlogistic than Hyponitrite of Protoxyd of Etherogen.

II. ANTIPHLOGISTICA ACIDA.

VEGETABILIA—ORGANICA.

Neuragica, Oræsthetica.

{ ACIDUM HYPOCARBONICUM,
{ *Acidum Oxalicum.*

ACIDUM TARTARICUM.

ACIDUM MALICUM.

ACIDUM CITRICUM.

ACIDUM ACETICUM.

ACIDUM CARBONICUM.

There are doubtless numerous other vegetable-organic Acids that might be as usefully employed as Antiphlogistics as those I have specified; but they are either not as abundant, or not as conveniently obtainable. But after all, Tartaric, Malic and Citric Acids may be considered as the best types of the vegetable-acid Antiphlogistics. As ordinarily used, no neuragic or oresthetic effect is produced, so that for all practical purposes, they may be

reckoned as pure Antiphlogistics. There are no vegetable Antiphlogistics except those which are such, in consequence of containing some of the Antiphlogistic vegetable Acids, or some of the Antiphlogistic Salts, that are employed for this purpose, in a separate state. Very many plants abound in uncombined Acids in such quantities as to be thereby rendered most unequivocally Antiphlogistic.

ACIDUM HYPOCARBONICUM vel *Acidum Oxalicum*, is said to be contained in

- OXALIS ACETOSELLA. (*Linn.*)
- ? OXALIS AMERICANA. (*Big. De Cand.*)
- ? OXALIS STRICTA. (*Linn.*)
- ? OXALIS VIOLACEA. (*Linn.*)
- OXALIS CORNICULATA. (*Linn.*)
- ? RUMEX ACETOSA. (*Linn.*)
- ? RUMEX ACETOSELLA. (*Linn.*)
- PELARGONIUM ACIDUM. (*Sw. Geran.*)
- ? CICONIUM ACETOSUM. (*Sw. Geran.*)
- ? CICONIUM OXYPHYLLUM. (*Colv. Cat.*)
- CICER ARIETINUM. (*Linn.*)

(*Th. Thom. Chim. 5th Edit. 1818. Vol. iv. Pgs. 11 & 12.*)

I believe that the HYPOCARBONIC or *Oxalic Acid* always exists in plants in combination with Potassa in the form of Binhypocarbonate or *Binoxalate* of Potassa. Scheele is said to have enumerated twenty-two roots and fifteen barks, in which he found Hypocarbonic or Oxalic Acid.

ACIDUM TARTARICUM is said to be contained in

- ? OXYCOCCUS MACROCARPUS. (*Pursh.*)
- VITIS VINIFERA. (*Linn.*)
- ? MORUS NIGRA. (*Linn.*)
- ? MORUS RUBRA. (*Linn.*)
- MORUS ALBA. (*Linn.*)
- RHEUM RHAPONTICUM. (*Linn.*)
- RHUS CORIARIA. (*Linn.*)
- RHUS TYPHINA. (*Linn.*)

according to Thomas Thompson. (*Ibidem, Pg. 12-13.*)

ACIDUM MALICUM is said to be contained in

- BERBERIS VULGARIS. (*Linn.*)
- ? BERBERIS CANADENSIS. (*Pursh.*)
- SAMBUCUS NIGRA. (*Linn.*)

- ?SAMBUCUS CANADENSIS. (*Linn.*)
- ?SAMBUCUS PUBENS. (*Mich.*)
- ?SAMBUCUS EBULUS. (*Linn.*)
- SORBUS AUCUPARIA. (*Linn.*)
- ?SORBUS DOMESTICA. (*Linn.*)
- ?SORBUS AMERICANA. (*Pursh.*)
- MALUS COMMUNIS. (*De Cand. Fl. Gal.*)
- PRUNUS SPINOSA. (*Linn.*)
- ?PRUNUS AMERICANA. (*Marshall.*)
- PRUNUS DOMESTICA. (*Linn.*)

—*Ibidem*, Pg. 12-13.

ACIDUM CITRICUM is said to be contained in

- ? CITRUS MEDICA. (*Risso and De Cand.*)
- CITRUS LIMONUM. (*Risso and De Cand.*)
- ? CITRUS ACIDA. (*Hortus Bengalensis.*)
- ? CITRUS LIMETTA. (*Risso and De Cand.*)
- CITRUS BIGARADIA. (*Duham. and Risso.*)
- CITRUS AURANTIUM. (*Risso and De Cand.*)
- CITRUS DECUMANA. (*Linn. and De Cand.*)
- CITRUS MARGARITA. (*Linn. and De Cand.*)
- VACCINIUM VITIS-IDÆA. (*Linn.*)
- ROSA CANINA. (*Linn.*)
- SOLANUM DULCAMARA. (*Linn.*)
- CERASUS PADUS. (*Linn.*)

—*Ibidem*, Pg. 12-13.

TARTARIC and MALIC ACIDS are said to be contained in

- AGAVE AMERICANA. (*Linn.*)
- ? AGAVE MEXICANA. (*La Marck.*)

—*Ibidem*, Pg. 13.

MALIC and CITRIC ACIDS are said to be contained in just about equal proportions in

- RIBES RUBRUM. (*Linn.*)
- ? RIBES ALBUM.
- ? RIBES NIGRUM. (*Linn.*)
- ? RIBES FLORIDUM. (*L' Her.*)
- RIBES GROSSULARIA. (*Linn.*)
- FRAGARIA VESCA. (*Linn.*)
- ? FRAGARIA VIRGINIANA. (*Linn.*)

- ? FRAGARIA ELATIOR. (*Willd.*)
- VACCINIUM MYRTILLUS. (*Linn.*)
- RUBUS IDÆUS. (*Linn.*)
- ? RUBUS STRIGOSUS. (*Mich.*)
- ? RUBUS OCCIDENTALIS. (*Linn.*)
- ? RUBUS CHAMÆMORUS. (*Linn.*)
- ? RUBUS VILLOSUS. (*Aiton.*)
- ? RUBUS CANADENSIS. (*Linn.*)
- ? RUBUS TRIVIALIS. (*Mich.*)
- CRATÆGUS ARIA. (*Linn.*)
- CERASUS HORTENSIS. (*Salisb. or F. Gray.*)
- CERASUS AVIUM. (*Salisb. or F. Gray.*)

—*Ibidem*, Pg. 12–13.

TARTARIC, MALIC, and CITRIC ACIDS are said to be contained in

TAMARINDUS INDICA. (*Linn.*) —*Ibidem*, Pg. 13.

TARTARIC and CITRIC ACIDS are said to be contained in

OXYCOCCUS PALUSTRIS. (*Persoon.*)

—*Ibidem*, Pg. 12–13.

ANTIPHLOGISTICA ACIDA.

VEGETABILIA—ORGANICA.

Neuragica, Oræsthetica.

Succi Vegetabiles,—

FRUCTUS.

- CITRUS MEDICA. (*Risso and De Cand.*)
Citron Tree.
- CITRUS LIMONUM. (*Risso and De Cand.*)
Lemon Tree.
- CITRUS ACIDA. (*Hort. Bengal.*)
Lime Tree.
- CITRUS LIMETTA. (*Risso and De Cand.*)
Bergamotte Tree.
- CITRUS BIGARADIA. (*Duham. and Risso.*)
Myrtle-leafed Orange Tree.
Seville Orange Tree.
Bitter Orange Tree.
- CITRUS AURANTIUM. (*Risso and De Cand.*)
China Orange Tree.

*Sweet Orange Tree.*CITRUS DECUMANA. (*Linn. and De Cand.*)*Shaddock Tree.*CITRUS MARGARITA. (*Lour. and De Cand.*)*Sweet Lemon Tree.*PUNICA GRANATUM. (*Linn.*)*Pomegranate Tree.*OXYCOCCUS MACROCARPUS. (*Pursh.*)*Large Craneberry Plant.*OXYCOCCUS PALUSTRIS. (*Persoon.*)*Small Craneberry Plant.*BERBERIS VULGARIS. (*Linn.*)*European Berbery Tree.*BERBERIS CANADENSIS. (*Pursh.*)*Southern Berbery Tree.*RIBES RUBRUM. (*Linn.*)*Red Currant Bush.*

RIBES ALBUM.

*White Currant Bush.*RIBES NIGRUM. (*Linn.*)*European Black Currant Bush.*RIBES FLORIDUM. (*L' Herit.*)*American Black Currant Bush.*RIBES GROSSULARIA. (*Linn.*)*Goose Berry Bush.*VACCINIUM MYRTILLUS. (*Linn.*)*Bull Berry Bush. Wine Berry Bush.*VACCINIUM VITIS-IDÆA. (*Linn.*)*Red Whortle Berry Bush.*FRAGARIA VESCA. (*Linn.*)*European Straw Berry Plant.*FRAGARIA VIRGINIANA. (*Linn.*)*United States Straw Berry Plant,*FRAGARIA ELATIOR. (*Willd.*)*Haut-Bois Straw Berry Plant.*RUBUS IDÆUS. (*Linn.*)*Rasp Berry Bush.*RUBUS STRIGOSUS. (*Mich.*)*Red Rasp Berry Bush.*

- RUBUS OCCIDENTALIS. (Linn.)
Black Rasp-Berry Bush.
- RUBUS CHAMÆMORUS. (Linn.)
Cloud-Berry Bush.
- RUBUS VILLOSUS. (Aiton.)
High Black-Berry Bush.
- RUBUS CANADENSIS. (Linn.)
Dew-Berry Bush.
- RUBUS TRIVIALIS. (Mich.)
Dew-Berry Bush.
- VITIS VINIFERA. (Linn.)
Common Vine.
- ROSA CANINA. (Linn.)
Dog Rose Bush. Hip. Hep.
- SOLANUM DULCAMARA. (Linn.)
Bitter Sweet. Woody Nightshade.
- SAMBUCUS NIGRA. (Linn.)
European Black Elder Bush.
- SAMBUCUS CANADENSIS. (Linn.)
New England Elder Bush.
- SAMBUCUS PUBENS. (Mich.)
Red Elder Bush.
- SAMBUCUS EBULUS. (Linn.)
Dwarf Elder Bush.
- SORBUS AUCUPARIA. (Linn.)
Rohan Tree. Quicken Tree.
- SORBUS DOMESTICA. (Linn.)
Sorb Tree. Servis Tree.
- SORBUS AMERICANA. (Pursh.)
American Rohan Tree.
- CRATÆGUS ARIA. (Linn.)
White Beam Tree.
- MALUS COMMUNIS. (De Cand. Fl. Gal.)
Apple Tree.
- CERASUS HORTENSIS. (Salisb. or F. Gray.)
Garden Cherry Tree.
- CERASUS PADUS. (Linn.)
Hedge Cherry Tree.
- CERASUS AVIUM. (Salsb. or F. Gray.)
Bird Cherry Tree.
- PRUNUS SPINOSA. (Linn.)

*Sloe-Tree.*PRUNUS AMERICANA. (*Marshall.*)*Red Plum Tree. Yellow Plum Tree.*PRUNUS DOMESTICA. (*Linn.*)*Common Plum Tree.*MORUS NIGRA. (*Linn.*)*Black Mulberry Tree.*MORUS RUBRA. (*Linn.*)*Red Mulberry Tree.*MORUS ALBA. (*Linn.*)*White Mulberry Tree.*

PLANTÆ.

OXALIS ACETOSELLA. (*Linn.*)*Wood Sorrel (Shamrock ?)*OXALIS AMERICANA. (*Big. and De Cand.*)*Wood Sorrel.*OXALIS STRICTA. (*Linn.*)*Wood Sorrel.*OXALIS VIOLACEA. (*Linn.*)*Wood Sorrel.*OXALIS CONICULATA. (*Linn.*)*Wood Sorrel.*RUMEX ACETOSA. (*Linn.*)*Sorrel.*RUMEX ACETOSELLA. (*Linn.*)*Small Sorrel.*PELARGONIUM ACIDUM. (*Sw. Geran.*){ CICONIUM ACETOSUM. (*Sw. Geran.*){ *Pelargonium acetosum.* (*Ait.? De Cand.*){ *Geranium acetosum.* (*Linn.*){ CICONIUM OXYPHYLLUM. (*Colv. Cat.*){ *Geranium miniatum.* (*Andrews's Geran.*){ ? *Pelargonium oxyphyllum.* (*De Cand.*){ ? *Pelargonium lævigatum.* (*L' Herit.*)CICER ARIETINUM. (*Linn.*)AGAVE AMERICANA. (*Linn.*)AGAVE MEXICANA. (*La Marck.*)RHEUM RHAPONTICUM. (*Linn.*)

PULPA FRUCTUS.

TAMARINDUS INDICA. (*Linn.*)*Tamarind Tree.*

INFUSUM FRUCTUS.

RHUS TYPHINA. (Linn.)

Stag's Horn Sumach.

RHUS GLABRA. (Linn.)

Smooth Sumach.

RHUS COPALLINA. (Linn.)

Mountain Sumach.

RHUS CORIARIA. (Linn.)

Tanners' Sumach.

For employment in medicine and as Antiphlogistics, these acid juices usually require more or less dilution, in some instances but a little, in others a great deal; and often they are made more agreeable by the addition of a little of the best refined Sugar. For all diseases except those of a phlogistic character, these juices would be far better for the digestible and nutritious matter, which they contain; but in exquisitely phlogistic diathesis, I doubt whether this circumstance gives them any superiority over the pure Acids suitably diluted. But this digestible nutritious matter is a substitute for considerable dilution. Those who employ the vegetable Acids and these juices, equally in non-phlogistic and non-atonic, and even in positively atonic diseases, have often noticed the superiority, as respects kindness of operation, of the Vegetable Juices over the pure Acids, though ever so much diluted; but I do not recollect that any body has ever remarked the superiority of the pure Acids, if only diluted enough, in exquisitely phlogistic cases.

The preceding list of vegetables whose juices are acid and are employed as Antiphlogistics, may be deemed unnecessarily large by some, and not sufficiently extensive by others. At this moment, I have before me one European work on materia medica, that has seventy-five additional articles of this character and class, and an other work that I should judge had as many more; but interspersed in an alphabetical order, with other articles belonging to all the other classes, so that I have not been at the trouble to number them. The articles, which I have selected, with only about a dozen exceptions, are such as are easily obtainable in the northern and middle U. S. A. Perhaps equally valuable articles may be in use at the South and in the West; but what I have specified I trust will be sufficient to illustrate the subject amply and fully.

ANTIPHLOGISTICA ACIDA.

ANIMALIA—ORGANICA.

Neuragica, Oræsthetica.

ACIDUM LACTICUM.

ANTIPHLOGISTICA ACIDA.

CHIMICA—INORGANICA.

Neuragica, Oræsthetica.

ACIDUM CHLORICUM.

ACIDUM CHLOROSUM.

ACIDUM SULPHURICUM.

ACIDUM NITRICUM.

ACIDUM CHLOROXYNITRICUM.

ACIDUM PHOSPHORICUM.

ACIDUM CHLOROHYDRICUM.

ACIDUM BORACICUM.

Salts usually derive their medicinal characters from their bases, because these are far oftener medicinal, and medicinal in such a way as to retain their powers, in their Salts. But sometimes Salts derive prominent peculiarities of power from their Acids. However, Salts of the same base often possess powers, which can not be ascribed to the diversity of the Acids, which enter into their constitution. There is apparently no difference in the medicinal powers of Sulphuric and Nitric Acids, by which we can explain the fact that Sulphate of Potassa is Cathartic and Nitrate of Potassa is destitute of this power, at least to such a degree as to be available in medicine for this purpose. I shall therefore found my genera of the Salts upon their base, and my species upon their Acid. In a few instances however, when a group of Salts with different bases derives its most important power or powers from a particular Acid, I may perhaps arrange such groups into genera founded on the Acid, the species being founded upon the base.

The ANTIPHLOGISTICA SALINA may be subdivided into three groups, viz.

1. ANTIPHLOGISTICA SALINA.

NON—EVACUANTIA.

Neuragica, Oræsthetica.

2. ANTIPHLOGISTICA SALINA.

CATHARTICA.

Neuragica, Oræsthetica.

3. ANTIPHLOGISTICA SALINA.

NAUSEANTIA, EMETICA, CATHARTICA.

Neuragica, Oræsthetica.

Nitrate and Chlorite of Potassa may be considered as the best types of the non-evacuant saline Antiphlogistics. As ordinarily used, no neuragic or oresthetic effect is produced, so that for all practical purposes they may be reckoned as pure Antiphlogistics. John Murray says that "Nitrate of Potassa is an example of a Refrigerant" (i. e. an Antiphlogistic) "acting by the sensation of cold it produces in the stomach, which is equivalent to a partial abstraction of stimulus, which being extended by sympathy to the heart, occasions a transient reduction in the force of the circulation, and by this, or by a similar sympathetic affection, causes a sensation of cold over the body." Murray continues, "it excites a sensation of cold in the stomach even when taken dissolved, and still more in the solid state; and this is followed by reduction in the number and force of the" (arterial) "pulsations." "Hence" (Murray says) "Nitrate of Potassa acts more suddenly than the other Refrigerants" (Antiphlogistics) "and is more transient in its operation." "But" he adds, "it may also operate in some degree more permanently in the same manner as the Vegetable Acids; as it is probable, from the florid color it gives to the blood, that it parts with Oxygen readily." (*John Murray's Syst. Mat. Med. from 4th Edinb. Edit. N. York 1828, Refrigerants, Pg. 262.*) I esteem the whole of these notions as utterly chimerical. I do not think that any portion of them is true; but I have already repeatedly stated my objections to them. I do not think that there is any foundation whatever for the opinion that the exhausting effects of Nitrate of Potassa are more transient than any other non-evacuant Antiphlogistic; nor for the opinion that it operates in two ways as an Antiphlogistic, its effects being less permanent from one mode and more permanent from the other. I do not think that the florid color which it gives to the blood, is of any more importance to its medicinal effects, than the florid color which Carmine gives to the face, when it is applied with a pencil.

It is to be remarked that those Salts of the following group, whose electro-negative proximate principle, or in one word Acid, is of vegetable-organic origin, as Hypocarbonic or Oxalic, Subhypocarbonous or Croconic, Acetic, Malic, Citric, etc. are less

perfectly non-evacuant than the rest. In fact, when used freely they are sub-cathartic, in view of which fact, I once made them a distinct group. This arrangement I abandoned however, on the grounds that they could not be employed to any useful purpose as Cathartics, and that the group, in which I have merged them, if taken in inordinate doses and quantities, is likewise sub-cathartic, though less so than the group in question.

ANTIPHLOGISTICA SALINA.

NON-EVACUANTIA.

Neuragica, Oræsthetica.

POTASSÆ NITRAS ANHYDRUS.

POTASSÆ HYPONITRAS ANHYDRUS.

POTASSÆ CHLORIS ANHYDRUS ($K^1 O^1 + Cl^1 O^5$.)

POTASSÆ CHLORAS ANHYDRUS ($K^1 O^1 + Cl^1 O^7$.)

POTASSÆ BICARBONAS ANHYDRUS.

POTASSÆ BICARBONAS UNHYDRUS.

{ POTASSÆ SUBHYPOCARBONIS, } BINHYDRUS.
 { Potassæ Croconas, }

{ POTASSÆ HYPOCARBONAS, } UNHYDRUS.
 { Potassæ Oxalas, }

{ POTASSÆ BINHYPOCARBONAS, } BINHYDRUS.
 { Potassæ Binoxalas, }

{ POTASSÆ QUATERNHYPOCARBONAS, } SEPTENHYDRUS.
 { Potassæ Quaternoxalas, }

POTASSÆ BOAS ANHYDRUS.

POTASSÆ BIBRAS ANHYDRUS.

POTASSÆ ACETAS BINHYDRUS.

POTASSÆ BINACETAS SENHYDRUS.

POTASSÆ MALAS ANHYDRUS.

POTASSÆ BIMALAS ANHYDRUS.

POTASSÆ CITRAS ANHYDRUS.

{ POTASSÆ FORMICICAS, } ANHYDRUS.
 { Potassæ Formas, }

{ SODÆ NITRAS ANHYDRUS.

{ Qu.? Subcatharticus vel Catharticus.

SODÆ HYPONITRAS ANHYDRUS.

SODÆ CHLORIS ANHYDRUS ($Na^1 O^1 + Cl^1 O^5$.)

SODÆ CHLORAS ANHYDRUS ($Na^1 O^1 + Cl^1 O^7$.)

SODÆ BICARBONAS UNHYDRUS.

{ SODÆ HYPOCARBONAS, } ANHYDRUS.
 { Sodæ Oxalas, }

{ SODÆ BINHYPOCARBONAS, }
{ *Sodæ Binazalas,* } TERNHYDRUS.

SODÆ BORAS ANHYDRUS.

SODÆ BIBORAS QUINHYDRUS.

SODÆ BIBORAS DENHYDRUS.

SODÆ ACETAS SENHYDRUS.

SODÆ MALAS ANHYDRUS.

SODÆ BIMALAS ANHYDRUS.

SODÆ CITRAS ANHYDRUS.

SODÆ et AMMONII OXYDI SULPHAS SENHYDRUS.

CALCIÆ NITRAS TERNHYDRUS.

CALCIÆ NITRAS SENHYDRUS.

CALCIÆ ACETAS SENHYDRUS.

CALCIÆ BINACETAS.

CALCIÆ BIMALAS SENHYDRUS.

AMMONII OXYDI NITRAS UNHYDRUS.

AMMONII OXYDI HYPONITRAS UNHYDRUS.

AMMONII OXYDI SULPHAS UNHYDRUS.

AMMONII OXYDI SULPHAS BINHYDRUS.

AMMONII OXYDI BORAS BINHYDRUS.

AMMONII OXYDI BIBORAS.

{ AMMONII OXYDI HYPOCARBONAS, }
{ *Ammonii Oxydi Oxalas,* } BINHYDRUS.

{ AMMONII OXYDI HYPOCARBONAS, }
{ *Ammonii Oxydi Oxalas,* } TERNHYDRUS.

{ AMMONII OXYDI BINHYPOCARBONAS, }
{ *Ammonii Oxydi Binoxa'as,* } OCTONHYDRUS.

AMMONII OXYDI ACETAS SEPTENHYDRUS.

AMMONII OXYDI BINACETAS.

AMMONII OXYDI MALAS.

AMMONII OXYDI BIMALAS.

AMMONII OXYDI CITRAS.

AMMONII CHLORIDUM.

The group of Salts that immediately follows has, among my acquaintance in the medical profession, been commonly reckoned to be Adenagic, or according to the term employed in the case, Deobstruent, by which was undoubtedly intended Adenagic. I have however carefully studied these articles in reference to this point, and I am compelled to say that I do not think there is the

least foundation for the opinion in question. All the peculiarity of this group that I could ever perceive is a little greater degree of Oresthetic power, but not sufficient to separate them from the preceding group, among which, they might very properly be distributed under the two genera to which they really belong.

ANTIPHLOGISTICA SALINA.

NON-EVACUENTIA.

Neuragica, Oræsthetica.

Habita Adenagica non recte.

POTASSÆ CARBONAS ANHYDRUS.

POTASSÆ CARBONAS BINHYDRUS.

POTASSÆ SESQUICARBONAS SENHYDRUS.

POTASSÆ SESQUICARBONAS DUODENHYDRUS.

SODÆ CARBONAS UNHYDRUS.

SODÆ CARBONAS SESQUIHYDRUS.

SODÆ CARBONAS OCTONHYDRUS.

SODÆ CARBONAS DENHYDRUS.

SODÆ SESQUICARBONAS BINHYDRUS.

The electro-negative proximate principles, or in one word, the Acids of the following group of Salts, have the character of being Euphrenics, or as they are ordinarily termed Nervines, or Antispasmodics, and they have been supposed to impart this power to the Salts into whose composition they enter. On this ground I was induced to make them a distinct group, but I now doubt whether on good foundation; for I have never perceived any Euphrenic effects from them; and therefore I am inclined to think that all of them, except those that have Magnesia for a base, should be distributed into their proper genera among the non-evacuant Saline Antiphlogistics; while those with Magnesia for a base should be placed in their proper genus among the Cathartic Saline Antiphlogistics.

ANTIPHLOGISTICA SALINA.

NON-EVACUENTIA.

Neuragica, Oræsthetica.

Habita Euphrenica non recte.

{ POTASSÆ BENZHYLAS, }	} ANHYDRUS.
{ Potassæ Benzoäs, }	

{ POTASSÆ BIBENZHYLAS, }	} ANHYDRUS.
{ Potassæ Bibenzoäs, }	

{ SODÆ BENZHYLAS, }
 { Sodæ Benzoäs, } ANHYDRUS.

{ CALCIE BENZHYLAS, }
 { Calcie Benzoäs, } ANHYDRUS.

{ MAGNESIÆ BENZHYLAS, }
 { Magnesiæ Benzoäs, } ANHYDRUS.

Qu.? Cathartic.

{ AMMONII OXYDI BENZHYLAS, }
 { Ammonii Oxydi Benzoäs, } UNHYDRUS.

{ AMMONII OXYDI BIBENZHYLAS, }
 { Ammonii Oxydi Bibenzoäs, } UNHYDRUS.

POTASSÆ SUCCINAS.

SODÆ SUCCINAS.

CALCIÆ SUCCINAS.

MAGNESIÆ SUCCINAS.

Qu.? Cathartic.

AMMONII OXYDI SUCCINAS BINHYDRUS.

POTASSÆ CAMPHORAS.

CALCIÆ CAMPHORAS OCTONHYDRUS.

MAGNESIÆ CAMPHORAS (SESQUIHYDRUS ?)

Qu.? Cathartic.

AMMONII OXYDI CAMPHORAS.

AMMONII OXYDI BICAMPHORAS.

The neuragic effects of all these Salts are perceived only from a comparatively protracted use of them, or from single inordinate or excessive doses. From none of the preceding articles, unless possibly the Nitrate of Oxyd of Ammonium, are any Oresthetic effects immediately perceptible, when they are administered in the ordinary moderate doses and quantities in the twenty-four hours; but if the doses and quantities are in any degree inordinate or excessive, the Oresthetic operation is decided and even prominent.

ANTIPHLOGISTICA SALINA.

CATHARTICA.

Neuragica, (Oræsthetica ?)

ALIAS

Cathartica Salina ad Antiphlogisticandum adhibita.

POTASSÆ TARTRAS BINHYDRUS.

POTASSÆ BITARTRAS UNHYDRUS.

POTASSÆ BITARTRAS BINHYDRUS.

POTASSÆ ET SODÆ TARTRAS OCTONHYDRUS.
 POTASSÆ ET SODÆ TARTRAS DENHYDRUS.
 POTASSÆ SULPHAS ANHYDRUS.
 POTASSÆ HYPOSULPHAS ANHYDRUS.
 POTASSÆ HYPOSULPHIS.
 POTASSÆ PHOSPHAS UNHYDRUS.
 POTASSÆ PHOSPHAS BINHYDRUS.
 POTASSÆ BIPHOSPHAS BINHYDRUS.
 POTASSÆ ET MAGNESIÆ SULPHAS ANHYDRUS.
 POTASSÆ ET MAGNESIÆ SULPHAS SENHYDRUS.
 POTASSÆ ET MAGNESIÆ SULPHAS SEPTENHYDRUS.
 POTASSÆ ET AMMONII OXYDI SULPHAS QUATERNHYDRUS.
 POTASSÆ ET AMMONII OXYDI TARTRAS BINHYDRUS.

SODÆ SULPHAS ANHYDRUS.
 SODÆ SULPHAS OCTONHYDRUS.
 SODÆ SULPHAS DENHYDRUS.
 Rhombicus { SODÆ DIPHOSPHAS UNHYDRUS.
 SODÆ DIPHOSPHAS QUINDENHYDRUS.
 SODÆ DIPHOSPHAS VICENI-QUATERNHYDRUS.
 SODÆ HYPOSULPHAS BINHYDRUS.
 SODÆ TARTRAS BINHYDRUS.
 SODÆ BITARTRAS TERNHYDRUS.
 SODÆ ET MAGNESIÆ SULPHAS SENHYDRUS.
 SODÆ ET AMMONII OXYDI SULPHAS SENHYDRUS.

MAGNESIÆ NITRAS ANHYDRUS.
 MAGNESIÆ NITRAS SENHYDRUS.
 MAGNESIÆ CHLORIS BINHYDRUS.
 MAGNESIÆ SULPHAS SEPTENHYDRUS.
 MAGNESIÆ HYPOSULPHAS SENHYDRUS.
 MAGNESIÆ DIPHOSPHAS UNHYDRUS.
 MAGNESIÆ PHOSPHAS SEPTENHYDRUS.
 MAGNESIÆ BICARBONAS.
 { MAGNESIÆ HYPOCARBONAS, }
 { *Magnesiæ Oxalas,* } BINHYDRUS.
 { MAGNESIÆ BINHYPOCARBONAS, }
 { *Magnesiæ Binoxalas,* } SEPTENHYDRUS.
 MAGNESIÆ ACETAS QUINHYDRUS.
 MAGNESIÆ BITARTRAS UNHYDRUS.
 MAGNESIÆ CITRAS.

MAGNESIÆ MALAS.

MAGNESIÆ ET AMMONII OXYDI SULPHAS ANHYDRUS.

MAGNESIÆ ET AMMONII OXYDI SULPHAS SEPTENHYDRUS.

MAGNESIÆ ET AMMONII OXYDI NITRAS.

AMMONII OXYDI TARTRAS ANHYDRUS.

POTASSII PROTOCHLORIDUM.

SODII PROTOCHLORIDUM.

MAGNESII PROTOCHLORIDUM.

Neuragic effects are perceived from a protracted use of the Cathartic Saline Antiphlogistics, but I suspect not so much from inordinate or excessive doses, because they pass-off so speedily by catharsis, when so taken. From numerous circumstances, I am strongly inclined to believe that all the Cathartic Saline Antiphlogistics would produce Oresthetic effects in large doses, provided they could be retained in the alimentary canal for a sufficient length of time. Their external sensible properties are very much like those of the Bicarbonates of Potassa and of Soda, which produce decided Oresthetic effects, and are capable of irritating Ulcers and tender surfaces. I cannot doubt therefore that if they did not pass-off so speedily by Catharsis, they would prove as actively Oresthetic as a large portion of the non-evacuant Saline Antiphlogistics. On the whole, I am inclined to believe that there is no Antiphlogistic agent known, which does not possess Neuragic, and some degree of Oresthetic power (Depletion of Blood, which is a process in contradistinction from an agent, excepted); though some articles exert such a moderate degree of the latter power, as they are ordinarily employed and managed, that it is not noticed from common doses and quantities in the twenty-four hours. I notice this fact, because I shall hereafter state that all the purest Antisbestics likewise possess more or less Oresthetic power; and because, from this last fact, an Oresthetic power has been maintained to be identical with an Antisbestic power. If the foregoing statement is true however, there is just as much reason for considering an Oresthetic power to be identi-

cal with an Antiphlogistic power, as with an Antisbestic power. The truth is that an Oresthetic power is much oftener found intirely distinct from either Antiphlogistic or Antisbestic powers, than in connexion with either, which sufficiently proves that it is not identical with both. And yet, it is from the operation of Oresthetic-Antiphlogistics, and Euphrenic-Antiphlogistics which have been mistaken for pure stimulants (commonly so called) that the false dogma has arisen that stimulants, if continued for any considerable length of time, necessarily exhaust, reduce or debilitate. But no pure Oresthetic, no pure Euphrenic, no pure Oresthetic-Antisbestic, or pure Euphrenic-Antisbestic is capable of exhausting, reducing or debilitating at all, or at least when given in medicinal doses and quantities.

A considerable number of plants contain some Antiphlogistic salt in sufficient abundance to render the expressed juice, and sometimes the whole plant, sufficiently Antiphlogistic to be capable of being useful in medicine, for this power. Binoxalate of Potassa exists in many plants, and renders them Antiphlogistic. This salt is believed to be the active principle of *Rumex Acetosa*, *Rumex Acetosella*, numerous species of *Oxalis*, and doubtless very many other plants. Does not Bitartrate of Potassa exist in the Grape; and is not this the compound which renders its unfermented juice Antiphlogistic? I have much oftener found perfect crystals of Nitrate of Soda (Cubic Niter) in vegetable extracts from Solanaceous plants, than Nitrate of Potassa; but Nitrate of Potassa exists in a number of plants, as is believed, in sufficient quantity to render them decidedly Antiphlogistic.* I have found it in some vegetable extracts from certain Solanaceous plants, though in too small quantity to render such plants Antiphlogistic. It exists much more abundantly in plants of other natural orders, as in some Nitrariaceæ, some Urticaceæ, etc. I do not purpose at present to mention any considerable number of these, but only just enough to illustrate my statement, since the pure salt in its separate state must always be preferable to any crude vegetable containing it; to say nothing of the difficulty and inconvenience of supplying our shops with such plants, from obscure and not easily accessible parts or regions.

* "Planche interdum octavam partem Potassæ Nitratiss in extracto foliorum Cichorii Intybi reperisse narrat." (*Ph. Batav. Lips. 1824, Vol. 1, Pg. 122.*)

PLANTS THAT ARE ANTIPHLOGISTIC IN CONSEQUENCE OF CONTAINING
NITRATE OF POTASSA.

NITRARIA SCHÆBERI. (*Linn.*)

NITRARIA TRIDENTATA. (*Des Font.*)

NITRARIA CASPICA. (*De Cand.*)

NITRARIA SENEGALENSIS. (*La M.*)

NITRARIA BILLARDIERII.

PARIETARIA OFFICINALIS. (*Linn.*)

? PARIETARIA CRETICA. (*Willd ?*) (*Linn. ?*)

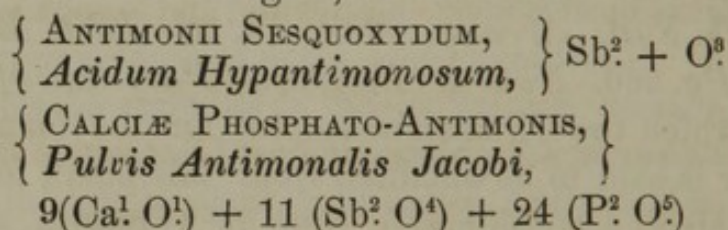
We are informed by naturalists and travelers that "Nitraria" Schœberi (*Linn.*) "contains Nitrate of Potassa, and is said not to flourish unless this salt is mixed with the soil, in which it grows." (*Wm. With. Syst. Arrang. Brit. Pl. 5th Edit. Birm. 1812. Vol. II. Pg. 260. Note of Page Sub Parietar. officin.*) All the Niter, which the Circassians employ for the manufacture of Gun-powder is said to be obtained from this plant, for Mr Longworth, who spent a considerable time in Circassia, says "Niter the Circassians obtain from a shrub, which is found all over their country." "It blazes when ignited, like Touch-Paper." (*J. A. Longworth. "A year among the Circassians." Lond., 1840. Vol. I. Chap. XII. Pg. 251.*) Now Nitraria Sibirica and Nitraria Caspica have commonly been considered as mere varieties of one species, though De Candolle says of the latter "verosimiliter species propria" and Willdenow before him said "forte species distincta ulterius examinanda." As varieties of one species these plants have both been called Nitraria Schœberi (*Linn.*) As distinct species the name Nitraria Schœberi must be left to the supposed variety Sibirica, since it was first applied to this, by Linnæus, as I believe. In the preceding list then, let Nitraria Schœberi (*Linn.*) be identical with Nitraria Sibirica (*De Cand.*) notwithstanding the application of this name to Nitraria Caspica in this place. A species of Nitraria is said to be employed in the Northern States of Africa as a medicine, on account of the Nitrate of Potassa, which it contains. But Persoon says of Parietaria Cretica, another Niter-producing plant, that its habitat is "In Cretaet ad Mare Caspicum." William Withering says of Parietaria officinalis (*Linn.*) "I have been informed, contains a considerable quantity of Nitrate of Potassa, so that, in making an extract from it, the mass has taken fire." (*Wm. With. Syst.*

Arrang. Brit. Pl. 5th Edit. Birm. 1812. Vol. II. Pg. 260. Note at foot of Page.) "The juice contains Nitrate of Potassa, sometimes in considerable quantities." (*S. F. Gray Nat. Arrang. Brit. Pl. Lond. 1821. Vol II Pg. 254 Ord. Nat. Urticaceæ.*) *Parietaria officinalis* "contains a considerable quantity of Niter." (*Hort. Anglic. Lond. 1822. Vol. II. Pg. 568.*) *Parietaria officinalis* was once much used in medicine, and its effects were those of Nitrate of Potassa.

ANTIPHLOGISTICA SALINA.

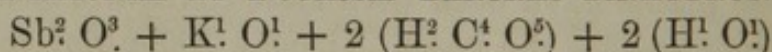
NAUSIATICA, EMETICA, CATHARTICA.

Neuragica, Oræsthetica.



This is according to the best analysis of which I have knowledge; but it is a very old one. If this is the true constitution of this salt, it is an extraordinary compound. If there were one equivalent more of one or the other of the two Acids, there would be just three equivalents of the Acids to one equivalent of base, thus making a *Terni-Salt*; but perhaps this might not render the composition any more probable.

ANTIMONIÆ ET POTASSÆ TARTRAS BINHYDRUS.

POTASSÆ ANTIMONAS, $\text{K}^1 \text{O}^1 + \text{Sb}^2 \text{O}^5$.SODÆ ANTIMONAS, $\text{Na}^1 \text{O}^1 + \text{Sb}^2 \text{O}^5$.MAGNESIÆ ANTIMONAS, $\text{Mg}^1 \text{O}^1 + \text{Sb}^2 \text{O}^5$.CALCIÆ ANTIMONAS, $\text{C}^1 \text{O}^1 + \text{Sb}^2 \text{O}^5$.AMMONII OXYDI ANTIMONAS, $\text{N}^1 \text{H}^4 \text{O}^1 + \text{Sb}^2 \text{O}^5$.AMMONII OXYDI BINANTIMONAS, $\text{N}^1 \text{H}^4 \text{O}^1 + 2(\text{Sb}^2 \text{O}^5)$.POTASSÆ ANTIMONIS, $\text{K}^1 \text{O}^1 + \text{Sb}^2 \text{O}^4$.SODÆ ANTIMONIS, $\text{Na}^1 \text{O}^1 + \text{Sb}^2 \text{O}^4$.MAGNESIÆ ANTIMONIS, $\text{Mg}^1 \text{O}^1 + \text{Sb}^2 \text{O}^4$.CALCIÆ ANTIMONIS, $\text{Ca}^1 \text{O}^1 + \text{Sb}^2 \text{O}^4$.AMMONII OXYDI ANTIMONAS, $\text{N}^1 \text{H}^4 \text{O}^1 + \text{Sb}^2 \text{O}^5$.AMMONII OXYDI BINANTIMONAS, $\text{N}^1 \text{H}^4 \text{O}^1 + 2(\text{Sb}^2 \text{O}^5)$.ANTIMONIÆ ANTIMONAS, $\text{Sb}^2 \text{O}^3 + \text{Sb}^2 \text{O}^5$.

As respects the number and proportion of its elements, this salt is just equal to two equivalents of Antimonous Acid ; and yet I am informed by an able chemist that it is quite a different thing, with different habitudes and relations.

ANTIMONIÆ ANTIMONIS, $\text{Sb}^2 \text{O}^3 + \text{Sb}^2 \text{O}^4$

NAUSEA PROTRACTA AB ANTIPHLOGISTICIS NAUSIATICIS.

Continuous and protracted nausea produced by articles otherwise Antiphlogistic very much augments the Antiphlogistic effects of such agents ; but continuous and protracted nausea produced by non-Antiphlogistic agents can hardly be made Antiphlogistic, though it may be more or less exhausting ; but the consideration of this subject belongs elsewhere, and is soon to be treated of.

ANTIPHLOGISTICA ÆTHEREA.

Euphrænica, Sub-oræsthetica, (Neuragica ?)

{ ÆTHEROGENII PROTOXYDI HYPONITRIS.
 { *Æther Hyponitrosus.*
 { *Spiritus Nitri dulcis.*
 { FORMICIGENII TRITOCHELORIDUM.
 { *Chloroform.*

Some writers on the materia medica consider Water as an Antiphlogistic ; as Pearson for example ; and even those who do not specify it, in their list of Antiphlogistics, usually hold it as such. Thus John Murray says—"if a draught of cold water be swallowed, the sensation of cold it produces in the stomach is equivalent to a partial abstraction of Stimulus, which being extended by sympathy to the heart, occasions a reduction in the force of the circulation, and by this, or by a similar sympathetic affection, causes a sensation of cold over the body." As I have already said, the essence of an Antiphlogistic operation is the diminution and abatement of preternatural vital energy and strength of action in the circulating system. Now articles that merely produce "a sensation of cold" are not Antiphlogistics, nor are they Refrigerants in the full sense of this latter term, as it is employed by all the writers upon the materia medica, who, within my knowledge, employ this term as the name of a class of remedial agents. But Water possesses no exhausting power, either in health or disease, and therefore it can not possibly be Antiphlogistic. If it were so, or if it were exhausting, it could

not possibly be employed as it is, for the purpose of mere drink, and for the mechanical offices which it constantly performs within the animal economy. Any decided Antiphlogistic however feeble in its powers, used as Water constantly is, would inevitably do irreparable mischief. But cold Water may be reckoned a mere cooler whenever there is preternatural heat, whether in phlogistic or atonic diseases, and it cools without diminishing vital energy and strength of action on the one hand, or increasing it on the other. I trust that I have already shown satisfactorily that Antiphlogistication does not by any means consist in cooling, and that there may be cooling without any degree of Antiphlogistication, or of exhaustion short of Antiphlogistication. The Antiphlogistics abate phlogistic diathesis, and they obviate the heat of this diathesis merely because heat is one of the individual component parts of it. Articles that will not abate this diathesis generally, will not abate the heat of it. Mere heat is very far from constituting the essence of this diathesis; it is merely a part of it, and it is a part probably of the least consequence of any of the aggregate of symptoms, of which it is composed. Heat, and very intense heat too, is often a part of the exquisitely atonic diathesis; and like the heat of phlogistic diathesis, it is incapable of being obviated, except by agents that obviate the general diathesis of which it is a part. In this case, the Antiphlogistics increase instead of diminish the morbid heat that may happen to exist. Cold Water, in disease, is mainly useful to allay morbid thirst, which is often found in the atonic diathesis, in which it is in general as effectual as in the thirst of the phlogistic diathesis. There are some exceptions to this however. I have seen epidemics of some exquisitely atonic diseases, in which Cold Water scarcely had any influence upon the thirst, but in which Brandy only moderately diluted was quite effectual for this purpose; so that even Cold Water is by no means a specific for thirst.

Artuum Ligaturæ, or *Ligatures upon the limbs*, have sometimes been considered as constituting an Antiphlogistic process; but, as I am quite sure, erroneously. Their application continued for any safe length of time, by no means exhausts either in a moderate or an intense degree; and their employment is utterly incapable of abating phlogistic diathesis to the least extent whatever. Some other place therefore must be found for the operation and

effects of this process, beside among the Antiphlogistics. Obviously this is not the place for the consideration of this subject.

INDICATIONS FOR THE USE OF ANTIPHLOGISTICS.

Although John Murray gives us clearly to understand that, by the term Refrigerants, he means what were formerly called Antiphlogistics, his list of Refrigerant agents being indentical with that of the Antiphlogistics of all authors, who have employed the latter term, yet unquestionably misled by the import of the term Refrigerants, he makes the following statements in regard to the indications for the employment of this class of remedies. He says "it is evident that the indication to be fulfilled by the use of the Refrigerants is the reduction of morbid heat." "Hence the propriety of their administration in Synocha, and other pure inflammatory" (meaning phlogistic, sthenic or entonic) "diseases, and" (also) "in Typhus, in all of which the temperature of the body is increased, though from different causes." Now I should have supposed that the recognition of this last fact, for it is undoubtedly a fact, viz. that the preternatural heat of Synocha and Typhus is due to different causes, would have led to a suspicion at least, that it could not be proper to treat both in the same manner, and with the same agents. Murray says, "in inflammatory" (phlogistic) "diseases, being so much more rapid than usual, a greater quantity of blood is sent through the whole body, and through the lungs, in a given time; and the usual alterations of the blood taking place, the evolution of caloric, which is the consequence of these alterations, must be increased, and the temperature raised." "In such cases, the use of Acids, by lessening the disposition of the blood to consume Oxygen in the lungs, may be useful in reducing the temperature, and Nitrate of Potassa may be of advantage, as it diminishes the force of the contractions of the heart." "These means however can have only a trivial effect compared with those evacuations, by which the force of the circulation is lessened." Murray continues — "The increased temperature in Typhus seems to be owing to the absorption of the animal solids" [the animal solids are absorbed as rapidly and as extensively in Synocha as in Typhus]—"which containing comparatively little Oxygen, causes the blood to consume more of it in the lungs." "The introduction of Acids into the system, by affording this element in a concrete state, to that mat-

ter, will lessen the consumption of it in respiration, and of course moderate the morbidly increased temperature." "In either of these forms of disease therefore Refrigerants may be useful; and accordingly we find them generally used in all the species of febrile affection, though they are still to be regarded as medicines of weak power." I consider it certain that "the reduction of morbid heat" is not the principal indication to be fulfilled by the use of the Refrigerants, or in a better term the Antiphlogistics. It is rather the abatement of phlogistic diathesis in all its parts, the essence of which is morbidly increased vital energy, and morbidly increased strength of action in the sanguiferous system, and not morbidly increased heat. It is true there is morbidly increased heat in phlogistic diathesis; but this is not the essence of it. This morbidly increased heat cannot be obviated till the morbidly increased vital energy and strength of action is obviated; and when the latter is gone, the former necessarily ceases to exist. The morbid heat is a small and comparatively unimportant part of phlogistic diathesis. Whatever relieves phlogistic diathesis generally, always relieves the heat of it; but mere coolers which do not diminish vital energy and strength of action such as cold water, render no material, or at least no essential or permanent service. The propriety then of the Antiphlogistics in Cauma or Synocha, and in all pure phlogistic diseases, does not depend upon their being coolers, but upon their power of abating phlogistic diathesis generally, and especially upon their power of diminishing morbidly increased vital energy and strength of action. This mistaking of one of the effects of the Antiphlogistics, and by no means their most important effect, for what is in reality their principal operation, has occasioned much bad practice. Authors have thus lost sight of the manner in which the Antiphlogistics really produce their remedial effects, and, like John Murray, have erroneously supposed that they must be just as appropriate to the treatment of Typhus, as to that of Cauma or Synocha, because they are injudiciously called Refrigerants, and because there is often morbid heat in some cases of Typhus, as well as in Cauma. But the morbid heat of true Typhus, of whatever species, is never connected with any phlogistic diathesis, but always, on the contrary, with more or less atonic diathesis. It is true that there are some mere irritative diseases intirely destitute of phlogistic or atonic

diathesis, which are attended with morbid heat; but the Antiphlogistics are as inappropriate to such cases as to Typhus, though they do not produce as much injury in them as in Typhus. But neither John Murray, nor any other author with which I am acquainted, pretends to inculcate that the heat of Cauma, Typhus and mere irritative diseases, is occasioned in the same manner, or connected with the same state or condition of the system. On the contrary, I believe it would be admitted that the morbid heat of these three sorts of diseases, is altogether of a different character, and the effect of different causes; but then as the notion is, Refrigerants, i. e. coolers, cannot fail to be appropriate in all these cases, and Antiphlogistics being Refrigerants or coolers, must be the remedies proper and necessary. In several places, the principles on which this discrimination in relation to the use of the Antiphlogistic depends, are recognized, and even actually laid-down by the most commonly received authors upon the materia medica, as Duncan, A. T. Thompson, Edwards and Vassasseur, and even by Murray himself; and yet, to all appearance, all of these gentlemen, able and deservedly distinguished as they are, seem to be misled by the etymological signification of the term Refrigerant i. e. cooler, and then what is called in medicine a theory is formed, and this fixes and justifies the practice, however absurd and bad it may be. Next, it is handed down from author to author, and at last the profession is in dutybound to receive and adopt it, because there is so much authority in its favor. I doubt not in the least, indeed I am quite confident that a considerable portion of the contents of our books rest upon just such a foundation, and upon nothing else. This is one of the modes in which words become things. As to John Murray's chimerical theory in regard to the different modes in which the Antiphlogistics operate in Cauma and Typhus, it is too obviously absurd either to require or to merit a refutation.

It must be observed that the Antiphlogistics are medicinally incompatible with the Antisbestics, and the Tonics, and that when the first are really and truly indicated, the second and third can not possibly be, and vice versa. I have very often heard it maintained that when medicines of opposite and incompatible powers are given in conjunction, if they are in equivalent quantities, they will just neutralize each other; and that if one

predominates, only a certain degree of the effects of that article will be produced. For my part, I am utterly incredulous of this dogma. In an exquisite case of true Cauma or Synocha for example, I do not believe that it would be possible to give a sufficient quantity of Nitrate of Potassa, or Tartrate of Antimonia and Potassa, or of both in conjunction, or of any other Antiphlogistic, to counteract the ill effects of an efficient use of Alcohol or of Disulphate of Oxyd of Quinum, and much more of both in conjunction. On the other hand, in an intensely atonic disease, as in Typhus Carcerum, or any other equally atonic species of Typhus, whether of the nervous or putrid type, I do not believe it possible to counteract the ill effects of an efficient use of Nitrate of Potassa and Tartrate of Antimonia and Potassa, by any amount of invigorants, either from the class of Antisbestics or the class of Tonics that any patient can possibly swallow. In both sets of cases, the inappropriate remedy will inevitably do more or less injury, in defiance of the freest possible use of the truly appropriate agents, though in all probability, not quite as much as it would do, if not accompanied with the appropriate remedies.

Antiphlogistics are indicated, and are in fact indispensable in all phlogistic, sthenic or entonic diseases; and they are very frequently employed, though very rarely with advantage, in the early stages of diseases, that are neither entonic on the one hand, nor materially atonic on the other. Such cases will usually tolerate them; but according to my belief, a belief founded upon my own observation and experience, it is much better to treat such diseases with mere counter-agents, that is, articles which are neither Antiphlogistic nor Antisbestic, than to reduce the patient to a decidedly atonic state by Antiphlogistics, for the sake of rendering him capable of being benefitted by Antisbestics and Tonics. In all non-phlogistic and non-atonic diseases, patients will always bear (as physicians are in the habit of saying) either moderately exhausting, or moderately invigorating remedies, and if they are only *borne*, this is commonly esteemed as satisfactory evidence, not only that they are proper, but that they are useful, or even necessary. Even positively but moderately malignant diseases, will sometimes *bear* (as the favorite term is) small quantities of moderately exhausting medicines or processes, such as Hyponitrite of Protoxyd of Etherogen, or in other words Spiritus Nitri

dulcis, moderate Catharsis, moderate Depletion of Blood, etc., and therefore, in the opinions of many physicians, they are proper, useful and often necessary.

Nothing is more common than that a patient with Typhus nervosus should be dosed with the weaker Antiphlogistics, such as the vegetable-organic and the chymical-inorganic Acids, the Carbonates of the metallic Alcalies, the neutri-saline effervescing mixtures, Hyponitrite or Protoxyd of Etherogen; and this even at a time when invigorating agents are confessedly indicated. I have seen each one of the above-named agents given in conjunction with Wine and even Alcohol, which were considered to be positively needed, and even required. Because this sort of stuff does not obviously and palpably do immediate and pressing mischief, and because many patients *bear* it, and yet ultimately recover after it, it is commonly supposed that there is a vast accumulation, and a vast weight of testimony in its favor. If any practitioner of medicine happens to have investigated this practice, and found as a result of comparative trial, and comparative observation, that the use of these Antiphlogistics in such cases, is not only useless, but positively injurious, and publishes a statement to this effect, he is immediately pronounced a mere theorist, and denounced as destitute of all practical skill in the medical profession; and all this is commonly considered as abundantly proving the utility, in fact the indispensable importance of the practice in question.

After evacuants, I have been in the habit of seeing more mischief done, in atonic diseases, by the metallic Alcalies and their Carbonates, than by any other of the Antiphlogistics. The false dogma of so many that they are Tonics, and the circumstances that their ill effects are not manifest immediately after a single dose, or even after a short series of moderate doses, and also that these effects take place gradually and insidiously, together with their reputed Antoxyntic power, leads to their continuous and protracted use, and induces the prescriber to attribute the ultimate ill effects, which they produce, to the disease, rather than to the real and true cause, viz. the Alkali.

By late writers on the materia medica, more especially since the last change in the general diathesis of our diseases as respects entony and atony, the power of almost if not quite every agent

of this class, except Depletion of Blood, has been very much undervalued, in part (as would seem) from a false chymical theory, and in part from the observation of their effects in pure atonic diseases merely. Every Antiphlogistic, which has power sufficient to affect a person in health, invariably produces a greater or less diminution of the vital energies and of the strength of action, when given in operative quantities, and for any material length of time, both in non-phlogistic and non-atonic diseases, and in positively atonic ones. Those exhausting agents, which will not abate phlogistic diathesis, but which will debilitate persons in health, and still further exhaust in atonic diseases, though not Antiphlogistics should be studied and understood, that they may be avoided, both where they will render no service, and where they will do injury. There are agents which will not exhaust in health, but which will do so in atonic diseases. It is important that these also should be well known; but they cannot be treated-of among the Antiphlogistics, because they possess no Antiphlogistic power.

It is a prevalent opinion with some, that there are some agents which are Antiphlogistic, that is, exhausting in phlogistic diathesis, but not in health, or in atonic diseases. Of these Water is commonly mentioned as an example. I have already denied any exhausting power to Water, and the same may be done to every thing else supposed to have such a character. The habitual use, even of the weakest Antiphlogistics, as Carbonic Acid for example, and much more the vegetable-organic Acids, the Carbonate of Soda, neutri-saline effervescing mixtures however made, and above all, what within my knowledge, is called Seidlitz Powders, and which is Tartrate of Potassa and Soda, or in other terms, Sal Rupellense or Rochelle Salt, one of the most active of the Antiphlogistic purging Salts, will in the event invariably do more or less injury, and they will not injure with any the less certainty, because in many instances they do it slowly and insidiously. "A continual dropping will wear away rocks."

Since the change in the diathesis of our diseases, which took place between the years 1805 and 1812, as early as the former in some places, and as late as the latter in others, there has been little occasion for this class of remedies. It is highly important however, that their power should be well understood, in order to

avoid an improper administration of them, if for nothing else. But should our diseases again become phlogistic, as it is not improbable that they may, the same depletory, evacuant and Antiphlogistic course would be as indispensably necessary, and be attended with the same highly beneficial effects, as in the days of Sydenham.

PROËM TO THE CLASS NAUSIATICA.

It will be recollected that according to my plan, there are to be just as many classes in the materia medica, as there are peculiar and distinct powers, operations and effects. Now it is not to be expected that every power should be of equal importance in the practice of medicine. The importance of the several powers must obviously be very various, from those which are comparatively of little value, to those which are of the greatest value of the whole. The class of which I am now about to treat, I consider as one of the comparatively unimportant ones, but still it is founded upon a peculiar and distinct power, whose operations and effects are highly useful in all truly phlogistic or entonic diseases. This is unquestionably sufficient to entitle it to a brief consideration at least.

The term Nausiatica is derived from *Ναυοιάω*, ω. to be nauseated, and this verb is from *Ναυσία*, ας, ή. sea-sickness. I do not know that such a word as *ναυσιατικός* exists in Greek, but as appears to me, such a word is a regular formation from *ναυοιάω*.

Definition.—Nausiatica are articles which produce a distressing variety of common sensation called nausea, having its seat in the stomach, always attended with a loathing of all food, and a tendency to vomiting, and always aggravated by motion and exertion. It is often produced by the motion of a ship in a high wind; by riding in a carriage so hung upon its springs that its motions resemble those of a ship in a high wind; by swinging; by whirling; etc. It is often but not always a precursor of vomiting; often but not always a symptom of pregnancy; and it occurs very frequently as a symptom of various diseases, as for example, of all

the species of Gastritis, of all the species of Enteritis, often of all species of Diarrhoea, if there is more than one species, etc.

The continuance of nausea for any material length of time, especially if it is at all intense, is productive of a greater or less degree of positive and direct exhaustion of all the parts dependent upon the nerve of chymical action, nutrition and reproduction, commonly called the great sympathetic nerve. The definition of Nausea, as given by Sauvages, Von Linné, Vogel and Sagar, are far more applicable to retching than to Nausea. In fact they are not at all applicable to Nausea, while they are applicable to retching, as far as they go. Sagar's definition is wholly made-up of Sauvages's general remarks, subsequent to his definition. "Nausea." "Inanis vomendi conatus et flatus tantum per os emissio ; affinis est aëri fluxibus." (*Sauvages.*) "Nausea." "Rejectionis cibi flatuumque conatus inanis." (*Von Linné.*) "Nausea." "Inane vomendi desiderium." (*Vogel.*) "Nausea." "Inanis vomendi conatus, et tamen maximis licet nisibus nihil nisi flatus aut mucus exiguus exantlatur ; adest anorexia, gastrodynia, motus ossis hyoïdis ad superiora, ptyalismus ; capitis ad anteriora inclinatio, tremor labii inferioris, etc." (*Sagar.*) "Nausea est vomendi cupiditas et conatus frustraneus, seu qui respectu ad effectum spectatum maximus est ; ita et cum summis nisibus nil nisi flatus, aut quid paucum vomitum rejiciatur, et omnia assumenda fastidiantur." "Phænomena Nauseam comitantia sunt cardialgia, sensus ingratus ventriculi, aversatio seu abominatio intima ex hoc sensu oriunda, protensio capitis ad anteriora, oris diductio, motus ossis hyoïdis ad superiora, sonitus singularis expiratorius, depressio simultanea epigastrii, sæpe salivæ uber proventus, labii inferioris tremor ; et cum vehemens est morbus, virium imminutio, brachiorum ad posteriora protensio, corporis totius ad anteriora inclinatio, etc. omnis potus et cibi oblatus aversatio, et deglutiendi impossibilitas." (*Pg. 177, Col. 1. et 2. Tom. 2. Nosol. Method. Francisci Bossieri Sauvagesii Venetiis 1773.*) "Nausea definitur a Corræo per causam, scilicet depravatus motus facultatis expultricis, quo nititur per os excernere quæ ventriculo sunt molestæ ; quod est tenesmus respectu dejectionis ; tussis respectu anacatharsis ; sternutatio respectu coryzæ ; id tussis erga vomitum est ; et dysuria respectu enuresis ; et dystocia respectu partus." "Nausea differt a vomitu quod

in vomitu uber sit materiæ rejectio respectu conatum, in Nausea vero minima." "Differt a vomituritione, quæ vomitum aliosque morbus comitatur, quia in Nausea pro morbo essentiali spectata, inanis vomendi conatus sit constans et præcipuum symptoma, non ita in alia syndrome; ex principiis constantioribus diversamque medelam exigentibus deducuntur Nauseæ species seu varietates." (*Pg. 177, Col. 2. Tom. 2. Nosol. Method. Francisci Boissieri Sauvagesii Venetiis 1773.*)

There is no need of a specific criticism of these several definitions, since it will be sufficient to state what can not but be obvious, on a little consideration to every one, who has a correct knowledge of physiology, that nausea is a pure and unmixed sensation, not necessarily involving the least motion of any sort. The most intense nausea that I ever heard complained-of was unattended with the least action or motion. It seemed to transcend all action or motion, so much so that the patient felt as if retching and vomiting would be a great relief. I have said that the most intense nausea that I ever heard complained-of was of this character; but it is proper to state that I did not judge of its intensity wholly by the complaint of the patient, but in part by the prostration and also by the exhaustion which it produced. Here let it be remarked that I do not intend the same thing by prostration and exhaustion, as too many seem to do. The former implies a latent state merely of the vital energies, while the latter implies an extinction of them to a greater or less extent. Prostration may be compared to a spring under so great a weight as to be incapable of action, while exhaustion may be illustrated by a spring deprived of its elastic power. Every one at all well acquainted with the physiology of the nervous system very well knows that all sensation depends wholly and intirely upon nerves of sensation, and that action or motion depend wholly and intirely upon nerves of motion, and also that nerves of sensation and nerves of motion are distinct systems of nerves, not necessarily associated in their functions, but capable of association under certain circumstances. Now nausea undoubtedly has its seat in nerves of common sensation and consequently it is one of the numerous varieties of common sensation. On the other hand, retching appears to me to have its seat in the external involuntary motor nerves of expression,

and so, very certainly, has upward peristaltic action of the œsophagus and stomach, below which we have no actual knowledge that involuntary motor nerves of expression are ever sent to the alimentary canal. Vomiting as an action, consists in upward peristaltic action of the stomach and œsophagus, very often, though not necessarily, with a spasmodic action of the external muscles of expressory and respiratory motion associated. As I have already said, this upward peristaltic action of the stomach and œsophagus, and this spasmodic action of the external muscles of expression, commonly called retching, depends upon the involuntary motor nerves of expression. The upward peristaltic action of the stomach and œsophagus is often called inverted peristaltic action, but I think improperly, since it is performed by the instrumentality of a nerve which never produces downward peristaltic action, and since the downward peristaltic action is produced by the instrumentality of a nerve which never produces upward peristaltic action. It is a great mistake therefore to consider nausea as an incipient degree or an imperfect grade of vomiting. That this can not be correct will at once be perceived when we observe what I have just inculcated, viz. that nausea is a variety of common sensation, and as such, must have its seat in nerves derived from the two posterior columns of the spinal cord, while retching and vomiting are involuntary motions or actions of the expressory system of nerves, and as such, must have their seat in the nerves derived from the medullary or nervous tract situated between the two posterior and the two anterior columns of the medulla oblongata and of the spinal cord. A nauseant power is certainly different and distinct from an emetic power, since the former has its seat in a nerve or nerves of common sensation, while the latter has its seat in nerves of expressory motion. Should not a nauseant power then very clearly constitute the foundation of a class in the *materia medica*?

Nauseation is commonly considered as a remedial process, and is much employed for this purpose, by many practitioners. As a remedial process it directly produces languor and lassitude; directly diminishes appetite, hunger and digestive power; directly lessens vital energy and strength of action in the sanguiferous system; and in all probability, in all the other subordinate parts dependent, for their motive power, upon the nerve of

chimical action, nutrition and reproduction. As we have no direct measure of the strength of the action of any other of the parts dependent upon the great sympathetic nerve, except the sanguiferous system, we infer that their vital energy, and their strength of action is impaired, whenever their functions are impaired, in connection with a diminution of vital energy and strength of action in the sanguiferous system. As nausea is a mere variety of common sensation, it may perhaps be asked how it produces the effects just detailed? Whenever any man will show me how Nitrate of Potassa directly exhausts; how Tartrate of Antimonia and Potassa directly exhausts in certain doses and quantities, and how it vomits in certain other doses and quantities; how sulphate of soda exhausts and purges; how Dichlorid of Mercury salivates and purges; how Strychnos Nux-vomica produces Tetanic paroxysms; how Papaver produces somnolency and actual sleep, etc. I will undertake to be ready with an answer to this question.

Intense nausea without vomiting is not infrequently accompanied by the following aggregate of symptoms, viz. sensations of extreme languor and lassitude; anxiety, restlessness and jactitation; slight rigors; vertigo, particularly on exertion or motion; faintness; some affection or disturbance of the special senses, more particularly of vision; livor of the lips, and other parts covered by epithelium merely; great pallor; coldness of the surface, and particularly cold extremities; cold sweats, often profuse; pulse small, feeble, unequal and otherwise irregular, and sometimes fluttering; weakness and confusion of the intellectual functions, sometimes amounting to delirium, and sometimes even to cerebral stupor. I have specified these symptoms in about the most urgent degree, in which they are ever liable to occur. It must be distinctly understood that they commonly take place in a more moderate and milder form. I have likewise enumerated them in the order in which we commonly take cognizance of them, though it is probably quite a different order from that in which they are actually produced. I do not believe that this group of effects is capable of being produced by any agent whatever in genuine and decided phlogistic diathesis; at any rate I never saw any approach to them under such circumstances, and I think that the symptoms in question are pathologically incom-

patible with the symptoms which constitute phlogistic diathesis. When this aggregate of symptoms exists, there is very little liability to vomiting. In fact, I am inclined to think that vomiting can not be produced merely by giving additional quantities of the article that has occasioned the effects just specified.

I have heard it strenuously maintained that these effects prove Narcotic power in the article producing them ; but I am confident that this opinion is without any good foundation. I have known them produced by Tartrate of Antimony and Potassa much oftener than by any other Agent ; and assuredly this article is not Narcotic. I have known them produced by Protosulphate of Zinc, to which I do not think that any body ascribes Narcotic power. I never knew them produced by Protosulphate of Copper, which I have been very much in the habit of employing as an Emetic, because its operation is never preceded or accompanied or succeeded by much of any nausea, more especially if full doses are given ; because it is very speedy, usually sufficiently thorough, and always kind in its operation. It will be obvious to every one that an emetic with so little tendency to occasion nausea must be very little liable to produce the group of effects under consideration. I have known them produced in an exquisite degree by Cephælis Ipecacuanha unaccompanied by any other agent, which, of all Emetics, I consider as the least likely to occasion such effects. This article however is more likely to be Narcotic than any of the chymical inorganic emetics. I have known them produced by *Urginea maritima*, which, so far as I know, nobody supposes to be Narcotic. I have known them produced by *Lobelia inflata* ; and always when this has happened to be the mode of its operation, I have heard it ascribed to the awfully Narcotic power of the *Lobelia*. But as I shall inculcate in its proper place, I know of no sort of evidence that *Lobelia* is Narcotic in any degree, unless the fact that it sometimes produces a greater or less number of these symptoms, constitutes evidence. I never knew them produced by the Distilled Water of *Ranunculus Flammula*, which I have been much in the habit of using as an Emetic. This article, like Protosulphate of Copper, is speedy in its Emetic effect, and is never preceded, accompanied or succeeded by much of any nausea ; and though it

empties the stomach thoroughly, I have never known it otherwise than kindly done, and without either exhaustion or even prostration. Now I consider it as very certain that none of these agents possess any degree of Narcotic power. I am inclined to doubt whether any decidedly Narcotic Emetic would be capable of producing these effects. I think the sedative effects of a Narcotic would have a strong tendency to prevent and counteract them. At all events Papaver is very effectual for their relief. I never knew them produced by any of those Emetics, which in general, operate speedily, and without being preceded, accompanied or followed by much nausea. I believe that this aggregate of symptoms may be produced by a sufficiently free use of any powerfully nauseating article so managed that it does not vomit.

When I began the study of medicine (as I have already said more than once) the general diathesis was phlogistic i. e. all the acute diseases were of this character. For about two years I saw only such acute diseases treated. During my professional pupillage the general diathesis underwent a change from entonic to atonic i. e. all the acute diseases became atonic. Again when I began the practice of my profession, it was in a place where this change had not fully taken place. There had been a brief epidemic Pneumonitis Typhodes-notha, but when this ceased, the acute diseases which occurred sporadically, were again phlogistic, though probably not as much so, as those before the epidemic above mentioned. I mention these facts to show what opportunities I have had of studying the phlogistic diathesis at the bed-side, and of observing the effects of medicines in it. The medical gentlemen, whose practice I saw, during my pupillage, usually bled their patients once quite freely at the outset of a phlogistic acute disease, and then depended upon common Antiphlogistics, and much more especially nauseating ones, to prevent the rekindling of the phlogistic symptoms. The articles of this character which were most used were Nitrate of Potassa and Tatræte of Antimonia and Potassa, the latter always pushed to greater or less nausea. It is not necessary for me to go into any of the details of the practice of these gentlemen, and I have mentioned thus much of it merely to explain how I was let into the course of investigation of the effects of nauseants, that I am about to mention. On witnessing this practice, the first question

that occurred to me, was whether nausea enhanced the Antiphlogistic effect of the Tartrate of Antimonia and Potassa. I made so many comparative observations, that I thought there was not the least room left for doubt upon the subject. The Antiphlogistic effect of this Salt was decidedly greater when it was pushed to the production of nausea, than when it fell short of this point; and the same was equally true of what is commonly called Pulvis Antimonialis Jacobi. The second question was whether every article that nauseated in an equal degree was equally efficacious for the relief of phlogistic diathesis; and contrary to what I expected, I found that no article, however nauseant it might be, rendered any service, unless it was Antiphlogistic besides. This I verified by numerous comparative observations. It was however equally true that the nauseant Antiphlogistics were much more Antiphlogistic when pushed to the production of nausea, than when they fell short of it. As it always required a greater quantity of an Antiphlogistic to produce nausea, than just to fall short of it, the question arose here whether it was not the greater quantity of the article taken, when nausea was produced, rather than the nausea, that produced the greater Antiphlogistic effect. This question was not as easily answered as most of the other questions relating to this subject; but from the best observations that I could make, my conclusion was that the greater effects were in fact due to both.

At this period of my knowledge of medicine, I had a vague kind of notion that Cephælis Ipecacuanha had a specific sort of febrifuge power in the different species of Typhus, and in the several Typhoid Phlogotica, not febrifuge in the sense in which Cinchona is febrifuge in Intermittent and Remittent, but in an intirely different manner. If I could explain this vague notion, it would not be necessary to do it, since I have habitually met with physicians who seemed to have just about the same ideas and notions upon this subject. Indeed I derived all my notions upon this point from others. I certainly did not originate them. The third question that occurred to me was whether Cephælis Ipecacuanha is more effectual for the purpose under consideration when pushed so as to produce nausea; and here ample comparative trial gave a decision in the negative. It did not occur to me here to make the inquiry whether this agent in any quantity ever

rendered any service at all, in any species of Typhus, or in any of the Typhoid Phlogotica. Some time after I came to the conclusion that the production of nausea in Typhus was injurious rather than beneficial. I found that Wilson Philip had made the same inquiry and come to the same conclusion. As near as I recollect, this gentleman's observations were upon nausea as produced by Ipecacuanha. His results are stated in few words, I believe in the first edition of his work on Fevers.

Pure nausea, if sufficiently continued, is always more or less of an exhausting process; and as occasioned by certain articles, it is sufficiently exhausting to be efficiently Antiphlogistic. Unless however it is produced by articles that are more or less Antiphlogistic independent of producing nausea, this process cannot be much relied-on for the relief of any thing like phlogistic diathesis. This same degree and quality of nausea is however sufficiently exhausting to impair the tone of health, and to aggravate very greatly a decidedly atonic disease. These results I arrived-at, as the effect of trial, observation and experience, and mostly from the recommendations of distinguished members of the medical profession. My first observations on the effect of nausea in atonic diseases, I endeavored to make with the purest nauseants that I could find. It is well known that there are very many articles in the materia medica, that have a strong tendency to produce nausea, and that are in fact very liable to produce it, which however do not vomit readily, and are altogether incapable of being used with any convenience and advantage as Emetics. These articles, provided they are not materially Carthartic, and much more if they are not Cathartic at all, are the best agents for the production of pure and intense nausea. An article may be nauseating with but little tendency to vomit; but if it is decidedly Cathartic, this circumstance will be an objection to its use for the production of nausea merely.

Euphorbia Ipecacuanha has been strongly recommended by high medical authorities as a Febrifuge and Diaphoretic in Typhus of all species or sorts; and in the Typhoid Phlogotica. It has been alleged to be just as good in these diseases as Cephaëlis Ipecacuanha, and declared to operate in an analogous, if not in an identical manner. I therefore considered myself as justified in making trial of it. As a result of such trial I found that Euphor-

bia Ipecacuanha is very easily made to nauseate and to almost any degree that may be desired, and this without any great liability to prove Emetic. But it possesses quite an efficient Cathartic power, and cannot therefore be much employed as a nauseant without too much Cathartic effect, and hardly even without the production of actual Diarrhea, and that of an obstinate and rather unmanageable sort. These peculiarities of operation prevent its use as a mere nauseant in atonic diseases, and they do not render it of any value in phlogistic diseases. I also tried several other agents of a similar character to Euphorbia Ipecacuanha, but with no better success than I had with that article, and therefore I shall omit specifications. I subsequently tried various and even numerous other articles, that are in almost universal use as medicines, and with the results subsequently stated.

The most intense nausea that can be produced by Cephaëlis Ipecacuanha, is not sufficiently exhausting to relieve decided phlogistic diathesis, and consequently it can not be said to be Antiphlogistic. Nausea when produced by Protosulphate of Iron can never be Antiphlogistic; but it is more or less exhausting in health; and still more so in atonic diseases. Nausea produced by Protosulphate of Zinc can never be Antiphlogistic, but it is exhausting in health, and still more so in atonic disease. I think that the Salts of Zinc when taken in nauseating doses and quantities, are more exhausting than the Salts of Iron. Nausea produced by Protosulphate of Copper is not Antiphlogistic; but it is exhausting in health, and much more so in atonic disease. The nausea produced by the Disarsenite of Potassa, though not Antiphlogistic, is decidedly exhausting in health and much more so in atonic disease. The nausea produced by Veratrum viride is not Antiphlogistic, but it debilitates in health, and much more in atonic disease. The nausea produced by Sanguinaria vernalis is more exhausting than that of Veratrum viride. The nausea produced by Lobelia inflata is not Antiphlogistic, but it debilitates in health, and much more in atonic disease. I think that Lobelia inflata, when pushed so as to nauseate, is more exhausting than Sanguinaria vernalis. The nausea produced by Urginea maritima is not Antiphlogistic; but it debilitates in health, and much more in atonic disease. I think that Urginea maritima, when pushed so as to nauseate, is more

exhausting than *Lobelia inflata*. I have never seen *Colchicum autumnale* employed in nauseating doses and quantities, in any decided phlogistic disease; but from the extent to which it exhausts in atonic disease, and even in health (for I have seen it employed in mere local affections, when there could not be said to be any constitutional disease) I should think that it might be capable even of proving Antiphlogistic though I never witnessed this effect from it. After trying many articles for the production of nausea for medicinal purposes, I came to the conclusion at last that nothing but Tartrate of Antimonia and Potassa could be relied-on for this operation, at least with any degree of uniformity, or with any considerable intensity. But there are many cases, in which the production of nausea is recommended, but in which the Tartrate of Antimonia and Potassa would be inadmissible on account of its exhausting effects.

A catalogue of the Nausiatica or Nauseants would coincide mainly, but not intirely, with a catalogue of the Emetica or Emetics. It would comprise a few articles that are Nauseating without being sufficiently Emetic to be employed for this purpose, while it would exclude all those articles, which produce their emetic effects suddenly, without being preceded, accompanied or succeeded by much, if any nausea. Instead of such a catalogue, I shall mention the pathological conditions, and the specific diseases, for which the production of nausea has been commonly commended and employed.

For several years I watched the effects of nausea, with all the care and attention in my power, and I was never able to detect any thing but prostration and a greater or less amount of exhaustion in conjunction, from the more active and especially from the Antiphlogistic nauseants. Every other effect that I have ever heard ascribed to nausea, I have invariably failed of producing.

In *Cauma vulgare*, and in all the entonic phlegmonous Phlogistica, I do not think that there is room to doubt the utility of nausea, provided it is produced by the right agents. I never however found it to answer the desired purpose unless it was produced by articles that were Antiphlogistic in doses and quantities that fell short of the production of Nausea. But pushing these articles to the nauseating point I have always found to enhance their efficacy as Antiphlogistics.

In all the several species of Typhus, and in all the Typhoid Phlogotica (as I have elsewhere said) I have long been convinced, as the result of comparative trials and observations, that the production of nausea is not serviceable, but positively injurious.

I have known the production of nausea for relieving the disease called by John Mason Good, *Dipsosis avens*, for which to all appearance it proved effectual for the time being, even if it did not effect a radical cure; for the patient had no desire for drink, while under intense nausea. But I have had no opportunity to treat a sufficient number of cases of this disease, to acquire a knowledge of the real *juvantia* and *lædientia*. In fact all the cases, that I ever had to treat, were but moderate ones, so much so, that I never had opportunity to ascertain the true pathology of the disease, or to decide upon the best mode of treating it.

The production of nausea has been recommended as a remedy for *Limosis avens*. As I never had a very decided case of this disease to treat, I can say nothing of its proper remedies, from my own experience. Under strong nausea however, I should not think that any patient would desire food. Whether this nausea would be a mere temporizing one, or more radical in its operation and effects, I am unable to form even a probable conjecture, without more knowledge of the pathology of this malady than I ever had means of obtaining.

I have known one physician of high character and standing, who attached considerable importance to the use of Tartrate of Antimonia and Potassa in nauseating doses, in *Limosis Dyspepsia*. At one period I was well acquainted with this gentleman's views, and the grounds, on which he employed this practice, so singular to me; but at the present time, I have pretty much forgotten them. I am not now sure whether he considered the nausea as an essential part of the practice or not, though I think that he did, since I believe he commonly pushed the Tartrate of Antimonia and Potassa to its production. This gentleman was himself a dyspeptic subject, and the fact that he treated himself in this manner furnished sufficient evidence of his conviction that it was at least a good method. I have often seen this gentleman's cases, and was well acquainted with himself; but I never met with any good reason for adopting his practice, instead of other

modes that I have seen employed by other practitioners, or that I have used myself.

The production of nausea is relied-on by some physicians for the relief of the pain of Chololithus meäns; but I know of no reason to believe that this measure ever renders the least service in this disease.

The production of nausea has been recommended for facilitating and expediting the passage of renal calculus through the ureters. It is supposed by some to occasion relaxation and dilatation of the ureters, and thus to relieve the pain very materially and essentially. For myself, I have never accomplished anything in this disease by this measure. Papaver very freely administered is the only agent with which I have ever been able to do any good in such cases.

Nausea is often produced for the purpose of relieving cough; but if nothing was conjoined with it, I never knew the least benefit to result from it, in any case whatever. I have often employed it in my own practice, and I have much oftener witnessed its operation in the practice of my friends and acquaintances. Where a certain amount of Papaver has been conjoined with it, I have very often found that the Papaver did not render near as much benefit to the cough, in conjunction with the nauseant, as the same quantity of Papaver would render, when unaccompanied by the nauseant.

Nausea has been recommended as a palliative, and even as remedial in Bex theriödes or Whooping-cough. I have certainly tried it often enough to have witnessed benefit from it, if it is capable of producing any; and yet I never saw the least service from it. I know of nothing in the pathology of this disease, that at all indicates this measure, or that would lead to a suspicion that it is capable of doing good.

Nausea is often produced for the purpose of promoting expectoration commonly so called. This effect I never saw it produce, except in entonic Phlegmonous Pneumonitis (viz. Pneumonitis Caumatode Phlegmonea) but when employed alone, I have known it, in a considerable number of instances, to arrest expectoration wholly and intirely, which was gradually restored on suspending the nauseating process. I have not infrequently been called in consultation, when it was considered to be an indica-

tion to promote expectoration in some atonic disease, and when the physician in attendance was endeavoring to accomplish this purpose by various articles, with nauseating agents conjoined. I have invariably facilitated the process, and promoted the desired object, when I have caused a suspension of the nauseating articles.

Nausea is very frequently produced for the relief of Dyspnœa exacerbans, more especially, and also for the relief of Dyspnœa continua. In the early part of my practice, and before I had learned anything of these diseases from observation and experience, I tried the production of nausea very thoroughly alone and by itself, but without the slightest benefit, till at last my patients began to require that I should do something else. The two nauseants that I most commonly employed were Tartrate of Antimonia and Potassa, and *Urginea maritima*, from both of which, this process seemed to be equally without beneficial effect. I mention this particularly, because at that time I had no precise and definite knowledge of that operation and effect, which I have since called Adenagic, and which certainly does produce some benefit in these two species of disease. After I understood this subject better, I again tried *Urginea maritima* in doses short of the production of nausea, but repeated as frequently as could be tolerated without purgation, and this method of managing this article certainly did render some service, though not sufficient to establish the practice with me. In short, I became satisfied that pushing *Urginea maritima* to the production of nausea hindered what little benefit it seemed capable of producing, when it fell short of nauseating. As to the Tartrate of Antimonia and Potassa, I could perceive no benefit from it, either when it nauseated or when it did not. At a subsequent period therefore, I attributed what little service *Urginea maritima* produced, to its Adenagic power, a power not possessed by Tartrate of Antimonia and Potassa; and later trials of Adenagics in this disease appears to me to have justified this conclusion.

The production of nausea is very often recommended in Asthma; but by the term Asthma, practitioners of medicine in the U. S. A. seem to mean what all the nosologists call Dyspnœa. In truth Dyspnœa and Asthma, though they agree in certain respects, yet they differ materially in others. Dyspnœa, as

a disease, is perfectly continuous, but irregularly exacerbating and remitting; while *Asthma verum* is perfectly paroxysmal and intermittent, the paroxysms being only exacerbating and remittent. *Dyspnœa* is a perfectly chronic disease, exacerbations and all; while *Asthma* is a chronic disease with exquisitely acute paroxysms. An *Asthmatic* subject frequently has only a single paroxysm a year; while a *Dyspnoïc* subject usually has a multitude of exacerbations within the same period of time. *Dyspnœa* is a disease of very frequent occurrence; while *Asthma verum* occurs very rarely. I have had opportunity to treat a multitude of cases of *Dyspnœa*, but never yet had a case of *Asthma* under my charge. I have however seen a case or two, that were the patients of other physicians, who however did not recognize the disease. These facts will furnish sufficient reason why I should not know the effects of nausea upon this malady. However, as far as I can judge from the analogy of *Dyspnœa*, I do not believe that nausea would be of the least service in *Asthma verum*.

Nausea has been recommended as remedial of *Sternalgia suffocativa*, sometimes called *Asthma dolorificum*, sometimes *Syncope Anginosa*, but more commonly *Angina Pectoris*. I never saw it tried but once in this disease, and then it was decidedly injurious. I should not suppose, *a priori*, that any thing either prostrating or exhausting could be useful in this disease.

I have known the production of nausea employed in the treatment of *Mania*, but never with any permanent beneficial effects, nor so far as I now recollect, with any temporary or transient ones.

Nausea is often produced for the relief of spasm. In the early part of my practice I often employed it for this purpose, not only in a moderate, but in an intense degree; but when it was depended-upon exclusively, I never had any reason to conclude that it rendered the least service. When other more appropriate means were conjoined with it, the benefit was usually in proportion to the amount of those means employed.

Nausea has been recommended for *Entasia Convulsio*, and *Entasia Hysteria*.* I have not only seen it tried, but have often

* Good puts these diseases with *Epilepsia*, in his genus *Sypspasia*. But *Epilepsy* consists of clonic spasms, while these are a variety of tonic spasms. They should therefore be transferred to the genus *Entasia* and associated with *Trismus*, *Tetanus*, etc., and I have so arranged them.

prescribed it myself, in the latter; but I never yet saw this disease in any degree benefited by it. In the former malady I have seen it prescribed, but never advised it myself. As the laws of *Entasia Convulsio* differ materially from those of *Entasia Hysteria*, and differ in such a manner as to make it more difficult to decide upon the *juvantia* and the *lædientia*, I cannot pretend to speak with the same certainty in this case as the other, in regard to the effects of nausea; but still it is my conviction that it does no good, even in this disease. The two maladies are too nearly related for one to be much benefited by treatment that is of no service in the other.

Nausea is often produced for the purpose of suspending Hemorrhage. I have often tried it in obstinate cases, but I never had any reason to conclude that it ever rendered the least service.

I have known physicians who produced Nausea for the relief of Good's *Paramenia difficilis*; but as the remedies mainly relied on for a radical cure of this disease, are of such a different character, indeed so intirely opposite in their effects, I cannot think that this measure can be very well adapted even to the relief of a paroxysm. I never tried it however and can therefore say nothing of it from my own experience. I have certainly received much less testimony in its favor, than in many cases where, notwithstanding testimony, I have been totally disappointed as respects any beneficial effects from nausea.

Nausea is often produced for the purpose of occasioning relaxation, and of facilitating and expediting parturition. In the early part of my practice I made trial of it a few times, without any apparent benefit. Perhaps I did not try it thoroughly; for my patients uniformly preferred the pain of parturition, to any thing like intense nausea. But it was about a year after I began my professional career that I met with an account of the topical application of *Atropa lethalis* or *Bella-donna* for this same purpose, and this I found decidedly preferable.

I have known surgeons who attached great importance to the production of nausea for the facilitation of the reduction both of incarcerated and strangulated Hernia. I never however saw the desired purpose accomplished by this method in a single instance; but after its total failure I have seen perfect success by means of a cataplasm of *Nicotiana Tabacum*, or quite a full dose of *Papa-*

ver and rest for a few hours. These measures, it is true, are more especially adapted to the relief of incarcerated Hernia; but I have known them succede when there were symptoms of strangulation. Assuredly a cataplasm of Nicotiana, the degree of whose effects can be regulated with great accuracy, and a full dose of Papaver, must be quite as safe in strangulated Hernia, as the exhausting process of nausea with an article otherwise highly exhausting; for I never knew any other nauseant than Tartrate of Antimonia and Potassa employed for this purpose.

Nausea I have sometimes seen produced for the purpose of occasioning relaxation, and thus facilitating the reduction of certain dislocated bones. Not having witnessed its trial very many times, I am not entitled to decide upon its efficacy as positively as in many other cases; but when I have witnessed it, any benefit has never been obvious, at least as far as I could judge.

In all the cases for which this process is recommended, but in which I have been able to obtain no benefit from it, all the medical gentlemen of my acquaintance, whom I have seen employ it, always conjoin with it other measures, and other agents, that are of more or less real value in the diseases treated, measures and agents, which, according to my observations and experience, would do more good without nausea, than with it.

It should be distinctly understood and recognized in this place that Nausea is also a disease, as well as a remedial process; but is Nausea ever truly idiopathic, i. e. does it ever occur without being preceded, or accompanied, by some other disease? If it never occurs idiopathically, it must be improper to give it place as a species in nosology. Unless Sea-sickness (*Anglice vulgariter sic dictum*) is idiopathic Nausea, I have no knowledge of any such thing. If Sea-sickness is not idiopathic, of what disease is it symptomatic, i. e. what other disease precedes or accompanies and occasions it? I have formerly been in the habit of supposing that it is symptomatic of some transient disorder of the hemispheres of the cerebrum, occasioned by the motion of a ship in a high wind, etc. but is this opinion correct, or well founded? At present, I have much less confidence in the correctness of this view than I once had. I have myself been severely and protractedly sea-sick (as popular language is) not only at sea, but in an open boat on the river Connecticut; on the land in a close

carriage so hung upon its springs as to have a motion resembling that of a ship; and also from swinging; and I never yet had either vertigo, or any unusual sensations in the head preceding or accompanying the Nausea, or any morbid symptoms of any sort, that can by any means be referred to the head. I have received testimony to the same effect from several physicians, and from numerous non-medical persons. Can there be disorder of the hemispheres of the cerebrum, or of any part of the contents of the cranium, without any symptoms of it? If Nausea or Sea-sickness, so called, can occur in one individual, without any preceding disorder of the contents of the cranium, can any preceding disorder of any of these parts, be necessary or essential to Nausea or sea-sickness? I have known persons to be affected with vertigo in connection with sea-sickness; and some even with delirium also; but, so long as all cases are not attended with either one or the other, or some different symptom equally indicative of disorder of the brain, I can see no propriety in considering Sea-sickness as symptomatic of disorder of the brain, since what is not found in all cases of a given disease, can not be essential to such disease.

But in what function is Nausea or Sea-sickness seated? Are its primary manifestations in the nervous function exclusively, or, are they in some other function? It is assuredly a mere and pure sensation; and as it is plainly not a variety either of olfaction, vision, audition or gustation—the only special senses—it must be a variety of common sensation, and if its peculiarities as a variety of common sensation are not due to the peculiarities of the structure and organization of the part or texture, in which the nerves in which it has its seat terminate, then we are ignorant of that, on which they do actually depend. At all events, Nausea or Sea-sickness is a disturbance of a part of that aggregate of functions, upon which primary digestion depends, and therefore (as I formerly thought) it may very well be reckoned as a disease of the digestive function. At the present time however, I do not feel the same confidence in the correctness of the preceding conclusion, that I once did; and I must add, that I think this point requires further investigation and consideration, before it can be considered as in any way well settled. But, in which pair or pairs of nerves of common sensation, has Nausea or Sea-sickness its precise and exact seat? The stomach is commonly supposed to receive

filaments from every pair of nerves of common sensation (except the common sensor part of the *par tertium* of Willis) as they accompany the great sympathetic nerve (so called) and also special filaments from one or more pairs of nerves of common sensation which do not accompany the great sympathetic nerve. Can we determine whether it has its seat in these special filaments from one or more pairs of nerves of common sensation, or in those filaments from every pair of nerves of common sensation (except the sensor part of the *par tertium* of Willis) which accompanies the great sympathetic nerve so called? If Nausea or Sea-sickness is then an idiopathic affection, it should take rank in nosology, as a species of disease. Does it clearly and plainly belong to any established group or genus of diseases, or must it constitute a new genus of a single species? As a species of disease it seems to have but one primary symptom; though this primary symptom invariably produces a greater or less number of secondary symptoms, and these such as are common to several of the species of the group or genus *Limosis*. This being the fact, it once appeared to me that the disease under consideration might very properly be arranged as a *Limosis*, with the term *Nausea* as its trivial name. As I once thought, *Limosis Nausea* might require to have several varieties arranged under it. At present however, these conclusions are not to my mind quite as clearly true, as I once thought them. But, it is not necessary for me to go into this subject any further in this place. I should not have mentioned it at all, except to show my recognition of *Nausea* as a specific disease, and to afford an opportunity of inculcating that an investigation of the pathology and nosology of *Nausea* can not fail of throwing light upon it, as a remedial process.

PROËM TO THE CLASS *LEÄNTICA*.

The etymology of the term *Leäntica* is obvious. We find *Ἀσυντιχός, κή, χόν*, in ancient Greek, signifying alleviating, lenitive; (*Donnegan*) assuaging; soothing; calming; etc. This attribute is connected with the verb *Ἀσάτω, ανω* signifying to alleviate; to calm; to sooth; (*Donnegan*) to assuage; etc.

Definition.—Leäntica or Leäntics are articles which independent of any Antiphlogistic, Neuragic, Narcotic or Euphrenic powers, but by virtue merely of a peculiar impression upon a mucous membrane or the cutaneous surface of the body, made either by a mucilaginous or a gummy principle, by a farinaceous or amylaceous one, by an oily, a saccharine, a gelatinous or an albuminous one, or by moisture and heat conjoined, or by some conjunction of these, allay irritation in various parts of the system, alleviate phlogotic soreness, swelling and pain, and contribute either to a resolution, or a suppuration, according to the stage and other circumstances of the case.

As appears to me, the operation of the Leäntics consists unquestionably in a mere antirritant and soothing effect, though doubtless a peculiar and specific one, upon the nerves of the part irritated, taking place either by continuous, contiguous, nervous, textural or perhaps sensorial sympathy. The operation of the Leäntics is in itself strictly a vital operation, as much so as titillation, though it is produced by the mechanical qualities of the Leäntic agent, rather than by proper medicinal qualities, such as Antiphlogistic, Neuragic, Narcotic and Euphrenic powers. This last remark furnishes an explanation why the Leäntics are arranged by some as mere mechanical remedies; and it is only in the way here alluded-to, that any article of the *materia medica* can ever be considered as a mechanical remedy. The operation of the Leäntics is exerted mainly, if not exclusively, upon the nerves of common sensation. Possibly also the nerves of involuntary motion, both of the expressory and nutritive systems, may be affected in a less degree; though I think not. The Leäntics appear (at least so far as our present knowledge extends) to be utterly incapable of directly destroying life.

The following terms have been employed as names of this class of remedial agents, viz.: *Lenitiva*; *Demulcentia*; *Emollientia*; *Sedantia*; *Sedativa*. The first objection to the whole five is that the law of the nomenclature of the classes of the *materia medica* requires Greek instead of Latin, which law is implicitly followed in a great majority of cases, and for uniformity's sake, should be followed in the whole. Now all these terms are pure Latin. If the law required that the terms should be Latin, I perceive no objection either to the term *Lenitiva*, or

to the term Demulcentia. By the writers of the present day, the term Demulcentia has been limited to this class of articles used internally, while Emollientia has been applied only to this class of articles applied externally. The Demulcents and the Emollients of the authors of the present time having been thus limited, this fact, as appears to me, constitutes an objection to the use of these terms employed with greater latitude. At all events, Emollientia is not a very appropriate name for this class of agents taken internally, whatever may be the fact with the term Demulcentia in application to the class applied externally. It is a serious objection to the terms Sedantia and Sedativa in application to this class of agents, that they have been applied also to the Neuragics and the Narcotics, and even to the Euphrenics, all of which are classes that require distinction, as much as any other classes.

It will be recollected that the definition of Leantics which I have given, is intended to include not only the Demulcents of Cullen and Murray, but also the Emollients of both these authors. In my opinion, the operation of each is absolutely the same, only in the case of the Emollients the agent is more generally applied immediately to the irritated or inflamed part, i. e. it is applied topically and externally, while in the case of the Demulcents, it is frequently taken internally, and does not come at all into contact with the irritated or inflamed part, unless it is situated somewhere in the upper part of the alimentary canal. But when mucilaginous or oily applications are made topically to inflamed eyes or to a scalded surface ; or when the same substances are applied topically by injection, in irritations, or inflammations of the uretha, the vagina, or the large intestines, such a use of these agents has been considered rather as falling under the term Demulcents than under that of Emollient. The term Emollient has been more especially employed for warm, farinaceous, oily and mucilaginous applications to irritated or inflamed external parts, to allay swelling, soreness and pain, and to promote either a resolution or a suppuration, according to the state of the disease. I do not think however (as according to the opinions of Cullen and Murray expressed in their definition) that the portion of the Leäntics which commonly goes by the name of Emollients ever “diminishes the force of the cohesion in the particles of the solid matter of the human body, thereby rendering them more lax and flexi-

ble." (*Pg. 267, Jn. Murr. Syst. Mat. Med. fr. 4th Edinb. Edit. N. Y. 1828. Demulcents.*) Suppose a warm farinaceous, mucilaginous and oily cataplasm is applied to a Phlegmone communis in its early stage. Under such circumstances it lessens pain and soreness, diminishes the swelling and often produces a perfect resolution of the Phlogosis or Inflammation. Now what has all this to do with any diminution of the force of cohesion in the particles of the solid matter of the human body? Certainly nothing at all. The soreness and pain are relieved, either by virtue of a peculiar and specific antirritant and soothing operation upon the nerves of the diseased part, or by a diminution of the Phlogosis or Inflammation, or most probably both. The swelling is only to be relieved by diminishing the deposition of new matter in the diseased part, and by producing an absorption of the previous morbid deposits. This must be accomplished by an operation upon the secernents and absorbents of the diseased part. Diminution of the force of cohesion, a consequent greater relaxation and greater flexibility have nothing to do with the matter. The operation upon the nerves and upon the secernents and absorbents of the diseased part constitutes the whole process, and the whole is a vital, and not a mechanical operation. Suppose this warm farinaceous, mucilaginous and oily cataplasm is applied to a Phlegmone communis after the Phlogosis or Inflammation has passed its height. Under these circumstances suppuration takes place, and relief of the soreness, pain and swelling immediately follows. Now the absorption of solid matter to produce a cavity for containing the pus, and the closing of the communication between this cavity and the cells of the cellular substance, and the secretion of the pus into this cavity thus formed, are the exclusive work of the secernents and absorbents; and if an Emollient cataplasm contributes to the production of these effects, it must do it by virtue of some vital operation upon the secernents and absorbents of the diseased part, and not by mechanically diminishing the force of cohesion in the particles of the solid matter of the human body. The soreness and pain in this case, as in that previously specified, must be alleviated by a vital, peculiar and specific antirritant and soothing operation of the remedy upon the nerves of the diseased part.

But Cullen says "the Emollients seem to act upon the parts to which they are immediately applied in one of two ways." "The one is by being insinuated into the substance of the" (living) "solid, thereby diminishing its density and its force of cohesion." "The other is by being insinuated in to the interstices of the particles," (when) "they diminish the friction that might otherwise occur, and thereby render the" (living) "solid more flexible." Cullen says that "the former seems to be the operation of Water, the latter of Oil." Cullen says that "the action of the Emollients is most evident upon the simple solid; and that they may possibly act also upon the solid matter of the moving fibers; but except it be by the heat, that is frequently joined with them, they do not seem by their chymical qualities to act upon the nervous power. (*Cul. Mat. Med. Philad. 1812. Vol. II. Pg. 85. Ch. III. Emollients.*) Again I would ask what shadow of evidence there is for the opinion that Water ever mechanically insinuates itself into the substance of the living solid, and thereby diminishes its density or its force of cohesion; what shadow of evidence there is for the opinion that Oil is ever mechanically insinuated into the interstices between the particles of the living solid, thereby diminishing the friction, that would otherwise occur among the particles, and thereby rendering the living solid more flexible. I can scarcely imagine anything more at variance with the laws of physiology, any thing more impossible, any thing which is *prima facie* more absurd. I do not think that by their chemical qualities, the Emollients act upon the nervous power, any more than Cullen does; but I do not doubt that they act upon this power, by their other qualities, and that this is their sole operation. Why may not an emollient cataplasm produce an effect upon the nervous power, as well as titillation with a feather?

Cullen says that "the operation of Emollients is most considerable in the parts to which they are immediately applied; but as the whole of the solid matter of the body is constantly in a preternaturally extended state, and as at the same time the several parts are so connected, as to form one continuous body, so the tension of the whole must in some measure depend upon the tension of every particular part." "It is therefore that the relaxation of any one part must in some measure affect the whole." "It is indeed in this way that the effect of

Emollients is often extended much beyond the part to which they are immediately applied." (*Ibidem.*) In my view, there can be neither doubt nor question that the Emollients produce the greatest effects upon those parts, to which they are immediately and directly applied; nor that they produce more or less effects beyond the parts to which they are immediately and directly applied; but that this operation is utterly unconnected with any mechanical tension and relaxation of the living solid, appears to me to be susceptible of the most positive proof. Cullen says: "As however the effect of Emollients is still most considerable in the part to which they are immediately applied, it will be evident that their effect will be most considerable upon the surface of the body; and it is a question how far they can be rendered so, in the internal parts." "Upon this subject it may readily be imagined that as they may be applied to the internal surface of the alimentary canal, their effect then may be very great, and although I would not maintain that they must be none at all, yet I am disposed to think that except in the mouth and fauces, or in the great guts" (lower and larger intestines) "to which they may be immediately and copiously applied, they can not, in the other parts of the alimentary canal be very considerable." "The internal surface of the stomach and intestines are very constantly coated with a considerable quantity of mucus not readily diffusible in water, and therefore likely to prevent the insinuation of water or oil into the substance of their coats." "Their effects here also must be less, as they are unassisted by any additional heat, which is often required in their action upon the external parts; and an other circumstance, which may prevent their action upon the alimentary canal, is that their application to any particular part can never be very durable, as Water, the chief form of Emollients, must be very quickly carried-on, in progressive motion, or quickly withdrawn by absorption." (*Ibidem.* Pg. 85, 86.) I will just pause here to say that I do not consider Water as either the principal Emollient or Demulcent, or in a word Leäntic, though the presence of a certain amount of it may be absolutely necessary to reduce the several Leäntic principles to a fit state for their proper operation. For example Gum is truly Leäntic, and far more so than Water, and yet it must be reduced to a mucilage by water, before it can exert its Leäntic operation and produce its Leäntic

effect. I say this here once for all, as remarks to a different effect are often made by authors, and may occur in several of the quotations that I may have occasion to make.

It appears to me that in irritations and Inflammations of those viscera, which are situated near the surface, as of the peritoneum, the liver, the urinary bladder, etc., Emollients, when applied to the surface immediately over the viscus irritated or inflamed, are capable of producing their specific operative effects in a greater or less degree, though undoubtedly in a less degree than when the irritation or Inflammation is on the surface itself, and the Emollient comes into actual contact with the parts irritated or inflamed. As to the production of Emollient effects upon internal parts, by the swallowing of any Emollient, I can not discover in what such effects can be distinguished from what has been heretofore considered and discussed as a Demulcent effect; and therefore nothing additional need be said upon it in this place. Cullen continues "if the action of Emollients in the alimentary canal be in any measure doubtful, it must be still more so with respect to the blood-vessels." "Here even a large quantity is slowly introduced; is soon very much divided; can never be applied in large quantity to any one part; and must always be mixed with a large quantity of fluid not very penetrating." "At the same time it is applied to a surface constantly covered with an exudation, not readily miscible with water; and with all this, it is constantly in a rapid progressive motion, by which it must be soon carried intirely out, by the several secretions and excretions." "From all these circumstances, it would appear that Emollients as Watery" (medicines) "can hardly ever have any action in the system of blood-vessels; and therefore to explain the action of Emollients upon the system of solids, I am almost confined to consider only their action upon the external surface of the body, or in the parts immediately subjacent." (*Ibidem. Pg. 86.*) It appears to me also that the question of the operation of the Emollients by means of being taken into the mass of the circulating fluids, is identical with that of the operation of the Demulcents in the same manner, a question that has already been sufficiently discussed.

John Murray says "Demulcents are defined medicines used to obviate and prevent the action of acrid and stimulant matters;

and that not by correcting or changing their acrimony, but by involving it in a mild and viscid matter, which prevents it from acting upon the sensible parts of the body, or by covering the surface, to which they may be applied." (*Pg. 267. J. Murr. Syst. Mat. Med. fr. 4th Edinb. Edit. N. Y. 1828. Demulcents.*) It must be noted that all but the last clause of Dr. Murray's definition of Demulcents is quoted from Cullen's, and that by both these gentlemen it is intended to apply only to articles taken internally; most of those which are applied externally, for the same or a similar purpose, being denominated Emollients. John Murray says that "the class of Emollients, according to the definition given by Cullen, includes those medicines which diminish the force of the cohesion in the particles of the solid matter of the human body, and thereby render them more lax and flexible." "Their operation" (adds Dr. Murray) "is evidently mechanical." "They are insinuated into the matter of the solid fiber, and either diminish its density, or lessen the friction between its particles." (*Ibidem. Pg. 274. Emollients.*) In my opinion these definitions, as well that of Demulcents, as that of Emollients, are palpably incorrect and unfounded, notwithstanding the high source from which they emanate. For illustration, in Catarrhus communis whether the variety epidemicus or the variety sporadicus, a small quantity of some Leäntic taken frequently will sometimes lessen the frequency and diminish the violence of the cough. Now is it the fact in this disease that there is any acrid or stimulant or even in any way irritant matter upon any part with which the Leäntic comes into contact, which is at all instrumental in occasioning the Cough or which the Leäntic involves, as the common phrase is, so as to prevent its action? At the time of the beginning of the Cough in Catarrhus communis, there is usually an actual diminution, if not a suspension of the natural mucous secretion into the fauces; but in its progress there is a preternaturally increased secretion, either of this mucus in a thinner and more watery state, or of a mucipurulent matter, either from the Schneiderian or bronchial membrane, or both, each of which is intirely destitute of any acrid or stimulant or even irritant properties, and in fact seems to possess all the physical qualities of the best Leäntics. Assuredly in this case the Leäntics can not act according to the definition. This is in fact

pretty much conceded, for Dr Murray himself says the action of Demulcents "has been supposed to be exemplified in Catarrh, where the irritation at the top of the trachea occasioning Cough is removed by mucilaginous substances." What acrid or stimulating or irritating matter is here involved by the Leäntic? I do not perceive that Dr Murray even pretends that there is any. But is the irritation which occasions Cough in Catarrh, in fact seated at the top of the trachea? For my part I do not believe that it is. The fact that our sensations refer it to this part, no more proves that this is its real seat, than the fact that we refer the irritation of vesical calculus to the outer extremity of the urethra, proves that this is the real seat of that irritation. The irritation which produces Cough in Catarrh is doubtless within the trachea, or most probably within the bronchial ramifications, and I can not but think it occasioned altogether by the pathological condition of the solid parts, in which the disease is seated, and not by the presence of any acrid stimulant or irritant matter. At all events, the Leäntic is never admitted within the larynx; and this proves conclusively that it can not act according to Cullen's and Murray's definitions. These remarks are equally applicable to the operation of Leäntics in all the species and varieties of Pneumonitis, of Phthisis and of Bronchhemmitis. In Gastritis and Enteritis of all species and varieties, in Diarrhœa, Cholera, etc. there is certainly no acrid and stimulant or irritant matter in the alimentary canal, which either produces or keeps-up the disease, or which the Leäntic can involve. In all these diseases, the benefit rendered by Leäntics must consist in the peculiar and specific antirritant and soothing impression made by the Leäntic upon the nerves of such parts of the mucous membrane, as it comes into contact with, which impression is propagated (in a diminished degree without doubt) throughout the whole extent of the membrane, with a part of which, the Leäntic comes into actual contact, and this by what has sometimes been called continuous sympathy i. e. by mere extension in the same continuous texture—a texture having the same organization and susceptibility throughout, and discharging the same function. In all these cases the operation of the Leäntic certainly differs very widely from the definitions of Cullen and Murray, and instead of being merely mechanical, as they suppose, it is most unequivocally vital, at least so far as concerns the

operation itself, though the impression producing the operation may be mechanical, in contra distinction from a remedial impression, as unquestionably vital as the operation of any remedies in the whole materia medica.

But how does the operation of the Leäntics in Nephritis, Cystitis, Urethritis, Lithia renalis, Lithia vesicalis correspond with the definitions of Cullen and Murray? Do they pass the stomach and duodenum unchanged; are they taken-up by the lacteals unchanged; do they pass the mesenteric glands unchanged; are they mingled with the lymph in the receptaculum chyli, and with the blood when emptied into the left subclavian vein unchanged; do they pass the lungs unchanged; and are they finally excreted by the kidneys unchanged? "Judæus Apella credat hoc sed ego non." But even Cullen himself seems to doubt whether the Leantics ever operate according to his definition, except when they are applied externally or received into the cavities, by which they come into immediate and direct contact with the part in which the irritation or inflammation which they are intended to relieve is seated. Thus, Cullen says "the effects of Demulcents are sufficiently evident with respect to external parts, and it may be presumed that the same may happen with respect to the internal parts, so far and so long as the acrid continues mixed with the Demulcent. (*Pg. 287. Vol. II. Cul. Mat. Med. Philad. 1812.*) "But here the difficulty occurs to suppose that the Demulcent matter retains its mild and inviscating quality after it has been taken into the body." "To cover accrimony, it is necessary that the Demulcent should be of a considerable degree of viscosity; and when it is such as can be diluted with water, a considerable dilution greatly diminishes its power, and renders it almost none at all." "But" continues Cullen "the greatest part of the Demulcents can not be long in the stomach, or in passing through the intestines and other passages into the blood-vessels, without suffering a dilution that must take-off their viscosity altogether. (*Ibidem. Pg. 288.*)

Cullen says "it is further probable that Demulcents being commonly of a nutritious character, must, by the powers of the gastric liquor and perhaps by a fermentation" (which) "they" (may) "undergo in the stomach, be rendered of the same fluidity with the other aqueous fluids of the body." Before I finish this quotation I

will stop here to say that fermentation in the living human stomach is impossible. The gastric liquor is the most perfect and complete antizymic, as Cullen would say, that is known. I have known the gastric fluid of a domestic brute animal kept twenty years without any apparent change. But the vitality of the stomach is a still more powerful antizymic. We may therefore consider fermentation as wholly out of the question in this discussion. "All this reasoning" says Cullen "I can employ with respect to the Demulcents, such as the mucilages and sweets, which are of an aqueous nature, to afford this conclusion, that such Demulcents can have no effect as such, in the mass of" the "blood, or in passing by the various excretions." (*Ibidem*. Pg. 28.) Now this conclusion is in perfect conformity with the well known and established physiological laws of the animal system; and I entertain no sort of doubt that it is strictly true. But continues Cullen "with respect to the Oily Demulcents, the matter is not so obvious. Considering what we have said above, of the diffusion and even mixture of oil in our fluids, it will be probable that no quantity of it can be commonly present in the mass of the blood, so as to act as a Demulcent, or in their oily state to pass off by the excretions." "We have indeed alleged above that oil is a matter fitted to inviscate the vegetable acid taken into the body; but by that very mixture, the form of the oil is changed and it loses its fitness to be an inviscating matter." "There is indeed an other argument, that might be employed in favor of the inviscating, and if you will, Demulcent nature of the oil." "It has been observed, and we have pointed it out above, that when acrimony, in consequence of certain diseases, prevails in the mass of the blood, an absorption of the oil, which has been formerly laid-up in the adipose membrane takes place, and it is with great probability supposed that in this, nature intends that the absorbed Oil should cover the prevailing acrimony, and this supposition presumes that the Oil is fitted for this purpose." Cullen says that "all this is probable; but that it will apply to show that oil taken-in by the mouth, will act as a Demulcent, is to me very doubtful." "To conclude the subject of the Demulcent power of oil" (says Cullen) "I must observe that the oil commonly present in the blood, or even copiously introduced, is not a Demulcent with respect to some acrimonies taken into the body." Cullen says that "the Vitriolic Acid

passes copiously by the skin, in its Acid state, when it cures the Itch ; and the Muriatic Acid is found ready to irritate Issues and open Ulcers ; and we might give other instances of acrid matters ' passing by various secretions in their acrid state, not withstanding that a great deal of Oil is at the same time taken-in." Cullen says "from these considerations, it seems probable that the operation of the Demulcents in covering acrimony in the mass of the blood, must be very inconsiderable." (*Ibidem. Pg. 288, 9.*) In this instance, it is my opinion that Cullen, instead of being too skeptical, has in reality admitted too much. For my part, I know of no sort of evidence, or even of any plausible reason for concluding, that oil is ever received into the mass of the circulating fluids through the medium of the 'organs of primary digestion, as oil, or that, as a general rule, it is ever capable of passing any of the digestive organs unchanged and undigested, even into the lower and larger intestines, except in cases where the digestive function is temporarily suspended, as in particular diseases, or where a cathartic dose of it is taken at once. However, Cullen says "it is certain that the taking of Demulcents often allays Cough and suspends the repetition of it for some time ; and this, without having recourse to their operation in the mass of the blood, may be accounted-for in an other way." He says "as Cough is ordinarily occasioned by a halitus or vapor of some acrimony, arising from the lungs, and irritating the very sensible parts of the glottis and its neighborhood ; so by besmearing these parts with a Demulcent matter, we may often avoid the irritation we speak of, and therefore the frequency of the cough. (*Ibidem. Pg. 289.*) I certainly agree with Cullen that the swallowing of Leantics is often capable of somewhat allaying Cough, and this without passing into the circulation i. e. the blood, and therefore without doing it by virtue of being excreted in the precise form, in which they were swallowed, upon the irritated or Inflamed part of the pulmonary apparatus, whose diseased condition occasions the Cough.

Cullen's hypothesis that "Cough is ordinarily produced by a halitus or vapor of some acrimony arising from the lungs and irritating the very sensible parts of the glottis, and its neighborhood," I imagine, will find few advocates, at the present day. I consider it certain that Cough is not ordinarily occasioned by any

acrimony exterior to the bronchial membrane, but is rather produced, in most cases by the pathological condition of some part of this membrane, or of parts subjacent to it. The "besmearing," as Cullen terms it, of the glottis, and the parts in its neighborhood, can not then allay Cough, by defending these parts from the action of any irritant external to them, and therefore it must do it by a peculiar and specific antirritant and soothing impression upon their nerves.

In regard to the operation of this class of remedies, Murray says that when Demulcents "are directly applied to any part, it may be" (easily) "understood how this operation is obtained from them; but where they are received by the medium of the stomach into the circulating system, it has been supposed that they can have no such effect." "They must be changed by the process of digestion, and lose that viscidty, by which only they operate, so that they can not afterwards be separated, by any secretion, in their original form." "Hence their utility in Gonorrhœa, and similar affections has been altogether denied." Murray continues "it is not clear however that such a conclusion is just." "It is sufficiently certain that many substances, which undergo the process of digestion, are afterwards separated, in their intire state from the blood, by particular secreting organs." "There is no gland that has this power more particularly than the kidneys, substances received into the stomach, and digested, afterwards passing-off in the urine with all their peculiar properties." "For example" Murray says "there is reason to believe that Saccharine matter can be separated in this manner; yet there is no substance, which can be supposed to be assimilated more completely by digestion, or to be more easily changed in its composition, by the chiminal operations of the system." "If it can therefore be reproduced by secretion, it is equally probable that mucilaginous or oily substances, which form the principal Demulcents, are" (also) "capable of such a separation. But Dr. Murray admits that "no alteration is perceived in the quality of the urine, from the use of these substances;" (and he says) "there can be no doubt but that a great share of the relief, which Demulcents afford, in irritation or inflammation of the urinary passages, is owing to the large quantity of water, in which they are diffused, or by which the urine is diluted and rendered less "stimulating"

(rather less irritating.) He says "perhaps the relief is to be ascribed solely to the dilution." (*Pg. 268. J. Murr. Syst. Mat. Med. fr. 4th Edinb. edit. N. Y. 1828. Demulcents.*) If this is the fact, why not use mere and pure water instead of mucilages, etc.? Now what examples, I would ask, can possibly be adduced, in which any substance, (I will not say "many substances") which undergoes the process of digestion, is afterwards separated in its intire state, from the blood, by particular secreting organs? I certainly have no knowledge of any such case; and from this circumstance, together with a consideration of the ordinary and well known laws of the animal economy as respects digestion, secretion and excretion, I do not believe that any such case actually exists. I certainly know of no evidence whatever that the kidneys are any more capable of performing such an extraordinary synthesis of a vegetable organic proximate principle than any other secretory or excretory. What shadow of reason is there for believing that sugar, for example, can be perfectly digested by the stomach, and pass into the mass of the circulating fluid as chyle, and then be separated again as perfect sugar? As well might we believe almost any absurdity that can possibly be thought-of. That in one specific disease, viz. Paruria Diabetes, the kidneys have the power of secreting Sugar of a peculiar sort, is universally known. But this is purely the effect of disease—an effect occurring under no other circumstances; and even here it is not formed out of Sugar received into the stomach, and there and elsewhere converted into chyle; but it is formed from any vegetable aliments, and often even from animal food, though with much less facility.

But according to Murray, oily matters, that have been once digested, and are again restored to their original oily state by the kidneys, produce "no alteration" that can be "perceived, in the quality of the urine!"—a truly extraordinary circumstance in regard to this hypothesis. But after all this "labor of the mountains" Murray thinks that "there can be no doubt but that a great share of the relief, which Demulcents afford, in irritation or inflammation of the urinary passages, is owing to the large quantity of water in which they are diffused, and by which the urine is diluted," and he adds "perhaps the relief is to be ascrib-

ed solely to this dilution. (*Ibidem.*) Why not then, I repeat, use pure Water as a Demulcent or Leantic?

Murray says that "in general, Demulcents may be considered as substances less stimulating than the fluids usually applied to the parts, that are in a state of irritation." (*Ibidem.*) This I very much doubt. What stimulant or even irritant powers belong to the mucus and the mucipurulent matter, that is excreted from the Schneiderian membrane in Catarrhus communis, whether of the variety epidemicus or the variety sporadicus; or from the bronchial membrane in Pneumonitis Catarrhalis, or any other species of Pneumonitis? I do not believe that we possess any single Leantic so little irritating, or (if the term is insisted-upon), so little stimulating. What Irritant or Stimulant powers belong to the excretion from the mucous membrane in Diarrhœa, Cholera, or even what is called Dysentery? Doubtless less than belong to any Leantics in common use. But even Dr. Good has attempted a defense of this absurd hypothesis. He says "notwithstanding the difficulty of conceiving how a few drachms of bland oil or a few ounces of mucilage of Gum Arabic can be intermixed with many pounds of serosity, and still retain their sensible quality of inviscating sedatives, it is by no means more difficult to conceive this, than that moderate doses of Sulphuric Acid introduced into the stomach should pass copiously by the skin in its Acid state, as Dr. Cullen allows it to do, to cure the Itch; or that the Common Salt employed as an ingredient in the manufacture of glass, should, in the melting of this material, impregnate the atmosphere of the glass-house, be inhaled by the lungs of the workmen, and passing by the matter of perspiration, through the pores of the skin, once more concrete into crystals on the foreheads." (*Pg. 549, 50, 51. Vol. I. Good's Stud. Med. 4th Amer. Edit. Bost. 1826. Bex sicca.*) But where, I repeat, are the proofs that "a few drachms of bland oil, or a few ounces of mucilage of Gum Arabic, can be intermixed" at all or by any means in the living body, and by the natural and regular operation of the digestive apparatus, "with many pounds of serosity" as Good (oddly enough to be sure) terms the mass of the circulating fluid, or in a word the blood. When it is shown that these substances can at all be mixed unchanged with the blood of a living person, in the manner and by the means above

mentioned, it will then be time enough to inquire whether in such circumstances they (in the very language of Dr. Good) "still retain their sensible qualities of inviscating sedatives," and whether they are still capable of passing the kidneys unchanged and of operating as Leantic in irritations and inflammations of the urinary organs. I can by no means agree that there is as little difficulty in supposing that vegetable oil and vegetable mucilage, both extremely digestible and decomposable substances, can pass the stomach, the lacteals, the whole circulation, and even the secretory and excretory vessels of the lungs and kidneys, without undergoing any changes, as that Sulphuric Acid and Common Salt, both not easily digestible or decomposable substances, may do it. Vegetable oil and vegetable mucilage are not only easily digestible, but they actually consist of highly nutritious materials. Besides, in their ordinary state, vegetable oil and vegetable mucilage are not ingredients of the human system. On the other hand, Sulphuric Acid and Common Salt, being chiminal-inorganic substances, are not only far more difficult of decomposition, and their elements (for they have no proximate principles) are not nutritious; but in their undecomposed state, they are usually found in a small quantity, in every healthy human subject. Besides, they are physiologically capable, in proper solution and with suitable dilution, of being discharged by more than one of the emunctories, at least if we may take testimony upon the subject; whereas vegetable oil and vegetable mucilage in their intire state, can in all probability, pass-off only by the intestines, and not by any means by the lungs, skin or kidneys. But though I conceive it much more possible that small quantities of Sulphuric Acid and Common Salt may be excreted by the skin and lungs, yet I hold it to be an absurdity to suppose that the Common Salt, which crystallizes upon the foreheads of the workmen in a manufactory of glass, should consist of Common Salt, which has been vaporized, inhaled and again passed-off by perspiration. Why is it not much more reasonable to believe that the vapor of Salt, which is supposed to float in the air, probably in union with water, is directly precipitated upon the faces of the workmen, which are cooler than the air of the manufactory, or that the perspired matter, which is always supposed naturally to contain more or less Common Salt, has deposited it, in consequence of the increased

discharge, and more speedy evaporation of such perspired matter. The assumption that Sulphuric Acid, when taken internally for the cure of the Itch, passes-off by the skin, I believe, is pretty much gratuitous.

I perfectly agree with Good that "it would be a waste of words to bring examples to prove that, by some means or other, Demulcents are capable of allaying irritation in organs remote from the stomach;" but still, I think with Cullen that where the irritation is in the lungs, by swallowing these substances leisurely (probably slowly is here intended) we necessarily besmear the fauces and upper part of the glottis, and thereby directly relieve the irritation in these organs; and that the quietude here produced in the upper extremity of the trachea, is propagated by continuous sympathy, through the whole bronchial ramifications, and that it is in this manner, that Leantics prove remedial in all pulmonary irritations. I will add that their operation in irritations of the urinary organs, which, I think with Cullen, is inconsiderable, depends intirely on their impression upon the stomach.

But even where Leantics are externally and topically applied, as in Ophthalmitis, Isthmitis and Causis, or by injection, as in Elytritis and Urethritis, it does not appear to me, as I have already said, that they are of any service by virtue of involving any acrid or irritant, or, as authors always say, Stimulant matters, as according to Cullen's and Murray's definition. The secretions of an inflamed eye or of inflamed fauces, or of a scalded surface, I repeat, are certainly neither acrid nor irritant, nor stimulant, as common language is, nor do they at all contribute to keep-up these diseases respectively, unless they are suffered to accumulate and become sour, or dry into a crust. Even in Elytritis and Urethritis Pyoblennorrhœica, the mucipurulent matter excreted, though containing the contagion of the disease, does not appear to be acrid or irritant, or stimulant, as authors will have it; nor is the thin bloody mucus or sanious excretion from the mucous membrane of the upper and smaller intestines, in what is called Dysentery, at all of this character. The real fact then seems to be that even here Leantics operate intirely by the peculiar and specific antirritant and soothing impression, which they make upon the irritated nerves of the diseased membrane, which is certainly a vital, and not a mechanical operation. It appears to me

perfectly plain that the operation of the Leantics is always a vital, and never a mechanical operation; and that it always consists in a peculiar and specific antirritant and soothing impression upon the nerves of the part, on which they exert what powers they possess. Murray says that the Demulcents, as he calls them, "are evidently not medicines of any great power." To this we need not hesitate a moment to give full assent.

Edwards and Vavasseur say that "Demulcent substances are the remedies, the action of which has a tendency to relax the tissues, with which they came into contact, to lessen their tonic action, and to blunt their sensibility. [*Edwards' and Vavasseur's Man. Mat. Med. and Pharm. Phil.* 1829. *Chap. XIII. Pg.* 425.] Edwards and Vavasseur say that Emollients" [are articles] "which tend to soften the tissues, with which they are in contact." [*Ibidem Gener. Consider. Pg.* 59.] The class Emollients and its definition occurs in a synopsis of classification near the beginning of Edwards's and Vavasseur's work, while the class Demulcents and its definition occurs in the body of the work, where the Emollients should be treated-of, if this name were not abandoned. From this fact, and the essential identity of the definitions of these two terms, it is very evident that these gentlemen think as I do, in regard to the medicinal unity of Demulcents and Emollients. Indeed, under "Demulcent remedies," these gentlemen say that "their mode of action seems to be the same, whether applied to the skin, or introduced into the digestive canal, and to depend mostly upon the water, which is their common menstruum." [*Ibidem. Ch. XII. Pg.* 425.] Edwards and Vavasseur say that "in the first instance, Demulcents or Emollients appear to relax the cutaneous tissue, to swell it, and to diminish its redness and sensibility, and subdue more or less completely the inflammatory symptoms of which it is the seat." [*Ibidem.*] They also say that "in the second" (place) "Demulcents and Emollients produce similar changes in the parts with which they come into contact, and abate the internal heat, thirst, Cough, etc. at the same time that they prove a light aliment best suited to the inflammatory stage." [qu? state] "of the organs." [*Ibidem.*] I do not think that these definitions exhibit a very philosophical or luminous physiology or pathology. Edwards and Vavasseur say that although the most marked effects of the Demulcents are local,

they have others." "In fact" (say these gentlemen) "the relaxing action of these remedies seems to be susceptible of being transmitted by contiguity of organs. (*Ibidem.*) I doubt not that their effect, though to a considerable extent local, is in a less degree constitutional, though as I shall say hereafter, I must wholly deny its propagation by mere contiguity of organs. Edwards and Vavasseur say that "the recent experiments which have been made on absorption, have proved that the tissues during life may be penetrated by liquids," (viz. by endosmosis and exosmosis.) "It is then very easy to conceive how some remedies are capable of extending their sphere of action to a certain distance around a point to which they have been applied, or in other words, of propagating their influence by a contiguity of organs, without producing the same effects on the whole economy." "Thus Emollient Fomentations and Cataplasms which are applied to the abdomen when ever some one of the organs contained in this cavity is inflamed, at first induce a relaxation of the skin, and afterwards gradually extend their action to the parts more deeply situated." (*Ibidem. Chap. I. Sec. 17, Pg. 29.*) I discredit the whole of these opinions that, in a living subject not in the article of death, either Leäntics or any other remedial agents ever operate by endosmosis from external application or affect different textures by exosmosis when they are taken internally. What takes place in a brute animal in the article of death, in consequence of being half cut-up for the purpose of physiological experiment, affords no safe analogy for the *modus-operandi medicaminum*. If a urinary bladder of a dead brute animal, is filled or partially filled with weak Water of Ammid of Hydrogen, it is said the Ammid of Hydrogen may be strained through the parietes of the bladder, though neither Hydrogen-gas nor Nitrogen-gas will pass in this manner if not combined. But does any body at all acquainted with physiology suppose that Ammid of Hydrogen will pass through the parietes of the bladder of a living animal not in a dieing state? I do not think there is the least evidence of the spread or diffusion of medicinal or remedial influence by means of mere contiguity of organs, unless the effects of Epispastics of Cantharis upon the surface, for the relief of an inflamed viscus, is considered to be a case of this sort; which I take it for granted, it will not be. At all events, I do not think

that such a view could be defended. This however is no place to discuss the laws of secretion and excretion or of absorption—a topic which probably belongs to the Proëm to that class of diseases which Good calls *Eccritica*.

“The internal administration” (of Demulcents or Emollients, say Edwards and Vavasseur) “continued for a certain time, produces also more or less debilitating effects on the general economy.” “Thus we frequently see them diminish the strength and frequency of the pulse, subdue the irritation of organs distant from those with which they are in contact,” etc. “These secondary effects are principally owing to the sympathies they induce, and to the absorption of the large quantity of water in which they are administered.” These gentlemen add that “by the digestive action of the stomach, these substances are changed into chyme; and besides, we know from numerous and accurate experiments, that the increase of the proportion of water in the blood relative to that of the red globules, is a powerful means of lessening the vital energy.” (*Ibidem*, Chap. XIII, Pg. 425.) If it were strictly true that Leäntics were directly exhausting, it would constitute a sound objection to their use in all atonic diseases of any thing like a serious character. But I do not think that the pure Leantics are in any degree directly exhausting. It is only indirectly that they produce this effect. They undoubtedly relax generally, as Edwards and Vavasseur say, while they also diminish appetite and digestive power. I never saw them continued for any length of time, in even a moderately atonic disease, without undesirable effects; and in an intensely atonic case, their relaxing effects are immediately perceived. I have often seen the effects of more appropriate remedies intirely counteracted by a very free and protracted use of Leantics, and as I think cases decided fatally, which might have been saved by better treatment. I do not believe that increasing the proportion of water in the mass of the circulating fluid, by drinking an additional quantity of water, does in fact debilitate. It is increasing the proportion of water by diminishing the crassamentum of the blood, that produces the effect in question.

Edwards and Vavasseur say correctly that “all the Demulcent remedies are furnished by organic snstances containing certain proximate principles, to which they are indebted for

their" (Demulcent) "properties." (*Ibidem.*) In short Demulcents and Emollients, as I have already said, appear to me to have but one single power, and to be but one single class of remedies, the former name being applied to them when they are taken into the alimentary canal, and the other, when they are topically applied to the external surface of the body. Occasionally, it is true, some author or practitioner does not draw the line in this place exactly, sometimes applying the term Demulcent to an article externally employed; and more rarely using the term Emollient for some thing taken internally.

The Leantics, as has been so often said, are in fact weak and inefficient remedies, in all probability only worthy of being considered as mere palliatives and auxiliaries, and as they are often employed, as mere placebos. They can therefore never be relied on exclusively in any serious disease. Even as auxiliaries or palliatives, it appears to me that their internal use ought to be confined to 1, Irritation or Phlogosis connected with a phlogistic diathesis, and consequently itself phlogistic; and in such cases only after Depletion of Blood. 2. Irritation or Phlogosis not connected either with phlogistic or atonic diathesis; and also for scarcely any other purpose than keeping the mouth moist a little longer than it can be done with simple or pure water. The Leantics are barely admissible in Irritation or Phlogosis connected with a moderately atonic diathesis, though they are seldom of any material service under such circumstances. In Irritation or Phlogosis connected with an exquisitely atonic diathesis I never saw them render any degree of service whatever; but I have not infrequently seen them do considerable injury. Edwards and Vavasseur say (as I have elsewhere quoted) of this class of remedies, that "their internal administration continued for a certain time, produces more or less debilitating effects on the general economy." This I believe is correct to a moderate extent, in all cases, and to a greater extent, in intensely atonic ones; and hence Edwards and Vavasseur specify particularly that they are "contraïndicated in cases of atony, and toward the end of certain chronic diseases maintained by general debility." (*Edw. and Vavass. Man. Mat. Med. Philad. 1829, Ch. XIII, Pg. 425, Demulcents et alibi.*) As I elsewhere inculcate, the debility or exhaustion produced by the Leantics, is never direct but

always indirect, though not in the Brunonian sense of this latter term. In the Brunonian sense of indirect debility, I do not think that such a condition exists in pathology. John Murray says that "the Demulcents may be freely used in as large quantities as the stomach will receive." (*Jn. Murr. Syst. Mat. Med. fr. 4th Edinb. Edit. N. Y. 1828, Pg. 268, Demulcents.*) This is assuredly injudicious advice. Dr. Murray could never have seen them much used in atonic diseases, or he could never have studied their effects very carefully, for if he had, it appears to me that he would have seen evil from "as large quantities as the stomach will receive," certainly if continued for any length of time. I believe that Dr. Murray did not survive late enough to see much of Broussaisism. In feeble and exhausted subjects having Limosis of almost any species or variety, Bex, Dyspnœa, Asthma, Catarrh, whether epidemic or sporadic, Pneumonitis Typhodes of any species, Phthisis of any species, Rosalia, Rubeola, Paruria, Lithia, etc. in all of which diseases, Leantics are recommended by authors and employed by numerous practitioners, evil will inevitably result from them, if they are taken freely and for any considerable length of time. In those species of Phthisis, in which the topical affection is limited and confined to the bronchial membrane, I have witnessed mischief from them much the most frequently. I have seen the appetite and digestive power greatly impaired by them, and indirectly great weakness produced; and I can not say that I ever knew any good at all result from their use in such cases. *Cæteris paribus* Leantics are always of much more utility in the acute stages of disease than in the chronic. Broussais brought himself into much notoriety by pretending to place great reliance upon a free and protracted use of Leantics in numerous cases, in which they had previously been considered as absolutely worthless. He even made them principal remedies in many serious and positively dangerous diseases. Dr. John B. Beck, late professor in one of the medical schools of New-York, makes the following truly and literally just remarks in regard to Broussais, his doctrines and his practice. He says "a celebrated theorist" (speculatist would have been a more appropriate term) "of the present day, although the taste of the age does not permit him to consign to the flames the labors of those who have gone before him" (as was done by Paracelsus) "with

quite as little ceremony denounces the whole of them as useless and vain, and holds himself-up as the first and the true founder of medicine." "Trusting to the representations of the author of the new doctrine, as it is styled," (both by himself and his disciples) "we should be led to suppose that he is the last, the true prophet, sent to expose the errors and delusions of all preceding times; that for centuries, men have been wandering in darkness, and that he has been commissioned to hold before their astonished eyes the torch of truth, that he has found the key to the paradise of knowledge," and at his "Open Sesame" all its flood-gates are to be thrown open; that he is the mighty necromancer, who is to pour-out his oil, and calm the troubled waves of controversy and disputation; that he is to extend his magic wand over the medical world, and the millenium of our science is to commence." "And what is the wonderful revelation, which is to effect all this?" "What is this 'philosopher's stone' which after the search of ages, has at last been discovered?" "Why substantially nothing more nor less than this—that what physicians have been in the habit of calling fever, time immemorial, is nothing more than Gastro-Enteritis, and that the whole of the practice of medicine is to be reduced-down to the use of Leeches and Gum-water, a discovery about as profound as Paracelsus flattered himself that he had made, in his famous Elixir, by means of which, he gravely maintained that he could prolong the life of man to the age of Methusaleh, and a practice about as rational as the blood-letting and hot-water of Dr. Sangrado." Dr. Beck says that "it is highly honorable to the independence and discrimination of the profession on this side of the Atlantic, that of the numerous schools of medicine, which we have in the United States, only one has been infected with the Gum-water theory." But says Dr. Beck "even the 'new doctrine' is already sinking into a premature grave," and this seems to be the fact even in Paris, if we may judge from the periodicals, and from the testimony of foreigners who visit Paris.

CULLEN'S CATALOGUE OF DEMULCENTS.

SYMPHYTUM.

CYNOGLOSSUM.

GUMMI ARABICUM.

TRAGACANTHA.

ICHTHYOCOLLA.

OLEOSA BLANDA.

(*Cull. Treat. Mat. Med. Philad.* 1812, *Vol. II, Pg.* 289, 290, 291.) Cullen gives no formal catalogue of Emollients, which he mentions as a distinct class from Demulcents; but in his discussion of the subject he admits

WATER

and

GREASY OILS

to be such; and as I understand him, he admits nothing else, unless it is FRICTION. (*Ibidem. fr. Pg.* 85 to 92.) This is assuredly a meager list of Leantics, not more than four of which are in common use in the U. S. A. at the present time, and one of these only to a very slight extent as a Leantic, though very often taken as a nutrient. I refer to Ichthyocolla.

JOHN MURRAY'S CATALOGUE OF DEMULCENTS.

ACACIA ARABICA.

ASTRAGALUS TRAGACANTHA.

LINUM USITATISSIMUM.

ALTHÆA OFFICINALIS.

MALVA SYLVESTRIS.

GLYCYRRHIZA GLABRA.

SMILAX SARSAPARILLA.

CYCAS CIRCINALIS.

ORCHIS MASCULA.

MARANTA ARUNDINACEA.

TRITICUM HYBERNUM.

LICHEN ISLANDICUS.

CORNU CERVI.

ICHTHYOCOLLA.

AMYGDALUS COMMUNIS.

OLEA EUROPÆA.

SEVUM CETI.

CERA.

(*Jn. Murr. Syst. Mat. Med. fr. 4th Edinb. Edit. N. Y.* 1828, *Pg.* 268.) It may be well to remark here that I never could

obtain any evidence that *Smilax Sarsaparilla* (Linn.) is ever used as a Leantic. This species, I believe, is confined to North-America, and perhaps even to the U. S. A. and so far as I can ascertain, is never collected for exportation, and never even found in our own shops. Its root may be farinaceous, and capable of proving Leantic with proper preparation; but I should be glad of some facts upon the subject; and certainly Murray gives none. But what is *Smilax Sarsaparilla* (Linn.)? Is it *Smilax spinulosa* (Smith)? Is it *Smilax glauca* (Walter)? Are *Smilax spinulosa* (Smith) and *Smilax glauca* (Walter) one and the same plant? It has been known fifty or sixty or even more years that this species does not afford any of the Adenagic roots that have been so highly celebrated in medicine, and that have been so long employed in such unscientific pharmaceutic preparations, that they can contain but little, if any of the active principle. Only one of these belongs to North America viz. *Smilax medica*, or Vera-Cruz Sarsaparilla; and this I believe is not indigenous to the U. S. A. at least so far as is now known.

Murray gives no catalogue of Emollients but he says that

HEAT
and
MOISTURE

are the principal Emollients. As both of these are better retained in conjunction with certain vegetable substances, he advises

CRUMB OF BREAD,
FLOUR OR MEAL,
FLORES CHAMÆMELI,
MALVA, ETC.

John Murray next mentions UNCTUOUS SUBSTANCES, and very particularly

AXUNGIA PORCINA.
OLEUM PALMÆ.
OLEUM COCOÏS BUTYRACEÆ.

OLEUM BACCARUM LAURI NOBILIS.

(Ibidem. Pg. 274, 5.)

John Murray's list of Leantics is a good one, far better than that of Cullen, though even this is very much too small. I very much doubt whether, in any portion of our country, the list of Leantics in universal popular use, is not at least four times larger than that of Murray's. The physician should certainly be well acquainted with the *medicina anilis*, at least when it is good; and even when it is not, he should know its demerits. Nothing commends a practitioner of medicine so effectually to favorable popular opinion, as a thorough knowledge of the *materia medica* of the good women and the Lady Bountifuls, where he resides and must find employment.

It is not often that Dr. Pearson gives any definition of his classes of the *materia medica*; and in the case of his *Emollients*, he gives none; neither has he any preceding disquisition upon the subject.

PEARSON'S CATALOGUE OF EMOLLIENTS.

A. DILUENTS.

JUS CARNIS BUBULÆ DILUTUM.

JUS PULLINUM DILUTUM.

SERUM LACTIS.

INFUSUM MELISSÆ.

INFUSUM THEÆ.

DECOCTUM AVENÆ.

DECOCTUM HORDEI.

B. DEMULCENTS.

ACIPENSER HUSO.

HELIX POMATIA.

PHYSETER MACROCEPHALUS.

ALTHÆA OFFICINALIS.
 AMYGDALUS COMMUNIS.
 ASTRAGALUS TRAGACANTHA.
 AVENA SATIVA.
 CYCAS CIRCINALIS.
 GLYCYRRHIZA GLABRA.
 HORDEUM DISTICHON.
 JATROPHA JANIPHA.
 LICHEN ISLANDICUS.
 LINUM USITATISSIMUM.
 MALVA SYLVESTRIS.
 MARANTA ARUNDINACEA.
 MIMOSA NILOTICA.
 OLEA EUROPÆA.
 ORCHIS MASCULA.
 PYRUS CYDONIA.
 TRITICUM HYBERNUM.
 TUSSILAGO FARFARA.
 CERA.

(*Rich. Pearson. Pract. Synops. Mat. Aliment. and Mat. Med. Lond. 1808. Pg. 286, 287.*)

The preceding catalogues I have given not only in the terms, but in the order employed by the authors quoted ; though I have varied the order of Edwards's and Vavasseur's catalogue. They put down their articles in natural history order, for which I deem their catalogue too small ; and yet it is too large to be put down without any method at all. I have taken the liberty to arrange it according to the affinity of the proximate principles, in which the Demulcent power resides, wherever the article really possesses any such power.

EDWARDS'S AND VAVASSEUR'S CATALOGUE OF DEMULCENTS AND EMOLLIENTS.

GUMMI.
 SACCHARUM.
 FÆCULA.
 OLEA FIXA.

ALBUMEN.

GELATINA.

These Edwards and Vavasseur consider as the Demulcent principles of plants and animal substances, though I think that they mention some articles that contain neither of them.

GUMMI ARABICUM.

GUMMI SENEGALENSE.

GUMMI NOTRAS.

GUMMI TRAGACANTHÆ.

ULMUS FULVA.

ULMUS AMERICANA.

MEDULLA SASSAFRAS.

RADIX OSMUNDÆ.

RADIX FOLIA ET FLORES ALTHÆÆ.

RADIX FOLIA ET FLORES ALCEÆ ROSEÆ.

RADIX FOLIA ET FLORES MALVÆ SYLVESTRIS.

RADIX ET FOLIA CONSOLIDÆ MAJORIS.

SEMEN LINI.

SEMINA PSYLLII.

HERBA SENECTIONIS.

HERBA ACANTHI.

BULBUS LILII.

SEMINA CYDONIÆ.

BULBUS PORRI.

SEMINA CANNABIS.

SEMINA CUCURBITÆ LAGENARIÆ.

SEMINA CURCUBITÆ PEONIS.

SEMINA CUCUMERIS MELONIS.

SEMINA CUCUMERIS SATIVI.

HERBA ET FLORES BORAGINIS.

HERBA ANCHUSÆ ITALICÆ.

HERBA PULMONARIÆ OFFICINALIS.

HERBA CYNOGLOSSI OFFICINALIS.

RADIX GRAMINIS.

MELILOTUS OFFICINALIS.

SACCHARUM OFFICINARUM.

ACER SACCHARINUM.
BETA VULGARIS.
FRUCTUS PRUNI.
UVÆ PASSÆ.
CARICÆ PINGUES.
FRCTUS DACTYLUS.
FRUCTUS JUJUBA.
RADIX LIQUIRITIÆ.
FÆCULA SAGU.
FÆCULA TAPIOKA.
FÆCULA MARANTÆ.
FÆCULA SOLANI.
HORDEI SEMINA.
ORIZÆ SEMEN.
GRUTUM.
TRITICI SEMEN.
SECALIS SEMEN.
FURFUR.
RADIX SALEP.
OLEUM LINI.
OLEUM AMYGDALARUM DULCIUM.
OLEUM OLIVÆ.
OLEUM JUGLANDIS.
OLEUM PAPAVERIS.
BUTYRUM SEU OLEUM CACAO.
ADEPS SUINUS.
SEVUM OVINUM.
BUTYRUM.
SPERMA CETI.
CERA.
ICHTHYOCOLLA.
TAUROCOLLA.
LAC.
SERUM LACTIS.

(*Edw. & Varass. Man. Mat. Med. Philad. 1829, Pg. 425, 447.*)

Perhaps the Leäntics might be usefully sub-divided into two great groups, viz., 1st. Demulcentia, or those that produce their effects by internal use; and 2d. Emollientia, or those that operate intirely by external and topical application. But I shall make no display of the articles belonging to this class according to such a sub-division. So far as I have knowledge, the following are all the Leäntic principles, whether vegetable-organic, or animal-organic, viz.:

1. MUCILAGO.
2. GUMMI.
3. ACIDUM PECTICUM.
4. FÆCULA IN FARINA.
5. SACCHARUM.
6. OLEUM PINGUE.
7. ALBUMEN.
8. GELATINA.
9. CERA.
10. AQUA CALIDA.

As there are just ten Leäntic principles, simple or compound, so there is a good foundation for ten turmæ or groups of Leäntics, according to the principle upon which the Leäntic power depends, viz.:

1. LEANTICA MUCILAGINOSA.
2. LEANTICA GUMMOSA.
3. LEANTICA PECTICA.
4. LEANTICA FÆCULACEA.
5. LEANTICA SACCHARINA.
6. LEANTICA OLEOSA.
7. LEANTICA ALBUMINOSA.
8. LEANTICA GELATINOSA.
9. LEANTICA CEREÆ.
10. LEANTICA CALORI-AQUOSA.

In the subsequent catalogue, I mention no article as a Leäntic, which is not mentioned by some work now before me on the materia medica, and of a high character; but at the same time I fall very far short of enumerating all the articles specified in the

collection of books on this department of medicine in my own library, which is meager in this branch of the medical profession. The list which I am now about to give, is mostly selected from about five works, one of them truly scanty in its catalogue, while the rest are quite full; but in no case, do I take all the agents which they mention. It is true that a large proportion of the other books, that I possess, make out their catalogues from those from which I make-up my own list. Some of the articles are selected because they are believed to be excellent articles in themselves, though they are but little known and used; while others are admitted because they enjoy extensive popular favor, and are in constant popular use, and can not be rejected by physicians, even if they choose to reject them, though they are of very inferior value, and in reality scarcely worth retaining in comparison with far better articles at present scarcely employed. Although I endeavor to make a selection of articles from the authorities which I have mentioned, yet I do not feel it to be expedient to omit all mention of an article that I deem ineligible, when it is in almost universal popular use, and which physicians (as I have already said) are in reality absolutely constrained to employ, because their employers will have it so, and will use it of their own accord. The cause of useful knowledge will be far better subserved by admitting and treating of such an article, and carefully pointing-out the popular errors and mistakes, and even the professional ones. In no other way can such an article be got rid-of. Such a course will in due time produce correct notions upon the subject, and cause it to be neglected on just grounds, never to be re-called into our systems.

Some articles are admitted into the list because they are constantly mentioned by all our standard authors, though much better articles might be selected. It certainly behoves the practitioner of medicine to be acquainted with every article, about which inquiries are liable to be made, because ignorance of any such agents would assuredly contribute to his disparagement. Some articles that are utterly worthless, but still much employed, are occasionally mentioned. My principles may be well illustrated, in regard to this last set of articles by certain agents referred to quite a different class. The dried stigmata of *Crocus sativus* (Linnæus); *Crocus odoratus* (Bivona) etc. are decidedly Euphrenic,

and when of a good quality, and rightly administered, possess considerable value in the materia medica. But by almost universal mistake in the U. S. A., not only of the people, but of the apothecaries, and even of the physicians, the tubular corollæ of *Carthamus tinctorius* are employed for the dried stigmata of *Crocus sativus*, *Crocus odorus*, etc. Now it is impossible to make this subject intelligible to persons not acquainted with botany and materia medica; but by constantly mentioning the error, physicians generally will at last come to understand it, and then apothecaries. This is as far as we can expect to go. How many geological periods it will require to get-rid of the error among non-medical people generally I cannot pretend to conjecture. I have been trying to do it, where I have lived the whole of my professional life, but hitherto in vain.

I have also selected articles from every country where British or U. S. A. physicians are likely to be found, and wherever any work on the materia medica in the English language is likely to be read.

LEANTICA MUCILAGINOSA.

The Mucilaginous Leäntics are by far the most miscellaneous and heterogeneous group belonging to the class. Many articles that contain Mucilage in the greatest abundance, contain also, other individual proximate principles which are the foundation of other groups of Leäntics. For example, the subterranean tubers of *Solanum tuberosum* contain a great deal of Mucilage, which, evaporated to dryness, is said to differ a great deal, indeed very materially and essentially from Gum, or from any other individual vegetable proximate principle, which is the foundation of a group of Leäntics. These root-tubers also contain a large amount of *Fæcula*, which is the foundation of another group of Leäntics. This involves the necessity of mentioning these root-tubers under two groups at least. Some articles contain as many as three, and some times perhaps more of these individual proximate principles, and therefore require to be mentioned in as many groups.

Although I profess to found my turmæ or groups of Leäntics upon individual organic proximate principles, on which the power depends, yet my first turma or group is an exception to this rule;

and one or two other exceptions will occur hereafter. I have made the presence of what is called Mucilage the foundation of my first sub-division of the medicinal class of Leäntics, as if it were an individual organic proximate principle of vegetables, which I am perfectly aware it is not recognized as being, by any chymical authority within my present recollection. What it is I know not; but I have always suspected it to be Gum mingled with various other substances, the whole held in solution by water. As I do not certainly know this, however, it would not be proper to arrange Mucilaginous articles, the most numerous, if not the most important group of the class, according to a mere hypothesis. As the Mucilages differ, in their external sensible properties, about as much from the Gums, as each differs from Pectic Acid and Fæcula after being boiled in water, it appears to me that sound practical discrimination will be best promoted by keeping the Mucilaginous articles separate and distinct both from the Gums and the Fæculæ, if not from solutions of Pectic Acid. At all events, I make my arrangements according to the best of my knowledge; and I can only hope that my successors will have far more extensive and precise information, and will be able to put on a certain foundation, all of which I am obliged to write under uncertainty and doubt.

The Mucilaginous Leäntics are in much the most general use; and if they are not considered as actually the best articles of the class, they are at least considered as the most convenient. It is by no means easy to make a natural arrangement of the Mucilaginous Leäntics among themselves, so that, after various attempts to do this, I abandoned the idea of doing it. It will be obvious that such an arrangement of crude articles of complex composition must be nearly impossible. An arrangement, however, by natural orders is a natural arrangement, so far as respects the plants belonging to the same order; but I did not deem it worth while to attempt any natural arrangement among the orders themselves, since only now and then an order furnishes Mucilaginous Leäntics.

MALVACEÆ.

Lindley says of the Malvaceæ that “the uniform character of the order is to abound in Mucilage, and to be totally destitute of

all unwholesome qualities." (*Lind. Veg. King. Lond.* 1846, *Pg.* 369.) Now I am not acquainted with any fact in opposition to this statement; though as I know nothing of very many of the plants belonging to this order, there may be many exceptions. A number of the species possess additional powers; but this does not at all invalidate Dr. Lindley's statement. This is more than can perhaps be said of almost any other natural order, which comprises more than an exceedingly small number of species.

MALVEÆ.

- ALTHÆA OFFICINALIS, (*Linn.*)
- ALTHÆA ROSEA, (*Cavanilles.*)
- ALTHÆA FICIFOLIA, (*Cavanilles.*)
- MALVA SYLVESTRIS, (*Linn.*)
- MALVA ROTUNDIFOLIA, (*Linn.*)
- MALVA CRISPA, (*Linn.*)
- MALVA ALCEA, (*Linn.*)
- SPHÆRALCEA CISPLATINA, (*St. Hilaire.*)
- URENA LOBATA, (*Linn.*)
- URENA SINUATA, (*Linn.*)
- PAVONIA DIURETICA, (*St. Hilaire.*)

HIBISCEÆ.

- HIBISCUS ROSA-SINENSIS, (*Linn.*)
- ABELMOSCHUS ESCULENTUS, (*Wright & Arnott.*)
- { ABELMOSCHUS MOSCHATUS, (*Wright & Arnott.*)
- { *Abelmoschus Moscheutos.*
- { *Abelmoschus palustris.*
- { ABELMOSCHUS VITIFOLIUS,
- { *Hibiscus Vitifolius*, (*Linn.*)
- { ABELMOSCHUS OBTUSIFOLIUS,
- { *Hibiscus obtusifolius*, (*Willd.*)
- GOSSYPIMUM VITIFOLIUM, (*La Marck.*)

SIDEÆ.

- SIDA CORDIFOLIA, (*Linn.*)
- SIDA CARPINIFOLIA, (*Linn.*)

SIDA MICRANTHA.

SIDA RHOMBIFOLIA (*Linn.*)SIDA MULTIFLORA (*Cavanilles.*)SIDA JAMAÏCENSIS (*Cavanilles.*)SIDA ALNIFOLIA (*Linn.*)SIDA ALTHÆIFOLIA (*Swartz.*)SIDA ACUTA* (*Burmamn.*)ABUTILON ESCULENTUM (*St. Hilaire.*)ABUTILON INDICUM (*G. Don.*)ABUTILON AVICENNÆ (*Gærtner.*)

? { ABUTILON MAURITIANUM,
 Sida Mauritiana (*Jacquin.*)

? { ABUTILON POPULIFOLIUM,
 Sida Populifolia (*La Marck*) (*Linn.?*)

? { ABUTILON ASIATICUM,
 Sida Asiatica (*Linn.*)

Between the two genera *Hibiscus* and *Abelmoschus*, which formerly constituted but one genus, and between the genera *Sida* and *Abutilon*, which in like manner formerly constituted but one genus, I may not have distributed the species correctly; but I have done it according to the best authorities in my possession or to which I have access. There are supposed to be thirty-seven genera, and at least one thousand species of the *Malvaceæ*. Perhaps a better selection of *Leäntics* might have been made from this order; but I believe that all which I have named are in use for this purpose, though how many more I am unable to say exactly.

STERCULIACEÆ.

Lindley says that the *Sterculiaceæ* “are chiefly remarkable for the abundance of Mucilage they contain.” (*Ibidem*, Pg. 361.)

BOMBEÆ.

ADANSONIA DIGITATA (*Linn.*)ERIODENDRON ANFRACTUOSUM (*D. C.*)

* Lindley mentions as a *Leäntic* *SIDA ABUTILA*. I find no such species in any work of descriptive botany that I possess. Does he not intend

{ ABUTILON AVICENNÆ (*Gærtner.*)
 { *Sida Abutilon* (*Linn.*)?

HELICTEREÆ.

MYRODIA ANGUSTIFOLIA.

HELICTERES SACAROLHA (*St. Hilaire.*)HELICTERES OVATA (*La Marck.*)HELICTERES CORYLIFOLIA (*Nees ab Esenbeck.*)HELICTERES BREVISPIA (*St. Hilaire.*)HELICTERES VUARAME (*Martius.*)

STERCULIÆ.

STERCULIA ACUMINATA (*Palisot de Beauvois.*)STERCULIA CHICHA (*St. Hilaire.*)STERCULIA FETIDA (*Linn.*)STERCULIA LASIANTHA (*Martius.*)STERCULIA NOBILIS (*J. E. Smith.*)

STERCULIA TOMENTOSA.

CAVALLIUM URENS (*Schott.*)

PEDALIACEÆ.

Of this order Lindley mentions only two genera as furnishing Mucilaginous articles, each representing one of the two tribes into which the order is divided. Of these two genera the following two species may well stand as representatives.

PEDALIEÆ.

PEDALIUM MUREX (*Linn.*)

SESAMEÆ.

SESAMUM INDICUM (*De Candolle.*)

PLANTAGINACEÆ.

Lindley says of the Plantaginaceæ that "their seeds are covered with mucus," doubtless meaning Mucilage. (*Ibidem*, Pg. 643.)

PLANTAGO ISPAGHULA (*Rowburgh.*)PLANTAGO PSYLLIUM (*Linn.*)

PLANTAGO CYNOPS (*Linn.*)
 PLANTAGO MAXIMA (*Jaquin.*)
 PLANTAGO ARENARIA (*Kitaibel.*)
 PLANTAGO MARITIMA (*Delile.*)
 PLANTAGO MEDIA (*Linn.*)
 PLANTAGO CORDATA (*La Marck.*)
 PLANTAGO MAJOR (*Linn.*)

LINACEÆ.

Lindley says that "the Mucilage of the seeds is a striking character of the Linaceæ." (*Ibidem*, Pg. 485.)

LINUM USITATISSIMUM (*Linn.*)

CANNABINACEÆ.

This natural order is at present reckoned as comprising only two genera of one species each, viz. Cannabis and Humulus. I very strongly suspect that there is error as respects the number of species of the former. The seeds of Cannabis are said to furnish much Mucilage, and the root of Humulus is one of the articles used as a substitute for Sarsaparilla, and like all the articles so used, said to be Demulcent.

CANNABIS SATIVA (*Linn.*)
 CANNABIS INDICA.*
 CANNABIS SINENSIS.*

TERNSTRÆMIACEÆ.

Some of the species of this natural order contain Mucilage, some furnish Gum, and some furnish Oil, and doubtless the greatest portion furnish neither. Those which furnish the three principles above named, must be mentioned under three of my groups. The following will serve as a specimen of those which contain Mucilage.

KIELMEYERA ROSEA (*Martius.*)
 KIELMEYERA SPECIOSA (*St. Hilaire.*)

* Perhaps mere varieties of the first, but I think more probably distinct species. At any rate each deserves distinct mention in the materia medica

FABACEÆ.

The Fabaceæ often furnish Gum, some times Fæcula, and often Sugar of some species or variety, but seldom Mucilage, at least so far as I have knowledge. The following are alleged to contain Mucilage, which I know to be true of some of them, though I have not seen and employed the whole. Authors are often exceedingly careless in regard to their specifications in reference to such points, as I have often had opportunity and occasion to verify.

LOTEÆ.

ASPALATHUS INDICUS (*Linn.*)

TRIGONELLA FÆNUM-GRÆCUM (*Linn.*)

SESBANIA PICTA (*Poiret.*)

ASTRAGALUS EXSCAPUS (*Linn.*)

ASTRAGALUS GLYCYPHYLLUS (*Linn.*)

ASTRAGALUS MONSPESSULANUS (*Linn.*)

PHASEOLEÆ.

PUERARIA TUBEROSA (*De Cand.*)

CASSIÆ.

CASSIA ABSUS (*Linn.*)

CASSIA TORA (*Linn.*)

BAUHINIÆ.

BAUHINIA TOMENTOSA (*Linn.*)

CAULOTRETUS MACROSTACHYUS (*Martius.*)

AMARANTACEÆ.

Lindley says of the Amarantaceæ that "many of the species are used, on account of the wholesome Mucilaginous qualities of the leaves." (*Ibidem*, Pg. 511.)

GOMPHRENEÆ.

PHILOXERUS VERMICULATUS, (*Rees's Cyclop.*)

ACHYRANTHEÆ.

AMARANTUS BAHIIENSIS (*Schrader.*)

AMARANTUS BLITUM (*Linn.*)

AMARANTUS CAMPESTRIS (*Willdenow.*)

AMARANTUS MELANCHOLICUS (*Linn.*)

AMARANTUS VIRIDIS (*Linn.*)

AMARANTUS OBTUSIFOLIUS.

BYTTNERIACEÆ.

I do not know that the Byttneriaceæ have any common medicinal character. The following species are alleged to be Mucilaginous. Some other species are Leäntic in consequence of containing other Leäntic principles, and will accordingly be mentioned in another group.

BYTTNEREÆ.

GAZUMA ULMIFOLIA (*Lind.*)

HERMANNIEÆ.

WALTHERIA DURADINHA (*St. Hilaire.*)

MELOCHIA CORCHORIFOLIA (*Linn.*)

DOMBEYEÆ.

KYDIA CALYCINA (*Roxburgh.*)

Lindley says that "Pterosperma (of the tribe Dombeyeæ) are all Mucilaginous. (*Ibidem*, Pg. 364.) De Candolle recognizes and describes four species of Pterospermum, viz.

PTEROSPERMUM ACERIFOLIUM (*Willd.*)

PTEROSPERMUM SUBERIFOLIUM (*Willd.*)

PTEROSPERMUM LANCEIFOLIUM (*Roxb.*)

PTEROSPERMUM SEMISAGITTATUM (*Roxb.*)

TILIACEÆ.

Lindley says of the Tiliaceæ that "they all have a mucilaginous wholesome juice." (*Ibidem*, Pg. 372.) The following species of the order are used as Leäntics.

TILIEÆ.

TILIA AMERICANA (*Linn.*)

TRIUMFETTA LAPPULA (*Linn.*)

TRIUMFETTA SEMITRILOBA (*Linn.*)

SAUVAGESIACEÆ.

This natural order is small and at the time of the publication of Lindley's Vegetable Kingdom, was not known to comprise more than three genera and fifteen species, and yet, small as it is, its species are not known to possess any common medicinal character. The following species is alleged to be very mucilaginous, and is used as a Leäntic. I do not know that any other species possesses the same properties.

SAUVAGESIA ERECTA (*Linn.*)

CUCURBITACEÆ.

Lindley says of the Cucurbitaceæ that "the seeds of all the species are Oily and capable of forming very readily an emulsion." Some of them "when pressed yield an abundance of Oil, equal to that of the finest Olives." (*Ibidem*, Pg. 314.) I have never been acquainted in any part of the U. S. A. where these seeds were not in high popular favor, and in constant use as Leäntics; and I have visited, traveled-in and seen more or less of the original thirteen states. But they are more in use on account of the Mucilage which they contain, than on account of their Oil.

CUCURBITEÆ.

CUCUMIS SATIVUS (*Linn.*)

CUCUMIS MELO (*Linn.*)

CITRULLUS VULGARIS (*Schrader.*)

CUCURBITA PEPO (*Linn.*)

CUCURBITA POTIRO (*Persoon.*)

CUCURBITA CERATOCREAS (*Haberle.*)

MOMORDICA DIOICA (*Roxburgh.*)

BRYONIA ROSTRATA (*Rottler.*)

ULMACEÆ.

ULMEÆ.

ULMUS CAMPESTRIS (*Linn.*)

ULMUS EFFUSA (*Willdenow.*)

ULMUS AMERICANA (*Linn.*)

ULMUS SUBEROSA (*Mæench.*)

ULMUS FULVA (*Michaux.*)

CELTEÆ.

CĒLTIS AUSTRALIS (*Linn.*)

APIACEÆ.

SCANDICEÆ.

ANTHRISCUS CEREFOLIUM (*Hoffman.*)

DAUCEÆ.

DAUCUS CAROTA (*Linn.*)

AMYGDALACEÆ.

Lindley says of the Amygdalaceæ that "all of them yield a gum analagous to Gum Tragacanth." (*Ibidem*, Pg. 558.) They are not mentioned however, in this place, on account of this Gum, but on account of the emulsions, which are prepared with the kernels of their fruit.

AMYGDALUS COMMUNIS (*Linn.*)AMYGDALUS NANA (*Linn.*)

PYRACEÆ.

CYDONIA VULGARIS (*Persoon.*)

HYDROPHYLLACEÆ.

HYDROLEA ZEYLANICA (*Vahl. Linn.*)

LAURACEÆ.

SASSAFRAS OFFICINALE (*Nees.*)BENZOIN ODORIFERUM (*Nees.*)

CRASSULACEAÆ.

Lindley says that "the common quality of the order (Crassulaceæ) is refrigerant, by which as far as I can judge, is intended, in this place, mucilaginous." (*Ibidem*, Pg. 345.)

CRASSULEÆ.

CRASSULA TETRAGONA (*Linn.*)SEDUM TELEPHIUM (*Linn.*)

SEDUM ALTISSIMUM (*Poiret.*)
 SEMPERVIVUM TECTORUM (*Linn.*)
 BRYOPHYLLUM CALYCINUM (*Salisb.*)
 UMBILICUS PENDULINUS (*De Cand.*)
 CALANCHOE BRASILIENSIS (*Martius?*)

ARTOCARPACEÆ.

ARTOCARPUS INCISSUS (*Linn. fil.*)
 ARTOCARPUS INTEGRIFOLIUS (*Linn.*)
 POUROUMA BICOLOR (*Martius.*)
 POUROUMA ACUMINATA (*Martius.*)
 POUROUMA CECROPIIFOLIA (*Martius.*)

BORAGINACEÆ.

Lindley says of this order that "soft mucilaginous and emollient properties are the usual characteristics of the order." He adds, "some are also said to contain Niter, a proof of which is shown by their frequent decrepitation when thrown in the fire." (*Ibidem*, Pg. 656.)

ANCHUSEÆ.

SYMPHYTUM OFFICINALE (*Linn.*)
 SYMPHYTUM TUBEROSUM (*Linn.*)
 BORRAGO OFFICINALIS (*Linn.*)
 CERINTE MAJOR (*Linn.*)

ASTERACEÆ.

ENTUSSILAGINEÆ.

TUSSILAGO FARFARA (*Linn.*)

INULEÆ.

INULA HELENIUM (*Linn.*)

CONYZEÆ.

CONYZA BALSAMIFERA (*Willdenow.*)

SPHÆRANTHEÆ.

SPHÆRANTHUS COCHINCHINENSE (*Loureiro.*)

LILIACEÆ.

TULIPEÆ.

LILIUM CANDIDUM (*Linn.*)ERYTHRONIUM DENS CANIS (*Linn.*)

SCILLEÆ.

HYACINTHUS NON-SCRIPTUS (*Linn.*)ALLIUM CEPA (*Linn.*)

ASPARAGEÆ.

POLYGONATUM MULTIFLORUM (*Des Font.*)POLYGONATUM PUBESCEHS (*Pursh.*)POLYGONATUM LATIFOLIUM (*Des Font.*)

CONVOLVULACEÆ.

CONVOLVULEÆ.

ARGYREIA BRACTEATA (*Wallich.*)CONVOLVULUS GEMELLUS (*Linn.*)IPOMŒA PES-CAPRÆ (*Swartz.*)

ACANTHACEÆ.

Lindley says that “for the most part the Acanthaceæ are Mucilaginous and slightly bitter.” Lindley adds that “the genuine Acanths” (by which I suppose he means the Acanthi) “are Emollients.” (*Ibidem*, Pg. 679.)

ACANTHEÆ.

ACANTHUS MOLLIS (*Linn.*)

JUSTICIEÆ.

JUSTICIA TRIFLORA (*Vahl.*)

CORDIACEÆ.

Lindley says that “the flesh of the fruit of the Cordiaceæ is succulent, Mucilaginous and Emollient.” (*Ibidem*, Pg. 628.)

CORDIA LATIFOLIA (*Roxburgh.*)CORDIA MYXA (*Linn.*)

VERBENACEÆ.

VITICEÆ.

GMELINA PARVIFLORA (*Roxburgh.*)GMELINA ASIATICA (*Linn.*)

ILLECEBRACEÆ.

ILLECEBRUM LANATUM (*Linn.*)*vel potius.*

AMARANTACEÆ.

AËRUA LANATA	} <i>Jussieu.</i>
<i>vel</i>	
Aërva lanata	

The above names designate one and the same article. I think it probable that the last is the most appropriate.

POLYPODIACEÆ.

Lindley says of the Polypodiaceæ that "the leaves generally contain a thick Astringent Mucilage, with a little aroma, on which account many are considered Lenitive and Pectoral," etc. (*Ibidem*, Pg. 79.)

POLYPODEÆ.

PTERIS AQUILLINA (*Linn.*)POLYPODIUM CALAGUALA (*Ruiz.*)

OSMUNDEÆ.

OSMUNDA REGALIS (*Linn.*)OSMUNDA SPECTABILIS (*Willdenow.*)

CHENOPODIACEÆ.

AGATHOPHYTUM BONUS-HENRICUS (*Moquin Tandon.*)

VIOLACEÆ.

VIOLEÆ.

VIOLA PEDATA (*Linn.*)

LAMIACEÆ.

MONARDEÆ.

SALVIA HORMINUM (*Linn.*)

ROSACEÆ.

ROSEÆ.

ROSA CANINA (*Linn.*)

LARDIZABALACEÆ.

BURASAIA MADAGASCARIENSIS (*Petit-Thou.*)

SCROFULARIACEÆ.

SIBTHORPEÆ.

SCOPARIA DULCIS (*Linn.*)

CRESCENTIACEÆ.

CRESCENTIA CUJETE (*Linn.*)

SOLANACEÆ.

SOLANUM TUBEROSUM (*Linn.*)

ILICACEÆ.

ILEX AQUIFOLIUM (*Linn.*)

PORTULACACEÆ.

PORTULACA OLERACEA (*Linn.*)

MESEMBRYATEMACEÆ.

LEWISIA REDIVIVA (*Pursh.*)

NYMPHÆACEÆ.

NYMPHÆEÆ.

NYMPHÆA LOTUS (*Linn.*)

COMBRETACEÆ.

TERMINALIEÆ.

TERMINALIA CATAPPA (*Linn.*)

CINCHONACEÆ.

ISERTIÆ.

ISERTIA COCCINEA (*Vahl.*)

CASSYTHACÆ.

{ CASSYTHA FILIFORMIS (*Miller.*)
 { *Rhipsalis Cassytha* (*Gært. De Cand.*)

MENISPERMACEÆ.

{ COCCULUS VILLOSUS β (*De. Cand.*)
 { *Cocculus Sepium* (*Colebrooke*)?
 { *Menispermum hirsutum* (*Linn.*)

SMILACEÆ.

Lindley says that "the Diuretic and Demulcent powers of Sarsaparilla are well known." (*Ibidem*, Pg. 215.) At all events, where any of the species of Smilax grow they are almost invariably used as Leäntics, i. e. either as Demulcents or Emollients.

SMILAX OFFICINALIS (*Kunth.*)SMILAX SYPHILITICA (*Humbolt.*)SMILAX MEDICA (*Schlechtendahl.*)SMILAX PURHAMPUY (*Ruiz.*)SMILAX PAPYRACEA (*Poriet.*)SMILAX JAPICANGA (*Griesbach.*)SMILAX BRASILIENSIS (*Sprengel.*)SMILAX ZEYLANICA (*Linn.*)SMILAX GLABRA (*Roxburgh.*)

SMILAX LEUCOPHYLLA.

SMILAX EXCELSA (*Linn.*)SMILAX ASPERA (*Linn.*)SMILAX LANCEIFOLIA (*Roxburgh.*)SMILAX SYRINGOIDES (*Griesbach.*)SMILAX ROTUNDIFOLIA (*Linn.*)SMILAX PERFOLIATA (*Loureiro.*)SMILAX CHINA (*Linn.*)SMILAX PSEUDO-CHINA (*Linn.*)SMILAX GLYCYPHYLLA (*R. Br. Swartz, Smith.*)

Smilax glauca (*Walter*) has been commonly supposed to be identical with *Smilax Sarsaparilla* (*Linn.*) Dr. Torrey however mentions *Smilax spinulosa* (*Smith*) as being only in part the *Smilax Sarsaparilla* (*Linn.*) while he does not mention *Smilax glauca* (*Walter*) as a synonym. I am not able to explain the difficulty, but I suppose *Smilax Sarsaparilla* (*Linn.*) must, according to Dr. Torrey's view, have comprised more than one species. To one of these *Smilax spinulosa* (*Smith*) must be the appropriate name, while Torrey does not decide to which *Smilax glauca* (*Walter*) belongs, or whether it does not comprise both. This is my understanding of the subject; but I may be in an error about the whole matter. As I shall hereafter state more in detail, I have caused what was supposed to be *Smilax glauca* (*Walter*) *Smilax Sarsaparilla* (*Linn.*) to be employed in medicine, but according to report, with no effect, unless it might be a Leäntic one. I have however received similar testimony, and that too from highly judicious physicians in regard to all the species of *Smilax* that are ever found in our shops.

COPROSMANTHUS HERBACEUM (*Torrey.*)

There is a considerable number of articles that are commonly considered as having the same, or similar powers as *Sarsaparilla*, and that are constantly used as substitutes for it, which are also uniformly mentioned and employed as Demulcents, such as the following, viz.

ASCLEPIADACEÆ.

PERIPLOCEÆ.

HEMIDESMUS INDICUS (*R. Br.*)

ASCLEPEADEÆ.

SARCOSTEMMA FORSKÆHLIANUM (*R. & S.*)

SARCOSTEMMA STIPITACEUM (*R. Br.*)

GYMNEMA LACTIFERUM (*R. Br.*)

OXYSTELMA ESCULENTUM (*R. Br.*)

ASCLEPIAS TUBEROSA (*Linn.*)

STAPELIEÆ.

CEROPEGIA BULBOSA (*Roxburgh.*)

CEROPEGIA EDULIS (*Hortulanorum.*)

APOCYNACEÆ.

WRIGHTIÆ.

ICHTHOCARPUS FRUTESCENS (*R. Br.*)

LILIACEÆ.

ANTHERICEÆ.

HERRERIA SALSAPARILHA (*Martius.*)HERRERIA STELLATA (*Ruiz & Pavon.*)

ASPARAGEÆ.

LUZURIAGA RADICANS (*Ruiz & Pavon.*)

HEMEROCALEÆ.

PHORMIUM TENAX (*Forster.*)

PHILESIACEÆ.

LAPAGERIA ROSEA (*Ruiz & Pavon.*)

CANNABINACEÆ.

HUMULUS LUPULUS (*Linn.*)

CYPERACEÆ.

Lindley says that "the roots of the Cyperi are succulent and filled with a nutritive and agreeable Mucilage." (*Ibidem*, Pg. 118.) But he does not specify any particular species, though he mentions a considerable number of Cyperaceous plants. He says also that "the roots of those species of Carex, which are called German Sarsaparilla" (those which I name) "have Demulcent and Diaphoretic properties." (*Ibidem*, Pg. 118.)

CARICEÆ.

CAREX ARENARIA (*Linn.*)CAREX HIRTA (*Linn.*)CAREX INTERMEDIA (*Goodenough.*)

CAREX DISTICHA (*What is this?*)

GRAMINACEÆ.

HORDEÆ.

AGROPYRON REPENS (*P. de B.*)*AGROPYRON CANINUM* (*R. & S.*)*AGROPYRON JUNCUM* (*P. de B.*)*AGROPYRON GLAUCUM* (*R. & S.*)

CHLOREÆ.

CYNODON DACTYLON (*Richard.*)*CYNODON LINEARE* (*What is this?*)

The articles which immediately precede are the principal substitutes for Sarsaparilla that are mentioned by authors, all of which are commonly said to be Demulcent.

LEÄNTICA GUMMOSA.

The Gummy Leäntics are generally much preferred to the Mucilaginous, as they consist of a single and usually pure vegetable organic proximate principle, nearly destitute of flavor, while the various impurities commonly associated with Mucilage, give it a variety of tastes, and often such as are disagreeable. Beside this, the consistence of an aqueous solution of Gum, is more easily conformed to the exigencies of a case, or to the taste of a patient, than any ordinary preparation of a Mucilaginous article, and in addition is more easy of preservation, in its best and most perfect condition.

FABACEÆ.

As a natural order I do not think that the Fabaceæ can be said to possess any characteristic power or property, though Lindley assigns one, and in my view, is very unfortunate in his selection. Lindley says that "upon the whole" the Fabaceæ "must be considered Poisonous, and that those species which are used for food by man or" (brute) "animals are exceptions to the general rule, the deleterious juices of the order not being in such instances, sufficiently concentrated to prove injurious, and being in fact replaced to a considerable extent by either Sugar or Fæcula."

(*Lind. Veg. King. Lond.* 1846, *Pg.* 547.) Now from the best estimate that can possibly be made, there can not be less than six thousand five hundred species in the order Fabaceæ. This is in fact the number assigned to it by Mr. Bentham in 1845. Out of the whole of this number of species, I do not believe that there are a hundred articles, that can truly and properly be reckoned as Poisonous. But admitting two or three times this number, it would by no means justify us in saying that a Poisonous property is the characteristic of the order.

ACACIÆ.

ACACIA VERA (*Willd.*)

ACACIA ARABICA (*Willd.*)

ACACIA GUMMIFERA (*Willd.*)

ACACIA SENEGAL (*Willd.*)

ACACIA SEJAL (*Delile.*)

ACACIA EHRENBERGII (*Hayne.*)

ACACIA TORTILIS (*Forskæhl.*)

ACACIA HORRIDA (*Willd.*)

ACACIA VEREK.

ACACIA ADANSONII.

ACACIA DECURRENS (*Willd.*)

ACACIA MOLLISSIMA (*Willd.*)

ACACIA AFFINIS.

Qu? *Inga affinis* (*De Cand.?*)

ACACIA? GIRAFFÆ (*Willd.*)

ZYGIA SASSA.

PITHECOLLOBIUM GUMMIFERUM (*Martius.*)

VACHELLIA FARESIANA (*Wright & Arnott.*)

PARKIÆ.

PROSOPIS IULLIFLORA (*De Cand.*)

LOTEÆ.

ASTRAGALUS VERUS (*Olivier.*)

ASTRAGALUS CRETICUS (*La Marck.*)

ASTRAGALUS ARISTATUS (*Heritier.*)

ASTRAGALUS STROBILIFER.

ASTRAGALUS GUMMIFER (*Labillardiere.*)

ASTRAGALUS EXSCAPUS (*Linn.*)

ASTRAGALUS MONSPESSULANUS, (*Linn.*)

{ ASTRAGALUS TRGACANTHA (*Linn.*)
A. Massiliensis (*La Marck*) var *a* (*Linn.*)
A. Poterium (*Vahl.*) var *β* (*Linn.*)

BAUHINIÆ.

BAUHINIA RETUSA (*Roxburgh.*)

BAUHINIA EMARGINATA (*Miller.*)

DALBERGIEÆ.

PTEROCARPUS GUMMIFER (*Bertero.*)

CASSIÆ.

CASSIA AURICULATA (*Linn.*)

STERCULIACEÆ.

I have already mentioned that the Sterculiaceæ are in general remarkable for the amount of Mucilage which they contain, and I shall hereafter mention that their seeds abound in Oil. A few of them are known to yield pure Gum; and if the subject were properly investigated, I doubt not that a much greater number would be found to do this.

STERCULIÆ.

CAVALIUM URENS (*Schott.*)

SOUTHWELLIA TRAGACANTHA (*Schott.*)

BOMBEÆ.

OCHROMA LAGOPUS (*Swartz.*)

ERIODENDRON ANFRACTUOSUM (*De Cand.*)

OLEACEÆ.

I shall hereafter have occasion to mention the Oleaceæ as Oil-producing and as Sugar-producing articles. One species at least produces Gum; but I do not think that the production of either can be considered as characteristic of the order.

OLEÆ.

OLEA EUROPÆA (*Linn.*)

MELIACEÆ.

It is not a characteristic of this order to produce Gum, nor can it be said to have any characteristic property.

MELIÆ.

MELIA AZADIRACHTA (*Linn.*)

COMBRETACEÆ.

I do not think that the order Combretaceæ can be considered as having any characteristic property or properties.

COMBRETEÆ.

COMBRETUM ALTERNIFOLIUM (*Jaquin.*)

TERMINALIÆ.

TERMINALIA BELERICA, (*Roxburgh.*)

CEDRELACEÆ.

So far as I have information, the production of Gum by any species of this order is an exception to the products of the species generally ; and yet I am not apprised that the order has any characterizing property. Many of the species are however bitter and Cathartic.

CEDRELEÆ.

CHLOROXYLON DUPADA (*Buchanan or Hamilton.*)

ULMACEÆ.

The order Ulmaceæ has no characterizing properties ; nor am I apprised that more than one species produces true and pure Gum.

CELTEÆ.

CELTIS ORIENTALIS (*Linn.*)

CLUSIACEÆ.

This order has no characterizing property or properties.

CALOPHYLLÆ.

CALOPHYLLUM INOPHYLLUM (*Linn.*)

EPACRIDACEÆ.

This order has no characterizing property or properties.

EPACRIDEÆ.

ANDERSONIA PANSHMOUM (*Buchanan or Hamilton.*)

LILIACEÆ.

In all probability, different species might be found in this order, that possess a majority of all the powers of the materia medica; but I think that Gum is not a frequent product of the Liliaceæ, notwithstanding the order is supposed to comprise at least twelve hundred species.

LILLEÆ.

HYACINTHUS NON-SCRIPTUS (*Linn.*)

PINACEÆ.

It is apparently a prominent exception to what we should expect from a general view of this order, that any species of it should produce a perfect and pure Gum; nor am I apprised that more than a single species does this.

ABIETIÆ.

LARIX EUROPÆA (*De Cand.*)

CYCADACEÆ.

Although this order generally yields Mucilage and Fæcula, yet I know of but two species, that yields a clear transparent and apparently pure Gum.

CYCAS REVOLUTA (*Thunburg.*)

CYCAS CIRINALIS (*Linn.*)

CITRACEÆ.

The production of Gum, so far as I have knowledge, is not a

characteristic of this order, but an exception to its general properties so far as it may be said to have any general properties.

FERONIA ELEPHANTUM (*Correa de Serra.*)

ÆGLE MARMELOS (*Correa de Serra.*)

ANACARDIACEÆ.

If this order can be said to have any general characterizing property it is that of producing a peculiar variety of Erythema vesiculare upon the skins of those exposed to its influence. I believe that all the species which operate decidedly in this manner are Oresthetic, Antisbestic and Subdiuretic. But I greatly doubt whether any very large portion of the species commonly operate in this manner; and I believe that only a very few of them yield Gum. Lindley however admits only ninety-five species, while he enumerates forty-one genera. A small order is much more likely to have a characterizing property or properties than a large one.

ANACARDIUM OCCIDENTALE, (*Linn.*)

ANACARDIUM HUMILE (*Martius.*)

SPONDIAS MANGIFERA (*Persoon.*)

MANGIFERA INDICA (*Linn.*)

AMYGDALACEÆ.

I am inclined to think that the production of Gum may be considered as a characteristic of this order, of which Lindley numbers only five genera and one hundred and ten species. If this is not its characteristic I do not think it can be said to have one.

PRUNUS SPINOSA (*Linn.*)

{ CERASUS HORTENSIS (*Persoon.*)

{ *Cerasus vulgaris* (*Miller.*)

ARMENIACA VULGARIS (*La Marck.*)

PERSICA VULGARIS (*Miller.*)

CISTACEÆ.

The different species of this order seem to contain no proximate principles, and to possess no medicinal powers in common. So far as I have knowledge, the following is the only article that affords Gum, unless possibly it may be the other species of this same genus.

COCHLOSPERMUM GOSSYPIUM (*De Cand.*)

DIPTEROCARPACEÆ.

This order is decidedly a Resin-producing one and not a Gum-producing one; but I have heretofore mentioned a single instance of the production of Gum in a Resin-producing order, and even by a Resin-producing tree. For aught I know, one case may be as much an exception to a general rule as the other. This order contains only seven or eight genera, and about forty-seven species, and one of these produces the "hard Camphor of Sumatra." "Wood Oil" is an other product of this order, and what else I am unable to tell.

{ SHOREA ROBUSTA (*Roxburgh.*)
 { *Vatica.*

SMILACEÆ.

This order consists of two genera and about a hundred and twenty species. I have no knowledge that any of these, beside that which I mention, ever produces Gum, though all probably contain Fæcula.

SMILAX PSEUDO-CHINA (*Linn.*)

ASTERACEÆ.

I have no knowledge that the Asteraceæ as a whole, or any natural sub-division of them have any characterizing properties; and if they have I think they are certainly not Gum-producing.

CYNAREÆ.

CARLINEÆ.

CARLINA GUMMIFERA (*Lessing.*)

The next article stands as a valuable Gum-producing plant, in an excellent work on materia medica which is now open before me; and yet I do not find it described under this name, in any descriptive work on botany in my possession. This may be the fact, and yet the article be well known to gentlemen of more botanical knowledge, and better libraries than I possess. Perhaps some of those practitioners of medicine, who claim to be botanical by way of eminence (so much so that no others are worthy of this designation in comparison with themselves) might solve the

difficulty. Is there a typographical mistake in regard to the name? Was the name changed before it got into any systematic work, so as to be quoted as a synonym? We find *Acaroïs Resinifera* and *Acoroïdes Resinifera* in works on *materia medica*, but never in descriptive works on botany. I retain the article, whether the name is correct or not, because the Gum is said to be excellent. I may have mentioned it before under some other name. Doubtless many of my readers will understand it.

CHIRONGIA GLABRA (*Buchanan or Hamilton.*)

LEÄNTICA PECTICA.

The Pectic Acid Leäntics are a peculiarly valuable group. Preparations of them may be made either exceedingly thin or quite thick, according to the taste of the patient, or the advice of the physician. Pectic Acid is easily separated from the crude plant, and no proximate principle is liable to accompany it and render it in any degree disagreeable, which may not be as easily separated, at least from the crude plant. No group of this class is capable of being varied, and flavored in so many different ways, either as Leäntics, or as nutrients, as preparations of Pectic Acid, a circumstance which gives it a great superiority over most other medicines and most other articles of diet. Pectic Acid is capable of what appears to be quite perfect primary and secondary digestion; and I imagine that there are few articles of diet, so large a proportion of which is capable of ultimate assimilation. When only so much of it is taken, as may be indicated as a Leäntic, it may at the same time furnish the system with all the food that the case requires. To me scarcely any thing is more agreeable to the taste than preparations of Pectic Acid; but I do not find any of my patients who like it, when it is called Sea-Weed. It is relished very much better under the name of Moss, which some choose to call it; but I have always found that it is liked when it is called Custard for example, though without a particle of Egg in its composition. With many, names are all important, indeed almost every thing. A Radish may be liked, but Horse Radish is too vulgar to be eaten. At all events I have often witnessed this. A *medicine* will be submitted to, but a *Cathartic* is absolutely intolerable. It would seem to be much to be regretted that the popular names of the medicinal and edible

Algæ, Lichenes, Fangi, &c. can not from time to time be mutually interchanged, since I am well satisfied that a Sea-Weed would, as a general rule, be much better esteemed as a Moss, as a Mushroom, as a Lichen, etc. Fashion, in medicine, has never had due importance attached to it.

I do not know that all the Algaceæ, which I have enumerated among the Leäntica Pectica, contain Pectic Acid; and much less even that any of the Lichenaceæ contain it, though I think there is good ground for the strongest suspicion that they do. The Leäntic power of the latter may possibly be due to some variety of Mucilage, as may that of a part of the former, though I think not. It will be sufficient that an article contains some thing like Pectic Acid in Leäntic and nutrient powers, to entitle it to a place in my list or catalogue of this class. What have been known as "Edible Birds' Nests" in the extreme East, have been considered valuable, not only as agents of this class, but also as articles of diet. In connexion with articles that are Leäntic and nutrient in consequence of containing Pectic Acid, or some proximate principle similar or analogous to it, I shall endeavor to ascertain whether the essential principal of these Nests is any thing of this sort, or is furnished by any Alga, and shall consequently quote the best authorities upon the subject. Meyen in his voyage round the world, states that "the Japanese had long ago discovered that the costly Birds' Nests are nothing more than softened Sea-Weed; and that they now prepare the substance itself in an artistic manner." (*G. Sigmond, M. D., on Ceylon Sea-Weed, 2d Edit. Lond. 1841, Pg. 19.*) Meyen however does not inform us what species of Alga the Japanese "soften" for this purpose, nor how they "prepare the substance itself."

FUCACEÆ.

FUCEÆ.

FUCUS CARTILAGINOSUS.

Lindley says that "the Tsantjan or Kanten (*Fucus cartilaginossus*) is used in China as a substitute for the Edible Birds' Nests, and seems to have the composition of Pectic Acid, Gum, Starch, and a considerable quantity of inorganic matter, especially Sulphate of Calcia." (*Lindl. Veg. Kingd. Lond. 1846, Pg. 24.*) It

must be observed here that Lindley does not claim that this species enters into the composition of these Birds' Nests, or that any principle from it does so; but only that it "is used in China as a substitute for the Edible Birds' Nests, and seems to have the same composition," etc. Confessedly then, this article does not produce the essential principle of these Nests. But what is the species here intended? None under this name is mentioned in any descriptive work on botany, to which I happen to have access. In all probability it does not belong to the genus *Fucus*, for Lindley mentions it under *Ceramiaceæ*. Can Lindley have intended *Fucus cartilagineus* (*Linnaeus*) which I shall hereafter mention as *Gelidium versicolor* (*Lamouroux*) or *Fucus cartilagineus* (*Hudson*) which I shall hereafter mention as *Desmia Hornemanni* (*Lyngbye*.) I know not how this question can be answered with the least certainty.

DURVILLÆA UTILIS,

DURVILLÆA EDULIS (*Bory. St. Vincent.*)

"The same nutritious properties, which render *Plocaria candida* so valuable to the Ceylonese, are found by the natives of Chili in *Durvillæa edulis*." (*G. G. Sigm. on Ceyl. Sea-Weed, 2 Edit. Lond. 1841, Pg. 71-2.*)

LAMINARIÆ.

LAMINARIA DIGITATA (*Lamouroux*.)

LAMINARIA SACCHARINA (*Lamouroux*.)

"The same nutritious properties which render *Plocaria candida* so valuable to the Ceylonese, are found by the Japanese in *Laminaria Saccharina*." (*Ibidem, Pg. 71.*)

LAMINARIA BRACTEATA,

LAMINARIA POTATORUM (*Lamouroux*.)

ALARIA ESCULENTA (*Greville*.)

CYSTOSEIRÆ.

SARGASSUM ACANTHOCARPUM,

SARGASSUM CUNEIFOLIUM,

SARGASSUM PYRIFORME, (*Agardh*.)

SPOROCHNEÆ.

- { DESMARESTIA,
- { *Desmia Hornemanni*, (Lyngbye.)

I have mentioned this article heretofore. It is supposed to contain much Jelly, and may be one of those species, which have been used as a substitute for the Edible Birds' Nests.

CERAMIACEÆ.

SPHÆROCOCCEÆ.

- RHODYMENIA PALMATA (*Greville.*)
- { GRACILARIA SPINOSA,
- { *Plocaria.*
- { PLOCARIA TENAX (*Nees Von Esenbeck.*)
- { *Gracilaria.*
- { PLOCARIA COMPRESSA (*Nees Von Esenbeck.*)
- { *Gracilaria.*
- { PLOCARIA CANDIDA (*Nees Von Esenbeck.*)
- { *Gracilaria Lichenoides* (*Greville.*)

Professor George G. Sigmond says that the Swallow, whose Nest is esculent, prepares it from *Fucus Amylaceus* (*O'Shaughnessy*) as well as from several other species of the same order. (*G. G. Sigmond M. D. on Fucus Amylaceus, 2d Edit. Lond. 1841, Pg. 16.*) By *Fucus Amylaceus* (*O'Shaughnessy*) I understand what I have just called and mentioned as *Plocaria candida* (*Nees Von Esenbeck.*) We are told by Wm. H. Harvey M. D. (in his *Nereis Boreali-Americana, Washington, 1853, Part II. Pg. 106*) that "the name *Plocaria candida* was given by *Nees Von Esenbeck* to the Ceylon Sea-Weed of commerce, which he himself calls *Gracillaria Lichenoides*. *Lindley* also seems to recognize that *Plocaria candida* (*Nees Von Esenbeck*) and *Fucus Amylaceus* (*O'Shaughnessy*) are the same, for he says that *Plocaria candida* (*Nees Von Esenbeck*) or *Fucus Amylaceus* (*O'Shaughnessy*) "has been found to consist of Pectine" (Pectic Acid) "Gum and Starch, with a considerable quantity of inorganic matter, especially Sulphate of Lime" (or Calcia) (*Lindl. Veget. Kingd. Lond. 1846, Pg. 24.*) W. H. Harvey M. D. says that "many, perhaps all" (of the species of *Gracilaria* or *Plocaria*) "may be reduced by boiling to a tasteless Gelatine"

(doubtless Jelly is here intended and not Gelatine) "which when properly seasoned" (flavored) "is palatable and considered wholesome," and that "*Gracilaria Lichenoïdes*" (Greville) "yields a very tenacious Jelly." (*Wm. H. Harvey M. D. Nereïs Boreali-Americana, Washington, 1853, Prt. II. Pg. 107.*) Frederic Farre, M. D., says that the Agar-Agar of the Malays is identical with the Ceylon Sea-Weed, which he treats-of under the name *Gigartina Lichenoïdes* (*See a Paper by Fr. Farre M. D. in Prof. Sigm. on Ceylon Sea-Weed, 2d Edit. Lond. 1841, Pg. 68.*) It is my opinion that Dr. Farre is intirely mistaken in supposing that Agar-Agar of the Malays is identical with what has been called *Fucus Amylaceus* (*O'Shaughnessy.*) At all events Wm. H. Harvey, M. D. (*Nereïs Boreali-Americana, Washington, 1853, Introduction, Pg. 33*) says that the "Agar-Agar or Agal-Agal" of the Chinese is *Gracilaria spinosa*. He says of the genus *Gracilaria* that it was "originally proposed by Dr. Greville in his *Algæ Britannicæ*," and that it was amended by Professor J. Agardh, by the rejection of such species as do not accord with a stricter and more limited character. He adds that "it is the same as *Plocaria* (*Endlicher*) and to a great extent, as the first section of Kuetzing's genus *Sphærococcus*." "The name *Plocaria* has been extended to the *Grevillii*-Agardhian genus by Endlicher, on the plea that it had the priority in the order of publication." "But," (says Dr. Harvey) "I agree with Professor Agardh in regarding the mutation of an established generic name as being in this instance uncalled-for, in as much as the name *Plocaria* (which has very little the priority over *Gracilaria*)" (but observe here that is admitted to have the priority) "was given in ignorance of the natural affinities of the plant so called, Nees believing it to be a Lichen; nor is it very certain whether the author intended to include in his description one or many species." "The type of this genus is *Fucus Confervoïdes* (*Linn.*) a widely dispersed plant found from the tropics to a high latitude in both hemispheres." (*Wm. H. Harvey M. D. Nereïs Boreali-Americana Washington, 1853, Prt. II. Pg. 106.*) *Plocaria* then is admitted to be the prior name, and as a name, it is in itself unexceptionable. Why then should it be changed? The fact that the affinities of the plant were not understood by Nees Von Esenbeck, when he gave the name, is no sound objection. A

large portion of the names throughout botany were given under similar circumstances; but it furnishes no reason why they should not be received. However, as I happen to be ignorant whether all the plants which authors call *Gracilaria*, are true *Plocariæ*, and vice versa whether all plants which authors call *Plocaria* are true *Gracilaria*, I have determined to use the name employed by the author from which I quote, mentioning the other with a note of interrogation as a synonym. This would not be necessary if I could arrive even at a probability that the species was correctly referred, and could always obtain the authority for the reference. This will be an awkward method; but I shall have occasion to practice it only a few times.

In opposition to Dr. Harvey, Lindley merges *Gracilaria* in *Plocaria*, and as appears to me, very properly even upon Harvey's own principles, since it is the prior name, and certainly as little exceptionable as *Gracilaria*. It is of some importance to know that it has been believed by some that the essential ingredient of the Edible Birds' Nests is furnished by the article now under consideration, in order to understand various authors, and the references which they make to the subject, though I think I shall show in the sequel that the opinion in question is an error.

CRYPTONEMÆ.

GIGARTINA SPECIOSA (*Sonder.*)

IRIDÆA EDULIS.

CHONDRUS CRISPUS (*Lyngbye.*)

"The same nutritious properties which render *Plocaria candida* so valuable to the Ceylonese, are found in *Chondrus crispus*." (*G. G. Sigm. Ceyl. Sea-Weed, 2d Edit. Lond. 1841, Pg. 71-2.*)

HALYMENIA PALMATA (*Agardh.*)

"The same nutritious properties which render *Plocaria candida* so valuable to the Ceylonese, are found by the Icelanders in *Halymenia palmata* (*Ibidem.*)

GELIDIUM CORNEUM (*Lamouroux.*)

Lindley says that *Gelidium* "furnishes the Edible Birds' Nests of the Chinese." (*Lind. Flor. Med. 1838, Pg. 630.*) Again Lindley says that "the material out of which" (certain) "Swallows construct the esculent nests, which are so highly valued by the

Chinese, is supposed to be a species of *Gelidium*." (*Lindl. Veget. Kingd. Lond. 1846, Pg. 24.*) *Gelidium corneum* (*Lamouroux*) is the species named by Frederic Farre M. D. in Dr. Sigmond's work. He says that this is the species, filaments of which have been supposed to be found in the Edible Nests (*Fred. Farre M. D. in Prof. Sigmond's Work on Ceylon Sea-Weed, Lond. 1841, Pg. 72-3.*) "Fée is of opinion that the real plant" (of which the Edible Birds' Nests are composed) "is *Gelidium corneum*" (*Lamouroux*) (*G. G. Sigm. on Ceyl. Sea-Weed, 2d Edit. Lond. 1841, Pg. 72-3.*) W. B. O'Shaughnessy M. D. says "one of the most singular (of the Sea-Weeds) "is *Gelidium corneum* (*Lamouroux*) "valuable from the immense quantity of soluble and nutritive Pectine, which it contains, and from its entering (according to the statement of Fée) into the formation of the Edible Swallow's Nests." (*Ibidem, Pg. 81.*)

GELIDIUM VERSICOLOR (*Lamouroux.*)

I have had occasion heretofore to mention an article which may quite likely be this species, which is said to be used as a substitute for the Edible Bird's Nests. It is represented as containing much Pectic Acid, Gum, Starch, etc. If it contains these principles, it must be valuable not only as a Leäntic, but as an article of diet for the sick. I think it can be satisfactorily shown that the Edible Bird's Nests are an animal and not a vegetable production; but the fact that the Pectic Acid of so many Algæ is so near like these Nests, as to induce so many distinguished and highly intelligent naturalists to suppose them identical, is an interesting fact, and argues very strongly that these Algæ may very well be substituted for the Nests. As I purpose to mention these Nests subsequently, and in a more appropriate connexion, I shall here only say further, that it was necessary and proper to say what I have already said in order to correct some widely prevalent mistakes in regard to several valuable Algæ.

LOMENTARIEÆ.

LAURENCIA PINNATIFIDA (*Lamouroux.*)

LICHENACEÆ.

The medicinal and nutrient preparations of the Lichenaceæ are so much like those of the Fucaceæ, that I have ventured to put

them together as *Leäntica pectica*, though I do not know that the Jelly obtained from the *Lichenaceæ* is actually Pectic Acid.

USNEÆ.

CETRARIA ISLANDICA (*Acharius*.)

CETRARIA NIVALIS (*Acharius*.)

CETRARIA CUCULATA (*Acharius*.)

PYXINEÆ.

GYROPHORA PROBOSCIDEA (*Acharius*.)

GYROPHORA CYLINDRICA (*Acharius*.)

GYROPHORA PUSTULATA (*Acharius*.)

GYROPHORA MUHLENBERGII (*Acharius*.)

By *Acharius* the genus *Umbilicaria* is wholly merged in *Gyrophora*; but by *Endlicher* and *Lindley* the genus *Gyrophora* is wholly merged in *Umbilicaria*. Now the name which was first imposed ought to have the precedence; but at present I have no means of ascertaining this point, and therefore I follow *Acharius*, the eldest if not the best authority.

PARMELIÆ.

STICTA PULMONACEA (*Acharius*.)

ALECTORIA USNEOIDES (*Acharius*.)

No such genus as *Alectoria* is recognized by *Lindley*, and yet there are several species mentioned by *Acharius*, of which he gives no synonyms showing that they have ever been referred to any other genus.

LECIDEÆ.

CENOMYCE RANGIFERINA (*Acharius*.)

CENOMYCE PYXIDATA (*Acharius*.)

Acharius wholly merges the genus *Cladonia* in the genus *Cenomyce*, while *Lindley* wholly merges *Cenomyce* in *Cladonia*. When two names are equally eligible, that first employed should always be retained. But in this case, I have no means of ascertaining this point. As I have done in another instance, I follow *Acharius* as the oldest if not the best authority.

COLLEMEÆ.

{ COLLEMA LACERUM (*Acharius*.)
 { ? *Leptogium lacerum* (*Fries Tuckerman*.)

In a work on materia medica now before me several Lichenes are mentioned as valuable, that do not seem to be described in any work on botany to which I can now refer. These are

LICHEN CORALLOIDES,
 LICHEN LEPROSUS,
 LICHEN NIVEUS.

I have no knowledge of these articles. It is possible that Lichen niveus may be a mistake for Lichen nivalis (*Linnaeus*) Lobaria nivalis (*Hoffman*) Parmelia nivalis (*Sprengel*) Cetraria nivalis (*Acharius*) but this is only conjecture. As respects the preceding two, I can not even hazard a conjecture in regard to them.

LEÄNTICA FARINACEA.

The farinaceous Leäntics, as a general rule, are supposed to be more agreeable to the taste, and more nutritious, but they are commonly esteemed rather less efficacious, and they are rather less convenient of preparation. The principal reason however, why they are less used than the Mucilaginous Leäntics, I suspect, is to be sought-for in the fact that their nutritious properties strike both patients and physicians more prominently, than the nutritious properties of the Mucilaginous Leäntics, and therefore more palpably contravene the starvation principles in regard to regimen, which are now so often entertained by many practitioners of medicine.

Farina-producing articles.

By Farina-producing articles I intend those which are used in the form of Meal as Leäntics. All of the group which I call farinaceous would, by the proper process, yield Fæcula. Some times articles of this group are boiled soft and then reduced to pulp by mashing and moistening by way of preparation for use.

Cerealìa.

The next fifty-seven articles are regular Cereals, being in constant use by some people, and in some countries as Bread-Corn.

Some of them being always at hand, they are very often employed as Leäntics.

GRAMINACEÆ.

HORDEÆ.

- { TRITICUM VULGARE (*Villars.*)
- { *Triticum sativum* (*La Marck.*)
- V. ÆSTIVUM (*Linn.*)
- V. HYBERNUM (*Linn.*)
- TRITICUM COMPOSITUM (*Linn.*)
- TRITICUM DURUM (*Des Fontaines.*)
- TRITICUM SPELTA (*Linn.? Willd.*)
- TRITICUM AMYLEUM (*Seringe. Nees.*)
- TRITICUM TURGIDUM (*Linn.*)
- TRITICUM POLONICUM (*Linn.*)
- TRITICUM MONOCOCCON (*Linn.*)

The preceding species of Wheat are by far the most extensively cultivated; but more than a dozen other supposed species are likewise cultivated as Bread-Corn in some countries, all of which are doubtless as effectual Leäntics as those already named. I here refer only to such as are cultivated, and not to those which grow wild and spontaneously. Those to which I refer are the following, viz. *Triticum Linnæanum* (*Ræmer & Schultes*); *Triticum fastuosum* (*Lagasca*); *Triticum Gärtnerianum* (*Lagasca*); *Triticum platystachyum* (*Lagasca*); *Triticum Cochleare* (*Lagasca*); *Triticum Cevallos* (*Lagasca*); *Triticum Siculum* (*Ræmer & Schultes*); *Triticum Hordeiforme* (*Host*); *Triticum compactum* (*Ræmer & Schultes*); *Triticum atratum* (*Ræmer & Schultes*); *Triticum Zea* (*Host*); *Triticum Cienfuegos* (*Lagasca*); *Triticum Bauhini* (*Lagasca*).

- SECALE CEREALE (*Linn.*)
- HORDEUM VULGARE (*Linn.*)
- HORDEUM DISTICHUM (*Linn.*)
- HORDEUM HEXASTICHUM (*Linn.*)
- HORDEUM ZEOCRITON (*Linn.*)

ORYZÆ.

- ORYZA SATIVA (*Linn.*)
- ORYZA SUBULATA (*Nees Von Esenbeck.*)

*ORYZA GLUTINOSA (Rumphius.)**ANDROPOGONEÆ.**SORGHUM VULGARE (Persoon.)**SORGHUM CERNUUM (Willd.)**SORGHUM SACCHARATUM (Persoon.)**SORGHUM BICOLOR (Willd.)**PANICEÆ.**PANICUM MILIACEUM (Linn.)**PANICUM FRUMENTACEUM (Roxburgh.)**PANICUM PILOSUM (Swartz.)**SETARIA GERMANICA (Palisot de Beauvois.)**SETARIA ITALICA (Palisot de Beauvois.)*

{ *PENNISETUM TYPHOÏDEUM (Persoon.)*
Pennicillaria spicata (Ræmer & Schultes.
Holcus spicatus (Linn.)

*PASPALUM EXILE.**PHALAREÆ.**PHALARIS CANARIENSIS (Linn.)**ZEÀ MAYS (Linn.)**ZEÀ CARAGUA (Linn.)**STIPEÆ.**STIPA PENNATA,**ORYZOPSIS ASPERIFOLIA (Michaux.)**FESTUCEÆ.**GLYCERIA FLUITANS (R. Brown.)**CHLOREÆ.**ELEUSINE TOCUSSA.**ELEUSINE CORACANA (Gærtner.)**ELEUSINE STRICTA (Roxburgh.)**AGROSTEÆ.**SPOROLOBUS (Abyssinicus.)*

AVENEÆ.

AVENA SATIVA (*Linn.*)

AVENA NUDA (*Linn.*)

ZIZANIA AQUATICA (*Linn.*)

POLYGONACEÆ.

POLYGONEÆ.

PHEGOPYRUM ESCULENTUM (*Moench.*)

PHEGOPYRUM TATARICUM.

AMARANTACEÆ.

ACHYRANTHEÆ.

AMARANTUS FRUMENTACEUS (*Buchanan.*)

AMARANTUS ANARDANA (*Hamilton.*)

DIOSCOREACEÆ.

DIOSCOREA SATIVA (*Linn.*)

DIOSCOREA ACULEATA (*Linn.*)

DIOSCOREA ALATA (*Linn.*)

DIOSCOREA BULBIFERA (*Linn.*)

DIOSCOREA PURPUREA (*Hortus Bengalensis.*)

TAMUS COMMUNIS (*Linn.*)

CONVOLVULACEÆ.

CONVOLVULUS FARINOSUS (*Linn.*)

{ IPOMÆA MACRORRHIZA (*Michaux & Lindley.*)
{ *Batatas Julapa* (*Choisy.*)

BATATAS EDULIS (*Choisy.*)

NELUMBIACEÆ.

NELUMBIUM SPECIOSUM (*Willd.*)

NELUMBIUM LUTEUM (*Willd.*)

SOLANACEÆ.

SOLANUM TUBEROSUM (*Linn.*)

OXALIDACEÆ.

- OXALIS CARNOSA (*Molina.*)
 OXALIS CRASSICAULIS (*Zuccarini.*)
 OXALIS CRENATA (*Jaquin.*)
 OXALIS DEPPEI.
 OXALIS ESCULENTA.
 OXALIS TETRAPHYLLA (*Cavanilles.*)
 OXALIS TUBEROSA (*Molina. Savi.*)

The following Oxalides have tuberous roots, at least small ones, which would probably become large by culture, like those already cultivated, and be equally farinaceous, viz. *Oxalis debilis* (*H. B. & K.*); *Oxalis elegans* (*H. B. & K.*); *Oxalis grandifolia* (*De Candolle*); *Oxalis Jacquini* (*H. B. & K.*); *Oxalis latifolia* (*H. B. & K.*); *Oxalis violacea* (*Linn.*) The following Oxalides have large and fleshy roots, though they are not strictly tuberous, viz. *Oxalis conorrhiza* (*Jacquin*); “*Radice crassa, fusiformi.*” (*Sprengel*); *Oxalis megalorrhiza* (*Jacquin*); “*Radice crassa multiplicite.* (*De Candolle*); “*Radice crassissima ramosa.*” (*Sprengel.*)

APIACEÆ.

AMMIEÆ.

- CARUM BULBOCASTANUM (*Koch.*)
 BUNIUM DENUDATUM (*De Cand.*)

LAMIACEÆ.

OCYMEÆ.

- OCYMUM TUBEROSUM.

AMARYLLIDACEÆ

- ALSTRÆMERIA EDULIS (*Tussac. Andrews.*)
 ALSTRÆMERIA PALLIDA.
 ALSTRÆMERIA SALSILLA (*Linn. Willd.*)

ASTERACEÆ.

COREOPSEÆ.

- HELIANTHUS TUBEROSUM (*Linn.*)

CENTAUREÆ.

CENTAUREA BEHEN (*Linn.*)

LILIACEÆ.

ASPARAGEÆ.

POLYGONATUM VULGARE (*Des Fontaines.*)

POLYGONATUM CANALICULATUM (*Pursh.*)

TULIPEÆ.

ERYTHRONIUM DENS-CANIS (*Linn.*)

SMILACEÆ.

SMILAX CHINA (*Linn.*)

SMILAX PSEUDO-CHINA (*Linn.*)

CUCURBITACEÆ.

CUCURBITEÆ.

BRYONIA DIOICA (*Jacquin.*)

IRIDACEÆ.

GLADIOLUS COMMUNIS.

ARTOCARPACEÆ.

ARTOCARPUS INCISSUS (*Linn.*)

COMMELYNACEÆ.

COMMELYN AFRICANA (*Linn.*)

TRADESCANTIA (ÆTHIOPICA ?)

URANIA (Koba ?)

FABACEÆ.

CLITORIEÆ.

PUERARIA TUBEROSA (*De Cand.*)

VICIEÆ.

FABA VULGARIS (*Mæench.*)

ERVUM LENS (*Linn.*)ERVUM ERVILIA (*Linn.*)

LOTEÆ.

LUPINUS ALBUS (*Linn.*)

SWARTZIEÆ.

HYMENÆA COURBARIL (*Linn.*)HYMENÆA STILBOCARPA (*Hayne.*)HYMENÆA STIGONOCARPA (*Martius.*)

POLYPODIACEÆ.

POLYPODEÆ.

POLPODIUM CALAGUALA.

Tapioca-producing Articles.

By Tapioca-producing articles I intend those which are used in the form of Fæcula, as Leäntics. Of course I do not attempt any distinction between what is commonly called Arrow-root Fæcula (though obtained from other genera besides *Maranta*) and what is called Tapioca, since they are always essentially the same, and some times they are absolutely identical. What is commonly called Tapioca is any Fæcula, Amylum or Starch, first obtained pure, then "sprinkled with a little water, and boiled" (half cooked) "in steam." "In this way, it is converted into viscid irregular masses, which are to be dried in the sun till they have become quite hard, and then may be broken into small grains for use." (*Whitelaw Ainslie's Materia Indica. Lond. 1826, Vol. I, Pg. 428.*) But the term Tapioca is very often applied to mere Fæcula, which has not undergone the preceding process; and it may as well be extended to this, as to call pure Fæcula Arrow-root, as if Arrows had roots and grew-up as Arrows. It is true Tapioca is no better as a name than Fæcula; but there seems to be a great indisposition to the use of this latter term, and a great predisposition to the use of the former, not only among non-medical people, but among physicians. The profession generally avoids good Latin, in favor of any barbarous word that can be found. Thus, for illustration, physicians seem to prefer making

themselves unintelligible by calling both of the Alcaloids Nectandra and Rodiæina by the abominable term Beebeerim, instead of using their proper Latin names, which are immediately derived from the natural-history name of the plant producing them, viz. Nectandra Rodiæi, and which are so much more euphonous, to say nothing of their distinguishing the Alcaloids from each other, which is not done by the term Beebeerim, which is an Indian name with a Low-Dutch orthography, and is of course always mispronounced by all who speak the English language, so that it would not be understood either by Indians or Low-Dutch.

MARANTACEÆ.

MARANTA ARUNDINACEA (*Linn.*)

MARANTA PANICULATA.

CANNA EDULIS (*Ker.*)

? CANNA COCCINEA (*Roscoe.*)

ZINGIBERACEÆ.

CURCUMA AUGUSTIFOLIA (*Roxburgh.*)

CURCUMA LEUCORRHIZA (*Roxburgh.*)

CURCUMA RUBESCENS (*Roxburgh.*)

EUPHORBIACEÆ.

CROTONEÆ.

JANIPHA MANIHOT (*H. B. & K.*)

JANIPHA AYPI.

PORTULACACEÆ.

LEWISIEÆ.

LEWISIA REDIVIVA (*Pursh.*)

ARACEÆ.

DRACUNCULEÆ.

ARUM MACULATUM (*Linn.*)

ARUM ? MACRORRHIZON (*Linn.*)

ARISÆMA ATRORUBENS (*Blume*)*ARISÆMA DRACONTIUM (*Schott.*)TYPHONIUM TRILOBATUM (*Blume.*)

CALADIEÆ.

CALADIUM PÆCILE (*Schott.*)CALADIUM SAGITTIFOLIUM (*Ventenat.*)CALADIUM VIOLACEUM (*Des Fontaines.*)COLOCASIA ANTIQUORUM (*Schott.*)COLOCASIA ESCULENTA (*Schott.*)

ANAPOREÆ.

DIEFFENBACHIA SEGUINA (*Schott.*)HOMALOMENA AROMATICA (*Schott.*)

ORONTIACEÆ.

ORONTIÆÆ.

DRACONTIUM POLYPHYLLUM (*Linn.*)

CALLEÆ.

CALLA PALUSTRIS (*Linn.*)SCINDAPSUS OFFICINALIS (*Schott.*)*Sago-producing Articles.*

I suppose that Sago is Fæcula Amylum or Starch "sprinkled with a little water, and then boiled" (half cooked) "in steam," by which "it is converted into a viscid mass." It appears next to be graunlated by being passed through a coarse sieve, and after this either dried in an oven or by artificial heat. Some of the varieties of Sago, particularly those of which the granules are nearly as large as Coriander-seeds, are not infrequently baked a little brown, upon one side, at least to appearance. Perhaps I may be mistaken about the matter; but it appears as I state, which is similar to the preparation of proper Tapioca.

*The trivial name commonly employed for this species of Arisæma is triphylla (three leaved) though I believe it never has more than two leaves. The trivial name atrorubens is some times used. This appears to me to be less exceptionable, since this is very generally the color of the Spatha. Some times however, it is whitish or yellowish green.

PALMACEÆ.

CALAMEÆ.

SAGUS LÆVIS (*Jack.*)SAGUS FARINIFERA (*Roxburgh.*)

SAGUS GENUINA.

SAGUS INERMIS.

SAGUS RUMPHII (*Willd. & Murray?*)

This last I think may be synonymous with *Saguerus Rumphii* (*Roxburgh.*)

CORYPHEÆ.

CORYPHA GEBANGA (*Blume.*)CORYPHA UMBRACULIFERA (*Linn.*)PHŒNIX FARNIFERA (*Roxburgh.*)

ARECEÆ.

SAGUERUS RUMPHII (*Roxburgh.*)CARYOTA URENS (*Linn.*)

BORASSEÆ.

BORASSUS GOMUTUS.*

CYCADACEÆ.

CYCAS CIRCINALIS, (*Linn.*)CYCAS INERMIS (*Loureiro.*)CYCAS REVOLUTA (*Thunberg.*)ZAMIA AUGUSTIFOLIA (*Jacquin.*)ZAMIA CAFFRA (*Thunberg.*)†ZAMIA DEBILIS (*Aiton.*)ZAMIA FURFURACEA (*Aiton.*)ZAMIA LANUGINOSA (*Jacquin.*)†

* As appears to me, this must be synonymous with *Saguerus Rumphii* (*Roxburgh.*) though I find the two mentioned as distinct by respectable authority.

† According to Whitelaw Ainslie, *Zamia Caffra* (*Thunberg*) and *Zamia lanuginosa* (*Willd.*) are synonyms; but in his "Species Plantarum" Willdenow does not mention the former, either as a synonym or a distinct species, while Sprengel in his "Systema Vegetabilium" mentions them as distinct species. I therefore retain them as such.

ZAMIA MEDIA (*Jacquin.*)ZAMIA PUMILA (*Linn.*)ZAMIA TENUIS (*Willd.*)DION EDULE (*Lindley.*)*Salep-producing Articles.*

For the preparation of Salep, it is said that we must wash the recent root-bulbs in water ; separate the fine brown cuticle either by a brush, or by dipping into scalding hot water and then rubbing them with a coarse cotton or linen cloth ; spread them on platters and dry in an oven heated to the usual degree for baking, which will require from six to twelve minutes, in which time they will have lost their milky whiteness, and will have acquired a translucency like that of horn, without any diminution of bulk. They must then be removed and dried and hardened in the air, which will require several days. This process however may be finished in a few hours by using a gentle preternatural heat. (*See Thom. Green's Botan. & Agricult. Dict. 2d Edit. Lond. 1823, Vol. II, Pg. 206, Col. II, Article Orchis mascula (Linnæus.)*) The root-bulbs of the Orchidæ should always be taken from the ground immediately on the decay of the herbaceous part of the plant in the summer or autumn, and only the living root-bulbs, which is to produce the next year's stem, leaves, flowers and seeds should be collected, and not the old shriveled and in fact dead root-bulbs which produced the herbaceous tops just withered and decaying. I suppose that any species of Orchis with undivided ovoid root-bulbs, may be prepared as Salep, though the species with the largest root-bulbs must, cæteris paribus, be the best.

Salep appears very unlike Farina and Fæcula, whether the latter remains in the regular form of Amylum or Starch, or has been converted into Tapioca, distinctively so called, or Sago. Salep is said to consist almost wholly of Bassorine in contradistinction from Fæcula proper ; and certainly the external sensible properties of the two are quite different ; and yet I believe that both are composed of the same elements in the same proportions, and even of the same number of equivalents. But this will be more fully considered in a more proper place perhaps, though it is sufficiently appropriate doubtless to consider it under Salep generally, rather than under any individual article of which Salep is made, since it is made of so large a number.

ORCHIACEÆ.

OPHREÆ.

ORCHIS MASCULA (*Linn.*)ORCHIS MILITARIS (*Linn.*)ORCHIS MORIO (*Linn.*)ORCHIS PYRAMIDALIS (*Linn.*)

ORCHIS TAURICA.

ORCHIS VARIEGATA (*Jacquin.*)PLATANThERA BIFOLIA (*Richard.*)

ARETHUSEÆ.

GASTRODIA SESAMOÏDES (*R. Brown.*)

VANDEÆ.

{ EULOPHIA (*virens?*) (*Sprengel.*){ *Limodorum virens* (*Roxburgh.*)

It is not at all probable that the species of *Eulophia*, which I have mentioned, is that which produces the Salep of the East Indies; a species (so far as I am informed) not even at present known. As however, *Eulophia virens* (*Roxburgh*) is the only East Indian species mentioned in Sprengel's "Systema Vegetabilium" and as it is quite likely that its roots may be made into Salep, I have ventured to name it, with a note of interrogation. Sprengel's happens to be the latest descriptive work, that I possess, which contains the whole Orchiaceæ, and I suspect that the principal Salep-producing *Eulophia* could not have been heard of, previous to the publication of this work.

LEÄNTICA SACCHARINA.

The saccharine Leäntics are much more pleasant and agreeable than any other group, and they are sufficiently easy of preparation; but a patient is supposed to become weary of them sooner than of any other group of this class. But beside this there was formerly a strong prejudice among physicians, in regard to much use of saccharine matter. It was supposed that they injured the tone of the stomach, contributed to produce Caries of the teeth, and in children at least, favored the production of intestinal En-

tozoä. Though I believe that physicians do not now entertain any of these opinions, still the effects of ancient prejudices (whatever may be the fact with the prejudices themselves) has not yet passed-by, but still continue to exert a considerable influence upon our prescriptions.

GRAMINACEÆ.

ANDROPOGONEÆ.

SACCHARUM OFFICINARUM (*Linn.*)

SACCHARUM SINENSE (*Roxburgh.*)

SACCHARUM ÆGYPTIACUM (*Willdenow.*)

SACCHARUM VIOLACEUM (*Tussac.*)

Common Sugar, as ordinarily obtained from the several species of Saccharum, is believed to be the fourth variety of the first species, viz. $H.^9 C.^{12} + O.^9 + 2 (H.^1 O.^1)$ that is Saccharum Binhydrium. The black liquid incrySTALLIZABLE Sugar commonly called Melasses has never been successfully analyzed, I suppose because it has never been obtained in sufficient purity to permit a reliable analysis. In all probability, it is the same (impurities excepted) with the liquid incrySTALLIZABLE Sugar of Honey, Agave, Salmalia, and various other plants. If it were not for the fact that the first or common species of Sugar seems to be easily and readily convertible into it, I should be inclined to suspect that it might be a distinct species. It may be merely isomeric with it, just as Quinædine is isomeric with Quinine, i. e. it may consist of the same elements in the same proportions, and in the same number of equivalents, and yet with very different external sensible properties. Melasses seems to be so very impure, as to make it just about impossible to arrive with any accuracy at its elementary composition, but I should think that Honey might admit first of a proximate analysis and then of an ultimate one, both of which might be reliable.

ARUNDINEÆ.

GYMNERIUM SACCHAROÏDES (*Humboldt.*)

This article is believed to produce Sugar of the common variety and species.

PHALAREÆ.

ZEA MAYS (*Linn.*)ZEA CARAGUA (*Linn.*)

The Sugar of these articles is believed to be of the common variety and species. It is asserted by some to be always liquid and incrySTALLIZABLE, but this is contradicted by others. As I have already stated, common Sugar is believed to be easily convertible into the liquid and incrySTALLIZABLE sort, during the process of preparation. This fact, I suspect, will explain the above-mentioned discrepancy of testimony.

ACERACEÆ.

ACER SACCHARINUM (*Wangenheim.*)ACER NIGRUM (*Michaux.*)ACER DASYCARPUM (*Ehrhart.*)ACER RUBRUM (*Linn.*)ACER MACROPHYLLUM (*Pursh.*)ACER PLATANOIDES (*Linn.*)ACER PSEUDO-PLATANUS (*Linn.*)

I believe that the Sugar of the *Acera* or Maples is of the first or common species, and that it is also of the most common variety, viz. *Saccharum Binhydram*. The Sugar made from the Maples, quite the latter part of the Sugar-making season, is sometimes moderately Cathartic. From the best information that I have been able to obtain, I am inclined to think that the sap which furnishes the Cathartic Sugar is always derived from the liber or inner bark, and that the sap from this part always possesses more or less Cathartic power. In a few instances where I have made investigation, the arrangements for collecting the sap were such as to obtain none from the inner bark till the sap became sparing, when they were changed so as to obtain this. All this however may be a mistake, for previously I had supposed that the sap of the alburnum or sap-wood did not afford much Sugar, and that it was mainly obtained from the sap of the liber. What the Cathartic principle that occasionally exists in Maple-Sugar made very near the end of the Sugar-season may be, I believe has never been investigated. It is by no means improbable that it is

the same as that which exists in Butternut-Sugar, and perhaps it is the same as that which exists in Manna.

PALMACEÆ.

CORYPHEÆ.

PHŒNIX SYLVESTRIS (*Hortus Bengalensis.*)

PHŒNIX DACTYLIFERA, (*Linn.*)

Lindley says that *Phœnix sylvestris* affords annually in Bengal 100,000 cwt. of Date Sugar (*Lindl. Veget. Kingd. Lond. 1846, Pg. 137.*) It appears to me that there must be a mistake in this statement, and that a hundred thousand pounds must have been intended, instead of a hundred thousand hundred weight. Date Sugar I believe, is of the first or common species and also of the most common variety viz. *Saccharum Binhydrum*. At any rate I have met with no statement to the contrary.

ARECEÆ.

SAGUERUS RUMPHII (*Roxburgh.*)

CALAMEÆ.

MAURITIA VINIFERA (*Martius.*)

ZALACCA EDULIS (*Reinwardt.*)

RAPHIA VINIFERA (*Palisot de Beauvois.*)

BORASSEÆ.

BORASSUS FLABELLIFORMIS (*Linn.*)

HYPHÆNE THEBAÏCA (*Lindley ?*)

COCOEÆ.

COCOS NUCIFERA (*Linn.*)

ELÆIS GUINEËNSIS (*Jacquin.*)

ELÆIS MELANOCOCCA (*Gærtner.*)

We often hear Date Sugar mentioned, as if it were a peculiar variety of the first or common species, or even a distinct species. But I do not recollect ever hearing Palm-Sugar spoken-of. And yet all the Palm-Sugar that I have ever seen, has appeared to me

to be pretty much the same thing, and as I judged, was the common variety (*Saccharum Binhydrium*) of the first or common species. However I do not know that the exact variety has ever been determined by a chimical examination. The external sensible properties are doubtless sufficient for the determination of the species.

CHENOPODIACEÆ.

BETA VULGARIS (*Linn.*)

BETA CYCLA (*Linn.*)

BETA MARITIMA (*Miller?*)

BETA MACRORRHIZA (*Steven.*)

All the preceding *Betæ*, though formerly reckoned as distinct species, are now generally believed to be mere varieties. They certainly differ from each other, and seem to require to be distinguished. If such distinction is made it is perfectly immaterial to medicine whether they are called species or varieties. In botany it may be of more importance.

BRASSICACEÆ.

ORTHOPLOCEÆ.

BRASSICIDÆ.

BRASSICA RAPA (*Linn.*)

JUGLANDACEÆ.

JUGLANS REGIA (*Linn.*)

JUGLANS NIGRA (*Linn.*)

JUGLANS CINEREA (*Linn.*)

I believe that the Sugar of the *Juglandes* is of the first or common species, and of the variety *Binhydrium*; but I do not think that the latter is as certainly ascertained as the former. The Sugar of the sap of *Juglans cinerea* is abundant, but I believe always more or less Cathartic. I have seen cases in which this property was quite useful in connexion with its Leäntic power, viz. when there was habitual costiveness, whether spontaneous or produced by the *Papaver* which was indicated and employed at

the same time. I am not apprised that any inquiries have ever been made respecting the nature and character of the Cathartic principle of the Sugar of *Juglans cinerea*, but of course it must be the same as that of the common Extract of the bark. Whether the Sugar of any other species of *Juglans* is Cathartic or not, I am ignorant. I have known Sugar obtained from the sap of a *Carya*; but I do not now recollect the species.

BETULACEÆ.

BETULA ALBA (*Linn.*)

BETULA LENTA (*Linn.*)

CORYLACEÆ.

QUERCUS MANNIFERA.

The Saccharine product of *Quercus Mannifera* is said to contain no Manna at all, nor any of the Cathartic principle that so often accompanies Manna; but it is said to be Sugar of the first or common species. Nothing is said as respects the variety. Under such circumstances it is commonly *Saccharum Binhydrium*, since if there were any of the peculiarities of the other varieties, it would seem as if they would be noticed and particularized. Insects are said to have no instrumentality in the production of this Sugar, but it is said to be a secretion from the leaves. I believe it is always found in granules of greater or less size. This tree ought to have been called *Quercus Saccharipara* (Sugar-producing) and not *Quercus Mannifera* (Manna-bearing.) We occasionally meet with accounts in the public prints, of "Showers of Manna," during which large quantities of it are said to fall. I have now several such in my possession. I recollect one which is said to have occurred some where in the District of Anatolia. Do not the so-called "Showers," consist of such a secretion carried by a high wind to a considerable distance from where it is formed? As the Manna of the Hebrews was a Bread-Corn, so the inhabitants of the places, where these "Showers" happen, almost always attempt to make the substance into bread; and we are some times told that such bread is "tolerable" though "very glutinous," meaning doubtless clammy or viscid. Gluten is an essential ingredient of all good meal. I should think that the addi-

tion of a great deal of good meal would be necessary to make good bread out of impure Sugar. What is the "Small Oak" of Kermanshaw, which produces such a substance by the aid of an Insect, a Coccus, as I suppose?

TAMARICACEÆ.

TAMARIX ORIENTALIS (*Forskæhl.*)TAMARIX GALLICA (*Linn.*)TAMARIX MANNIFERA (*Ehrenberg?*)

The product of *Tamarix Mannifera* (incorrectly so called) and *Coccus Mannifera* (so called with equal incorrectness) has been ascertained by analysis to contain no Manna at all, but to consist of Sugar of the first or common species with a little Mucilage conjoined. The particular variety of this Sugar is not specified by any body within my knowledge. When this is the fact the Sugar is usually the variety Binhydrum. But this Sugar is not produced upon this tree without the aid of an Insect, which is commonly supposed to effect its purpose by mere punctures. Upon this hypothesis the Sugar ought to exist in the sap, and remain solid on the evaporation of its watery parts. This may be the fact for aught I know; but if it is, any punctures ought to produce the Sugar.

CLASSIS	INSECTA (<i>Linn. Cuv. Harris.</i>)
ORDO	HEMIPTERA (<i>Linn. Cuv. Harris.</i>)
TRIBUS	{ <i>GALLINSECTA</i> (<i>Cuvier.</i>) { <i>COCCIDEÆ</i> (<i>Harris.</i>)

COCCUS MANNIFER (*Ehrenberg?*)

Without doubt, this tree ought to be called *Tamarix Sacchariparus*, and the Insect likewise *Coccus Sacchariparus* i. e. Sugar-producing: not *Saccharifer* i. e. Sugar-bearing. The other two species of *Tamarix* are said to produce Sugar in certain countries, but not in others. Is the failure of producing it due to the absence of the Insect, or to peculiarity of climate or some other unascertained cause? It is gravely said, by a very learned author, of the Sugar of the *Tamarices*, that "whether this is similar to the Manna of Moses" (rather of the Hebrews) "can not be ascertained;" and yet the latter was a Bread-Corn, or a substitute for one, while the former is a somewhat impure Sugar.

RHAMNACEÆ.

- ZIZYPHUS JUJUBA (*La Marck.*)
 ZIZYPHUS JOAZEIRO (*Martius.*)
 ZIZYPHUS CENOPLIA (*Miller.*)
 ZIZYPHUS VULGARIS (*La Marck.*)
 ZIZYPHUS LOTUS (*La Marck.*)
 ZIZYPHUS ORTHACANTHA (*De Cand.*)

The leathery substance sold at the present time in our shops, as a preparation of Jujube, or at least under this name, is as unlike the genuine substance prepared as a Leäntic, first from Zizyphus Jujuba, and afterwards from the other species that I have enumerated, as Chalk is to Cheese. The article which has usurped this name is but little better as a Leäntic than Caoutchouc; while the genuine article is exceedingly pleasant and agreeable, and in all probability is as effectual for the purpose for which it is employed, as any other simple and pure article of this class.

MYRTACEÆ.

MYRTEÆ.

- PSIDIUM GUAJAVA (*Raddi.*)
 v. PYRIFERUM (*De Cand.*)
 v. POMIFERUM (*De Cand.*)
 PSIDIUM ARAÇA.
 PSIDIUM ACUTANGULUM (*Martius.*)
 PSIDIUM INCANESCENS (*Martius.*)
 PSIDIUM PUBESCENS (*Martius.*)
 PSIDIUM CATTLEYANUM (*Sabine.*)
 PSIDIUM ALBIDUM (*Cambessedes.*)
 JAMBOSA VULGARIS (*De Cand.*)

MORACEÆ.

- FICUS CARICA (*Linn.*)
 FICUS SYCOMORUS (*Linn.*)

The Sugar of Figs has a crystalline form different from that of common Sugar, nor is it as sweet by any means. In all probability it is not common Saccharum Binhydram. (*Thos. Thom. Chim. Org. Bod. Veg. Lond. 1838, 7th Edit. Vol. VII. Pg. 645.*)

ULMACEÆ.

CELTEÆ.

CELTIS OCCIDENTALIS (*Linn.*)

AMYGDALACEÆ.

PRUNUS DOMESTICA (*Linn.*)

ROSACEÆ.

ROSEÆ.

ROSA CANINA (*Linn.*)

POTENTILLEÆ.

RUBUS BRASILIENSIS (*Martius.*)

RUBUS JAMAICENSIS (*Linn.*)

RUBUS VILLOSUS (*Aiton.*)

RUBUS FRUTICOSUS (*Linn.*)

FABACEÆ.

MIMOSEÆ.

INGA TETRAPHYLLA.

INGA EDULIS (*Martius.*)

INGA DULCIS (*Martius.*)

INGA CORDISTIPULA (*Martius.*)

ACACIA CORNIGERA (*Willd.*)

DIMORPHANDREÆ.

CERATONIA SILIQUA (*Linn.*)

Globules of Sugar are found upon *Ceratonia Siliqua*. They have been examined and found to be common Sugar mixed with a little Tannic and Oxalic Acids (*Ibidem* Pg. 648.) These globules are doubtless exuded inspissated sap. But the fruit contains the greatest quantity of Sugar.

ALGALES.

There are a considerable number of the *Algaceæ* that afford Sugar (as would seem) of the first or common species, and of the

variety Binhydrum, unless such Sugar has peculiarities which have not attracted notice. The following will serve as specimens of the Sugar-producing Algaceæ viz.

FUCACEÆ.

LAMINARIÆ.

LAMINARIA SACCHARINA (*L'Amouroux.*)

CERAMIACEÆ.

SPHÆROCOCCEÆ.

RHODYMENIA PALMATA (*Greville.*)

There are a considerable number of Fungaceæ, that are constantly mentioned as producing a certain variety of Sugar, viz. Saccharum Quaternhydrum, commonly called Mushroom Sugar. Liebig however considers Mushroom-Sugar as being a different species, viz. Manna. In numbers the amount of authority is greatly against him. The Fungaceæ commonly mentioned as producing Sugar are *Suborder* AGARICINI (*Fries & Lindley.*) *Agaricus acris* (*Bolton*) *Agaricus campestris* (*Linn.*) *Agaricus Theiogalus*, but whether of Bulliard or Swartz, we are not informed though these gentlemen apply this name to different species. *Agaricus piperatus*, but whether of Batsch, Bulliard, Scopoli or Linnæus, we are not told, though each of these gentlemen applies this name to a different species. *Amanita virgata* (*Persoon*). *Suborder* POLYPOREI (*Fries & Lindley.*) *Boletus squamosus* (*Hudson*). *Merulius Cantharellus* (*Persoon*). *Suborder* PHALLOÏDEI (*Fries & Lindley.*) *Phallus impudicus* (*Lindley? Persoon*). *Suborder* HELVELLACEI (*Fries & Lindley.*) *Peziza nigra*, but whether of Schumacher or Bulliard, we are not told, though these gentlemen apply this name to different species. *Clavellaria Coralloïdes*, though I find no such species in any work on the Fungaceæ to which I have access. *Suborder* HYDNEI (*Fries & Lindley.*) *Hydnum repandum* (*Persoon.*) *Hydnum hybridum*; which last I find mentioned in no work on the Fungaceæ to which I have access. Many other Sugar-producing Fungaceæ might doubtless be mentioned; but these will suffice to show where this particular variety of the first or common species of Sugar is found in nature. I do not know

that the crude Fungaceæ are ever used in medicine as Leäntics, though all that are specified heretofore are to be found in books of materia medica in my possession. If Saccharum Quaternhydrium were entitled to any preference in medicine over the other varieties of the first species of Sugar, it might doubtless be obtained in sufficient abundance.

VITACEÆ.

VITEÆ.

VITIS VINIFERA (*Linn.*)

The Sugar afforded by Grapes (Saccharum Uvarum) is the same as that of Eucalyptus Mannifera, and that of the urine in most cases of Paruria Diabetes (Saccharum Diabeticum) that is, it is of the first or most common species, and of the variety Quinhydrium.

MYRTACEÆ.

LEPTOSPERMEÆ

EUCALYPTUS MANNIFERA.

The trivial name of this plant should be Saccharipara, since (as I am about to show) it is common Sugar and not Manna which it produces. We are expressly told that the Sugar of Eucalyptus Mannifera is not produced by the instrumentality of Insects. Dr. Mudie the first or one of the first to bring the Sugar of Eucalyptus Mannifera before the public, pronounced it exactly like the Manna of Ornus and Fraxinus; and this statement has often been repeated on his authority; but analysis by Professor F. W. Johnson has shown that it is the Quinhydrous variety of the common species of Sugar, which is called Saccharum Uvarum or Grape Sugar, and is the same as that of the urine of Paruria Diabetes, when Sugar is produced in that disease. It contains no Manna whatever; nor is it known to contain any of that "yellow matter with a nauseous odor, to which the purgative quality of Manna seems owing." However, if a considerable quantity of it is taken, it is said to be laxative upon some persons, though not upon others. This would seem to indicate that it contains some thing beside mere Sugar, that escaped detection in Professor Johnson's analysis.

The vegetable and animal organic principle called Glycyra, Glycyrine and Glycyrhyle (the last two names being glaringly incorrect) is undoubtedly a species of Sugar, and one very nearly related to Manna. It is much used in chimical combination as a Leäntic, but not at all uncombined, though doubtless it would be capable of producing valuable Leäntic effects. Glycyra is the salifiable base of the liquid compound Salts, commonly called Greasy Oils. But I believe it some times performs the functions of an Acid or Salifying principle with certain Bases. Glycyra is a white solid, of a sweet taste, with the general habitudes of a Sugar. Its composition, when anhydrous is the following, viz. $H.^7 C.^6 + O.^5$ As will be observed, its compound radical is the same as that of Manna, Glycyra being a Pemptoxyd of it, while Manna is a Hectoxyd (*Thos. Thom. Chim. Org. Bod. Veget. Lond. 1838, 7th Edit. Vol. VII. Pg. 645-6-7.*) As this species of Sugar is never separated from its compounds for medicinal purposes, it will not be necessary to enumerate in this place any of the Greasy Oils from which it may be obtained with the greatest facility, more especially as a considerable number of them will be soon named as Leäntica Oleosa.

OLEACEÆ.

FRAXINEÆ.

ORNUS EUROPÆA (*Persoon.*)

ORNUS ROTUNDIFOLIA (*Aiton. Link.*)

FRAXINUS EXCELSIOR (*Linn.*)

FRAXINUS PARVIFOLIA (*Willd.*)

The two species of Ornus and the two of Fraxinus just specified are believed to produce the true and genuine Manna of modern times, which is a species of Sugar very different from the common sort. In its anhydrous state it is composed of $H.^7 C.^6 + O.^6$ while the common sort is composed of $H.^9 C.^{12} + O.^9$ likewise in its anhydrous state. Beside the true and genuine Manna itself, which is at least three fourths of the crude article, there is conjoined "a yellow matter with a nauseous odor, to which the Cathartic quality of the" (crude) "Manna seems" (to be) "owing." (*Ibidem Pg. 641.*) Beside the true and proper Manna and its Cathartic principle, a slight amount of accidental impurities always

accompanies the crude article, as found in our shops. As a whole, it seems to be an exuded inspissated sap. Under certain circumstances a Leäntic with slight Cathartic powers is valuable, since Leäntic and laxative articles are not infrequently indicated at one and the same time.

MYRTACEÆ.

MYRTEÆ.

PUNICA GRANATUM (*Linn.*)

We are told that "Mitonart found a Sugar in the root of *Punica Granatum* (*Linn.*) which he called Grenadine." It is said that "Boutron Charlard and Guillemet have shown this to be identical with Manna." (*Ibidem*, *Pg.* 642.) At present I have not access to Neumann's Chemistry, from which, it is said that "all the accounts of Manna to be found in chiminal books have been copied." (*Ibidem*, *Pg.* 640.)

POLYPODIACEÆ.

POLYPODEÆ.

POLYPODIUM CALAGUALA.

POLYPODIUM VULGARE (*Linn.*)

The Sugar of *Polypodium vulgare*, though considerably like Glycyon, we are told, Berzelius has shown to be quite different chimically (*Ibidem*, *Pg.* 644.) But what is it? I have not access to any work that contains Berzelius's results. Des Fosses examined this root and he supposed that he found incrySTALLIZABLE Sugar, Manna, Sarcocolla, and various other non-saccharine proximate principles. (*Ibidem*, *Pg.* 930.)

APIACEÆ.

AMMIEÆ.

APIUM GRAVEOLENS (*Linn.*)

We are told that Manna may be obtained from *Apium graveolens* (*Linn.*) but is it true and proper Manna?

SIUM SISARUM (*Linn.*)

This root yields a considerable quantity of Sugar. Is it Manna?

PEUCEDANEÆ.

HERACLEUM SPHONDYLIUM (Linn.)

PASTINACA SATIVA (Linn.)

These roots yield much Sugar. Is it Manna?

LILIACEÆ.

SCILLEÆ.

ALLIUM CEPA (Linn.)

We are told that Manna may be obtained from *Allium Cepa (Linn.)* but is this true and genuine Manna, or some other species of Sugar?

PINACEÆ.

ABIETEÆ.

LARIX EUROPÆA (De Cand.)

LARIX AMERICANA (Michaux.)

LARIX CEDRUS (Willd.? Persoon?)

The first of these species of *Larix* furnishes what is called Manna of Briançon. We are told that the so-called Manna of Briançon is the same substance as the crude Manna of the Orni and the Fraxini; but I consider this as much more than doubtful. Such statements are very lightly made in the materia medica. The supposed Manna now under consideration, has not (so far as I have knowledge) been actually analyzed by a competent chemist, and therefore the probability is much against its being true and genuine Manna. It is an exuded and inspissated saccharine and terebinthaceous sap; and such articles in their crude state it is customary to pronounce Manna without examination. The conjunction of the Oleiresin with the Saccharine matter enhances its value as a Leäntic in some cases. The Sugar produced by *Eucalyptus Mannifera* is commonly, indeed universally said to be Manna; and yet if we may trust Dr. Mudie's analysis it is not such. The Sugar produced by *Coccus Mannifera*, or *Tamarix*

Mannifera, etc. is uniformly said to be Manna; and yet it has been ascertained by analysis to contain no Manna at all. The sap of each of these Larices contains both Sugar and Oleiresin. That of the American species is abundant and of a remarkably fine quality, and affords one of the pleasantest compounds of what seems to be a natural composition of Mucilage, Syrup and Oleiresin, which I have often tried in vain to imitate factitiously.

FABACEÆ.

HEDYSAREÆ.

? ALHAGI MAURORUM (*Tournefort*)?

The Sugar obtained either from *Alhagi Maurorum* (for this is said certainly to produce a Sugar) or from some nearly allied plant, is always called Manna; and yet I can not ascertain that it has ever been shown to be such by analysis, and therefore probability is very strongly against it.

<i>Linnæus</i>	{	INSECTA	}	<i>Mc'Murtrie's</i> <i>Cuvier.</i>
		HYMENOPTERA		
		ANTHOPHILA.		
		APIDÆ (<i>Harris.</i>)		

APIS MELLIFICA (*Linn.*)

AMARYLLIDACEÆ.

AGAVEÆ.

AGAVE AMERICANA (*Linn.*)

AGAVE MEXICANA (*Haworth.*)

The Sugar produced by several species of *Agave* is said to be liquid or incrySTALLIZABLE Sugar analogous to Honey. Thomas Thomson says that "the Sugar from the sap of *Agave Americana* bears a greater resemblance to Manna than to" (common) "Sugar." (*Ibidem*, Pg. 648.)

STERCULIACEÆ.

BOMBÆÆ.

SALMALIA MALABARICA.

Salmalia Malabarica yields only a liquid or incrySTALLIZABLE Sugar, analogous to Honey. Now the liquid or incrySTALLIZABLE Sugars have never been analyzed, and of course their species has never been ascertained. This sort now under consideration, is said to be Cathartic.

LOTEÆ.

GLYCYRRHIZA GLABRA (*Linn.*)

GLYCYRRHIZA GLANDULIFERA (*Waldtstein & Kitaibel.*)

GLYCYRRHIZA ECHINATA (*Linn.*)

Glycyon or the Sugar of Liquirice is said to be the species of this substance contained in *Glycyrrhiza glabra*, and other species of this genus. It is "a yellow translucent mass cracked in all directions and easily detached from the vessel in which it"(s aqueous solution) "was evaporated." "It tastes like Liquirice-root, burns like" (the pollen of) "Lycopodium, and has a great affinity for Salifiable Bases, Acids and even Salts." "Its" (own) "Salts are but little soluble." Its Sulphate and Acetate have been well examined; and so have its Salts with Potassa Calcia and Baryta, and also with Cupra, Plumba, Ferra (Sesquoxyd) and Protochlorid of Tin. Its Salts are all sweet like Liquirice-root. (*Ibidem*, Pg. 643-4.) Glycyon has not yet been satisfactorily analyzed, but there is no reasonable ground for doubt that it has quite a different composition from the first or common species of Sugar.

TRIFOLIUM ALPINUM (*Linn.*)

MELLILOTUS OFFICINALIS (*Willd.*)

TRIGONELLA CÆRULEA (*Seringe.*)

From the external sensible properties, more especially the last of these articles, I am strongly inclined to believe that the species of Sugar which they contain, is that of Liquirice, viz. that called Glycyon; but I am ignorant of any actual analyses.

ASTRAGALUS GLYCYPHYLLUS (*Linn.*)

ROBINIA PSEUD-ACACIA (*Linn.*)

From the taste, the species of Sugar contained by the last two articles would seem to be Glycyon or Sugar of Liquirice.

PHASEOLEÆ.

ABRUS PRECATORIUS (*Linn.*)

Glycyon is said to be the saccharine principle contained in the root of *Abrus precatorius*.

PERIANDRA DULCIS (*Martius.*)

HEDYSAREÆ.

ONOBRYCHIS SATIVA (*La Mark.*)

SMILACEÆ.

SMILAX GLYCYPHYLLA (*Swartz.*)

GALIACEÆ.

GALIMUM CIRCÆOÏDES (*Ræmer & Schultes.*)

The preceding four articles I think contain Glycyon or Liquirice Sugar.

ARALIACEÆ.

PANAX	{	SCHINSENG (<i>Quorundem.</i>)
		GINSCHEN (<i>Quorundem.</i>)
		GINSENG (<i>Meyer.</i>)

PANAX QUINQUEFOLIA (*Linn.*)

The Sugar of the Panaces, I think, can not possibly be of the first or common species, but as appears to me, it is probably of the same character as that of *Glycyrrhiza*, which is commonly called by the ill formed name Glycyon.

PENÆACEÆ.

? PENÆA MUCRONATA (*Linn. Thunb.*)

Sarcocolla is a peculiar species of Sugar whose source is unknown. It has most commonly been supposed to be yielded by several of the twenty-one species of what once constituted the Linnæan genus *Penæa*, but which are now distributed into the three genera *Penæa*, *Sarcocolla* and *Geisoloma*, which constitute

the whole order *Penæaceæ*. Endlicher thinks that this substance is not likely to be the product of this order—Lindley supposes it must be yielded by some Umbellifer, because he thinks it is something like Galbanum, in which I think he is quite incorrect. Dioscorides says that it is produced by a Persian tree. Ainslie and various others attribute it to *Penæa mucronata* (*Linn.*) while doubtless *Penæa Sarcocolla* was so named from the hypothesis that it was its source. It is affirmed to exude from all parts of these plants, and especially from the calyces. It is said to come from India, Persia, Turkey etc. It forms some Salts and in general its habitudes are like those of Sugar. *Sarcocolla* occurs in globules from the size of a grain of sand to that of a Pea. It is yellow and translucent like *Gummi Acaciæ veræ* (*Willd.*) It is incrySTALLIZABLE, subviscid, subnauseous and sweet, followed by more or less bitterness. Its composition is $H^{.19} C^{.22} + O^6$. Perhaps some of its H. O. exists as water; but this is unknown.

LEÄNTICA OLEOSA.

The Greasy or Fat Oils, which are the foundation of one turma or group of the *Leäntics*, are not individual organic proximate principles, but compound Salts consisting of two Acids combined with a single Base. The Oily *Leäntics* are both less convenient of preparation and less agreeable, at least to Angli-American palates, than either the Mucilaginous or the Farinaceous *Leäntics*; but whether it is from this cause, or the mere influence of fashion, I know not, but they are certainly much less in favor, both with patients and physicians, than any other group of the *Leäntics*. However I believe they are actually more liable to offend the stomach, as well as disgust the taste, than any other group of this class of remedies. They are commonly administered in the pharmaceutic form called Emulsion.

SAPOTACEÆ.

- BASSIA BUTYRACEA (*Roxburgh.*)
- BASSIA LATIFOLIA (*Roxburgh.*)
- BASSIA LONGIFOLIA (*Linn.*)
- ? BASSIA OBOVATA (*Forster.*)
- { BASSIA? PARKII (*G. Don.*)
- { *Vitellaria* (*potius.*)

- BASSIA SERICEA (*Blume.*)
 BASSIA CUNEATA (*Blume.*)
 { BASSIA OLEÏFERA (*De Cand.*)
 { PALAQUIUM OLEÏFERUM (*Blanco.*) *potius.*

PALMACEÆ.

COCOEÆ.

- COCOS NUCIFERA (*Linn.*)
 COCOS BUTYRACEA (*Linn.*)
 COCOS CORONATA (*Martius.*)
 COCOS FLEXUOSA (*Martius.*)
 ATTALEA COMPTA (*Martius.*)
 ATTALEA EXCELSA (*Martius.*)
 ATTALEA SPECTABILIS (*Martius.*)
 ASTROCARYUM AYRI (*Martius.*)
 ASTROCARYUM JAUARI (*Martius.*)
 ASTROCARYUM MURUMURÚ (*Martius.*)
 ASTROCARYUM TUCUMÁ (*Martius.*)
 ASTROCARYUM VULGARE (*Martius.*)
 ACROCOMIA SCLEROCARPA (*Martius.*)
 DIPLOTHEMIUM MARITIMUM (*Martius.*)
 ELÆIS GUINEËNSES (*Jacquin.*)
 ELÆIS MENLANCOCCA (*Gærtner.*)

ARECEÆ.

- ÆNOCARPUS BACABA (*Martius.*)

CARYOCARACEÆ.

- CARYOCAR BRASILIENSE (*St. Hilaire.*)
 CARYOCAR BUTYROSUM (*Willd.*)
 CARYOCAR GLABRUM (*Persoon.*)
 CARYOCAR TOMENTOSUM (*Willd.*)

LECYTHIDACEÆ.

- LECYTHIS GRANDIFLORA (*Aublet.*)
 LECYTHIS LANCEOLATA (*Poiret.*)
 LECYTHIS PISONIS (*Cambessedes.*)

BERTHOLLETIA EXCELSA (*Humboldt.*)

CORYLACEÆ.

CORYLUS COLURNA (*Linn.*)CORYLUS AVELLANA (*Linn.*)CORYLUS AMERICANA (*Walter.*)CORYLUS ROSTRATA (*Aiton.*)FAGUS SYLVATICA (*Linn.*)FAGUS FERRUGINEA (*Aiton.*)

JUGLANDACEÆ.

JUGLANS CINEREÄ (*Linn.*)JUGLANS NIGRA (*Linn.*)JUGLANS PTEROCOCCA (*Roxburgh.*)JUGLANS REGIA (*Linn.*)

{ CARYA SQUAMOSA,
 { *Carya alba* (*Nuttall.*)

LAURACEÆ.

LAURUS NOBILIS (*Linn.*)PERSEA GRATISSIMA (*Gærtner.*)AYDENDRON CUJUMARY (*Nees.*)TETRANTHERA ROXBURGII (*Nees.*)

BRASSICACEÆ.

ORTHOPLOCEÆ.

BRASSICEÆ.

BRASSICA NAPUS (*Linn.*)BRASSIBA RAPA (*Linn.*)SINAPIS DICHOTOMA (*Roxburgh.*)

SINAPIS GLAUCA.

SINAPIS RAMOSA (*Roxburgh.*)

NOTORRHIZEÆ.

CAMELINEÆ.

CAMELINA SATIVA (*Crantz.*)

PEDALIACEÆ.

PEDALIEÆ.

PEDALIUM MUREX (*Linn.*)

SESAMEÆ.

SESAMUM INDICUM (*De Cand.*)

SESAMUM LACINIATUM (*Klein.*)

SESAMUM LUTEUM (*Retz.*)

SESAMUM OCCIDENTALE (*Heer et Regel.*)

BYTTNERIACEÆ.

THEÖBROMA BICOLOR (*Humboldt et Bonpland.*)

THEÖBROMA CACAO (*Linn.*)

THEÖBROMA MICROCARPUM (*Martius.*)

THEÖBROMA SUBINCANUM (*Martius.*)

THEÖBROMA SYLVESTRE (*Aublet.*)

STERCULIACEÆ.

It is said that the seeds of all the species of Sterculia (and twenty-seven are described in De Candolle's Prodrömus) afford Oil of an excellent quality, both abundantly and easily. The following are specified in a work on materia medica now before me, viz.

STERCULIEÆ.

STERCULIA CHICHA (*St. Hilaire.*)

STERCULIA FÆTIDA (*Linn.*)

STERCULIA LASIANTHA (*Martius.*)

ASTERACCEÆ.

COREOPSEÆ.

HELIANTHUS ANNUM (*Linn.*)

HELIANTHUS TUBEROSUM (*Linn.*)

CARTHAMEÆ.

CARTHAMUS TINCTORIUS (*Linn.*)

SILPHIÆ.

- { GUIZOTIA OLEÏFERA (*De Cand.*)
- { *Polymnia Abyssinica* (*Linn.*)

CHRYSOBALANACEÆ.

- CHRYSOBALANUS ICACO (*Linn.*)
- MOQUILEA CANOMENSIS (*Martius.*)
- LICANIA TURIÚVA (*Chamisso & Schlechtendahl.*)

MALVACEÆ.

HIBISCEÆ.

- GOSSYPIUM HERBACEUM (*Linn.*)
- { ? ABELMOSCHUS CANNABINUS.
- { *Hibiscus Cannabinus* (*Linn.*)

SIDEÆ.

- { SIDA ABUTILA (*Medicorum.*)
- { ? *Abutilon Avicennæ* (*Gærtner.*)

MYRISTICACEÆ.

- { MYRISTICA OFFICINALIS (*Linn.*)
- { *Myristica aromatica* (*La Marck.*)
- { *Myristica Moschata* (*Thunberg.*)
- { VIROLA SEBIFERA (*Aublet.*)
- { *Myristica Sebifera* (*Willd. & Persoon.*)
- { *Sebophora* ——— (*Necker.*)

MELIACEÆ.

MELIÆ.

- MELIA AZADIRACHTA (*Linn.*)

TRICHILIÆ.

- { CARAPA GUJANENSIS (*Aublet.*)
- { *Persoonia Guareoides* (*Willd.*)

FABACEÆ.

HEDYSAREÆ.

- ARACHIS HYPOGÆA (*Linn.*)

EUCÆSALPINIÆ.

CÆSALPINIA OLEOSPERMA.

CLUSIACEÆ.

GARCINIÆ.

PLATONIA INSIGNIS (*Martius.*)

PENTADESMA BUTYRACEUM (*Hort. Transac.*)

DIPTEROCARPACEÆ.

{ *VATERIA INDICA* (*Linn.*)
{ *Elæocarpus Copalliferus* (*Retz.*)

Product between Tallow and Wax.

CROTONEÆ.

ELÆOCOCCA VERNICIA (*Adr. Jussieu.*)

ELÆOCOCCA VERRUCOSA (*Adr. Jussieu.*)

Oil of both the above very acrid.

AMYGDALACEÆ.

AMYGDALUS COMMUNIS (*Linn.*)

PRUNUS BRIGANTIACA.

COMBRETACEÆ.

TERMINALIEÆ.

TERMINALIA CATAPPA (*Linn.*)

TERMINALIA CHEBULA (*Retz.*)

CUCURBITACEÆ.

FEUILLEÆ.

vel

FEUILLEACEÆ.

{ *JOLIFFIA AFRICANA* (*De Candolle.*)
{ *Telfairia pedata* (*Hooker.*)
{ *Feuillea pedata* (*Smith.*)

DIOSPYRACEÆ.

DIOSPYROS EMBRYOPTERIS (*Persoon.*)

ANACARDIACEÆ.

PISTACIA LENTISCUS (*Linn.*)

ULMACEÆ

CELTEÆ.

CELTIS AUSTRALIS (*Linn.*)

CANNABINACEÆ.

CANNABIS SATIVA (*Linn.*)

PAPAVERACEÆ.

PAPAVER SOMNIFERUM (*Linn.*)

OLEACEÆ.

OLEÆ.

OLEA EUROPEA (*Linn.*)

ELÆAGNACEÆ.

ELÆAGNUS AUGUSTIFOLIUS (*Marschall a Bieberstein.*)

TERNSTRÆMIACEÆ.

CAMELLIA OLEÏFERA (*Abel.*)

LINACEÆ.

LINUM USITATISSIMUM (*Linn.*)

MORINGACEÆ.

MORINGA PTERIGOSPERMA (*Gærtner.*)

HUMIRIACEÆ.

HELLERIA OBOVATA (*Nees et Martius.*)

SOLANACEÆ.

CURVEMBERYÆ.

SOLANUM BRASILIENSE.

OCHNACEÆ.

OCHNEÆ.

GOMPHIA PARVIFLORA (*De Cand.*)

CLASSIS PISCES (*Linn.*)

ORDO JUGULARES (*Linn.*)

CLASSIS PISCES (*Mc'Murtrie's Cuvier.*)

ORDO MALACOPTERIGII (*Cuvier.*)

SUBORDO SUBBRACHIATI (*Cuvier.*)

TRIBUS GADITES (*Cuvier.*)

CLASSIS PISCES (*J. G. Wood.*)

SUBCLASSIS OSSEI (*J. G. Wood.*)

ORDO MALACOPTERIGII (*J. G. Wood.*)

SUBORDO SUBBRACHIATI (*J. G. Wood.*)

TRIBUS GADIDÆ (*J. G. Wood.*)

{ MORRHUA CALLARIAS (*J. G. Wood.*)

{ *Gadus Morrhua* (*Linn.*)

In one edition of the *Systema Naturæ* of Linnæus in my possession, the common Cod is called *Gadus Morhua*, and *Gadus Callarias* is given as the name of the Torsk. In another edition *Gadus Morhua* is still given as the name of the Cod, while it is denied that *Gadus Callarias* is the Torsk, which is said to be *Gadus Brosmé*. In a comparatively late work, in which the Linnean genus is divided, that genus which contains the Cod is called *Morrhua*, and this species is called *Morrhua Callarias*, which seems to make *Gadus Morrhua* and *Gadus Callarias* (*Linn*) the same.

OLEUM JECINORIS MORRHUÆ CALLARIÆ.

By the article which immediately precedes, I intend what is commonly prescribed under the name of Cod-Liver-Oil, but as I doubt not, I have referred it to the wrong animal. At least nine

hundred and ninety-nine thousandths of all that has been recently employed in medicine, has been derived not from a Fish, but from one or two Sea-Mammals, viz. the following.

CLASSIS	MAMMALIA, (<i>Linn.</i>)
ORDO	CETE (<i>Linn.</i>)
CLASSIS	MAMMALIA (<i>Cuvier.</i>)
ORDO	CETACEA (<i>M'Murtrie's Cuvier.</i>)
TRIBUS	ORDINARIA (<i>M'Murtrie's Cuvier.</i>)
CLASSIS	MAMMALIA (<i>J. G. Wood.</i>)
ORDO	CETE (<i>J. G. Wood.</i>)
TRIBUS	BALÆNIDÆ (<i>J. G. Wood.</i>)

{ PHYSETER MACROCEPHALUS (*Linn.*)
 { *Catodon macrocephalus* (*Griff. Cuv.*)

BALÆNA MYSTICETUS (*Linn.*)

Oleum pinque liquidum, Physeteris macrocephali.

Oleum pinque Balænae Mysticeti.

These last two Greasy Oils I believe are never used as Leäntics, except as substitutes for Cod-Liver-Oil, though they are undoubtedly as good for this purpose as any other Greasy Oil. People of the Hyperborean Race, I imagine, would make no objection to taking these Oils whenever a Leäntic should be needed, and would probably prefer them to any other Oil which I have mentioned. A very considerable number of physicians within my knowledge, after using what they considered as genuine Cod-Liver-Oil for a comparatively long time, and in connexion with this having opportunity to observe what were considered to be the effects of Sperma Ceti and Whale Oils (so called) have relinquished the employment of the former, and in its stead have adopted the latter two, on the ground that they are just as good, and considerably cheaper. All of these gentlemen profess to think highly of the efficacy of Cod-Liver-Oil; but they have arrived at the conclusion that every other liquid Greasy Oil has the same power and is capable of producing the same effects. For myself, I have watched often and long for the effects of Cod-Liver-

Oil (so supposed to be) not prescribed however by myself, but by my professional acquaintance, and yet I never witnessed any thing but a Leäntic, and perhaps nutrient operation. It is true I feel no confidence that it was true and genuine Cod-Liver-Oil that I saw employed; but still I do not imagine that this was of much importance. I have accidentally received information that some large dealers in Cod-Liver-Oil have been in the habit of purchasing large quantities of Lamp-Oil of a wholesale establishment in a large city, which they never sold as Lamp-Oil at their own place of business. I have therefore been in the habit of supposing that true and genuine Cod-Liver-Oil is just about as often sent at the prescription of a physician as true and genuine Wine, by which I mean the pure juice of the Grape duly and properly fermented. However I do not think that the difference between Cod-Liver-Oil and Lamp-Oil is of any material importance, while I should greatly prefer the properly fermented pure juice of the Grape to Turnip-juice and bad Brandy, or any of the more common substitutes for Wine. I never yet had opportunity to converse with a physician who had any notion of what the powers of Cod-Liver-Oil might be properly called.

Oleum pinque solidum, Physteris macrocephali.

By this last article I intend that beautiful white concrete or solid Greasy-Oil, which is obtained exclusively from the head of *Physter macrocephalus*, and which has been known so long by the absurd and even ridiculous name of *Sperma Ceti* or the Seed of the Whale. Like the liquid Greasy Oils it is a Saline compound of two organic Acids with a single organic Base, in such proportions as to constitute a neutral Salt. This has been celebrated time immemorial, as a Leäntic. Every body will at once recollect that it is mentioned as such by Shakespeare.

LEÄNTICA ALBUMINOSA.

The albuminous Leäntics are scarcely recognized as such, in books, and yet they have always been at least in moderate use wherever I have witnessed the practice of medicine. As a general rule, Albumen is considered as too highly nutritious to be an eligible Leäntic; but this can hardly be considered as a well

founded objection, since the quantity required for Leäntic purposes is much less than would be necessary as a nutrient, if Albumen alone were to be relied-on. I have often known preparations of Albumen used as Leäntics, seldom however, intirely alone and unconjoined with any other article of this class. One of the most common forms in which the Greasy Oils are employed for this purpose, is beaten with Albumen and a little Sugar, and the preparation flavored with some thing agreeably sapid, as some Essential Oil, a little Wine, etc. Such a preparation is as much one of the Albumen as of the Oil.

For medicinal purposes Albumen is always obtained from some sort of Eggs, and of course of some Fowls, that are in general use. The greatest portion of the Eggs employed are the produce of domesticated Fowls; and yet, in many places the Eggs of wild Fowls are extensively used. These wild Fowls are either such as are protected by game laws, in private parks, etc. or such as resort in great numbers to particular limited locations, for the purpose of breeding. Many Eggs of the former class are employed in England, Scotland, and I suppose in Ireland also, which are unknown in the U. S. A. while many of the latter are employed in the U. S. A. which so far as I am informed are not known in England, Scotland and Ireland. As Eggs are so important in diet, and as I purpose to treat briefly hereafter of the proper diet for the sick, I shall mention in this connexion pretty much the whole of the Eggs that are generally eaten, since, in all probability, I shall not hereafter give any catalogues of articles of food, under what I shall say upon dietetics for the sick. In some instances I may fail of referring some of the Aves, which I shall have occasion to mention, to the most modern genera at present received by the best ornithologists, since in my present situation, I have no access to any books on this subject, except the few that I happen to have in my own library, which contains nothing very recent in this particular department of natural history. To those generic names for which I have access to no recent authority I shall prefix an asterism. As all the names which I shall employ, if not the most recent, have never the less been employed within the last forty or fifty years, and are still to be found in many books that will always be works of reference, I think that I can not fail of being always perfectly intelligible.

CLASSIS	AVES.
ORDO	COLUMBÆ.
TRIBUS	COLUMBIDÆ.
SUBTRIBUS	COLUMBINÆ.
	*COLUMBA DOMESTICA.
	<i>Pigeon.</i>
	ECTOPISTES MIGRATORIA.
	<i>Passenger Pigeon.</i>
ORDO	GALINÆ.
TRIBUS	PHASIANIDÆ.
SUBTRIBUS	PAVONINÆ.
	PAVO CRISTATUS (<i>Linn.</i>)
	<i>Pea Cock and Pea Hen.</i>
SUBTRIBUS	PHASIANINÆ.
	PHASIANUS CHOLCHICUS.
	<i>Pheasant.</i>
SUBTRICUS	GALINÆ.
	GALLUS BANKIVA.
	GALLUS SONERATI.
	<i>Common domestic Fowl.</i>

The last mentioned two species are supposed to be the two originals of our common domestic Fowl commonly called *Gallus domesticus*. Most of our domestic animals, beyond all doubt, are derived from two, three or even more different and distinct species. The domestic Dog, in all probability is derived from even more than three species. The domestic Cat, and also the Sheep, are considered as derived from a plurality of species, by some of the best naturalists. It is therefore no way extraordinary that the common domestic Fowl should be derived from two distinct species.

SUBTRIBUS	MELEAGRINÆ.
	MELEAGRIS GALLOPAVO.
	<i>Turkey.</i>
	NUMIDIA MELEAGRIS.
	<i>Guinea Fowl.</i>

TRIBUS	TETRAONIDÆ.
SUBTRIBUS	PERDICINÆ.
	PERDIX CINEREA.
	<i>Partridge.</i>
	COTURNIX COMMUNIS.
	<i>Quail.</i>
SUBTRIBUS	TETRAONINÆ.
	TETRAO UROGALLUS.
	<i>Capercaillie.</i>
	TETRAO TETRIX.
	<i>Black Grouse.</i>
	*TETRAO? UMBELLUS (<i>Gmelin.</i>)
	<i>Partridge in U. S. A.</i>
	*TETRAO? TOGATUS (<i>Gmelin.</i>)
	*TETRAO? CUPIDO (<i>Gmelin.</i>)
	<i>Pinnated Grouse.</i>
	LAGOPUS SCOTICUS.
	<i>Red Grouse.</i>
	LAGOPUS ALBUS.
	<i>Ptarmigan.</i>
TRIBUS	MEGAPODIDÆ.
	MEGAPODIUS TUMULUS.
	<i>Mound-making Megapode.</i>
	TALEGALLUS LATHAMI.
	<i>Brush Turkey.</i>
ORDO	STRUTHIONES.
TRIBUS	STRUTHIONIDÆ.
SUBTRIBUS	STRUTHIONINÆ.
	STRUTHIO CAMELUS.
	<i>Ostrich.</i>

I have no knowledge that the Albumen of the Egg of the Ostrich has ever been used as a Leäntic, but it is extensively eaten, and may be used in medicine as well as any other Egg.

ORDO	ANSERES.
TRIBUS	ANATIDÆ.
SUBTRIBUS	ANSERINE.

	{ *ANSER.
	<i>Anas Anser.</i>
	<i>Common Gander and Goose.</i>
SUBTRIBUS	ANATINÆ.
	ANAS BOSCHAS V. DOMESTICUS.
	<i>Mallard.</i>
	*TADORNA MOSCHATA.
	<i>Muscovy Duck.</i>
TRIBUS	LARIDÆ.
SUBTRIBUS	LARINÆ.
	*LARUS CYANORRHYNCHUS.
	<i>Gull.</i>
	*LARUS CANUS.
	<i>Gull.</i>
	LARUS MARINUS.
	<i>Black-backed Gull.</i>

LEÄNTICA GELATINOSA.

The Gelatinous Leäntics, as well as the Albuminous, are generally considered as being too nutritious to admit of being used with the freedom which is necessary for the best effects of this class of agents; and I do not question but that this opinion is correct in relation to phlogistic diseases, and perhaps to those which are neither phlogistic nor atonic; but I think it is not exactly true in relation to diseases which are decidedly and considerably atonic.

As relates to their source, the principal varieties of Gelatine are Boöcolla and Ichthyocolla; and to these may perhaps be added Ornithocolla. I shall name only three or four domesticated Bovine animals, from some parts of which Gelatine is commonly obtained; two Pisces of some naturalists and Amphibia of others; and one species of the Aves, which seems to excrete Gelatine under certain circumstances, or some thing very much like it.

CLASSIS	MAMMALIA.
	{ RUMINANTIA.
ORDO	<i>vel</i>
	{ UNGULATA.
TRIBUS	BOVIDÆ.
SUBTRIBUS	BOVINA.

{ *Bos URUS* v. *DOMESTICUS* (*Herberstein*.)
 Taurus Urus v. *domesticus*.
 Bos Taurus v. *domesticus*.
 { *Bos GIBBOSUS*.
 Bos Urus v. *gibbosus*.
 Taurus Urus v. *gibbosus*.
 Bos Taurus v. *gibbosus*.
 Zebu.

At first view, it would seem not a little remarkable that there is no common popular name in the English language, for the domestic animal called in natural history *Bos Urus* v. *domesticus*, at least if we were not in the same predicament in several other cases. Edward Griffith in his edition of Cuvier's *Animal Kingdom*, gives *Urus* (I believe a term found in Cæsar's *Commentaries*) as the popular name. This I think would be good enough for this purpose, if it were not incapable of an English plural, and if it were not utterly impossible to get it into use. This species was once called in Germany *Aurox*, and in Poland *Thur*; but both of these names are intirely unknown in vernacular English. The full-grown unmutilated male is called *Bull*, while the young one is called *Bullock*. The spado or castrated male whether partially grown or wholly so, is called *Ox*, which term, (as would seem) because it is reckoned a little more delicate (though on what grounds I cannot imagine) is applied by certain writers of the present time, to the male in all circumstances.* The full grown female is called *Cow* (plural *Kine*) while the young female is called *Heifer*. The young, less than a yearold, of all Bovine animals, I believe, is invariably called *Calf*. By some naturalists *Bos gibbosus* is considered only a variety of the preceding; while by others it is reckoned as a distinct species, when

* At the present period the term *Bull*, *Ram*, and *Boar* seem to be absolutely proscribed, though the words *Buck* and *Hart* remain in favor, and are even grossly misapplied in the place of the three preceding. The terms *Sow* and *Bitch* seem to be pretty generally proscribed. I have known clergymen that always read "the creature was washed" instead of "the Sow that was washed," etc. There seems to be some tendency to view the words *Man* and *Woman* in the same light. If I were not more than twenty-one years of age, I should expect (according to the common course of nature) to outlive both these words. But *Male* and *Female*, words expressive of sex merely, and that absolutely in the abstract, seem at present to be the very quintessence of refinement.

these terms are intended to be employed in their strict natural history acceptation. It is much too often the fact that truly distinguished naturalists, and more especially those of less merit, have pronounced a particular animal a species or a variety, under very indefinite not to say very incorrect notions of the real difference between the two; so that such decisions are not always worthy of the respect, which the character of the naturalist ought to make them deserve. In my view, any primordial difference, whether great or small, which is permanent and unchangeable by accidental influences and agencies, and is invariably continued by natural reproduction, always indicates a true and genuine species, either in plants or animals, and in the latter whether brute or human. The same species may be reproduced by any number of pairs however great, provided they are themselves of the same species; but different species can not be produced by a single individual pair. Now whether *Bos gibbosus* tested by these principles would prove to be a mere variety or a true and proper species, I am unable to say; but for my purposes, it is desirable to distinguish it, and I distinguish it accordingly, without pretending to decide the question just referred-to. I make all this explanation because I think that species and variety should always be applied with accuracy and precision in every department of natural history.

Gelatine of a peculiarly delicate quality, as has commonly been supposed, has long been obtained from the feet of the Calves of all domesticated Bovine animals. Of late years however, full as good an article has been obtained from the full grown individuals.

{ *BUBALUS BUFFELUS* V. DOMESTICUS.
 { *BOS BUBALUS* V. DOMESTICUS.
 { *East India Buffalo.*

{ *BUBALUS ARNÉE* V. DOMESTICUS.
 { *Chinese and Malay Buffalo.*

Domesticated Buffalos, whether Calves or adult animals may undoubtedly be made to afford as much and as good Gelatine, as the domesticated Urus (to employ Cæsar's and Mr. Griffith's popular name in preference to none) and the domesticated Zebu. In fact I doubt not that Gelatine has very often been obtained from

both the species of Buffalo which I have mentioned and that a great multitude of times. At the same time it is proper to remark here that neither the muscle of the Urus or the Zebu, nor that of either of the species of Buffalo that I mention, contains sufficient Gelatine to be of any value for the purpose of making Broth as food for the sick, without the addition of some other material. The Broths commonly prepared for the sick from such muscle are commonly called Tea, and they are just about as nutritious (I do not say medicinal) as a simple infusion of *Thea Sinensis* without Cream, Milk or Sugar, notwithstanding so much importance is attached to them by many, as food for invalids.

CLASSIS	PISCES (<i>Linn.</i>)
ORDO	CHONDROPTERIGHI (<i>Linn.</i>)
CLASSIS	AMPHIBIA } (<i>Quorundum.</i>)
ORDO	NANTES }
SUBORDO	ELEUTHEROPOMI } (<i>J. G. Wood.</i>)
TRIBUS	ACIPENSERIDÆ }
	ACIPENSER STURIO.
	*ACIPENSER HUSO.

One or more species of Sturgeon has long been known as affording Gelatine not only abundantly but of an excellent quality.

CLASSIS	AVES	} (<i>Linn.</i>)
ORDO	PASSERES	
ORDO	FISSIROSTRES	} (<i>J. G. Wood.</i>)
	DIURNÆ	
TRIBUS	HIRUNDINIDÆ	} (<i>J. G. Wood.</i>)
SUBTRIBUS	CYPSELINÆ	
	*HIRUNDO ? ESCULENTA (<i>Linn.</i>)	

Wm. H. Harvey M. D. (in the Introduction to his *Nereïs Americana*, Part 1st, Washington 1852) says that "it was at one time supposed that the famous Edible Birds' Nests of China, the finest of which sell for their weight in Gold, and enter into the composition of the most luxurious Chinese dishes, were constructed of the semidecomposed branches of some Alga; but it has since been ascertained that these nests consist of an animal substance,

which is supposed to be disgorged by the Swallows which build them." This statement is undoubtedly correct, since it is well known that certainly nearly allied Birds prepare their Nests very much in the same manner. But the disgorging mentioned by Dr. Harvey is merely the employment of a regular secretion intended to assist in the formation of a Nest." The nest of the" (American) "Chimney Swallow" (says Alexander Wilson the ornithologist) "is of singular construction, being formed of very small twigs fastened together with a strong adhesive Glue which is secreted by two glands, one on each side of the hind-head, and" (which) "mixes with the saliva." "With this Glue which becomes" (as) "hard as the twigs themselves, the whole Nest is thickly besmeared," etc. (*A. Wilson's Ornithology, Philad. 1828, Vol. II. Pg. 427, Hirundo Pelasgia or Chimney Swallow.*) The Chinese *Hirundo*? *esculenta* undoubtedly has a secreting apparatus for this substance similar to that of the American Chimney Swallow, by which it produces the substance in question, though no naturalists (at least so far as appears) have detected it. But what is the substance secreted? I am not apprised that it has ever been analytically examined by any chimist; but I think there can be no reasonable doubt that it is Gelatine. Perhaps I am mistaken in regard to the conclusion that it has never been examined; for several authors either assert, or say what implies unequivocally that it is actually known to be Gelatine. An anonymous British work on Zoölogy (*Lond. & Edinb. 1801, Vol. I, Pg. 263*) says of the *Hirundo esculenta*, or Esculent Swallow that it makes "the Edible nests of which the Chinese are so fond." This author says that "these Nests are built of different species of Mollusca, and other marine Gelatinous productions." These are "something like Isinglass," etc. They are "used as food in Broths," etc. Now though this author is mistaken as to the source of the essential material of these Nests, because the real facts of the case never came within the range of his observation; yet he would not be any where near as likely to be mistaken in regard to the substance itself, since he could easily obtain it and examine it at his leisure, or obtain information from others who had previously examined it. William Turton M. D. (*Translation of Linnæus's General System of Nature, etc. Vol. I, Pg. 628 Lond. 1806*) says of *Hirundo esculenta* or Escu-

lent Swallow, that "it makes its Nest of Gelatinous marine substances," and that "these Nests are made by the natives" (of China and the natives of the Indian Ocean) "and luxurious Asiatics, into Broths, or otherwise cooked, and regarded as the greatest dainties of the table," etc. In these two quotations (and I might make others to the same effect) Gelatine is recognized as the peculiar and essential ingredient of these Nests, though we are not told on what precise grounds. Still the thing is so very highly probable that there can be said to be scarcely room to doubt it.

LEÄNTICA LACTEA.

Is there not foundation for still an other turma or group of Leäntics, viz. *Leäntica Lactea*, consisting of the several sorts of Milk as produced by various plants and several of the mammals? Should we admit Milk as the foundation of a turma or group of the Leäntics, this would afford an other example of a turma or group not founded upon an individual organic proximate principle; for Milk is quite a complex compound consisting of numerous individual proximate principles.

ARTOCARPACEÆ.

? *BROSIMUM ALICASTRUM* (*Swartz.*)

{ *BROSIMUM.*
Galactodendron utile (*H. B. & K.*)
Cow Tree.

ASCLEPIADACEÆ.

STAPELIEÆ.

GYMNEMA LACTIFERUM (*R. Brown.*)

APOCYNACEÆ.

PLUMIERIEÆ.

TABERNIMONTANA UTILIS.

EUPHORBIACEÆ.

EUPHORBIEÆ.

EUPHORBIA BALSAMIFERA (*Aiton.*)

The whole of these articles are copius in a rich and wholesome Milk. That of Brosimum (*Galactodendron*) utile, has been analyzed by various chimists, has been found to contain as much as 30.57 per centum of Galactine (incorrectly so called) and is declared to be as wholesome and as nutritious as Cows' Milk. If there is any one of the preceding articles, that all this is not strictly true of, it is Brosimum Alicastrum, since, at certain seasons, the Milk of this article is tenacious and Gummy, which I believe is not true of any of the rest of the list.

CLASSIS	MAMMALIA (<i>Linn.</i>)
ORDO	PECORA (<i>Linn.</i>)
TRIBUS	BOVIDÆ.

Here we may again mention

BOS URUS V. DOMESTIPUS.
BOS GIBBOSUS V. DOMESTICUS.
BUBALUS BUFFELUS V. DOMESTICUS.
BUBALUS ARNÉE V. DOMESTICUS.

all of which have been mentioned in the preceding turma or group, with a specification of their natural history class, order, tribe, subtribe etc. which need not be repeated in this place. Cows' Milk is constantly used as a Leäntic. Boiled till it is of the consistence of Milk-Porridge, and then sweetened to the taste of the patient, it is as good a Leäntic of that section called Demulcent, as any other; and it has the advantage of being in addition an excellent nutrient, so that two purposes are subserved by it, the latter being more important than the former. Every body, I believe, is acquainted with the common Cataplasm of crumb of Bread, boiled in Cow's milk, universally known in New England under the denomination of the Bread and Milk Poul-tice; though as it is commonly prepared, it is one of the worst Poul-tices in common use.

TRIBUS	CAPRIDÆ.
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Without doubt the Milk of various other domestic animals re-

lated to the Bovidæ would be equally valuable for Leäntic purposes ; as for example, that of some of the Capridæ, as

CAPRA ÆGAGRUS V. HIRCUS.

TRIBUS CERVIDÆ.

The same is the fact with that of some of the Cervidæ, as

RANGIFER TARANDUS.

TRIBUS CAMELIDÆ.

It is also the fact with that of some of the Camelidæ, as

CAMELUS ARABICUS.

CAMELUS BACTRIANUS.

I doubt not that there are other Camelidæ whose Milk would be just as good, viz.

{ AUCHENIA GLAMA.
{ *Llama Pacos.*

Llama.

AUCHENIA HUANACA.

Guanaco.

AUCHENIA PACO.

Pacos.

Alpaca.

AUCHENIA ARAUCANA.

Chilihuque (Molina.)

The preceding species have all been domesticated. Beside these, an author upon mastology now before me, mentions and describes two other supposed species, which, he says, have never been domesticated. These are Auchenia Vicugna, the *Vicugna*, and Auchenia Huemel, the *Huemel*, the latter being an animal whose characters and relations have never been thoroughly investigated. I believe that all the domesticated species are generally called Llama, and hence, as I suppose, some authors call the genus Llama or Glama. I do not know certainly that the Milk of any species of Llama is employed either in diet or medicine ; but I

believe that Milk of all the domesticated species is, at least by the Spaniards, though I suppose that it was not employed by the aborigines of the country. I am not apprised that the American race, the autochthons or aborigines of this continent ever used the Milk of any domesticated animals as food; but I should think that the Hispani-Peruvians must undoubtedly have continued this custom of their ancestors, after migration to America, since the Arabian inhabitants of Spain must have been familiar with the Milk of the Camel as an article of food. Now the Llama is so nearly allied to the Camel that, till quite recently both were comprised in one and the same genus; and even now the genera *Camelus* and *Auchenia* or *Llama* are admitted to be very nearly related, almost as much so as the genera *Equus* and *Asinus*, which have been so lately separated. If it were not for the disparity in their size, I doubt not that the species of *Camelus* and the species *Auchenia* or *Llama*, would procreate together, and the hybrid offspring would some times be prolific in warm climates, as the Mule and the Hinny are occasionally, under such circumstances.

ORDO	{ <i>BELLUÆ</i> (<i>Linn.</i>)
	{ <i>PACHYDERMATA</i> (<i>Cuvier.</i>)
TRIBUS	{ <i>SOLIDUNGULA.</i>
	{ <i>EQUIDÆ.</i>
	<i>EQUUS CABALLUS.</i>
	<i>ASINUS VULGARIS.</i>

The Milk of the Mare and of the She Ass have long held a place in the materia medica, from some supposed peculiar efficacy in certain diseases; and they must certainly be as good for making Cataplasms as any other Milk.

LEÄNTICA CEREÆ.

PALMACEÆ.

CORYPHEÆ.

CORYPHA CERIFERA (*Martius.*)

This article is affirmed to produce a very useful Wax; but exactly how pure it may be I know not, as I have seen no analysis of it.

ARECEÆ.

{ *IRIARTEA ANDICOLA* (*Sprengel.*)
 { *Ceroxylon Andicola* (*H. B. & K.*)

This Palm is said by good authority to furnish genuine Wax, and not a concrete greasy oil, i. e. a Suet or Tallow; but the exudation called Wax is found by analysis to be only one third Wax, and two thirds Resin. It is said to be all the better for the preparation of Cerates for the Resin which it contains.

MYRICACEÆ.

MYRICA CERIFERA (*Linn.*)

I believe that this article produces a genuine Wax, though of a sort differing very considerably from Bees' Wax, since when made into candles it is consumed more rapidly than Tallow which burns-out much faster than Bees' Wax. According to my own observations, the Wax of this plant makes a better Cerate than Bees' Wax. At all events such Cerate seems to be incapable of becoming rancid.

CUCURBITACEÆ.

CUCURBITEÆ.

BENINCASA CERIFERA (*Savi.*)

The fruit of this article is said to produce a Waxy substance, from which I infer it is not absolutely pure Wax.

OLEACEÆ.

OLEÆ.

LIGUSTRUM LUCIDUM (*Aiton.*)

The product of this plant is alleged to be pure Wax, which may perhaps be somewhat doubtful.

DIPTEROCARPACEÆ.

VATERIA INDICA (*Linn.*)

The product of this plant is often called Wax, though it is said to be between Resin and Wax, in all probability like the product

of Iriartea Andicola. It is said to make excellent candles which emit little or no smoke. This would seem to indicate that a very large proportion of it is Wax.

EUPHORBIACEÆ.

HIPPOMANEÆ.

STILLINGIA SEBIFERA (*Michaux.*)

This genus was named after Dr. Stillingfleet and therefore it should have been Stillingfleetia, which Bojer actually calls it. Its product is commonly called Tallow, from which I should be inclined to think that it might be a concrete greasy oil. But it is said to make excellent Cerates, from which I conclude it contains more or less Wax.

I have already given the natural history class, order and tribe of

APIS MELLIFICA (*Linn.*)

This insect is the chief source of most of the Wax in common use. I believe that less of this sort of Wax is necessary to give a Cerate a proper degree of consistence than of any other sort, and to this circumstance possibly may the fact be due, that Cerates made with it are more liable to become rancid than Cerates made from Wax obtained from vegetables. From this circumstance I have been in the habit of considering Bees' Wax as less eligible for this purpose than any other Wax. But I may possibly be in error in regard to this subject, since I have investigated it far less thoroughly than most of the topics in regard to which I entertain views more or less at variance with common usage.

LEÄNTICA CALORIAQUOSA.

Is Water truly Leäntic? I do not think it is Demulcent in the common acceptation of that term; and though I have commonly been in the habit of considering it as Emollient, as that term is commonly employed, yet I have great doubts whether it is truly such. If it is, it can not be referred to any of the preceding turmæ or groups, but (to use such a Hibernism, as is common in natural history) it must constitute a turma or group by itself. But I think that it is only warm or hot Water that can be reckoned as even Emollient, and in this, the Emollient power is due rather to

the heat than to the Water, though the Heat without the Water as its vehicle might not produce the effect in question; and certainly the Water without the heat would not do it.

To those who have not devoted more or less time to the study of natural history, the preceding catalogue of articles, without popular names or any accompanying explanatory matter, may seem rather unintelligible. It must be remembered however that it could not well be accompanied by either of these, without involving a great deal of unnecessary repetition, for all this must needs accompany what I shall have to say of each individual article hereafter. But it may be said that the popular names at least might have accompanied those of natural history. It should be remembered however that the popular names of all except a few of the universally used articles, such as Wheat, Rye, Maize and a few less common articles found in England and the U. S. A. are as unintelligible to all but naturalists, as the natural history names, and often even more so. Is the name *Wesuckapucky*, which is the popular name of a North American plant, in the region where it grows, any more intelligible than the most difficult natural history name that occurs in the preceding catalogue? Indeed how many persons in the U. S. A. (those who have studied natural history excepted) would understand the English popular name Rowhan Tree or its abbreviation Roan-Tree, or even an other of its English popular names, viz. Quicken Tree, any better than *Sorbus Aucuparia* or *Pyrus Aucuparia*, for a tree at present not uncommon among us as an ornamental tree.

But the catalogue may seem to be preposterously long. I have not however mentioned a single article not specified as belonging to this class, in some work of established character in my library. In a large work like that which I am writing (though it will fall far short of six octavo volumes, the extent of Baron Murray's *Apparatus Medicaminum*) the catalogue should be full, and at least approximate to completeness, according to the present state of our knowledge. Assuredly there ought to be an arranged display of most if not all the articles belonging to each class, that are ever employed in medicine, in any part of the world. It is true but a small portion of them will need full details of their powers and applications. For illustration, when a writer on the *materia medica* has given a full account of Sulphate of Magne-

sia (Sal Ebshamense or Epsom Salt) as an Antiphlogistic and Neuragic Hydragogue Cathartic, he will have occasion to treat of all the rest of the articles belonging to the same group only briefly and comparatively, for the purpose of pointing out their peculiarities (if they have any) in comparison with Sulphate of Magnesia. Just so it will be with the Leäntics. But when I come to give an account of the different articles of this class in the subsequent part of this work I shall give their synonyms and their popular names (when they have any) so at least as to be perfectly intelligible, and even to furnish a key to the nomenclature of any and every author in common use among us; and till this is accomplished I must crave the patience of my readers. I will only add that this is probably much the longest catalogue of articles belonging to any class, that I shall have occasion to make; and more than this, in my view it is the least important class in the whole materia medica; and yet it is as necessary in its proper place as any other class. We must have weak as well as strong medicines. We can not (to use Dr. Rush's language) "discharge a cannon to kill a Fly" though it may be quite desirable to kill the Fly. Weak medicines are in fact much oftener needed than strong ones. The man who travels with no smaller money than Eagles will be perpetually embarrassed for the want of Cents. Just so it would be with the physician who has nothing but what it is the fashion at present to call Herculean medicines. It would be necessary for him to be exceedingly dextrous to avoid doing injury rather than rendering service. By all means let the practitioner of medicine have at his command a large catalogue of moderate as well as powerful articles, for he will much oftener need the former than the latter.

PROËM TO THE CLASS NEURAGICA.

The term Neuragica is compounded of νεῦρον a nerve and ἄγω to influence; to affect; to lead; to conduct; to sway; to control; to rule; etc. Neuragia or neuragy I shall employ to denote the effects of the Neuragics. These terms will at once be perceived not to be ancient, but of my own formation. They do not please

me, but they were the best I could devise. As I have been in the habit of distinguishing several powers not previously reckoned as belonging to the *materia medica*, it was utterly impossible that as a public instructor, I should use the definition instead of a name, whenever I had occasion to refer to these powers. But notwithstanding this difficulty, I refrained as long as possible from devising and forming any new terms, till the repeated solicitation of a large number of my pupils compelled me to refrain no longer. After all, I can discover no good reason why a new term should not be formed whenever it is really needed, provided it is legitimately and judiciously done. If it is not of this character, nobody is obliged to employ it, and whenever a better term is devised, it should be preferred. The intrinsic import of the term *Neuragic* is an article that acts upon the nerves. Now all medicines act upon the nerves. But we can not expect such a term to imply a whole definition. I do not hesitate to pronounce it utterly impossible to make a term, that shall imply the peculiar manner in which this class acts upon the nerves. The same defect which belongs to this term, belongs also to several of the oldest and best established terms in the *materia medica*. I will cite *Narcotica* for an example. This term, as I have already explained, denotes articles which benumb, stiffen, and render insensible. Now I do not know that *Narcotics* even stiffen unless they are given inordinately and produce a convulsion. They are certainly never administered in medicine for this purpose. I do not know that, properly speaking, they ever benumb. I do not know that they ever produce insensibility except as the result of coma, an operation beyond the medicinal grades of their effects. The name of a power ought never to imply any thing at all erroneous; but we must frequently be content with one not diagnostically distinctive of the peculiarities of the power, which it is employed to designate. But whenever any man will suggest a better name for the power which is the foundation of this class I will gladly adopt it.

Definition. *Neuragics* are articles, which, without either Euphrenic, Narcotic, Leäntic or Antiphlogistic powers, operate prominently and peculiarly upon the nervous system generally; some of them,—in the *first degree of their operation*, efficiently allaying, in a manner peculiar to themselves, morbid susceptibil-

ity, morbid sensation and irritative action or motion, without any vertigo or general tremors in conjunction ; while others produce more or less vertigo and general tremors ; both at the same time obviating or contributing to obviate spastic or convulsive diathesis ;—in the *second degree of their operation*, they occasion, in addition to the preceding effects, more or less languor, lassitude and prostration, more or less neuralgic pain, more or less palpitation of the heart, and arteries, more or less torpor as respects the peristaltic action of the intestines, more or less stupor or numbness in some parts of the extremities, sensations of constriction in the feet and hands, peculiar weakness particularly in the lower extremities or rather the lower half of the body, many symptoms and phenomena resembling Chorea, morbid wakefulness, etc.—in the *third degree of their operation*, they produce violent neuralgic pains in various parts of the body and limbs, great anxiety, general agitation, spastic or convulsive affections, obtuseness of the special senses or positive abolition of some of them, dilatation of the pupils of the eyes, emaciation, paralysis of the extremities both as respects voluntary motion and common sensation, delirium, general exhaustion, and some times, even not infrequently, death.

As I have already said in an other place, the class Neuragica is much easier defined by its ultimate effects, i. e. those which transcend or go beyond its medicinal operation, than by its proper medicinal effects. But it will at once be obvious that a class in the *materia medica* can never be properly founded on non-medicinal operations. And yet whatever may be the obscurity of the diagnosis of this class of agents, by the abridged definition or character, there is none at all by the full account of them.

The power upon which this class is founded, has been recognized time immemorial, as belonging to the compounds of Lead ; but I do not now recollect that it has been known to be possessed by any other articles, and much less by a large group ; and I think that such power has never been made the foundation of a class of remedial agents, by any writer on the *materia medica*. This class then is new, as it may be said to be the fact that the power on which it is founded belongs to very numerous articles ; and under such circumstances, it will be obvious that the name must be new. With the latter I have already admitted that I am

not satisfied, on the ground of the too great generality of its signification, and its absolute destitution of every thing specific and diagnostic; but I have long since given-up in despair all hope that I shall of myself ever devise a better.*

If the Neuragics are taken in exceedingly small doses and quantities within a short time, so that they are long in producing their ultimate effects, the patient is generally affected with a kind of leucophlegmatia or an hæmia. or at least, has great pallor, some times preceding, and some times only accompanying the ultimate effects. As I have hinted in my definition of the Neuragics, at a certain stage of ultimate Neuragy, there is very often more or less morbid obtuseness of the special senses, and more or less morbid acuteness of common sensation. At a certain stage of ultimate Neuragy, there is often more or less Œdema, i. e. Hydrops cellularis v. artuum inferiorum, as some suppose, caused by a Paresis of the absorbents. In regard to this last point however, I would not be understood as hazarding an opinion. From single very large doses of the Neuragics, at least, if they are excessive or absolutely inordinate, many of the symptoms of the third grade of their operation may occur, without being preceded by the symptoms of the previous grades, particularly without the Neuralgic pains.

The order in which the operative effects of the Neuragics take place, is variable to a very considerable extent. As far as my observations contribute to prove any thing, the Neuragics would seem to act, first on the nerves of common sensation, at least when they are taken rather largely within a comparatively short time; next upon the involuntary motor nerve of chiminal action, nutrition and reproduction; next upon the involuntary motor nerves of expression and respiratory motion; next upon the nerves of voluntary motion; and finally upon the nerves of special sensation, and even upon the hemispheres of the cerebrum. But the order in which they affect different parts is considerably varied

* The two last paragraphs, as will doubtless be perceived, are to be found in a preceding number of this work. They are however necessary here, which they were not in the preceding number to which I refer. But many of our subscribers would have a definition of our classes, before we could arrive at the regular proëm to each class, in the ordinary course of the work. This, as will at once be seen, necessarily involves a repetition of these definitions, together with that of a few explanatory remarks which are often required to accompany them.

by the size of the doses, and the length of time that their use has been continued before any of their operative effects begin to be noticeable.

When taken in minute doses and quantities and continued for a very long time, the first manifestations of their operation are some times a peculiar form of Dyspepsia, so far as I could discover, consisting intirely in certain peculiar disturbances of some of the functions of the nerve of chimical action, nutrition and reproduction, manifested by a greater or less degree of indigestion, vitiated secretions and diminished peristaltic action. By further continuance of the Neuragics in this manner, the functions of the esophageal and gastric par-vagum begin to be disturbed, manifested by morbid irritability of this nerve and frequent small vomitings, seldom accompanied by any nausea, and more rarely still by any retching. If the nature of these effects is not understood, and the use of the Neuragic agent is still continued, the functions of the nerves of common sensation will begin to be disturbed, some times manifested by morbid sensibility of the stomach, but oftener by Neuralgia constituting Gastrodynia. When taken in minute doses and quantities and their use continued for a comparatively long time, the first morbid manifestaions are some times a peculiar sort of Rheumatalgia indicated by moderate pain and greater weakness of the muscles affected. If this goes-on, it passes slowly and gradually into Paresis, which sooner or later becomes a perfect Paralysis. Here lesions of the functions of both nerves of common sensation and voluntary motion are simultaneously the first effect, though the latter disturbances are more intense and permanent.

From the same method of exposure to the operation of the Neuragics as last described, some times, in exquisitely strumous habits, a kind of Chronic Rheumatismus is produced, manifested by a swelling, often a slight soreness and always a great weakness, first of the small joints of the fingers and toes, which sooner or later extend to the larger joints, as the wrists and ankles, and some times even to the elbows and knees. This is hardly distinguishable from what I have commonly heard called Strumous Rheumatismus, though I never could discover in what respect that differed from pure Struma of the joints of the most chronic sort.*

* I often mention Rheumatalgia and Rheumatismus in contradistinction from each other. I consider them as different and intirely distinct diseases. The former is a

Some may perhaps inquire for the evidence of the instrumentality of any Neuragic in the production of such a disease. I can only say that far the largest proportion of all the cases that I have ever seen, had been long exposed to the influence of some Neuragic, and did not happen to affect subjects that were very exquisitely Strumous, while all the cases that I have ever known to occur without any influence from a Neuragic, have been far less numerous and have occurred only in the most exquisitely Strumous habits. In these cases, as would seem, that part of the involuntary nerve of chiminal action, nutrition and reproduction, which is sent to the secernents and absorbents of the deceased parts, and the nerves of voluntary motion which flex and straiten the swelled joints, seem to be the parts of the nervous system, in which the burden of the disease is manifested. The ultimate effects just described, I have witnessed as produced by preparations or compounds of Lead mainly; but a greater or less number and amount of them are capable of being produced by all the rest of the Neuragics, unless they possess some other different and distinct power, which prevents their being used with sufficient freedom, or being continued long enough to operate in the manner described. These are by no means the whole of the morbid effects produced by this class of agents; but they will serve as specimens in this place; and hereafter I shall detail all the several pathological conditions occasioned by the Neuragics, without describing the several specific diseases, into which they are commonly combined.

Neuragia or Neuragy both medicinal and ultimate consists in 1st Sedation, of a peculiar sort; 2d Neuralgia; 3d Convulsion some times; 4th Paralysis or perhaps Acinesia rather; 5th Exhaustion. In the ultimate Neuragy of Lead, and perhaps of other agents there is some times, beside the preceding, 6th Delirium;

sort of Neuralgia having its normal seat primarily in the nerves of common sensation of some of the muscles, but sooner or later involving the nerves of voluntary motion, often producing a Paresis, and some times even a Paralysis. It is never attended with any Phlogosis nor, except accidentally, with any Pyrexia. The latter is essentially a Phlogosis, always having its normal seat in joints, the pain and immobility depending upon the Phlogosis. In its most acute form, it is essentially a Pyrectic disease, the Fever being acute Hectic; in its subacute form the Fever is moderate subacute Hectic; but in its chronic form there is either no Fever at all, or it is slight and obscure chronic Hectic. In their retrograde and aberrant forms, both of these diseases may affect other parts beside those which are their normal seats.

and following Neuralgia of the Intestinal Canal i. e. Colic; 7th Contractions of certain portions of the mucous membrane. It is to be particularly observed that the peculiar sedation of the Neuragics is the only medicinal grade of their operation. All the other grades must be considered as ultimate Neuragy i. e. as transcending all useful medicinal operation. The ultimate and (as they are popularly called) poisonous effects of all of those articles of the whole of the materia medica, that are sufficiently active to be reckoned poisonous, always consist of a certain number of pathological conditions so associated as to constitute some regular species or variety of disease well known in nosology. Diseases produced in such a manner however, always have certain peculiarities more or less prominent in different cases, by which, in general they may be readily distinguished from the non-factitious and normal cases, of whose causes we are commonly in utter ignorance. The following are some of the species or varieties of disease that occur normally, and so far as our knowledge extends, to all appearance spontaneously, counterfeits of which are produced by certain neuragics. Neuralgia notha and Neuralgia Rheumatalgica both occur normally, and to all appearance spontaneously, while both are produced by the Neuragics. The factitious disease has a very near resemblance to the apparently spontaneous and normal one, and I do not know certainly that they can always be distinguished, without a knowledge that the patient has been protractedly exposed to the influence of Neuragics; and yet the two never change into each other, at any time during their course and progress, but remain perfectly distinct in their middle and their end, as much as in their beginning. They are therefore distinct nosological species, however near may be their resemblance. According to its seat, Neuralgia notha produced by Neuragics, as well as the normal and apparently spontaneous form, takes different names, Cephalodynia when it is in the head; Gastrodynia when it is in the stomach; Enterodynia or Colica when it is in the intestinal canal. What is called Colica Ileus in nosology is the normal and apparently spontaneous disease, while what is called Colica Rachialgia is the disease as produced by Neuragics. These differ more from each other than the two forms usually do, though they are not always distinguished by all physicians. Neuralgia Rheumatalgica should always be associated

with the other species of Rheumatalgia. I believe that the Neuragics never produce Neuralgia vera. At least I never witnessed such a case.

The Neuragics produce an Entasia Convulsio, which is rarely distinguished from the normal and apparently spontaneous disease, without a knowledge of the fact that the patient has been long exposed to the influence of some Neuragic. The Neuragics produce a Synclonus Tremor and a Synclonus Chorea which are not always distinguished from the normal and apparently spontaneous diseases, unless the physician happens to learn that the patient has been exposed protractedly to the influence of some of the Neuragics. The Neuragics some times produce Paralysis particularis and even Paralysis Paraplegia, if these are not rather Acinesiae than Paralysees. They also produce Paropsis Amaurosis and Paracusis Surditas, if these are not Acinesiae instead of Paralysees. There is liable to be the same failure of discriminating these, as of discriminating other cases produced by the Neuragics, unless the physician ascertains that the patient has been exposed to a sufficient influence of some Neuragics. There is a supposed Marasmus whose trivial name is Tabes, which is produced by certain Neuragics. I have had doubts whether this is a true Marasmus or not, i. e. whether the failure of function, which constitutes the essential pathological condition, is in the ultimate assimilating organs or not; but whether it is or not, I suppose there is no doubt that the disease, whatever it may be, is produced by certain Neuragics. I have every reason to think that this malady is not very often distinguished from normal and apparently spontaneous Marasmus climactericus, which often occurs at the second climacteric period, viz. about the age of fourteen; much oftener at the third climacteric period, viz. about the age of twenty-one; very rarely at the fourth and fifth climacteric period, viz. about twenty-eight and about thirty-five years of age; not infrequently in women at the sixth and seventh climacteric periods, viz. about forty-two and forty-nine years of age; rarely at the eighth climacteric period, viz. about fifty-six years of age, but which is supposed to occur by far the most frequently, at the ninth climacteric period, viz. about the age of sixty-three, which is therefore called the grand climacteric. I can not say how many times I may have failed of distinguishing Marasmus Tabes (so called) from

Marasmus climactericus, but I trust I have not done it often, since I always endeavor to ascertain whether a patient has been at all exposed to the influence of any Neuragic, and a knowledge of such a fact, in conjunction with the peculiarities of the case, I have always supposed has led me to the true diagnosis and the right conclusion.

Diarrhœa vulgaris is often produced by the Neuragics. How far such cases are distinguishable from normal and apparently spontaneous cases, I know not. I do not now recollect ever distinguishing a case produced by Neuragics, without a knowledge that the patient had been exposed to their influence. This point has always been so easy to arrive-at, in all the cases that I have ever had, that afforded ground for suspicion of Neuragic influence, that I never omit to inquire about it, so that my ability to detect such a case, without this knowledge, has perhaps never been fairly tested.

Coprostasis is some times occasioned by the Neuragics. Where I meet with a peculiarly intense case, rather speedily succeeding to a healthy state of the intestinal canal as far as relates to peristaltic action, more especially if the case has other peculiarities, I always inquire whether there has been any exposure to the influence of Neuragics, which, with the aggregate of the symptoms of the case, as I have been in the habit of supposing, has always led to a true diagnosis.

Rather a peculiar Limosis Dyspepsia is occasionally produced by the Neuragics; and altogether the most harrassing and distressing form of Limosis Syncoptica that I have ever witnessed, has been first occasioned and subsequently kept-up by compounds of Lead protractedly taken, which I consider as the most active of all the agents of this class. If fortunately, it were not taken in such small doses and quantities, in our common aqueduct waters, it would doubtless be very destructive to life. I never met with a bad case of Limosis Syncoptica without making very thorough inquiries whether the patient had been exposed to the protracted influence of any Neuragic, and more especially Lead; and as I suppose, the facts in regard to this point always assist me to a true diagnosis. Although Limosis Dyspepsia is far less frequently produced by the Neuragics, yet as Lead-pipe is so common for receiving water into houses, from wells and aqueducts, I always

inquire, in every case of Dyspepsia, whether the patient has been in the habit of using such water, or has been exposed to the influence of Lead in any other manner, or has taken any compound of Zinc long and very protractedly, etc. in order to ensure correct diagnosis.

An Arthritis Rheumatismus v. chronicus is certainly some times produced by the Neuragics, more especially in exquisitely Strumous habits; but from my first knowledge of this disease as a normal and apparently spontaneous one, I have known that an analogue of it is producible by Neuragics, and therefore whenever I am consulted about such a case I always inquire whether the patient has been exposed to the influence of any agent of this class. When this particular disease is complicated with Struma, whether the Neuragics have had any instrumentality in its production or not, I believe it always begins in the joints of the smallest extremities, as of the fingers, and perhaps the toes, though I must admit that I have neglected to inquire about the latter. Although Rheumatismus in all its other forms is eminently metastatic, yet I never knew this variety either in a retrograde or aberrant form. It must be observed that Rheumatismus, in all its forms, is a Pyretic Phlogoticum, the normal seat of the topical affection being the joints; while Rheumatalgia is an Apyretic Neuroticum, the normal seat of whose topical affection is muscle. In all probability, in the former, the nerve of chiminal action, nutrition, etc. of the joints is first affected; while in the latter, the nerves of common sensation and voluntary motion of the muscles, are first affected.

I have not even now specified near all the forms of disease that are produced by the Neuragics; but those that I have mentioned must suffice. Although I am disposed to believe that all those forms of disease which are produced by the Neuragics, are really and truly distinct species from their analogues, which occur normally and to all appearance spontaneously, and of course are capable of certain diagnosis; yet I can not venture to claim that I can myself readily distinguish them at once, and with certainty, without carefully investigating whether the patient has been exposed to the influence of any Neuragic; and of course I always make such investigation. I am inclined to think that Colica (Enterodynia) Rachiälgia, has the most marked distinguishing char-

acters of any of them and therefore may be the most readily discriminated ; but this opinion may be due to the fact that I have studied this disease the most thoroughly of any of them.

It is not certainly known in what manner inordinate and fatal doses of the Neuragics would destroy life, since sufficiently careful and accurate observations do not appear to have been made, or if they have been, are not recorded, to illustrate this point. From some circumstances with which I am familiar, it seems to me quite probable that the powers and functions of the involuntary nerve of chiminal action, nutrition and reproduction fail first under poisoning by the Neuragics ; but I have no actual knowledge on this subject.

The medicinal operations which are remedial of ultimate Neuragy are 1 Narcosis, 2 Erethismus, 3 Oræsthesis, 4 Antisbesis, 5 Tonic effects, and perhaps, 6 Adenagia. The individual agents, which (so far as my experience and observation go) are the best for the production of these several effects, in such cases are the following, viz. 1 Papaver somniferum, 2 Camphor officinarum, 3 Conium maculatum, 4 Digitalis purpurea, 5 Nicotiana Tabacum, 6 Ignatia amara, 7 Strychnos Nux-vomica, 8 Aconitum Napellus, 9 Capsicum annum, 10 Argentæ Protonitras, 11 Cantharis vesicatoria, 12 Rhus venenata, 13 Ætherogenii Protoxydi Unihydri, scilicet *Alcohol officinale*, 14 Quinini Cyanidi Dicyanoferris, 15 Quinini Cyanidum, 16 Quinini Oxydi Disulphas, 17 Ferræ Diprophosphas, etc.

The recognition of this power as belonging to the materia medica and therapeutics, may be said to be new. As produced by Lead it has long been known as a poisoning agency. This ought to have led to its use in therapeutics, since, as a medicine and a poison are undeniably synonymous terms, the primary grades of the operation of all poisons are assuredly remedial or therapeutic, while the ultimate grades of all medicines are certainly poisonous in a greater or less degree. In proof of this I will only mention Strychnos toxifera, Rouhamon Gujanense and Rouhamon (? Curare,) known both as medicines and as the most active poisons in the world, under the name Woorara, together with Strychnos Tieutè and Antiaris toxicaria, known as the most valuable medicines, and as highly powerful poisons, under the name Bohun Upas.

Several of the Neuragics have been long in use in medicine, and for their Neuragic operation and effects, but without any recognition that they possessed this power. In fact, their remedial operation has invariably been ascribed either to some different and distinct power, or to some merely hypothetical one. Their therapeutic effects have often been ascribed to what has been called a Nervine power; but what is this? It is commonly explained as an agent acting on the nerves. According to this vague definition it would be about equally applicable to the Leäntics, the Neuragics, the Narcotics, the Erethistics and the Euphrenics. But I never read an author, or conversed with a practitioner of medicine who had any notion of this power as I view it. It may therefore be very properly considered as a newly recognized power. Perhaps it may be thought that in their intrinsic etymological import the terms Nervine and Neuragic are perfectly synonymous. This is very nearly true; but the term nervine has been spoiled, as a name for this class, by long application with much greater latitude; and besides, is of Latin origin, which is contrary to the canons in regard to the names of the classes. The term Neuragic is new, has never been applied with any greater latitude, and is of Greek origin. It ought therefore to be preferred.

The therapeutic effects of the Neuragics have often been ascribed to what has been called an Antispastic, or (by a great blunder as respects language) an Antispasmodic power. But is there any such peculiar and distinct power in the materia medica? The Narcotics are probably the most eminently Antispastic of any class in the materia medica. The Neuragics would perhaps come next after these, and the Euphrenics, as far as they are known, are generally put-down as Antispastics. But there is no such thing as a distinct and specific Antispastic power. All the agents that ever relieve spasms, do it by virtue of some power which is the foundation of some other class.

The therapeutic effects of the Neuragics have often been called Sedative (Sedativa and Sedantia) and so have the effects of the Antiphlogistics in phlogistic diathesis, of the Leäntics, and the Narcotics, and as I think I may say with propriety, of the Euphrenics. But there is no peculiar and distinct power entitled to the name Sedative, and this class certainly is not distinctively so,

and therefore can not properly be designated by this name. But the terms *Sedantia* and *Sedativa* are of Latin origin and should therefore be rejected on this ground. But the most singular reference of the therapeutic power and operation characteristic of this class, is to the *Narcotics*! What a strange effect to ascribe to a *Narcotic* is *Colica* (*Enterodynia*) *Rachiälgia*, and *Rheumatalgia*! Who ever heard of any *Narcotic's* ever producing *Arthritis Rheumatismus v. chronicus*, or any species or variety of *Paralysis*, or of true and proper *Acinesia*? But I will not stop to point out all the differences between the *Neuragics* and the *Narcotics*, but to avoid needless repetitions, I will refer my readers to the full account of the operation and effects of the *Neuragics*, as contained in this proëm, and to the full account of the operation and effects of the *Narcotics*, soon to come in the regular course of this work.

With the exception of the *Alcaloid Indigotina* and those plants which contain a notable quantity of the *Element Iodine*, all the *Neuragics* are of *chimical-inorganic* origin. But *Indigotina* fully formed does not exist in any plant living or dead, but only as a compound radical of *H. C. N.* It requires to be *alcalized* by oxydation in the dead plant, in connexion with the process of obtaining it in a detached and separate state. *Iodine* being an element, must actually exist in plants, as well living as dead, but in exactly what state of combination is not known.

The *Neuragics* may be very properly distributed into two primary *turmæ* or groups, viz. I. *Neuragica Non-Oræsthetica* or those which in every grade or degree of their operation, are entirely destitute of any *Oresthetic* power; and II. *Neuragica Oræsthetica* or those which, in some grade or degree of their operation, prove more or less *Oresthetic*. The *Non-Oresthetic Neuragics* may be subdivided into

1. *PURA.*
2. *NARCOTICA.*
3. *STYPTICA.*
4. *ADENAGICA.*
5. *ADENAGICA, SUBANTIPHLOGISTICA.*
6. *ADENAGICA, TONICA.*
7. *STYPTICA, EMETICA, SUBCATHARTICA.*

NEURAGICA NON-ORÆSTHETICA.

1. PURA.

Orginis Organicæ.

INDIGOTINA ALCALOÏDES.

*Orginis inorganica.*PLUMBÆ PROTOCARBONAS $\text{Pb}^1 \text{O}^1 + \text{C}^1 \text{O}^2$ PLUMBI DISOXYDUM $\text{Pb}^2 + \text{O}^1$ PLUMBI PROTOXYDUM $\text{Pb}^1 + \text{O}^1$ PLUMBI SESQUOXYDUM $\text{Pb}^2 + \text{O}^3$ { PLUMBI DEUTOXYDUM $\text{Pb}^1 + \text{O}^2$ { *Acidum Plumbicum.*PLUMBÆ DIPROTOPLUMBAS $2 (\text{Pb}^1 \text{O}^1) + 1 (\text{Pb}^1 \text{O}^2)$ PLUMBÆ PROTOSULPHAS $\text{Pb}^1 \text{O}^1 + \text{S}^1 \text{O}^3$

If taken in much too large a dose, these preparations of Lead are quite liable to offend the stomach in such a manner or degree, as to be rejected from it; but I do not think that they possess any true or proper Emetic power. There are very many articles throughout the materia medica, that will be rejected in the same manner from too large a dose, which no body has ever thought of reckoning among the Emetics, nor could they ever be made to produce the remedial effects of an Emetic in any disease whatever.

ZINCI PROTOXYDUM $\text{Zn}^1 + \text{O}^1$

ZINCI PROTOXYDUM NATURALE.

ZINCI PROTOCARBONAS $\text{Zn}^1 \text{O}^1 + \text{C}^1 \text{O}^2$

ZINCI PROTOCARBONAS NATURALIS.

VISMUTHÆ TETRACIPROTONITRAS BINHYDRUS.

 $4 (\text{Vi}^1 \text{O}^1) + 1 (\text{N}^1 \text{O}^5) + 2 (\text{H}^1 \text{O}^1)$

If too largely and protractedly used, these preparations of Zinc will produce Neuralgic pain and Acinesia or Paralysis. I have seen what is called Paraplegia, perhaps Acinesia, of the hinder parts of a brute animal, by eating habitually and for a long time, the dressings of a very extensive Scald, which consisted of a Cerate of the Natural Carbonate of Zinc spread upon pledgits of lint. Till the disease was produced, it was not noticed that the Cats eat these dressings, and then the fact was ascertained. The preceding preparations of Zinc undoubtedly possess a very slight degree of true

and proper Emetic power, too slight however, to allow them to be used medicinally as Emetics. If the dose is too large they are still more liable to be rejected from the stomach, than the preceding preparations of Lead. This limits their doses, and this is all that need be said of it. We are told that the Tetraciprotonitrate of Bismuth, in over doses or too long continued "produces alarming gastric distress, nausea, vomiting, Diarrhœa or Coprostasis, Colic (Enterodynia) heat in the breast, slight rigors, vertigo and drowsiness." (*Wood's & Bache's Dispensat. 2d Edit. Philad. 1834, Pg. 798.*) If this statement is correct, it is most decidedly a Neuragic. But I have known practitioners of medicine, who, after prescribing it in almost all possible doses and quantities in the twenty-four hours, and in almost all the diseases in which it is ever recommended, have at last come to the conclusion that it is inert. In fact my own experience is not essentially different from this. I have no reason to think that I ever succeeded in benefiting a single case of disease by it. Can it be possible that we have always had a spurious article? Other physicians have used from the same parcels that we have, and considered the article as genuine, giving it only in doses of about three grains, and as they considered, with satisfactory effects, though I could perceive no effects at all.

I should expect a priori that the pure Non-Oresthetic Neuragics would prove direct Antaphrodisiacs; but I am not acquainted with many observations on this subject.

2. NARCOTICA.

PLUMBI PROTOCYANIDUM $\text{Pl.}^1 + \text{C.}^2 \text{N.}^1$

ZINCI PROTOCYANIDUM $\text{Zn.}^1 + \text{C.}^2 \text{N.}^1$

3. STYPTICA.

PLUMBÆ PROTACETAS TERNHYDRUS.

$\text{Pb.}^1 \text{O.}^1 + \text{H.}^3 \text{C.}^4 \text{O.}^3 + 3 (\text{H.}^1 \text{O.}^1)$

PLUMBÆ DIPROTACETAS DENIHYDRUS.

$2 (\text{Pb.}^1 \text{O.}^1) + 1 (\text{H.}^3 \text{C.}^4 \text{O.}^3) + 10 (\text{H.}^1 \text{O.}^1)$

PLUMBÆ TRIPROTACETAS BINHYDRUS.

$3 (\text{Pb.}^1 \text{O.}^1) + 1 (\text{H.}^3 \text{C.}^4 \text{O.}^3) + 2 (\text{H.}^1 \text{O.}^1)$

PLUMBÆ HEXACACETAS TERNHYDRUS.

$6 (\text{Pb.}^1 \text{O.}^1) + 1 (\text{H.}^3 \text{C.}^4 \text{O.}^3) + 3 (\text{H.}^1 \text{O.}^1)$

The four preceding salts are slightly Oresthetic, not sufficiently so to act upon the sound skin, but only upon Ulcers. With only this degree of Oresthetic power it is not worth while to reckon them as medicinally and positively Oresthetic.

4. ADENAGICA.

PLUMBI PROTIODIDUM $\text{Pb}^1 + \text{I}^1$

ZINCI PROTIODIDUM $\text{Zn}^1 + \text{I}^1$

5. ADENAGICA SUBANTIPHLOGISTICA.

POTASSII PROTIODIDI IODOPLUMBAS.

$\text{K}^1 \text{I}^1 + \text{Pb}^1 \text{I}^1$

6. ADENAGICA TONICA.

FERRI PROTIODIDUM $\text{Fe}^1 + \text{I}^1$

FERRI SESQUIODIDUM $\text{Fe}^2 + \text{I}^3$

7. STYPTICA EMETICA SUBCATHARTICA.

ZINCÆ PROTACETAS SEPTENHYDRUS.

$\text{Zn}^1 \text{O}^1 + \text{H}^3 \text{C}^4 \text{O}^3 + 7 (\text{H}^1 \text{O}^1)$

ZINCÆ PROTOSULPHAS SEPTENHYDRUS.

$\text{Zn}^1 \text{O}^1 + \text{H}^3 \text{C}^4 \text{O}^3 + 7 (\text{H}^1 \text{O}^1)$

Neuragic effects, as produced by these several articles, differ very considerably in quality, as well as intensity. I have seen only the primary grades of a Neuragic operation from the Alcaïoid Indigotina or Indigotine, and of course, I do not certainly know that it is capable of producing any of the ultimate grades. It is probably more moderate in its effects, at least in convenient doses and quantities in the twenty-four hours, than any of the chîmical-inorganic Neuragics. It was a long time before I was able to determine satisfactorily its true powers, on this account.

Neuragic effects are the most marked, prominent and peculiar, as produced by the compounds of Lead, and from these more particularly the Protocarbonate, from which there is the greatest hazard of failing to avoid very troublesome and painful, if not dangerous ultimate effects. I believe that this salt is altogether the most convenient and sure agent in the whole materia medica, for felonious and fatal poisoning. I think it might easily be given in such a manner, as to produce certain and speedy death, or in such

a manner as to produce certain and lingering or tardy death. Indeed it is said to have been formerly employed in Italy, to produce death in both of these ways. Being insipid and inodorous, it can be mingled with meal or flour, with powdered white Sugar, and with very numerous other articles of food. And yet most people would seem to be absolutely in love with this poison, since they insist on taking it in the water which they employ daily, and in various other ways, and feel great indignation at the physician who warns them of their danger.

In its Neuragic effects Zinc has the nearest resemblance to Lead in quality; but it is much less active—much less virulent, and much easier of management. I have not infrequently however seen mischievous Neuragic effects from the compounds of Zinc used incautiously and protractedly, under the mistaken notion that they are tonic. In general, their morbid effects are much less hazardous, and much more easily curable than those of Lead; and yet if they have been used so long as to have produced what is commonly called Paraplegia (more probably mere Acinesia of the lower extremities) I should doubt whether such an affection would be very easily curable.

From observation and experience I have no knowledge of the Neuragic effect of Binhydrous Tetraciprotonitrate of Bismuth. I can therefore say nothing of its quality or intensity. I have never witnessed any but the primary Neuragic effects of Iodine; but its ultimate Neuragic effects have been often described. From such descriptions as I have read and studied, I should think these ultimate Neuragic effects must be quite peculiar, in comparison with those of Zinc, and even those of Lead, and very intense in degree; but they can not be very easily produced, for I have often seen Iodine used more freely and more protractedly than is often mentioned by European authors; and yet I never witnessed the slightest trace of any ultimate Neuragy from it. I have known it used freely for eighteen months; but then it was accompanied by an equally free use of Disulphate of Oxyd of Quininum, and often with good Extract of Conium maculatum. How far these contributed to counteract its ultimate and undesirable effects, I will leave my medical readers to judge. Of one thing I am sure, viz. that they greatly promoted its remedial effects. I value Iodine in Struma; but if Iodine were to be left to me and I were to be

deprived of the compounds of Quinum; or the compounds of Quinum were to be left to me and I were to be deprived of Iodine, I should very greatly prefer Quinum so far as the successful treatment of Struma is concerned. However, Iodine does not benefit Struma by its Neuragic power, but by a power widely different and far more important.

NEURAGICA ORÆSTHETICA.

The two articles which follow are neuragic and orethetic merely, without any additional power.

AMMONII PROTOSULPHIDUM $H.^4 N.^1 + S.^1$

{ AMMONII PROTOSULPHIDI DISULPHIHYDRIS.
 { *In Water it is the Fuming Liquor of Boyle.*
 $2 (H.^4 N.^1) + H.^1 S.^1$

These two articles have but two powers, viz. neuragic and orethetic ones; but the former predominates so much over the latter, that they can not be used for the effects of the latter, where the effects of the former are contraïndicated, or even not indicated.

I do not think that any of the articles belonging to this second great division of the Neuragics, ought ever to be employed for the purpose of producing Neuragy merely, but being in constant and protracted use for other purposes, they very often produce very considerable Neuragy. Some times, this aids the other power, for which the article is employed, in its remedial effects, and thus may be considered as truly indicated; at other times, their Neuragic operation is positively injurious, and then they hinder the remedial effects of the power for which the article is used. At all events, it is important always to know when Neuragic effects are to be expected from any article administered for the effects of some other power; and as the most effectual way of inculcating this, I have judged it expedient to enumerate the articles which follow, in this class, since they are all absolutely Neuragic, though (as I have already said) it is rarely if ever expedient to use them as Neuragics merely.

The Orethetic Neuragics may very properly be subdivided into

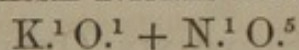
1. Antiphlogistica vera Neuragiam producentia.
2. Erethistica, Adenagica, Tonica, Neuragiam producentia.

3. Adenagica vera Neuragiam producentia.
4. Adenagica, Subantiphlogistica, Neuragiam producentia.
5. Adenagica, Narcotica, Neuragiam producentia.
6. Adenagica, Narcotica, Subantiphlogistica, Neuragiam producentia.
7. Adenagica, Tonica, Neuragiam producentia.
8. Adenagica, Tonica, Subemetica, Subcathartica, Neuragiam producentia.
9. Adenagica, Tonica, Subemetica, Subcathartica, Subantiphlogistica, Neuragiam producentia.
10. Adenagica, Subcathartica, Neuragiam producentia.
11. Adenagica Cathartica, Neuragiam producentia.
12. Adenagica, Emetica, Cathartica, Neuragiam producentia.

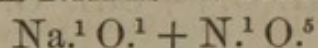
This subdivision, it must be particularly observed, is founded upon the number and sort of different and distinct powers possessed by each individual article. This is a sort of knowledge indispensable to judicious prescription; and though such an arrangement makes a large number of groups, yet this is unimportant in comparison with the information which it exhibits.

1. Antiphlogistica vera Neuragiam producentia.

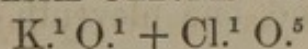
POTASSÆ NITRAS ANHYDRUS*



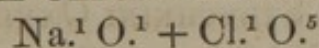
SODÆ NITRAS ANHYDRUS



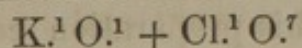
POTASSÆ CHLORIS ANHYDRUS



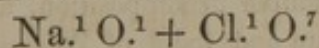
SODÆ CHLORIS ANHYDRUS



POTASSÆ CHLORAS ANHYDRUS

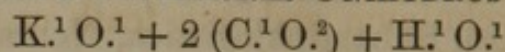


SODÆ CHLORAS ANHYDRUS

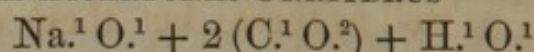


* Although I attempt to give the number of equivalents of Water contained in each Salt, yet this is often variable in conformity with variations in the manner of preparing the Salt, and particularly in conformity with the manner of managing it, during the process of crystallization. For use as medicine it is very immaterial whether a Salt contains a few more or less equivalents of Water; but for the prepa-

POTASSÆ BICARBONAS UNIHYDRUS



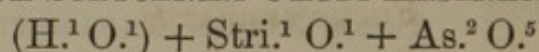
SODÆ BICARBONAS UNIHYDRUS



This group of articles possesses a considerable amount of Neuragic power, and it is therefore capable of producing decided Neuragic effects; but they ought never to be used for this purpose, on account of their Antiphlogistic or Exhausting power. Many Neuragics not recognized as such, are used for their Neuragic operation, under some other name, as Antispastic for example, though this supposed operation is commonly called Antispasmodic by mistake, though this latter term means some thing quite different. But this group of articles is not even used in this way, I suppose from the fact that too much exhaustion would result. The question may arise here why I enumerate these articles at all under the Neuragics? I answer because when used for widely different purposes, it is often the fact that considerable Neuragy results, so that it becomes important that their Neuragic power should be thoroughly known and recognized. It is probable that their Neuragic effects are useful along with their Antiphlogistic operation, when this is truly indicated.

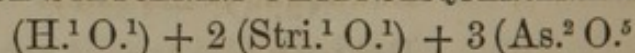
2. Erethistica, Adenagica, Tonica, Neuragiam producentia.

AQUA STRYCHNINI OXYDI ARSENATIS



About ten minims of this preparation contain one eighth of a grain of Strychninum and one eleventh of a grain of Arsenic Acid.

AQUA STRYCHNINI OXYDI SESQUARSENATIS



About ten minims of this preparation contain one eighth of a

ration of new compounds, by double decomposition, as of Iodid of Quininum from Disulphate of Oxyd of Quininum and Iodid of Potassium, it becomes important to know whether the former contains six or eight equivalents of Water. This can generally be decided (if it is not recollected) by observation of the state and form of the Salt, and by reference to some competent authority containing good accounts of all the Salts. But then such authorities must be possessed. Even the mere elementary works contain tolerably good accounts of a considerable number of the most common Salts.

grain of Strychninum and one tenth or eleventh of a grain of Arsenic Acid. Several other analogous compounds might be enumerated, as well as these; but these are sufficient for illustration.

3. *Adenagica vera Neuragiam producentia.*

IODINUM ELEMENTARIUM.

BROMINUM ELEMENTARIUM.

CHLORINUM ELEMENTARIUM.

I will here mention a few Fucaceæ and Ceramiaceæ that belong to this group, by virtue of the Iodine which they contain. I shall specify only a few species, that I happen to remember at this time, though in all probability they may not be the species that contain the greatest quantity of this element, and they are very far from being the whole number of species that contain it. A fuller catalogue will be given in the proëm to the Adenagics, or at all events under Iodine, when I come to the consideration of that article individually.

FUCACEÆ.

FUCEÆ.

FUCUS CERANOÏDES.

FUCUS NODOSUS.

FUCUS SERRATUS.

FUCUS VESICULOSUS.

SARGASSUM BACCIFERUM.

HALYSEREÆ.

ECKLONIA BUCCINALIS.

This contains more Iodine than the European species.

LAMINARIA.

CERAMIACEÆ.

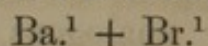
SPHEROCOCCEÆ.

PLOCARIA HELMINTHOCORTON.

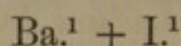
BARYTI PROTOCHLORIDUM.

Ba.¹ + Cl.¹

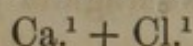
BARYTI PROTOBROMIDUM.



BARYTI PROTIODIDUM.



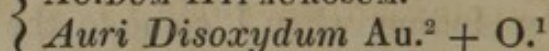
CALCII PROTOCHLORIDUM.



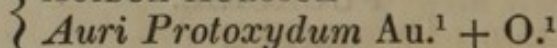
? AURUM ELEMENTARIUM.

Elementary Gold, in a state of very minute division, I suppose, procured by precipitation in a reguline form from a solution, is said to be active. As I have never verified this myself, as such a preparation, so far as I have knowledge, has been employed by only a few practitioners of medicine, and therefore there is a possibility of error, since every thing in medicine requires numerous trials to establish it, I put it down with a note of interrogation.

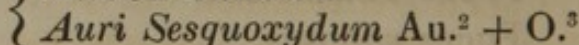
{ ACIDUM HYPAUROSUM.



{ ACIDUM AUROSUM



{ ACIDUM AURICUM



If the acidifying element is directly recognized in the names of these Acids, they will stand as follows, viz.

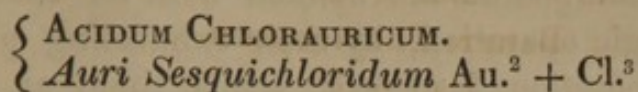
ACIDUM HYPOXAUROSUM.

ACIDUM OXAUROSUM.

ACIDUM OXAURICUM.

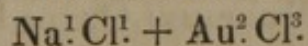
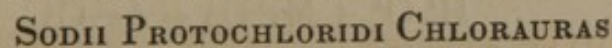
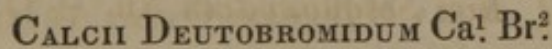
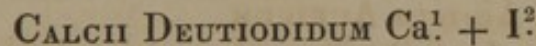
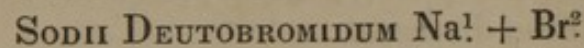
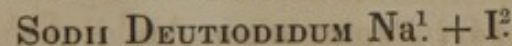
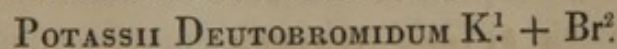
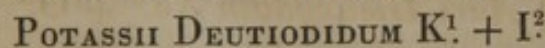
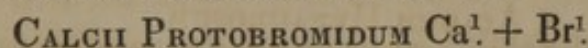
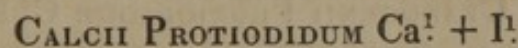
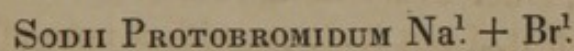
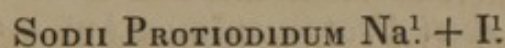
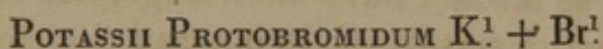
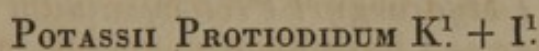
As far as I know, I have given these compounds the name of Acids for the first time; but they ought to have had these appellations long ago, since most probably, they are never basic, but always salifying. Even in the well known compound of one equivalent of Chlorid of Hydrogen (so called when a salifiable base, but Chlorohydric and Muriatic Acid when a salifying compound) and one equivalent of Sesquoxyd of Gold, the former is the electro-positive, and the latter the electro-negative proximate principle of the Salt, so that it is an Aurate or Oxaurate of Chlo-

rid of Hydrogen, and not a Chlorohydrate of Sesquoxyd of Gold, or as it is usually called, a Muriate of Gold.



This group is never used, except for its Adenagic operation; and yet I am inclined to think that the Neuragy which, under suitable circumstances, is produced, contributes more or less to the remedial effects.

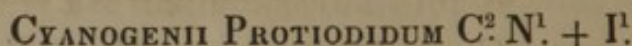
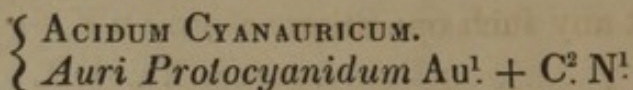
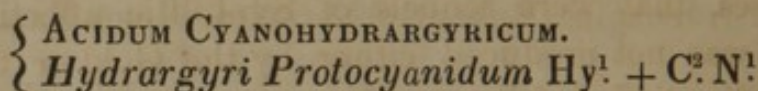
4. Adenagica, Subantiphlogistica, Neuragiam producentia.



I put down these articles as Neuragics, because they possess this power in as high a degree as they possess any power, though I do not think that it is ever expedient to employ them for their Neuragic effects, on account of the degree of exhausting power, which they always exert, when employed at all. Exhaustion, even in the most trifling degree, is never indicated along with Neuragy, though perhaps a moderate degree of it may be tolerated in some cases where Neuragy is required. Still, even in these cases, it is better to avoid the exhaustion if practicable, which it certainly is, since we have more powerful Neuragics without any exhausting power. These articles, I believe are always employed as Adenagics by all those who employ them internally. But their ex-

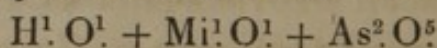
hausting effects are probably as much contraïndicated along with Adenagy, as along with Neuragy, and perhaps even more. In truth these compounds are spoiled both for their Neuragic and their Adenagic operation, by their exhausting power; and their Adenagic operation, I suspect, spoils them as Antiphlogistics. But I shall insist upon and enforce more fully the inexpediency of using these compounds as Adenagics, in my proëm to that class, where the consideration of this subject will be more appropriate.

5. Adenagica, Narcotica, Neuragiam producentia.

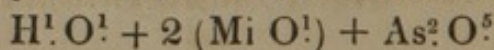


The first compound is particularly recommended by Pourche, as much better adapted to the cure of Struma and Syphilis than the Sesquichlorid of Gold, otherwise more properly called Acidum Chlorauricum. Its Cyanogen is supposed to modify its operation favorably for these diseases.

AQUA MORPHINI OXYDI ARSENATIS.



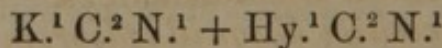
AQUA MORPHINI OXYDI DISARSENATIS



About ten minims of the former contains an eighth of a grain of Oxyd of Morphinum, and one twentieth of a grain of Arsenic Acid. About ten minims of the latter contains one eighth of a grain of Oxyd of Morphinum, and one fortieth of a grain of Arsenic Acid.

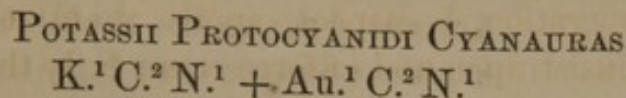
6. Adenagica, Narcotica, Subantiphlogistica, Neuragiam producentia.

POTASSII PROTOCYANIDI CYANOHYDRARGYRAS



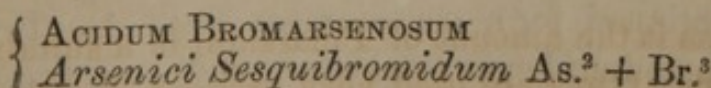
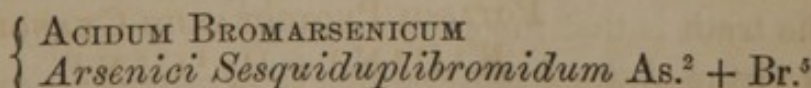
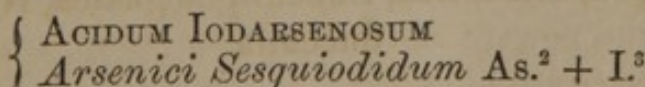
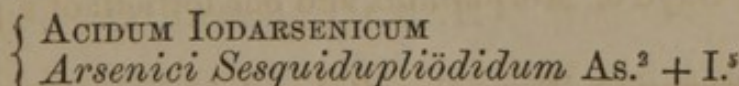
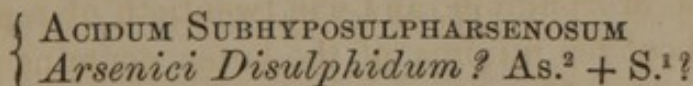
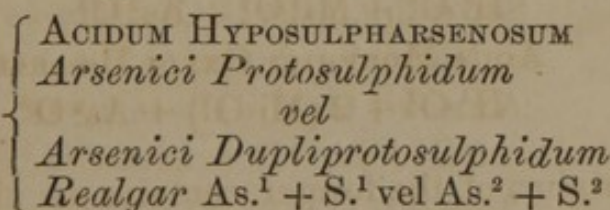
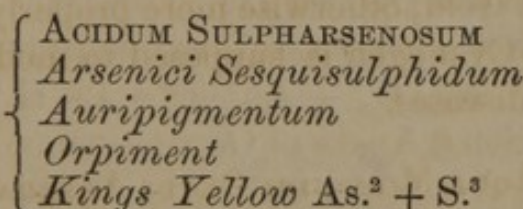
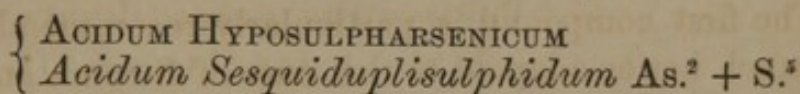
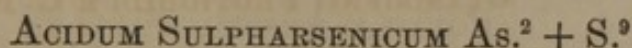
Small as is the amount of Potassium in a suitable dose of these compounds, if they are used very protractedly, they will certainly

impair the tone of the stomach. But perhaps the mercury in the former has some instrumentality in the production of this effect, as well as the Potassium.

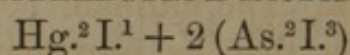
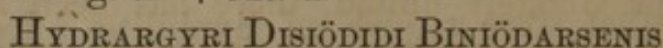
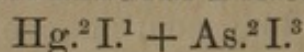
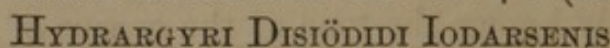
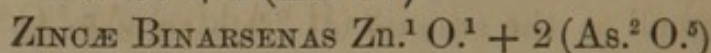
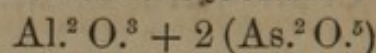
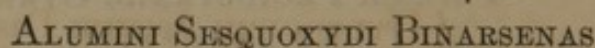
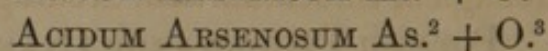
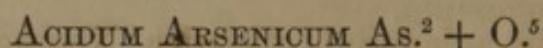


As small as the amount of Potassium is in this compound, I have certainly known the tone of the organs of primary digestion decidedly diminished by a long continuance of it for cutaneous diseases, that were sequels of Syphilitic affections. That this effect was not produced either by the Gold or the Cyanogen, I am well satisfied, because I have often used both, and that a long time, without any such operation.

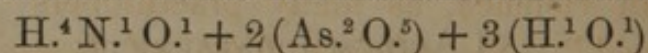
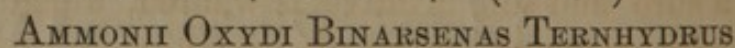
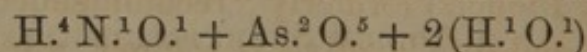
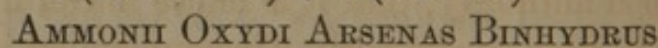
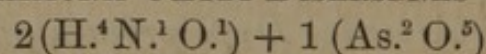
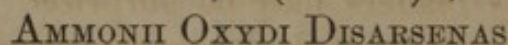
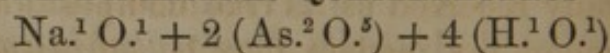
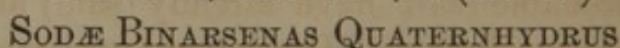
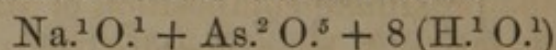
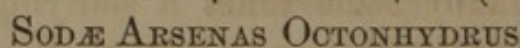
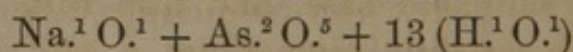
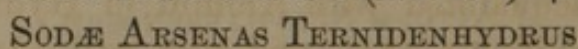
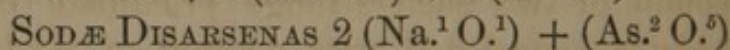
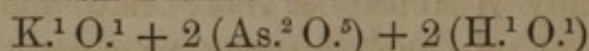
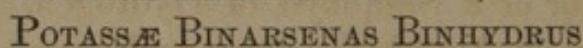
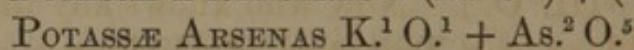
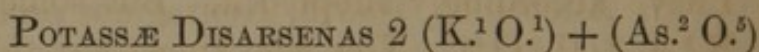
7. Adenagica, Tonica, Neuragiam producentia.



8. Adenagica, Tonica, Subemetica, Subcathartica, Neuragiam producentia.



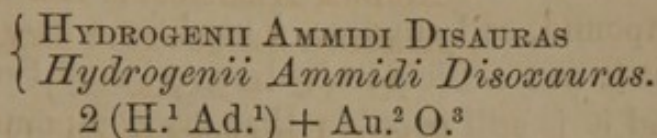
9. Adenagica, Tonica, Subemetica, Subcathartica, Subantiphlogistica, Neuragiam producentia.



At first view there may seem to be a contradiction or inconsistency in the powers ascribed to this group, since it is said to be Tonic and Subantiphlogistic, though not both at the same time. The truth is that the early operation of these compounds is Tonic, by virtue of the Arsenic which they contain; but when they have been continued for a considerable time, the Potassium, Sodium and Ammonium begin to exhaust, and soon transcend and super-

sede the Tonic effects that were produced before the last three principles began to exhaust.

10. Adenagica, Subcathartica, Neuragiam producentia.

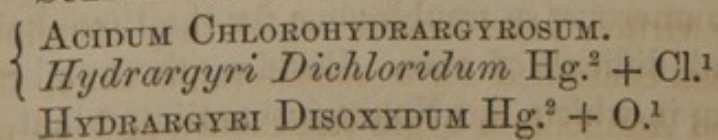


In regard to the manner in which the elements of this compound are proximately united, there has been a diversity of opinion among good chimists. The ultimate composition is $\text{H}^6 \text{Au}^2 \text{N}^1 \text{O}^3$. Turner supposes that its proximate composition is $2 (\text{H}^1 \text{Ad}^1) + \text{Au}^2 \text{O}^3$ i. e. a Disaurate or Disoxaurate of Ammid of Hydrogen. This is certainly its most probable composition, and I have accordingly adopted it. But some suppose it to be $\text{Au}^2 \text{N}^2 + 3 (\text{H}^2 \text{O}^3)$. Now who knows any thing of a Duplinitrogid of Gold, or of a Sesquoxyd of Hydrogen, provided Hydrogen is atomically reckoned as *one*; and much less of such a compound of two such strange proximate principles? But every body is acquainted with Ammid of Hydrogen, which is some times a salifiable base, and also with Auric or Oxauric Acid, which is certainly some times a salifying principle. The first view of the proximate composition of this Salt is altogether the most probable (as I have already said) and I believe it is now generally received as its true composition.

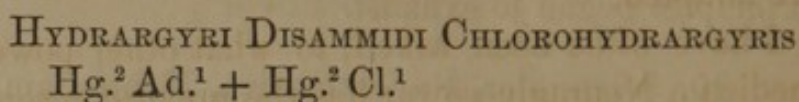
Some of my readers may be surprised to find this compound specified as an article of the materia medica; but we are informed on good authority, that it is in use in Germany. A medical writer (M. A. Plenciz) formerly "recommended it, in every case where a sure and safe laxative is wanted, alleging that it does not act with violence, as many practitioners" (of medicine) "have asserted." (*See Aikin's Edit. of Lewis's Mat. Med. 4th Edit. Lond. 1791, Pg. 189, Note at foot of page, et alibi.*)

11. Adenagica, Cathartica, Neuragiam producentia.

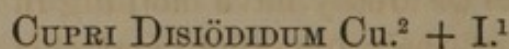
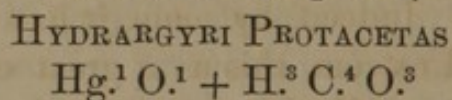
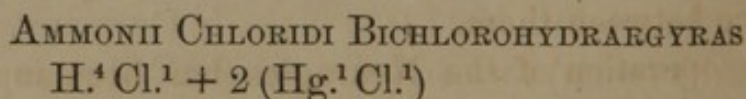
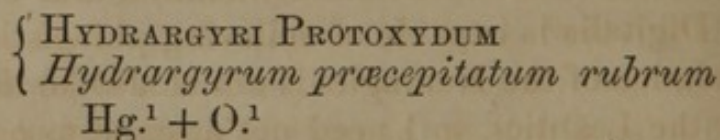
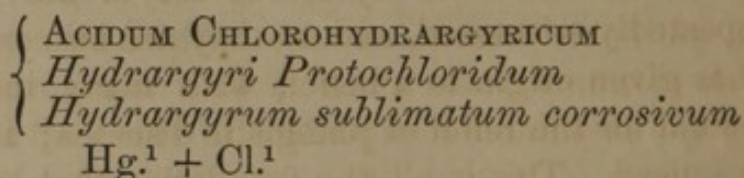
SULPHUR ELEMENTARIUM.



12. Adenagica, Emetic, Cathartica, Neuragiam producentia.



This compound, as I suppose, I used for a long time, under the opinion that it was a Bichlorohydrargyrite of Protammid of Hydrogen; and it is still possible that my compound was different from that which I have just specified. The first specimen of this Salt that I ever had, was sent to me by a physician, as Disoxyd of Mercury, prepared by boiling together, without regard to quantities, Dichlorid of Mercury and Sesquicarbonate of Oxyd of Ammonium in Water. It was considered as much better Disoxyd of Mercury than the common form, because it was very much more active. I perceived at once, that it could not be what was supposed; and after preparing it several times myself, and with definite quantities of the materials, I at last conjectured that it was probably Bichlorohydrargyrite of what is now called Ammid of Hydrogen. Subsequently Kane's researches changed my views of its composition.



This is a peculiarly difficult class for investigation, since it produces but one grade of medicinal effect, and this attended with very few symptoms, and since the ultimate grades of its operation are so numerous, so troublesome for the time being, and so obstinate and difficult of cure. Besides, the number and variety of diseases, in which the Neuragics are important, are much fewer

and much less diversified, than those to which several other classes are adapted.

I have often been asked, to what other power in the *materia medica* a Neuragic power has the greatest resemblance? This is a question difficult to be answered. A medicinal grade of a Neuragic operation is sedative, but its sedation is peculiar, and unlike that of any other power. An Antiphlogistic operation is direct sedation of the morbid irritability, and irritation, the morbid mobility, restlessness and jactitation, and the morbid wakefulness of the phlogistic diathesis, and of this alone. It has been supposed by some that the Neuragics are capable of subduing the phlogistic diathesis, and of course are substitutes for the Antiphlogistics. This, I am persuaded, is a great error. The supposed phlogistic diseases, that are relievable by the Neuragics, are Fevers of the Synochous or subputrid type, the most moderate grade of *Tuphus putridus* of old authors. In consequence of the very considerable heat attendant upon such cases, they are often mistaken for phlogistic diseases, though in reality, they have not one single diagnostic symptom of true phlogistic diathesis. These cases are relievable by a free use of the Neuragics, as I have repeatedly witnessed, and so they are by certain Narcotics, which has given origin to a belief, with many, that these agents are effectual for the relief of phlogistic diseases; a very erroneous opinion indeed. This is all the foundation that exists for the belief that *Digitalis* is capable of relieving phlogistic diseases.

The sedation of the Neuragics is not at all like that of the Nauseatics or the Leäntics, so I need not attempt to point-out any difference between them.

The operation of the Neuragics has been supposed to be like that of the Narcotics. Indeed, by some, it has been supposed to be identical with it. I esteem this as a great error. The Neuragics and Narcotics assuredly have nothing in common, except the sedative grade of their operation, and this varies widely in quality. The sedation of the Neuragics is never either Anodyne or Soporific; that of the Narcotics is always more or less both Anodyne and soporific. Vertigo is an occasional, but not essential symptom of ultimate Neuragy, and ultimate Narcosis, and so are Convulsions of some sort. The Narcotics never produce either Neuralgia or Paralysis in any grade of their operation, while these are very uniformly symptoms of ultimate Neuragy.

Very many of the Neuragics possess other different and distinct powers, for which they are commonly prescribed, and which acquire all the credit of the remedial service, which they render, while this power, not being a recognized one, gets no credit at all, though it often produces all the benefit. Hence it is very ill understood how far this power really contributes to the remedial efficacy of most of the articles, which possess it in conjunction with some other power or powers. Even in perfectly pure Neuragics, in consequence of this power's being called by the name of some other different and distinct power, the article possessing it is misapplied in therapeutics, so that its real value is not ascertained. Thus Lead is prescribed as a Styptic merely, though none of its compounds, that I now think of, are truly Styptic, except the several Acetates. Now the cases which require Styptics are not those which are the most benefited by Neuragics. Zinc is commonly prescribed as a Styptic, a Tonic and an Emetic. Now many of the compounds of Zinc are not at all Styptic. I think that none of them are at all Tonic, and but a few of them can be used advantageously as Emetics. But the cases in which Styptics, Tonics and Emetics are indicated, are not those in which the Neuragics are the most useful. Chlorid of Barytum is probably always prescribed for its Adenagic operation, though this is very inadequately understood by the profession at large; but at all events, its Neuragic power being wholly unrecognized, never receives either credit or blame, for any good or evil that it may produce. Arsenous Acid is reckoned Tonic by the great body of the medical profession, while its Adenagic operation is had reference-to, under the vague and loose denomination of Alterative. All its therapeutic effects are therefore ascribed to these two powers, and none of them to its Neuragic power, because it is utterly unknown. Iodine and Bromine are recognized only as Adenagics under the denomination of Alteratives; and though nearly, if not quite the whole of their ultimate effects is produced by their Neuragic operation, yet this power being unrecognized, it gets neither credit nor blame. Now a thorough knowledge of the operation and effects of a Neuragic power, could not possibly fail of teaching how to obtain the effects that we desire; and how to avoid those that we do not desire; and above all, it would teach when to select an Adenagic intirely destitute of any degree of Neuragic power; and when this power may be a useful auxiliary.

Therapeutic Applications of the Neuragics.

As this is a new class in the materia medica, founded upon a power that has never before been defined (at least so far as I know) it becomes almost necessary that I should say some thing of its therapeutic applications. It must be remembered that this work professes to treat of therapeutics, as well as of materia medica or pharmacology, so that a few specifications of the therapeutic applications of a newly recognized power, can not be much out of place in it. As appears to me, the pathological conditions of the nervous system which the Neuragics seem well adapted to relieve are 1. Irritation 2. Hyperæsthesia 3. Erethismus 4. Spasmus.

Ecphronia Aphrosyne (Mihi) var. furibunda? As perhaps there is both Irritation and Erethism in some of the varieties of Ecphronia Aphrosyne, a free use of the most efficient Neuragics such as Protocarbonate of Lead, for example, might possibly be useful; but I have never heard of their being tried in any form of this disease, so that what I suggest is founded only on conjecture. But admitting that they are capable of rendering more or less service, it would still be a matter of inquiry [whether other remedies might not be much more eligible. In some of the varieties of Ecphronia Aphrosyne as Melancholia, for example (for I consider it as a mere variety of the species of Insanity just particularized) I should apprehend that they would be much more likely to aggravate, than relieve.

Paroniria Salax (Good.) In this disease (resulting from, and dependent upon, a conjunction of an exquisitely susceptible or irritable temperament and strong venereal or sexual appeties, an affection, as I think, rightly associated by Dr. Good, with Somniloquy and Somnambulation, and hardly a disease) I should think that a continuous use of the Neuragics might cure. This I state without pretending to decide whether such a course would constitute the best remedy or not. Here there seems to be Hyperæsthesia, perhaps Irritation, and doubtless Erethismus. I should think that a continuous use of the Neuragics would obviate the diathesis on which this affection depends. But we are not in possession of facts upon this subject; and how should we be, in regard to an unrecognized power. Impotence is certainly one of

the symptoms of ultimate Neuragy, which proves the opinion that the Neuragics may be remedial of *Paroniria salax* (Good.)

Tenesmus intestinorum (Mihi.) I should think that this disease, whether idiopathic or symptomatic, might be relieved by Neuragics; and I think I have accomplished this, in cases symptomatic of Dysentery. Certain preparations of Lead and Zinc are the Neuragics which I have employed. I consider it necessary that they should be taken into the stomach, in order to produce much effect in this difficulty, since (as I doubt not) *Tenesmus intestinorum* has its seat above the valve of the colon.

Tenesmus vesicæ urinariæ (Mihi.) In idiopathic cases of this disease, without any tendency of the bladder to Paresis or Paralysis, or perhaps Acinesia, I should think that Neuragics might be useful; but I am not quite sure that I ever met with any such cases, since all that I have ever known, after the occurrence of a greater or less number of paroxysms, have passed into vesical retention of urine requiring the use of a Catheter, though there was nothing of the kind in connexion with the earliest paroxysms. From this fact, I infer that it is a regular tendency of *Tenesmus vesicæ urinariæ* to pass into Paresis or Paralysis, or perhaps Acinesia. I think we should naturally suppose that such a painful and harrassing affection of the muscular fibers of the bladder would impair their tone, and ultimately occasion Paralysis or Acinesia. I question the expediency of employing the Neuragics, since the production of Paralysis or at least Acinesia, is one of their prominent tendencies. I can not but think that I have met with some cases of this disease, that were unequivocally retrograde or metastatic Rheumatalgia. I think likewise that I have seen other cases, which were decidedly aberrant, or primarily misplaced Rheumatalgia. In all such cases (if I am not in error in regard to their nature and character) I should consider the Neuragics as contraïndicated, because Rheumatalgia inclines to pass into Paralysis or Acinesia, and I should apprehend that the tendency of the Neuragics and of the Rheumatalgiæ would coïncide and do mischief. After all I am not quite sure that the treatment of *Tenesmus* of any species or variety, with Neuragics is altogether judicious, since this disease approaches too near one or the other of the varieties or species of Neuralgia; and Neuralgia I should fear, might be exaggerated by much use of the Neuragics.

There has been more written upon the efficacy of the Neuragics in Spastic diseases than in any other; and for their supposed power over Spasm, I suppose of all sorts, many of them have been called Antispastics. But it has always appeared to me that the evidence of such power is more or less deficient. And yet a Neuragic power operates mainly upon the nervous system, and as we should suppose, ought to possess power over some sorts of Spasm. I shall therefore consider more particularly what Spastic diseases they are most likely to be capable of benefiting. There are certainly more than two sorts of Spasm, viz. Clonic and Tonic, of the former of which, *Epilepsia* furnishes the type, and of the latter, *Tetanus*. *Oscitation* and *Extension*, and also *Sternutation* I will not for the present reckon as Spasm; though there is reason for controversy on this point. *Pedesis Subsultus*, *Pedesis Choreoides*; and *Pedesis Singultus*, are undoubtedly Spasm, though they are neither Clonic nor Tonic. If *Bex* is Spasm (which seems to be universally admitted, but which may be strongly controverted) it must constitute still an other sort, making in the whole, four at the least, viz. that of *Pedesis*; that of *Bex*; that of *Epilepsia*; and that of *Tetanus*. The first two may then be called 1. Pedetic 2. Bechic; as the second two are called 3. Clonic 4. Tonic.

Pedesis Subsultus (Mihi) var. chronicus. I have seen an idiopathic chronic *Pedesis Subsultus* more than once, I now do not recollect how many times; but I was never asked but once to prescribe for it, and then I was to see the case but a single time, and was to have no physician in attendance to manage my medicines. Under such circumstances I declined prescribing. I have heard of cases of this disease, several times. It was always called irregular or abnormal *Chorea*, and had always been treated as such, but so far as I could learn, without a cure. As I think this is a truly Spastic disease, though of a very peculiar character, why might it not be curable under the use and by means of Neuragics? But I never had any knowledge of their employment in it, though they are alleged to have cured other Spastic diseases.

Pedesis Choreoides (Mihi). As this disease is a nondescript, so far as printed authorities within my knowledge are concerned, it is necessary to give here the most brief account of it, that will possibly serve to make the name intelligible. What I am in the habit of calling *Pedesis Choreoides* is a very singular affection.

The first manifestations of it are a sudden and involuntary jerk, very decidedly Spastic, of the foot and leg forward, as in the act of kicking some thing, or a similar jerk of the elbow upward and outward, as in the act of elbowing some body out of the way. The patient has no consciousness that such an act is about to take place, till it is actually performed, so that he has no opportunity to exercise his will, in its restraint. At first, the motions are few and far between, but they gradually become more frequent, till they are finally performed by the muscles moving the head, and the individual muscles of the face. Finally even some of the muscles of the trunk are affected. For a considerable time these motions do not prevent walking-about, and riding on horseback. The bystanders at first suppose that the subject of this disease has fallen into awkward and queer tricks, which have become confirmed by long habit; but as the malady progresses, it becomes obvious to every body, that what have been considered as odd motions merely, are truly and wholly disease. These motions at last increase to such a degree, that the patient can neither walk-about, nor ride on horseback. At last the whole voluntary muscular system is in a constant jerking motion the whole time, and to a greater extent when the patient is asleep, than when he is awake. The disease begins at an earlier or later period of life, in different subjects; its progress is more or less rapid; and its final intensity is less or greater, in different cases. But all these bodily symptoms are only a part of the disease. I believe that it is always constitutional and hereditary in particular families; and those who are to be its future subjects, if not carried-off prematurely by some acute disease, or by some casualty, always have great strength or vigor of intellect, with many and prominent peculiarities or eccentricities. This superiority and peculiarity or eccentricity of intellect increases with the disease, to a certain stage of it, when the powers of the mind begin to fail; and if the disease is intense enough (as it usually is) and if the patient survives long enough, he gradually becomes imbecile, and some times passes into a state of perfect Amentia. This malady in its involving the mind so deeply, seems to have some affinity with that disease which I have been in the habit of calling *Synclonus calculatorius* (the disease of Zerah Colburn, and several other persons that I have happened to know); and yet *Pedesis Choreoides*

is decidedly Spastic and not at all Paretic; while *Synclonus calculatorius* is as decidedly Paretic, and not at all Spastic. Both have their seat in the nerves of voluntary motion, and in the hemispheres of the cerebrum. The character of the motions in the two diseases is of an intirely different character. In *Pedesis Choreoïdes*, the motion is a sudden jerk, while in *Synclonus calculatorius* the motions are those of loss of power, and incapability of obedience to the will, being irregular and tremulous, and so long as any effort is made, as in sitting, standing or any greater exertion, they are perfectly continuous; though they cease in a passive state, as in a horizontal posture on the back, and especially in sleep. In short any exertion of the will aggravates this disease; while in *Pedesis Choreoïdes*, an exertion of the will diminishes the motions; and they are the greatest, when the subject is lieing passive on the back, and especially when he is asleep. And yet both these diseases are commonly confounded with *Chorea*, and so are several other maladies equally different and distinct, as *Sypasia Epilepsioides (Mihi)* and *Sypasia Choreoïdes (Mihi)*; but this is no place for the consideration of such topics. All the families, within my knowledge, that have been tainted with this disease, have gradually become extinct, since no body would intermarry with them. The Uniprotocarbonate of Lead I have known recommended as a remedy for this disease, under the notion that it is a powerful Antispastic. As this Salt has but one single power, viz. that of a Neuragic, if it cures disease at all, it must be by virtue of this power. But though I have known it recommended for this malady, I never knew it have a fair trial, and therefore am obliged to suspend my judgment of its efficacy, though I consider it quite certain that it is efficacious in some Spastic affections.

Pedesis Singultus (Mihi.) I have seen some very obstinate and dangerous idiopathic cases of this disease, which, I suppose, must be reckoned as Spastic. Whenever my patients could command a sufficient quantity of good Moschum, the *Singultus* has always been readily controllable, at least so long as the patients kept under its influence; and if they kept under it long enough, the disease has always been cured. But then the uniform adulteration of Moschum renders such as we have, precarious in its effects; and its high price almost occasions a prohibition of its use. If there-

fore, on trial, any of the Neuragics should be found capable of restraining such cases of *Singultus* as I have specified, it would be a happy discovery in therapeutics.

Bex theriödes (Auctorum.) Very little is known of the pathology of this disease. I should think that it might be considered as quite certain that there is some powerful irritation, either actual or potential, of the pulmonary par vagum, the involuntary motor nerve of expression, on which the act of coughing depends. Upon the hypothesis that this is the whole pathology of this malady, I should think that the Neuragics might be considered as indicated in it. I have known the Uniprotacetate of Lead prescribed in Whooping-Cough under the notion that this agent is Antispastic. Of course its Neuragic power must have been intended. As Narcotics operating more especially upon the involuntary nerves of expression were employed in conjunction with the Salt of Lead, it was impossible for me to determine whether any thing was accomplished by the latter. Admitting that the efficacy of the Neuragics is established in this disease, the practice just mentioned is certainly good.

I should think that the Neuragics must be positively bad in *Bex Dyspnoïca*, because, in that species of Cough, there would seem to be a Paresis of the pulmonary branch of the nerve of chymical action, nutrition, etc. for I believe that the Neuragics are liable to coincide-with and aggravate all Pareses.

The Neuragics are not supposed to be indicated in *Bex vulgaris*, because that species of Cough is not considered as being a Spastic disease. But if the act of coughing is ever a Spastic affection, it is always such. A particular motion or action can not be Spastic at one time, and Nonspastic at an other. A variation in intensity can not alter the essential nature and character of an action or motion. However I should have no expectations from the Neuragics in Common Cough; but I have never seen them employed in it.

I include under the genus *Syspasia* all those Spastic diseases which consist in Clonic Spasms, of precisely the same character as those of *Epilepsia*. This causes me to make *Epilepsia* the type of the genus *Syspasia*, and to exclude from it *Convulsio* or Common Convulsion and *Hysteria* or Hysterical Fit, which Good puts into it, but which are certainly Tonic Spasms though he calls

them Clonic. I include under *Entasia* all those Spastic diseases, which consist in Tonic Spasms. This leads me to make *Tetanus* the type of a genus, and to rank *Convulsio* or Common Convulsion, and *Hysteria* or Hysteric Fit under the genus *Entasia*, since they are most certainly Tonic Spasms, though Good very erroneously, calls them Clonic. I think however that it is Clonic Spasms, rather than Tonic ones, that the Neuragics have the greatest power over.

Syspasia Epilepsia (Good.) I have known Neuragics given with great effect in Epilepsy. I once had knowledge of a case of Epilepsy in a young man of twenty or twenty-five years of age, that had been of such long continuance, as to have greatly impaired his intellect, which was treated successfully with the Uniprotacetate of Lead. There was not only a total suspension of the Fits, but the powers of the mind seemed to be restored. In this case, the remedy was used alone, and was carried to such an extent as to produce a formidable *Colica* (*Enterodynia*) *Rachiälgia*. The physician (a distinguished man) judged that this was necessary for success; though I greatly doubt it. I could never satisfy myself that it is ever necessary to push medicine to the production of urgent disease, in order to obtain its remedial effects. At all events I have known several cases of Epilepsy cured by Uniprotacetate of Lead without the production of a single morbid symptom. It is my present belief that the Uniprotocarbonate of Lead would be much speedier, and much more effectual for the treatment of Epilepsy; but I have never verified this belief by actual trial. My judgment is founded on the observation of the speed and intensity of the morbid effects of this Salt. It appears to me to be far more active than any other Salt of Lead with whose effects I am acquainted. The Protoxyd of Zinc, a pure Neuragic, once had considerable reputation for the treatment of Epilepsy. I think that I have seen essential service rendered by it, and I have received much testimony to the same effect. It is rarely given with sufficient freedom, or continued any where near long enough. When I have supposed that it rendered the most service, it was not employed alone, but had other remedies, of established character in this disease, conjoined with it. Under such circumstances there is always room for fallacious conclusions as to what agent renders the most benefit, or what one effects the cure.

Syspasia Epilepsioïdes (Mihi.) I am familiar with a sort of Fit consisting of pure Clonic Spasms, much more prominent in one side than in the other, which is very clearly not Epilepsy, and which, I never knew even suspected of being such. This disease is mainly a nondescript so far as I am acquainted with books. It is commonly called *Chorea* (which seems to be a common sink for all nondescript diseases of the nervous function) though it is much less like *Chorea* than it is like Epilepsy. I have been in the habit of calling it *Syspasia Epilepsioïdes*. I can not here go into the symptomatology, diagnosis and pathology of it. It must suffice to state that it consists in Clonic Spasms as certainly as Epilepsia, and yet assuredly it is not that disease. Now I have commonly seen this malady treated with Protoxyd of Zinc (because that article is Nervine and Antiphlogistic) and Tonics; and every case that I have seen has recovered sooner or later. But how much was due to the Zinc, and how much to the Tonics, I never could decide, because I never saw them used separately. However, I think I have long had good ground for believing that Protoxyd of Zinc is capable of benefiting diseases attended with Clonic Spasms, or consisting in such essentially. But it will by no means relieve all cases of this sort.

Syspasia Choreoïdes (Mihi.) As the Neuragics are usually considered to be capable of rendering service in Spasm, would they not be useful in what I have been in the habit of calling *Syspasia Choreoïdes*, a disease, which, at first view might be, and actually is mistaken for *Chorea* of an inordinately violent character. It consists of Clonic? Spasms, of a peculiar character, of the voluntary muscular system which are mitigated by exertion, and which are at their worst when the patient is lying passive on the back. They seem to be produced by some urgent irritation at the origin of the nerves of voluntary motion in the anterior columns of the spinal cord. True *Synclonus Chorea* is not a Spastic disease but a Paretic one. The irregular motions take place when there is exertion, and are perfectly suspended when the patient lies passive on the back. They seem to be produced by want of power in the voluntary motor nervous system, which is probably at its origin in the anterior columns of the spinal cord, since the whole voluntary muscular system is involved. It is not an Irritation but a Paresis i. e. an imperfect or incomplete Paralysis. I

have never seen any Neuragic employed in this disease, so I know nothing of their effects upon it, from observation or experience.

Entasia Tetanus (Good.) I have seen reports in periodicals of supposed cases of Tetanus cured by Uniprotacetate of Lead. The details of the cases were not in general sufficient to prove that the disease was Tetanus, though the Spasms were undoubtedly truly Tonic. Hysteria counterfeits almost every thing, and I have known it counterfeit Tetanus very exactly, so as to deceive several physicians. I believe that the cases to which I refer, were considered as traumatic, but then a person may have a slight wound previous to a Hysteric Fit, without converting such Fit into Tetanus. The cases in question were supposed to have been cured by much too small quantities of the Uniprotacetate of Lead. I have seen ten times the quantity taken in Hemorrhage for the time being. But the reporter of the cases seemed to entertain the opinion that the Salt employed was a most tremendous poison in exceedingly small quantities. On the whole the reported cases did not appear to me to prove any thing. Had the medicine been employed in much larger quantities, there would have been better ground for believing that the cases were cured by it, and that they might have been true Tetanus.

Entasia Lyssa (Good.) I knew of a report, in a periodical, of one case of Lyssa or Rabies, supposed to have been cured by Uniprotacetate of Lead. If the case was true Lyssa or Rabies, it was certainly a very mild one; and according to my judgment, too little of the medicine was employed to cure any serious disease. But the same notion heretofore mentioned, was entertained by the reporter of this case, viz. that the Salt used was a tremendous poison, even in quite small quantities. We can not decide that a sufficient quantity of the Uniprotocarbonate of Lead may not accomplish some thing in this disease, till we have known it fairly and thoroughly tried; but it is certain that Lyssa or Rabies is not an easily curable disease. For myself, I should repose far more a priori confidence in *Strychnos toxifera*, *Rouhamon Gujanense* or *Rouhamon*? *Curare*, for the treatment of this disease, than in any preparation of Lead. However, if it should be ever established that any compound of Lead exerts any favorable influence over this disease, I would conjoin it with those agents, which, according to my present knowledge, seem to promise more.

Entasia Convulsio (Mihi.) As *Entasia Convulsio* or Common Convulsion usually consists of a single paroxysm of Tonic Spasms, it is generally ended before a dose of any Neuragic, however large it may be, can be expected to operate. If Convulsive or Spastic diathesis happens to exist, and the patient is subject to frequent attacks, such diathesis may possibly be capable of obviation by a regular and more or less protracted use of some of the most efficient of the Neuragics; but, I am in possession of no facts upon this subject, as it is an unexplored field, so far as my knowledge extends.

Entasia Hysteria (Mihi.) Very much the same remarks may be made in regard to the use of the Neuragics in *Hysteria* or Hysterical Fit, as have been made in regard to their use in *Convulsio* or Common Convulsion. They promise more for the obviation of Hysterical diathesis, than for the relief of a paroxysm or Fit so called; but still I am in possession of no facts upon the subject, that are founded on observation and experience.

There are several species of disease belonging to the genus *Entasia*, which Good does not describe there; and he describes several there, which belong elsewhere. I need not mention the former at present, since they are all milder affections than the *Entasiæ* that I have specified; so that if those that I have specified are relievable by the Neuragics, the milder ones will doubtless be so too. I do not think that any Paretic or Paralytic disease; any Acinetetic one; any species of *Synclonus* (for I consider all *Syncloni* as Paretic;) any Neuralgic affection, whether true, spurious or Rheumatalgic; nor any disease connected with the hemispheres of the cerebrum (except those which I have specified) as at all likely to be benefited by a Neuragic power operation or effect.

Synclonus Chorea (Good.) Although I have just mentioned the several species of *Synclonus* as not at all likely to be benefited by any pure Neuragic; yet a few words more seem to be required upon *Synclonus Chorea*, about which several mistakes seem to have been made. In the first place this disease has been groundlessly assumed to be Spastic, and nearly allied to Epilepsy, and therefore all the remedies proper for one of these diseases, have been supposed to be equally proper in the other. But in fact, Epilepsy is truly and unequivocally a Spastic disease, and

Chorea is with absolute certainty purely Paretic. Certain Neuragics have an established character in Epilepsy, and hence have been employed in *Chorea*, not only without benefit, but with positive injury, as Protoxyd of Zinc for example. On the other hand, certain Oresthetics have an established character in *Chorea*, and hence have been employed in Epilepsy, at least without any benefit, and some times with injury. The Protonitrate of Silver is one of the remedies that have been thus misapplied by being transferred from *Chorea* to Epilepsy.

Phlogotica externa.

I believe that the topical application of Aqueous Solutions of certain Styptic Neuragics to external Phlogoses is very nearly if not quite universal practice. Those who employ this measure have indeed entertained quite a diversity of opinions as to the manner in which it renders benefit. I have conversed with physicians who maintained that the Aqueous Solutions of these Salts are Antiphlogistic, and that all Phlogoses are necessarily phlogistic, and therefore that the whole benefit resulted from Antiphlogistication. For myself I do not by any means consider all Phlogoses as phlogistic, nor do I consider these Salts as at all Antiphlogistic. I have conversed with physicians who were well apprised that some Phlogoses are entonic and some atonic, but who considered these Lotions as Antiphlogistic and adapted only to entonic cases. Again I have conversed with others who recognized both entonic and atonic Phlogoses, but considered these Lotions as Stimulant, and adapted only to atonic Phlogoses. I have also conversed with those who considered these Lotions as operating solely by their Styptic power, and therefore as equally adapted to entonic and atonic cases. All this diversity of opinion sufficiently shows that there is no unanimity among the members of the profession on this subject, and that, of course there is error some where, for the correction of which there is still room. My own researches have led me to the conclusion that the beneficial effects of these Salts as topical applications in the external Phlogoses depend, to a very considerable extent, upon their Neuragic power, and that so far they are equally adapted to entonic and atonic cases. No doubt however, their Styptic power mate-

rially assists their discutient operation. Now I do not think that the Styptic operation of articles of chymical-inorganic origin, is at all contraïndicated by phlogistic diathesis, whatever may be the fact with the Styptic operation of articles of vegetable-organic origin. I do not believe however that pure Tannic Acid in any quantity would aggravate phlogistic diathesis. But I do not come to this conclusion from having tried it in such cases, but from the observation of its effects in a great variety of other cases.

Phyma Furunculus var. Phlegmonodes-non-malignus. I have very often seen Aqueous solutions of the Protacetates of Lead, of Protacetate and Protosulphate of Zinc, of Chlorid of Ammonium, etc. applied with decided advantage in this Phlogosis. For aught that I could discover, the Chlorid of Ammonium rendered as much service as the Salts previously mentioned. As this is not at all Styptic, it must have benefited intirely by its Neuragic power.

Phlegmone communis and *Phlegmone Bubo.* Aqueous solutions of these Neuragic articles applied sufficiently early will often completely discuss *Phlegmone communis* and also *Nonsyphilitic Phlegmone Bubo.* They will often palliate *Syphilitic Phlegmone Bubo.* If first employed later than a certain stage of these diseases, they usually render no service.

Mastitis Typhodes-Phlegmonea. I mention this species, because it is the most common at the present period, and is therefore that for which prescription is oftenest made. I am acquainted with at least three other species, to two of which (I imagine) Neuragics might be about equally well adapted. That a Neuragic power contributes to abate Phlegmone Phlogosis is evident from the effect of Natural Protocarbonate of Zinc when topically applied in *Mastitis.* I have received a great deal of testimony from a considerable number of very judicious practitioners, to the efficacy of this Salt in this disease, applied in the form of Liniment or Ointment. I have also prescribed it myself, and as I thought with the most decided advantage. I have no knowledge that this article possesses any power but a Neuragic one. If we consider it as established that by topical application pure Neuragics are capable of abating and relieving Phlogosis, we shall be led to conclude that the benefit rendered by Aqueous Solutions of the Protacetates of Lead, the Protacetate and the Protosulphate of

Zinc, etc. is not all due to their Stypticity but is in part owing to their Neuragic power. I consider it not a little singular that all the physicians of my acquaintance who have been in the habit of applying the Natural Carbonate of Zinc in *Mastitis* did not, so far as I could ascertain, employ it in any other Phlogosis. I never could discover why it should not be as useful in any other Phlogosis, as in *Mastitis*.

Acrobystitis Phlegmonea-acuta. This species is some times intirely cured by these agents. Such would not be the fact with *Acrobystitis Syphilitica*. For this other agents are required.

Orchitis Typhodes-Phlegmonea. If taken in sufficient season this disease is some times intirely cured by these agents.

Erythema. *Erythema* is a genus of diseases and not a species. I am acquainted with at least eight or ten species of external *Erythema*. As respects internal *Erythema*, I have no knowledge of more than two species, and for these, I do not know that the Neuragics are ever employed; but for several of the external species, I have often known them useful. Never the less it is considerably debated among physicians whether these articles can ever be judiciously employed in any species of *Erythema*.

Erythema vesiculare var. vulgare (Mihi). The most common form of *Erythema vesiculare* is usually relievable by certain Neuragics. I have seen this accomplished by Liniments of Natural Carbonate of Zinc, and of Carbonate of Lead, Lotions of the Protacetates of Lead, and Liniments of the same, will often effect the same purpose.

Erythema vesiculare (Good) var. Rhœinum (Mihi). I have seen Lotions of the Protacetates of Lead with Acetate of Oxyd of Morphinum, and Liniments of an Aqueous Solution of these Acetates both of Lead and Oxyd of Morphinum with an equal quantity of Olive Oil added, employed with the most decided benefit, in these cases.

Erythema Causis var. humida (Mihi). If applied before the skin is denuded of the cuticle, I have seen the same Lotion and the same Liniment mentioned under *Erythema vesiculare var. Rhœinum*, highly useful in Scald; and indeed I have often known it effect a cure. I believe it may be considered as well settled that in the Gangrenous Erythemata, the Neuragics are of no material service.

Phlogotica interna.

Pneumonitis Typhodes-notha (Mihi). *Pneumonitis Typhodes-Dysenterica*. This disease which is a Dysenteric Phlogosis of the bronchial membrane, the constitutional Febrile affection being a pure Typhus nervosus, I have repeatedly seen arrested suddenly, just at the time the disease was fully formed, by a few full doses of Uniprotacetate of Lead and Papaver. I am inclined to think that this may be done in a large proportion of the non-malignant cases. If it is inquired what I reckon a full dose for an adult person of ordinary susceptibility, when only a few doses are to be employed, I answer a drachm. Till the patient has taken from two to four such doses, I would repeat them every three hours, and then I would stop. Perhaps some may think that the Styptic power of the Salt employed, renders the service. No mere Styptic, however, in any quantity that the patient can conveniently take, will accomplish the same purpose. As appears to me it is the Neuragic power that produces the effect. But I can not pretend to decide that the Styptic power contributes nothing. It is certainly important that the Uniprotacetate of Lead should be accompanied with an equally free use of Papaver. After the disease has progressed for several days, it is in vain to think of arresting it in this manner.

Enteritis Typhodes-notha (Mihi). *Enteritis Typhodes-Dysenterica*. *Dysenteria Typhodes-nervosa*. Common Dysentery is a Phlogosis of a peculiar and specific character (the same as that of the bronchial membrane in *Pneumonitis Typhodes-notha*) of the lining mucous membrane of the upper and smaller intestines, the constitutional Febrile affection being ordinarily, in our climate, a Typhus nervosus. This disease like *Pneumonitis Typhodes-notha*, may some times be arrested suddenly, either in its primary stage, or when it is just fully formed, by a few very full doses of the Uniprotacetate of Lead and Papaver. These agents require to be managed in Dysentery, just as I have specified for *Pneumonitis Typhodes-notha*. Malignant Dysentery, and Dysentery that has progressed without interruption for several days, can not well be arrested in this manner. I am not prepared to decide as to the comparative value of this practice, either in *Pneumonitis Typhodes-notha* or in *Enteritis Typhodes-notha*. I

have occasionally employed this method, but not generally. In the few cases in which I have employed it, I accomplished my purpose with it. For myself, I never saw much benefit from any of the Protacetates of Lead, in small and uniform doses, at regular and short intervals, in the course and progress of Dysentery; but I have often seen it irritate and aggravate the intestinal symptoms, and oftener still, offend the stomach, diminishing its tone and greatly lessening appetite and digestive power.

Phthisis. (species omnes.) I have often known one or more of the Protacetates of Lead used with considerable freedom, in conjunction with Papaver for the diminution and restraint of profuse pyoblennorrhœic excretion from the lungs, and with success. The Disulphate of Oxyd of Quininum will greatly assist in producing this effect. In this case I doubt not that the Styptic operation of the Salt of Lead contributes greatly to the remedial effect produced; but I feel confident that it is not wholly due to this, as I have received ample testimony of the same effect's being produced by Protocarbonate of Lead, without any auxiliary at all; and this Salt is not Styptic at all. Now if a Neuragic alone will produce this effect, and if the Protacetates of Lead are Neuragic, these facts afford good ground for concluding that when this effect is occasioned by one of the Protacetates, it must be the effect both of its Neuragic, and of its Styptic powers.

Hæmorrhagia. (species et varietates omnes.) The Protacetates of Lead possess considerable power for the relief of Hemorrhage, which is commonly supposed to depend intirely upon their Styptic operation. This, I am satisfied, is an error, and I doubt not that, in certain cases, the Neuragic operation of these Salts contributes as much to the benefit rendered, as the Styptic operation. I once supposed that these Salts were adapted only to relieve mere and pure atonic Hemorrhage; but I now know that their application is by no means so limited. They are as effectual for pure irritative Hemorrhage as for pure atonic Hemorrhage, and of course, for Hemorrhage with atony and irritation conjoined. Now I think that they relieve irritation by their Neuragic power exclusively. I think also that they are not only admissible, but useful in entonic Hemorrhage. In such cases, I doubt whether Stypticity is of any service. I think that all must be accomplished by a Neuragic operation. In all cases except those which are the

most purely and the most exquisitely atonic, I doubt not that the Neuragic operation benefits as much as the Styptic one.

Diarrhœa vulgaris (Mihi.) The employment of certain Neuragics in obstinate cases of this disease is common and favorite practice, with many physicians. I have tried it very often, and some times not without more or less beneficial effects; but still I am convinced that the utility of the articles commonly employed have been very much overrated. The Protacetates of Lead, the Protacetate and Protosulphate of Zinc, the Biprotosulphate of Alumina and Potassa, etc. are specimens of the articles usually selected. These are all Styptic as well as Neuragic, and their Styptic power always takes all the credit of the benefit which they render. I have been long satisfied however, that it is much more due to their Neuragic than to their Styptic operation. Perhaps their Styptic operation may contribute more than I suppose; but pure Styptics, however intense, do very little good in such cases. All of the articles, which I have here enumerated, if used to any considerable extent, certainly exhaust more than is desirable, and very often produce a good deal of irritation. Upon the whole, I do not like the practice, and think that better may always be employed.

Blennorrhœa nasalis, Blennorrhœa faucialis, Blennorrhœa bronchialis, Blennorrhœa intestinalis, Blennorrhœa vesicalis, Blennorrhœa vaginalis, Blennorrhœa urethralis. These mucous fluxes though so precisely like each other, are widely separated in the works on nosology, and are called by widely different names. They occur in an acute and irritative form (the latter mistaken by Good for an entonic or phlogistic one) and in a chronic and irritative one. The first is usually called Coryza; the second I have never heard a name for; the third is one of three or four quite different diseases, that it is now the fashion to call Bronchitis, though it is no more a Phlogoticum than an old case of Leucorrhœa; though there are several other diseases, that this name would be much more appropriate for, as *Pneumonitis Typhodes-notha*, and *Pneumonitis Typhodes-Erythematica*, both of which are different specific Phlogoses intirely confined to the bronchial membrane, and therefore true Bronchitides; the fourth is commonly called *Diarrhœa mucosa*; the fifth is called Catarrh of the bladder; the sixth is called Leucorrhœa or Fluor albus; while the seventh

is called Gleet, and several other equally whimsical names. All of these diseases are often treated very successfully with the Styptic Neuragics and the Styptic power takes all the credit. But I have good reason to conclude that the Neuragic power accomplishes as much if not more than the Styptic one.

Ptyalismus acutus var. idiopathicus. I have known Idiopathic Acute Ptyalism treated by various Neuragics, and some times with success. I have known the Protacetates of Lead, the Protacetate and Protosulphate of Zinc, the Bisulphate of Alumina and Potassa, the Protoxyd and Protocarbonate of Zinc, the Tetracipronitrate of Bismuth, etc. all employed. The therapeutic effect of the first four is commonly ascribed to their Styptic operation, the effects of the last three are usually ascribed to a specific operation, or some times to an Alterative one, when they are ascribed to any operation whatever. According to my observations the Protoxyd and the Protocarbonate of Zinc are more effectual than any of the list that I have mentioned; but none of them are as effectual as Papaver.

Paruria Diabetes (Mihi.) Various Neuragics have been recommended in this disease; but as far as I have knowledge, their employment has not been attended with very encouraging success. A single one that has been highly recommended, I do not think has been sufficiently tried to determine its character. I intend the Disulphohydrite of Protosulphid of Ammonium. This should unquestionably be further investigated before its use is relinquished.

PROËM TO THE CLASS NARCOTICA.

There is a Greek noun-substantive equivalent in signification to the Latin *stupor*, which implies in English numbness, stiffness, torpor, but much more especially the first, viz. such a state as is produced by electric fishes. From this noun-substantive various other words are derived, and among these, a verb equivalent to the Latin *stupefacio*, and the English to benumb, to stiffen, to render torpid. The sensation produced by a blow or pressure

upon a nerve, which is commonly called sleep, when felt in a limb, will illustrate the meaning of the Greek noun-substantive to which I refer, and also the Latin *stupor*, as well as the sensation occasioned by the electric fishes. From this Greek noun-substantive comes the attribute, which is used as the name of this class, and the noun-substantive which is the name of its operation and effects.* The term Narcotic then is ancient, but by no means either definite or appropriate, as will be perceived by the definition. The Narcotics, strictly speaking, can not be said to benumb, i. e. to produce a sensation like that occasioned by a blow or pressure on a nerve, which, when in a limb, we commonly call sleep, nor do they even stiffen in the sense intended by the etymological import of the term. Possibly there may be true torpor in ultimate Narcosis, but this is not a medicinal grade of operation, and medicines are not to be classified by non-medicinal operations. Although I consider the term Neuragic as not a very appropriate name of the class to which it is applied, yet it is decidedly preferable to the term Narcotic for this class; and yet the medical profession perfectly acquiesce in the term Narcotic, making no objection whatever to it. I mention this by way of extenuation of what inappropriateness there may be in my terms Neuragic and Adenagic. The etymological signification of the term Narcotic no more constitutes a definition of this class of remedies, than the etymological significations of the terms Phlogosis or Inflammation constitute a definition of all the species and varieties of the topical affection bearing these names; or the etymological significations of Pyrexia or Febris constitute a definition of all the species and varieties of constitutional affection bearing these names; and yet we perpetually find medical authors and practitioners making the mistake in question. It is a matter of course that the definition of the class must be contained in the proëm, even though it has been previously given in the synopsis of the classification.

Definition. Narcotics are articles which *in the first degree of their operation*, directly allay morbid irritability and irritation, and irritative actions generally, morbid sensibility and sensation, morbid mobility, restlessness, jactitation and wakefulness, when they are connected with a non-phlogistic or a positively atonic

* I shall attempt no more Greek etymologies, either of the names of the classes, or of the names of individual articles, for reasons that will be obvious to the reader.

condition of the system ; *in the second degree of their operation*, they directly relieve pain ; *in the third degree of their operation*, they directly produce more or less somnolency, or even positive sleep ; *in the fourth degree of their operation*, they produce vertigo, headache, faintness, dimness of sight, the sensation of a cloud before the eyes, or some imperfection of vision, either with considerable dilatation, or great contraction, or an immovably fixed, but otherwise natural state of the pupils, nausea and retching, with epigastric uneasiness, especially when the head is raised, or otherwise much moved, accompanied with small and irregular pulse, cold extremities, cold, clammy and slippery sweats, delirium, convulsions, either clonic, tonic or of some other sort, succeeded by coma, and some times death ; and when the Narcotic has no other medicinal power conjoined, without any other accompanying operations.

The term Narcosis denotes the operative effects of a Narcotic.

The first degree of a Narcotic operation I shall hereafter call Antirritant ; the second degree I shall call Anodyne ; the third degree I shall call Soporific or Hypnotic ; and the fourth degree I shall call Ultimate-Narcotic. In moderate doses and quantities, in relation to the subject and the case, only the Antirritant grade of the operation of a Narcotic will be produced. In larger doses and quantities in relation to the subject and the case, the Anodyne grade of the operation will take place. In larger doses and quantities still, in relation to the subject and the case, the Hypnotic or Soporific grade of the operation will result. But though the property of directly producing sleep belongs to this class alone, yet not more than five or six of their number, or some where there about, are ordinarily used for this purpose, or are in fact capable of answering this intention to any considerable degree, and with any material certainty ; and even one of these very far surpasses all the rest, and is more employed for this purpose than the whole besides. The power of directly diminishing, to a greater or less extent, both morbid irritability and irritation, morbid sensibility and sensation, together with irritative activity, restlessness and jactitation, when these conditions are connected with a non-phlogistic, or a positively atonic diathesis, belongs only to the narcotics, in any thing like an eminent degree. The Antiphlogistics are Antirritant only in phlogistic or entonic diathesis.

The Leantics are only very slightly Antirritant, too slightly to be mentioned in competition with the Narcotics. The Neuragics are comparatively slow and gradual in their Antirritant operation, and it is of a very different quality when produced, being adapted only to a very different set of cases. The Euphrenics and Antisbestics are only indirectly Antirritant, which is also true of the Tonics. The last are very slow and gradual in the production of Antirritant effects. Even the Oresthetics by their strong and peculiar impression, some times obviate morbid irritability; but they can not very properly be reckoned even as indirect Antirritants, since their normal operation is to obviate torpor and insusceptibility. But the Narcotics are peculiarly valuable for this power, though much more especially Papaver.

All the Narcotics, when freely used, almost if not quite always diminish morbid frequency of the pulse, when connected with a non-phlogistic or positively atonic diathesis, and they produce this effect directly. Unlike the Antisbestics and Tonics, which operate only indirectly in this way, the Narcotics often reduce the frequency of the pulse much below the natural standard. It always appeared to me that this effect is a part of their Antirritant operation. I have often met with physicians who supposed that this peculiar power belonged to Digitalis and to no other Narcotic; but such is certainly not the fact. I do not know that Digitalis diminishes the frequency of the pulse any more prominently than Papaver, but it renders it irregular, i. e. unequal and intermittent. Under an extreme ultimate degree of its operation, almost any Narcotic may do this, but it is produced by Digitalis in the medicinal grades of its effects.

The Narcotics are the only direct Anodynes and Hypnotics or Soporifics. All the different and distinct subordinate parts of a Narcotic operation that are ever produced by any agents belonging to this class, are 1. Antirritant, already explained, one of the effects of most Narcotics. 2. Anodyne, already explained, produced by most Narcotics. 3. Coprostatic, that is directly diminishing the peristaltic action of the intestines, produced by Papaver only. 4. Ecbléseostatic, that is directly diminishing-parturient contractions of the uterus, produced by Papaver alone. 5. Cysteostatic, that is, occasioning vesical retention of urine, produced the most prominently by Papaver, but also in a less degree by

Atropa lethalis, most of the species of *Datura*, etc. 6. *Eccriseostatic*, that is directly diminishing most of the secretions, produced more eminently by *Papaver*, but also in a less degree by *Atropa lethalis*, most of the species of *Datura*, etc. 7. *Antaphrodisiac*, that is directly diminishing venereal appetite and power, produced by *Papaver* perhaps alone. 8. *Cnestic*, that is, causing great itching of the skin, produced by *Papaver*. 9. *Hypnotic* or *Soporific*, already explained, produced in a greater or less degree by all, but far more eminently by *Papaver*. 10. *Ultimate-Narcotic*, already explained, and produced, in all probability, by every article of the class. If the *Strychni* and certain other articles of more or less analogous operation, are reckoned as *Narcotics*, then an *Erethistic* operation must be considered as a subordinate part of a *Narcotic* one, and these agents must be specified as *Narcotic*, though destitute of any direct *Antirritant*, *Anodyne* and *Hypnotic* or *Soporific* powers, which appears to me to be tantamount to denying them any *Narcotic* power, which I deem correct.

The term *Enterostatic* is some times correctly used as synonymous with *Coprostatic*. *Hysterostatic* is some times used as synonymous with *Eccriseostatic*, and perhaps with propriety. *Urostatic* is some times used as synonymous with *Cysteostatic*, as I think incorrectly. The former denotes a suspension of renal secretion, while the latter implies a vesical retention of urine.

It is a great error to suppose every agent that is capable of producing cerebral stupor, anæsthesia or coma, is a *Narcotic*.

Single full and rather too large doses of the *Narcotics*, after about twelve hours, more or less according to circumstances, produce a temporary prostration, manifested by a good deal of languor and lassitude, with vertigo and nausea from an erect position, and much more from motion. This state does not generally continue more than twenty-four hours, some times not more than twelve hours, and if the affection is slight, not more than six; and it is produced in a more eminent degree by *Papaver* than by any other *Narcotic* in common use. This state or condition appears to have contributed towards giving John Brown the first notion of what he calls indirect debility, produced by excess of Stimulus. Indeed I think he derived the very first idea of such a sort of debility from this particular operation of *Papaver*, of which I am now treating; but it is not debility, nor is it produced

by Stimulus in any legitimate sense of this term. It is mere prostration, a regular operation of a Narcotic managed in a certain injudicious manner, and some thing quite different from debility. That Brown's supposed indirect debility of Papaver has no sort of connexion with what I call Antisbesis, the only true, proper and legitimate Stimulation, may be proved by the fact that the same state may be just as readily produced by a Narcotic intirely destitute of a particle of Antisbestic power. I have seen the effect in question produced in an intense degree, by a single full and large dose of Extract of Conium maculatum, taken to test its quality. I have likewise seen the effect in question produced by just such a dose of Hyoscyamus niger, taken for the same purpose. Now I have employed both of these articles often enough and long enough to know that they are utterly destitute of any degree of Antisbestic power, even the slightest. I have seen the effect in question produced by single excessive doses of Gelseminum nitidum and of Amianthium Muscitoxicum; and I have employed both these articles often enough and long enough to know that they are utterly destitute of even the most trifling degree of Antisbestic or true Stimulant power.

After Brown adopted the opinion that I am now considering, he applied the appellation indirect debility to certain cases of genuine exhaustion, which he fancied were produced in an analogous manner. In relation however to those cases of true exhaustion, to which Brown applied this phrase, indirect debility, I have else where fully considered them, and expressed my fullest conviction that their distinction as indirect debility, is intirely destitute of all good foundation, and that in all cases, such a distinction is a distinction without a difference, a distinction of no practical value, either in therapeutics or pathology, as we should expect all distinctions to be that have no good foundation.

In inordinately large doses and quantities, much larger than the exigencies of the case require, a greater or less number of the effects, which transcend the medicinal grade, and which I call ultimate-Narcotic, may occur. These consist of a greater or less degree of disturbance of the functions of the brain and spinal cord, and of the several sets of nerves respectively. It should always be remembered that the aggregate of symptoms, which constitutes Ultimate-Narcosis is specifically distinct from all other

disturbances of function in the brain, spinal-cord and nerves generally, and is sufficiently diverse to be always discriminated with facility; though not withstanding this, I think they are frequently confounded by authors and practitioners of medicine. Dimness of sight or disturbance of vision of some sort, is a symptom of Ultimate-Narcosis, and one to which I think an undue degree of importance has been attached. The manner in which the Narcotics produce dimness of sight, without any disturbance of the intellectual operations, and certainly none of the power of volition, can not be even plausibly conjectured. We can only say that this class of agents disturbs the function of the optic nerves, or of the nervous tract from which this nerve of special sensation takes its origin, before it disturbs the functions of any other part within the cranium. The fact that dimness of sight takes place equally when the pupil is immovably fixed in its natural state, as well as when it is extremely dilated or extremely contracted, is an interesting one towards a true appreciation of the nature and character of the symptom. It would seem that the pupil becomes fixed in just the condition that it happened to be in when the Narcotic produced its fullest effect, and that the dilatation, contraction, etc. must have been due to some other agency previously exerted. This fact explains why there is so much discrepancy of testimony among physicians as to the particular condition of the pupils produced by individual Narcotics. One gentleman has told me that *Digitalis* always produces dilatation of the pupils, another that it always produces contraction; one gentleman has told me that *Papaver* always produces contraction of the pupils, another that it always produces dilatation. Now I have as often seen one of these states as the other, produced (as is commonly said) by both of these agents; and I have much oftener seen a natural state under the free use of each of them. I do not doubt however, that a few Narcotics uniformly produce dilatation of the pupils, as *Atropa lethalis*, the several species of *Datura*, and a few others; but as appears to me, the great body of them when pushed to a considerable extent, merely fix the pupils for a certain time, in the state in which they happened to be, when the patient became intensely affected by the Narcotic.

But that amount of a Narcotic which may be only Antirritant in a given subject or case, may be Anodyne or Soporific, or even

Ultimate-Narcotic in another subject or case. It is by far too often the case that practitioners of medicine have a kind of medium dose, or at least a certain limited range as respects doses and quantities, from which they rarely deviate, and which is much too small for any quite insusceptible subject, or any case in which the system is powerfully occupied by the disease; but which may actually be too large for very susceptible subjects, or for cases of little intensity. From these circumstances, the proportion of cases that is benefited by the use of Narcotics under the charge of such a physician, is small, and both patients and physician become skeptical of their utility in any case, and finally their employment is wholly neglected, and in fact a strong prejudice against them is contracted. I have met with many a physician in this state who thought it would be well if the whole of the class were expunged from the *materia medica*. But this is not the only class of medicines in regard to which I have met with such opinions.

A greater or less number of the aggregate of symptoms making-up what I call Ultimate-Narcosis, in reality constitutes one of the varieties of prostration (not exhaustion) and may be produced to a considerable extent, not only by Narcotics, but by Nausiatics; by the ingestion of certain Fish; by the accession of certain malignant diseases; and doubtless by various other means. When these particular symptoms of prostration take place in an exquisite degree without having been preceded by powerful Nausea, but as the consequence of the ingestion of a comparatively large quantity of an agent possessing a greater or less degree of direct Antirritant, Anodyne and Soporific power, we may then know that they are the effects of a Narcotic operation. It must be distinctly understood that the production of the effects in question does not argue a Narcotic power, unless they take place without being preceded by powerful Nausea, and unless the agent is capable in smaller quantities, of producing a greater or less degree of direct Antirritant, Anodyne and Soporific effects. When these particular symptoms of prostration take-place in an exquisite degree, as an immediate sequel of powerful Nausea, produced by Sea-sickness, or Sick-headache so called, or the ingestion of any mere emetic agent, there can of course be no ground for ascribing them to any Narcotic operation. As I have already hinted, the same aggregate of symptoms is described by writers on the com-

plaints and maladies of inter-tropical climates, as being produced by the ingestion of certain Fish, found in the waters of such regions, and popularly said to be poisonous. Whether or not, in such cases, the Fish eaten first produces powerful Nausea, and the aggregate of symptoms under consideration is occasioned by the Nausea, I am unable to say, never, as I suppose, having seen exactly such cases as the writers in question would seem to describe. I imagine that few will be inclined to maintain that the Fish whose ingestion causes these symptoms of prostration, possess any Narcotic power. When these particular symptoms of prostration take place in an exquisite degree, without the ingestion of any thing whatever; without being preceded by powerful Nausea; and especially during the prevalence of a malignant epidemic, we may consider them as indicating an attack of such epidemic. At all events, under such circumstances, we can not possibly consider them as Narcotic effects. But the production of a greater or less degree of this sort of prostration, may never the less, be sometimes used as a test of the Narcotic power of an agent, if we carefully observe whether the article previously produces any degree of the first three grades of a Narcotic operation; and if we find that what appears to be Ultimate-Narcosis takes place without any Nausea, or at least without any material degree of it.

The aggregate of symptoms which I am in the habit of designating by the phrase Ultimate-Narcosis, is probably never of any remedial utility, any more than the ptyalism of the Mercurials; and it may very often be counteracted and prevented by Erethistics, Euphrenics, Oresthetics, Antisbestics, etc. without a particle of diminution of the real and true medicinal effects of the Narcotic. It is very often the fact that such a quantity of a given Narcotic may be necessary to obviate the symptoms of a particular disease, as would produce a troublesome degree of Ultimate-Narcosis, without some counteracting agent along with it. Now it is practically important to be known that this Ultimate Narcosis may generally, if not always, be perfectly counteracted, without at all diminishing the remedial influence of the Narcotic employed. This fact, for it is a fact, as I well know from repeated trials and experience, abundantly exposes the fallacy of Dr. Paris's notions of the remedial incompatibility of those articles which I call Erethistics, Euphrenics, Oresthetics, Antisbestics,

etc. especially with *Digitalis*, but also with most if not all other Narcotics. In truth, I believe it is pretty well known that Alcohol is often capable of counteracting, to a very considerable extent, the Ultimate-Narcotic effects of *Papaver*; and vice versa, *Papaver* is often capable of counteracting, to an equal extent, the Ultimate-Narcotic effects of Alcohol. Now both *Papaver* and Alcohol are both Erethistic, certainly Euphrenic, Antisbestic and Diaphoretic; and Alcohol but not *Papaver*, is Oresthetic. When I say that Alcohol counteracts the Ultimate-Narcotic effects of *Papaver*, and that *Papaver* counteracts the Ultimate-Narcotic effects of Alcohol, let no one make so great a mistake as to suppose that one of these articles, by its primary Narcotic operation, counteracts the primary Narcotic operation of the other; that one of these articles by its Erethistic, Euphrenic or Antisbestic operation, counteracts the Erethistic, Euphrenic or Antisbestic operation of the other. On the contrary, each of these several operations as produced by different articles, instead of counteracting, actually coincide with and promote each other. Hence the simple and pure Narcotics actually assist each other's operations. But Erethistic, Euphrenic, Oresthetic and Antisbestic operations, as I have already said, efficiently counteract an Ultimate-Narcotic operation. If the Ultimate-Narcotic operation of *Papaver* is to be prevented or counteracted by Alcohol, the Alcohol must be so managed as to produce only its Erethistic, Euphrenic, Oresthetic and Antisbestic effects; or if the Ultimate-Narcotic operation of Alcohol is to be prevented or counteracted by *Papaver*, the *Papaver* must be so managed as to produce only its Erethistic, Euphrenic and Antisbestic effects. When Alcohol is given in such a manner as to produce no other effects than Ultimate-Narcosis, it will not prevent nor counteract the Ultimate-Narcosis of *Papaver*; and in like manner, when *Papaver* is so managed as only to produce Ultimate-Narcosis, it will not prevent nor counteract the Ultimate-Narcosis of Alcohol. Similar stages of the operation of different pure Narcotics will not counteract each other, but they will coincide with and enhance each other's effects; and the same is doubtless true of the several stages of the operation of other powers, as produced by different individual agents.

I have elsewhere inculcated that Stimulant (i. e. Erethistic, Euphrenic, Oresthetic and Antisbestic) effects are not at all incom-

patible with true and proper Sedative effects as produced by the Narcotics. How often do we witness each of the above mentioned four effects in conjunction with the most decided Antirritant and Anodyne effects. Erethistic effects and certain grades of Euphrenic effect, may indeed be incompatible with positive Hypnotic or Soporific effects; but certain other grades of Euphrenic effect certainly are not so. How the dogma, that true Stimulation and true Sedation were necessarily incompatible, ever obtained credence and prevalence, was always mysterious to me. It has been alleged that Ultimate-Narcosis is capable of counteraction (and I suppose of prevention also) by those articles only which "increase action," as the established phrase is. But what is meant by increased action? If by this phrase is intended increased strength of action, the allegation is not true, since there are some articles which increase strength of action that will not relieve Ultimate-Narcosis, as for example the Tonics. On the other hand, the Oresthetics, the Euphrenics and the Erethistics when pure, have no power of increasing strength of action, and yet they are more effectual than Antisbestics, whose sole operation is to increase strength of action, for the prevention and obviation of Ultimate-Narcosis. The phrase "increased action" however is very loosely applied, not only to increased strength of action in the sanguiferous system; but also to increased frequency of pulse with great weakness; to increased activity, as in the secernents, in such cases as Diabetes, Blennorrhœa vaginalis or Leucorrhœa; to increased voluntary muscular activity, as under the Erethism of the exhilarant grade of the operation of a Euphrenic; to mental exhilaration as produced by the Euphrenics; to increased susceptibility, topical irritation and rubefaction, as produced by the Oresthetics; to Spasm or Convulsion of all sorts; and various other symptoms and conditions.

The most free Diaphoresis produced by simple and mild means does not appear to relieve Ultimate-Narcosis in any degree; but violent exercise producing violent sweating has done it; though, in all probability, the exercise was far more efficient for the purpose than the sweating. The continuous application of cold water to the head is very effectual for the relief of Ultimate-Narcosis, at least in many cases; but cold water thus employed does not produce "increased action" in any sense within my knowledge.

Violent processes making a strong impression, or giving a powerful shock to the system will often prevent, and as often obviate at once a high degree of Ultimate-Narcosis, though the processes themselves, independent of their violence, may be very ill adapted to the purpose. But such violent processes, it will at once be obvious, can not possibly be employed without great inconvenience, and usually great hazard, in the ordinary cases in which it is desirable to prevent or obviate a greater or less degree of Ultimate-Narcosis; but I can not now call to mind any case, in which we may not, with propriety and safety use Erethistics, Euphrenics, Oresthetics and Antisbestics.

The term Intoxication or Inebriation, was undoubtedly first applied, and long exclusively confined to the Ultimate effects, or more correctly to certain ultimate effects of Vinous liquors, taken in comparatively large quantity within a short time. In process of time, and after the discovery of distillation, the application of these terms was extended to the ultimate effects, or to certain ultimate effects, of Alcoholic liquors taken also in a comparatively large quantity, within a given time. This is still the only popular application of the term Intoxication and Inebriation, and in my opinion should continue to be so, because 1. The quality of these ultimate effects of Vinous, and Alcoholic liquors is materially and essentially different and distinct from any analogous ultimate effects of any simple and pure Euphrenic, or any Euphrenic-Narcotic. 2. The sequels of a frequent repetition of the production of the ultimate effects of Vinous and Alcoholic liquors are morbid, and equally different and distinct from the sequels of the frequent repetition of the production of the ultimate effects of any simple and pure Euphrenics, or any Euphrenic-Narcotics. 3. The ultimate effects in question of Vinous or Alcoholic liquors, are not only morbid, but are incapable of being employed for any useful purpose either in medicine proper or in surgery. It is true, there are material differences in the quality of the ultimate effects of Wine and Alcohol, not only while they are pending, but also in the sequels, which they respectively produce; but as the terms Intoxication and Inebriation have long been popularly applied to both, and to nothing else; and as neither is of any utility in medicine or surgery; and as both are morbific, though the sequels of a frequent repetition of the ultimate effects of Alcohol

are much more so than those of Wine; it appears to me to be proper that the two should continue to be classed together as Intoxication or Inebriation, but that it would be highly inexpedient to associate with them, under the same denominations, the ultimate effects of the other simple and pure Euphrenics, and of the Euphrenic-Narcotics. For example, what man of any true discrimination or sound judgement, would ever think of associating the ultimate effects of Protoxyd of Nitrogen with those of Wine or Alcohol? I think, therefore, that it would convey erroneous ideas, would give rise to groundless and injurious prejudices against other articles, and therefore would be highly improper to apply the terms Intoxication and Inebriation to the ultimate effects of any other agent than Vinous and Alcoholic liquors.

The term Intoxication is now however constantly applied to every article that will stupify, as the popular language is. The primary signification of stupify is to benumb, and thence, to suspend any function by benumbing. At present it is not commonly used in application to the suspension of any functions, except those of the hemispheres of the cerebrum. In the sense of stupefiërs in English, all the Narcotics that are sufficiently active to be capable of producing Ultimate-Narcosis, and all the Euphrenics that are sufficiently active to be capable of producing Ultimate-Euphrasy, are Intoxicating agents; and in all probability too, all the Erethistics. Every article that is considered capable of stupifying Fish, when thrown into the water which they inhabit, is always said therefore by all writers to be Intoxicating; and very many articles produce this effect which are neither Narcotic nor Euphrenic. If stupefaction, produced by an agent taken into the stomach, constitutes Intoxication, I can not discover why stupefaction produced by disease should not likewise constitute Intoxication. The conditions popularly called stupefaction, as produced by disease, are identically the same as those so called that are produced by medicinal agents. If the condition is the same, it is obvious that it should be called by the same name. Upon this plan, people are intoxicated by Catalepsy, Hysteria, Lethargy, Acinesia, Paralysis, and indeed, a large portion of all the diseases which destroy life, produce Intoxication near their termination in death. Upon this plan, at least nine tenths, and probably nineteen-twentieths of all persons die Intoxicated. The term In-

toxicating is constantly applied to every article that will exhilarate in any degree, whether it is capable of stupefying (in the English sense of this term) in any degree or not. This acceptance of the term extends its application to all the Euphrenics that are not sufficiently active to be capable of producing any degree of Ultimate-Euphrasy.

A British gentleman of the name of Morewood has written a large octavo upon Inebriants, by which he means Intoxicants (for the two words are synonymous in English) in which he extends the application of the term far beyond the Narcotics and the Euphrenics. In fact, I never could discover where he limits its application. As well as I can judge he applies the term to every thing which any body takes statedly, regularly and habitually, except food. He gives no definition of the term inebriant or intoxicant; nor do I think that a definition is possible, with the latitude in which he applies the term. I once attempted to make a definition of Intoxication that should be wholly founded upon Intoxication as produced by Vinous and Alcoholic liquors. It was as follows.

Intoxication is made-up of extraordinary exhilaration, gradually increasing so as to be beyond the control of the will of the subject, with imperfect articulation, and inability to regulate voluntary motion generally, which finally passes into suspension of the functions of the hemispheres of the cerebrum.

It is to be remarked that so much of a truly Intoxicating agent may be taken at once, as to produce the suspension of the functions of the hemispheres of the cerebrum very speedily, without being preceded by any thing more than a slight and transient degree of the extraordinary exhilaration. This definition is imperfect in as much as it does not specify the peculiar quality of the exhilaration or of the suspension of the functions of the hemispheres of the cerebrum, which are undoubtedly different as produced by Vinous and Alcoholic liquors, from the analogous grades of the operation of other Euphrenics, as is abundantly proved by the difference of the sequels. The imperfect articulation, and the inability to regulate voluntary motion generally (as is said) distinguishes the effects of Vinous and Alcoholic liquors from the effects of the pure and simple Euphrenics, and also from the effects of all other Euphrenic-Narcotics. But if it does not, the sequels

of Vinous and Alcoholic Intoxication (the only true and proper Intoxication, in my opinion at least) ought to be brought into the definition, since nothing else which has erroneously been called Intoxication ever has any such morbid sequels. I repeat that I do not think that any thing ought to be called Intoxicating, except the ultimate effects of comparatively large quantities of Vinous and Alcoholic liquors taken within a short time, because nothing produces any thing like such sequels, etc. This is undoubtedly the popular application of the terms Inebriation and Intoxication, and as I think the only legitimate use of them.

It should not be concealed, however, that the terms are frequently used in various other senses. The terms Intoxication and Stimulation are constantly used, by very many persons, as if they were perfectly synonymous. The terms Intoxicating and exhilarating are constantly used by many persons as if they were perfectly synonymous. The term Intoxication is constantly applied to the effects of the Erethistics. The terms Intoxicating and Narcotic are constantly used by very many persons as if they were perfectly synonymous. Else where I consider and discuss the employment of these terms in the sense of Narcosis, and the employment of the terms Inebriantia and Intoxicantia in the sense of Narcotica. The term Intoxication is frequently applied to the effects of the Narcotics, Erethistics, Euphrenics, Oresthetics, and Antisbestics, the whole being considered as one class by many authors and practitioners, as for example by John Murray, and his disciples and followers. It will at once be obvious, that terms thus loosely employed are no better than blanks, and in many instances not as good.

It has been supposed that all Narcotics are peculiarly liable to disturb "the relation between the respiratory and circulating functions;" but as appears to me incorrectly. I do not think there is any very precise and uniform relation between the frequency of respiration and the frequency of the cardiac and arterial contractions, in different individuals, or in the same individual at different times and periods. It is true that more or less pulmonary dilatation and contraction is necessary, both for the circulation of the blood, and for its arterialization; but fuller and more protracted, and of course less frequent dilatations or expansions of the lungs will answer the purpose of less full, less protracted and more

frequent dilatations or expansions, and vice versa. But the frequency of cardiac and arterial contractions do not always indicate the frequency with which the whole mass of the blood goes the round of the circulation, and therefore the range of the variation in the relative frequency of respiration and cardiac and arterial contraction, may obviously be considerable, without any disturbance of either function. It is not the fact however, that all Narcotics are capable of being made to exert a torpifying influence upon the motive powers of the respiratory apparatus; and when they are capable of being made to do this, it is not by any means the fact that they always do it, in all doses, at all periods of repetition, in all quantities in the twenty-four hours, in all circumstances, and under all modes of management. I believe it may be considered as an established fact that only those Narcotics which act more especially upon the nerves of expression, and which destroy life by suspending their function, ever exert any direct torpifying influence upon the respiratory apparatus, or even render respiration slower or less deep.

It is supposed by many that the Narcotics are necessarily contra-indicated whenever there is any lesion of the functions of the brain, employing this term in its widest acceptation, viz. in the sense of the whole of the contents of the cranium. I have very often heard this opinion advanced, and that too by men of distinction in the profession, but never the less, I esteem it a great error. There may undoubtedly be morbid irritability and irritation, morbid sensibility and sensation, and even pain in the brain, and if so, why not relieve these conditions by certain Narcotics, the most effectual remedies in the whole materia medica, for the accomplishment of such a purpose? Certainly there is very often morbid wakefulness, which undoubtedly has its seat in the hemispheres of the cerebrum. If so, why not remedy this condition by those Narcotics which are the most effectual for the production of sleep? Indeed what is there besides, that can be at all relied-on for the accomplishment of this purpose?

Some times the powers and energies of the brain become greatly impaired by a shock of some kind, or by concussion. In such a case, we naturally look to the Erethistics and the Antisbestics as the appropriate remedies. But the condition in question is almost, if not quite always, accompanied with morbid irritability

and irritation, and very often with more or less morbid sensibility, morbid sensation and even pain. Now we have articles that possess a combination of Erethistic, Euphrenic and Antisbestic powers along with a Narcotic power. And does this Narcotic power contraïndicate the use of these articles? A priori, we should naturally suppose that such articles would be the most appropriate remedies for such morbid conditions; and a posteriori, they have been found to be such. I may here mention Papaver as an example of these articles. The predominating power of this agent is that of a Narcotic; but it may easily be so managed as to obtain its Erethistic, Euphrenic, and Antisbestic effects, without any undesirable degree of its Narcotic operation. This then would seem to be just the remedy for the cases under consideration; and as a matter of observation, experience and fact, I have always found it such. In reality, I have succeeded far better with it, in such cases, than equally Erethistic, Euphrenic and Antisbestic articles, that are destitute of any Narcotic power. But Papaver is Diaphoretic in addition to the above mentioned powers. This however, is of no disservice, even where it renders no benefit; and some times this operation is indicated along with all the other powers. It is true Papaver is not Oresthetic; but an article possessing this power can be easily conjoined, as Capsicum for example. It is not therefore true (as so many physicians suppose) that in all cases in which there is any lesion of the function of the brain or nervous system generally, the Narcotics are necessarily contraïndicated; but on the contrary, under such circumstances, certain of the Narcotics are among the most effectual within our knowledge for the perfect restoration of the functions in question, at least when the lesion is not organic or structural, but purely functional. The Erethistic, Euphrenic, Antisbestic and Diaphoretic Narcotics are certainly more effectual for restoring the natural energy, with which the functions of the brain and nervous system generally are ordinarily performed, and for bringing this energy into more powerful action, than any other agents at present in our possession. Ignatia amara, Strychnos Nux-vomica and several other species with analogous powers which are universally reckoned as Narcotics, by all writers on the materia medica, exert almost all their influence in this way; and yet, I am not apprised that this fact has served at all to correct professional opin-

ion upon this subject, as it certainly ought to have done. *Botrophis Actæoides*, several species of *Cimicifuga*, and several of *Actæa*, are likewise universally reckoned as Narcotics, and operate in the same general manner, and yet do not seem to have contributed at all to correct the erroneous views and opinions which I am now combating. But I do not reckon any of these articles as true Narcotics, though in this respect my views are perfectly peculiar, and so far as I know contrary to the whole medical profession. Still, admitting that these articles are not Narcotic, a sufficient number of articles will remain, that are unequivocally and undeniably such, that are very effectual for restoring lost energy, under certain circumstances, not only to the nervous system generally but to the brain.

I believe that all Narcotics of any material activity, if pushed to a certain extent, are capable of producing Convulsions of some sort. Some produce Convulsions of the common sort; and some of the Epileptic sort. The following are examples of those producing Convulsions of the common sort, viz. *Hyoscyamus niger*; *Spermædia Clavus*; *Papaver somniferum*. The following are examples of those producing Convulsions of the Epileptic sort, viz. *Cicuta maculata*; *Tanacetum vulgare*; *Myristica officinalis*; *Camphora officinarum*; *Nicotina Nicotianæ Tabaci*. I do not know of any true and proper Narcotics that produce convulsions of the Tetanic sort; though I consider it as much more than probable that there are such. Indeed this is affirmed by certain authors, of several comparatively common articles; but I happen to know from my own observations that they are in error upon the subject.

Some of the Narcotics produce Convulsions only as a primary part of their operation; and some only as a secondary part of it. The following is an example of those producing Convulsions as a primary part of their operation, viz. *Spermædia Clavus*. The following are examples of those producing Convulsions as a secondary part of their operation, viz. *Papaver somniferum*; *Hyoscyamus niger*.

Some of the Narcotics produce Convulsions first in the muscles of voluntary motion; and some first in the muscles of involuntary motion. The following is an example of those producing Convulsions first in the muscles of voluntary motion viz. *Papaver somniferum*. The following is an example of those producing Con-

vulsions first in the muscles of involuntary motion, viz. *Spermœdia Clavus*. Those Narcotics which produce Convulsions of the common sort; which produce them as a primary part of their operation; and which produce them first in the muscles of involuntary motion, are necessarily Ecbolics. All this is true of *Spermœdia Clavus*, with the additional fact that it affects the uterus even in its unimpregnated state, before any other muscles of involuntary motion.

It is almost necessary to state here that the same identical things may be affirmed with equal truth of a considerable number of the Erethistics, viz. if pushed to a certain extent, they produce Convulsions of some sort; some occasioning Common Convulsions; some Tetanic Convulsions; and quite probably, some occasioning Epileptic Convulsions, though I can not specify any individual article that does this; some producing Convulsions as a primary part of their operation; and some only as a secondary part; some producing Convulsions first in the muscles of voluntary motion; and some first in the muscles of involuntary motion. Those Erethistics which produce Convulsions of the common sort; which produce them as a primary part of their operation; and which produce them first in the muscles of involuntary motion, are necessarily Ecbolics; all which is true of *Botrophis Actæoïdes*; and various other nearly allied articles.

Narcotics and Erethistics are the only classes of medicines that, as a regular part of their ultimate operation, produce Spasms or Convulsions; and yet all those classes of medicines, whose powers are capable of an Erethistic grade of operation, may produce Spasms or Convulsions in this grade, provided it is sufficiently intense. If an Ecbotic operation is not a true and proper Convulsion of the uterus, it is on the very border of it. For my part, I can not discover why Convulsion, which, in almost all cases, is a morbid condition, may not, in some instances, be subservient and necessary, to the performance of a natural and essential function.

The Narcotics as well as the Euphrenics produce a sort of anæsthesia. That of the Narcotics is in general less intense than that of the Euphrenics, and it is equally variable as produced by different individual articles. The anæsthesia of the Narcotics appears to me to be materially and essentially different from that of the Euphrenics. So far as my information extends (and I have

taken considerable pains to arrive at the truth upon this subject) the anæsthesia of the Narcotics is very generally, if not always, true and proper Coma; while that of the Euphrenics is never Coma, but a suspension of the function of the hemispheres of the cerebrum of the same character as that of Hysteric Fit. The correctness of this statement however depends on the reference that is made of the anæsthesia of certain agents in regard to which perhaps there is room for a diversity of opinion. *Thea Sinensis*, for example, is capable of producing a perfect anæsthesia; but is this anæsthesia the effect of a Narcotic or of a Euphrenic power? I have always referred it to a Euphrenic power, because it is of the same character as that of the unequivocal Euphrenics, and because there is no other indication that this agent possesses a particle of Narcotic power, except the fact that it produces an anæsthesia. To assume that this article is Narcotic, and then to prove by it that the anæsthesia of a Narcotic is not always true and proper Coma, would be begging the question. There are a number of other articles in the same predicament as *Tea*, i. e. they produce an anæsthesia which differs from true and proper Coma. If they are Narcotics (as many consider them merely because they produce an anæsthesia) then the anæsthesia of the Narcotics is not always Coma. If they are not Narcotics, but mere Euphrenics instead, there is nothing abnormal belonging to them.

The Narcotics as a class of remedial agents are very often mentioned as possessing peculiar efficacy in *Diarrhœa* more especially, but also in *Dysentery*. This language is almost, if not absolutely universal with the medical profession. Ask most physicians what agents they most rely upon, more especially in *Diarrhœa*, but also in *Dysentery*, and the answer will be Narcotics. Call a brother physician in consultation, in either of these diseases, and inquire what will most effectually restrain them, and the answer will be Narcotics. From conversation among physicians, it would seem as if it were a matter of perfect indifference what individual Narcotic is selected. When I have received such advice, I have some times observed that *Humulus Lupulus* is universally considered a Narcotic, and therefore I suppose that article will answer as well as any other. I have some times known this objected-to, but still without any specification. I have then named *Lactuca* interrogatively. If all this happened to be

done with sufficient gravity, *Papaver* would commonly be named next. I have no knowledge that any simple Narcotic, with the single exception of certain species of *Datura*, possesses any remedial value in these diseases; nor are these worth mentioning to the exclusion of *Papaver*, though they are capable of being made useful, administered by way of enema, as auxiliaries to *Papaver*, in intense and very obstinate cases. I have no knowledge that any Narcotic possessing other additional and distinct powers, besides *Papaver*, is of any material value for the treatment of *Diarrhœa* and *Dysentery*. Why not then say *Papaver* at once? What need is there of mincing and picking words? At the final examinations of candidates for degrees, I have found more difficulty in getting the name *Papaver*, or any of its common substitutes, mentioned, than the most indelicate, or the most profane word or phrase in the language. I have no knowledge that any other Narcotic beside *Papaver*, whether simple or pure, or possessing other different and distinct powers in addition, directly diminishes the liquid secretions into the upper and smaller intestines, and even intirely suspends morbid ones; or directly diminishes peristaltic action of the whole intestinal canal, totally suspending that which is morbid, and even greatly diminishing that which is natural and healthy. These are the operations by which *Diarrhœa* and *Dysentery* are relieved, operations which are not exerted by the Narcotics generally; and therefore I think it is high time we ceased to ascribe them to the whole class, instead of a single, or at most two individuals. Precisely the same remarks may be made with regard to the Narcotics in uterine or parturient contractions. The power to diminish these is commonly ascribed to the whole class; whereas as it belongs exclusively to *Papaver*; and there is no reason why medical students and young practitioners, and even old ones, who never free themselves from the trammels of authority, should be any longer deluded and misled by such grossly incorrect language. Even those who actually know better, are often inconsiderately misled, by this same language.

It is commonly supposed that simple and pure Narcotics, when habitually used in considerable quantities, and for a protracted period, invariably produce a greater or less degree of inconvenient, injurious or positively noxious effects upon the constitution, or in

other words, upon the system at large. Among other things this has been a subject of considerable investigation with me, and as the result of a good deal of observation and much more testimony, and I think I may say a large amount of investigation on the whole, I can not but utterly deny any foundation to the opinion in question. In fact, I have never been able to obtain a particle of evidence in its favor, as universal as it is among physicians. If there is no evidence at all of its truth, I insist that it should be wholly rejected, however long it may have been in favor. It is barely possible that on more extended research, some evidence of it may be developed, though all probability is greatly against such expectation. I have often prescribed and superintended the administration of a considerable number of simple and pure Narcotics, for a period between one and two and some times even three years, with occasional short interruptions or intermissions, and this, without the production of any thing morbid, inconvenient or disagreeable, even in the slightest degree. Some of the simple and pure Narcotics used in this manner, have been *Gelsemium nitidum*; *Amianthium Muscitoxicum*; *Datura Metel*; *Datura Tatula*; and one other species at least. *Gelsemium nitidum*, and *Amianthium Muscitoxicum* are so active that in all probability fifteen grains of the recent bark of the root, and most likely of the top also of the former, obtained at the time of its greatest activity, and taken at a single dose, and about the same quantity of the recent bulb of the latter, likewise collected at the time of its greatest activity, and taken at a single dose, would endanger the life of an adult. I make this statement on testimony, being fully apprised that the danger from such articles is extremely liable to be over-rated. Whenever I have given either of these articles habitually and protractedly, I have always done it in such doses, and in such quantities in the twenty-four hours, that operative effects were constantly procured from them. I have repeatedly given *Datura* for more than a year in succession, to just such an extent as to keep the pupils of the eyes moderately dilated for at least nine-tenths of this time. On omitting its use for two or three days, the pupils have always been restored to their natural state. Indeed just the quantity of this Narcotic necessary to produce this effect at first, would after a certain continuance, fail of producing it.

Unlike *Gelseminum nitidum*, and *Amianthium Musciticum*, the several species of *Datura* may be given in comparatively large doses, much larger than is necessary for their ordinary medicinal effects, and so large as to produce their own peculiar Ultimate-Narcosis in a very intense degree without danger to life. I have known a child kept for more than a week under such a degree of the influence of *Datura*, as to be this whole time, in a high delirium, from a large quantity of the seeds taken without any other person's knowledge, and as children often take such things, and not removed from the alimentary canal by repeated quick Emetics and Cathartics, though finally discharged by a slow Cathartic; and when they were discharged all the unpleasant symptoms soon disappeared, and not a single undesirable sequel remained. Assuredly the articles which I have mentioned are active enough to do mischief, by long continuance, if in fact any mischief ever results from Narcotics used in this manner.

I shall mention a few other articles that are not simple and pure Narcotics, but which possess one or more additional powers. *Digitalis purpurea* is Adenagic in addition to its Narcotic power. This article I have given quite freely, eight or nine tenths of the time, for two, three or even four years, for advanced and bad cases of Strumous affection of the heart, cases commonly called by the absurd name of Hypertrophy. In these cases, I have usually instructed the patient to take so much of the *Digitalis* daily, as to produce and keep-up a slight irregularity of the pulse. When there is the least general exhaustion of the sanguiferous system, and the greatest fullness and irritation of the pulse, I have found *Digitalis* enough, to be altogether the best remedy within my knowledge, and I have therefore used it long, and with great freedom, and always without injury or inconvenience of any sort or degree. Indeed I have long been perfectly satisfied that the apprehensions which so many seem to feel in regard to the safety of this agent are perfectly groundless. I have occasionally, in fact frequently, been called in consultation with young men, who in reality, had no *materia medica*, having been taught that anatomy and physiology were perfect substitutes for the whole apparatus medicaminum. But in order to be retained by their patients, they must prescribe some thing, and not being familiar with the proper doses and proper quantities in the twenty-four hours, I have

seen four times as much as was proper administered, but never with any thing more than present inconvenience, never with any morbid sequels. I have seen *Digitalis* given inordinately between a week and a fortnight, without the least suspicion that the urgent symptoms produced could possibly be due to any thing that was prescribed. On abstracting the *Digitalis* the urgent symptoms subsided very readily, leaving neither morbid nor disagreeable sequels. In more than one instance I have known *Digitalis* put up, by mistake of an apothecary, for *Spigelia Marilandica*, and administered to small children accordingly, with urgent symptoms indeed, for the time being, but without any morbid or undesirable effect as sequels. In reality, I esteem *Digitalis* a far safer agent than *Spigelia*; for unless I am in a very great error, I am strongly inclined to think that I have known death suddenly produced, in several instances, by the administration of too much *Spigelia* within a short time; but of this I am not very sure, since a single visit near the closing scene, does not always enable a counsellor to form a correct judgment in regard to such a point. But at all events, I never heard of any ill effects from a continuous and protracted use of *Spigelia Marilandica*, which is a simple and pure Narcotic, without any other power in addition.

Conium maculatum, which, in the form of Extract, is Adenagic as well as Narcotic, I have known administered, with few and brief interruptions, for a year or more, and always without any morbid or disagreeable sequels. The most considerable power of *Nicotiana Tabacum* is that of a Narcotic; but in addition to this it has a Euphrenic, an Adenagic, an Emetic, and a Cathartic power. As habitually and protractedly used, its sole operation is that of a Euphrenic. I never met with but one case, in my life, in which this agent was taken to a sufficient amount to manifest any of the operations of a Narcotic; and the only manifestation in the case to which I refer, was an irregularity or inequality of the pulse, less than that which indicates that the constitution or system at large is under such a degree of the influence of *Digitalis* as to render it medicinal or remedial, in some of the most common cases in which it is employed. This irregularity or inequality of the pulse seems to be almost too inconsiderable or even trifling to deserve the name of morbid, although it ought perhaps to be reckoned as such, since no degree of Narcosis is derived from the or-

