CALDWELL, C

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1816

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MEDICAL THESES,

SELECTED FROM AMONG

THE INAUGURAL DISSERTATIONS,

PUBLISHED AND DEFENDED

BY THE

GRADUATES IN MEDICINE,

OF THE

UNIVERSITY OF PENNSYLVANIA,

AND OF OTHER MEDICAL SCHOOLS IN THE UNITED STATES:

WITH AN

INTRODUCTION, APPENDIX, AND OCCASIONAL NOTES.

BY CHARLES CALDWELL, M. D.
EDITOR OF THE WORK.

TO BE CONTINUED ANNUALLY.

PHILADELPHIA:

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PRELIMINARY DISCOURSE.

WHEN an Editor, about to engage in a new periodical publication, makes known his intention to the lovers of science, and solicits them to encourage the performance by their patronage, he may be considered as acting his part in the negociation of an important compact. Under such circumstances, justice requires, and custom bestows her sanction on the measure, that his proposals to the public should be accompanied, if not by a complete analytical prospectus, at least by some account, of the work. That such an account may prove useful, and afford the degree of satisfaction required, it ought to contain *an exposition of the ground on which the work is undertaken, a statement of the matter it is to embrace, and the manner in which it is to be conducted, and some information, as to the resources and prospects of the editor, in relation to a faithful compliance with his engagements.* It is only by being duly and correctly informed on these points, that the public can be enabled to judge, respecting the probable merit and utility of the work, and the claim, it is likely to present to their patronage.

Impressed as he is, with the truth of these sentiments, and subscribing to the principles which they inculcate, and the conduct which they prescribe, the Editor of the work herein announced to the public, is desirous to manifest his regard for them, in his intercourse with his fellow-citizens on the present occasion. This it shall be his object to do, by endeavouring, in this address, to communicate the necessary degree of information on the three foregoing heads. He will proceed therefore to state, as briefly as may be consistent with the nature of his subject, the reasons why the present work has been instituted, of what materials it is to be composed, and from what sources those materials are to be drawn.

The Editor would first, however, beg leave to observe, that he is aware of the impracticability of exhibiting, within the limits to which he must here confine himself, a complete epitome, or view in miniature, of the work in which he is about to engage. A performance so condensed, and yet complete in all its parts, never has been, and perhaps never can be,

But this is not sufficient for the purposes of America. In vain do we look into the mirror of a foreign page, for faithful images of our native maladies. As well might the description of an Englishman, or a Frenchman, be applied to one of our western savages, as the descriptions of the diseases of Great-Britain or France, be applied to the diseases of the United States. Though in each case the general outlines of the picture would be nearly the same, the finishing of it would call for very different shades.

It is, indeed, true, that during the seventeenth, and about the beginning of the eighteenth century, the condition of certain parts of Europe bore a nearer resemblance to that of the United States, than it does at present. The inhabitants of those countries were surrounded, then, as we are now, with marshes undrained, lands cleared but not cultivated, and vast tracts of territory overshadowed by deep and impenetrable forests. Added to this, their habits, manners, and general modes of life, do not appear to have been greatly dissimilar to those, which now prevail in some parts of the United States. We might hence be led to believe, that their diseases at that time exhibited no inconsiderable resemblance to ours at present. Nor am I inclined to think that such an inference would be very distant from the truth. Whoever will look into the foreign publications of that period, will find in some of them abler accounts, and more accurate descriptions of certain diseases that now prevail in the United States, than are contained in the writings of our own medical contemporaries of Europe. But as those old works are written, for the most part, in the Latin language, and greatly interspersed with visionary theories, fabulous stories, absurd practice, and obsolete philosophy, they will not, in this country, be generally read, nor can they be recommended as fountains of knowledge, free from exception. It is from that immaculate source of information alone, the book of nature, that the physicians of America can acquire, in the first instance, an adequate knowledge of the maladies that surround them.

But if it be true, that the diseases of the United States are different, in their nature and character, from those of other countries, they necessarily call for a corresponding difference in their modes of treatment. Hence, in foreign publications, we search in vain for able lessons of practical instruction, relative to the diseases of our own country. And hence European physicians, on their first emigration across the Atlantic, are incompetent to the successful treatment of the complaints they have to encounter. They are obliged to abandon many of the practical doctrines taught in the schools of their native country, and either embark on the hazardous sea of experiment, with nothing but their own observation for their guide, or avail themselves of the experience of the physicians of America. The modes of practice inculcated in the schools, and detailed in the medical writings, of Europe, are too feeble to arrest the course of the gigantic forms of disease, which frequently appear in the United States. They are suited only to the complaints of a climate free from extremes, or of a people enervated by luxury and an excess of labour. It is American practice alone, that appears to be accommodated to the character and strength of American diseases.

But it is not only by a difference in the character, form, and violence of her diseases, that the United States is distinguished from the countries of Europe. She is, perhaps, no less distinguished by the peculiarities of her native remedies. Though we do not mean to assert, that such is the balance between physical good and evil, that diseases and their antidotes uniformly spring from the same soil, yet perhaps the sentiment ought not to be too hastily rejected. It is unquestionably true, that certain countries remarkable for their diseases, have enriched the materia medica with some of its choicest articles. But be this as it may, we have seen enough to convince us, that, in a medical point of view, the bosom of our country has not been so bounteously peopled with vegetables to no purpose. The plants of the new world possess balsamic and healing virtues no less active, and in no less abundance, than those of the old. But, as yet, these virtues have not been so completely developed, nor their form and mode of exhibition in medicine, so accurately ascertained. It is still in the fields and forests of the western hemisphere, that the lines of the poet are most emphatically realized.

> " Full many a flower is born to blush unseen,
> " And waste its virtues on the desert air."

Nothing but time and attention are necessary to draw many of these plants from their native obscurity, and render them alike subservient to the purposes of ornament and health.

Such are a few of the principal considerations that have led to the projection of the work, which is herein offered to the patronage of the public. If it be true, that we are subject to forms of disease different from those that prevail in other countries, and requiring modes of treatment not to be learnt from foreign publications; and if it be further true, that we possess many domestic remedies exclusively our own, it is in the medical productions of our own country that such peculiarities should be faithfully recorded. As it is in British works that we must search for accurate accounts of British diseases, British remedies, and British practice, so it is in American publications alone, that a system of medicine truly American must be ultimately found. To continue in a state of medical pupilage to our parent country, long after the expiration of our national minority, is unmanly and humiliating. It is on our own talents and exertions, and not on those of foreigners, we must rely for the final accommodation of the healing art to the diseases of our country. As well might we look to the acts of the British parliament for laws suited to the political condition and exigencies of the United States, as to the writings of British, or any other foreign physicians, for authorities to govern us in the practice of medicine. The same motives of self-preservation, interest, and national pride, which urged us, as a people, to the attainment of political independence, at an incalculable expense of wealth and blood, call on us to assert, with equal firmness, our independence in relation to the healing art. As it is to the former, that we are principally indebted for the eloquence and lustre of our present statesmen, so it is the latter alone, that can ever raise the physicians of the United States to a corresponding grade of eminence in their profession. Inferiority is the natural concomitant of a state of dependence.

But I would not be here supposed to insinuate, that we ought to live and write exclusively for ourselves. The medical publications of the United States should be calculated for other purposes than merely to shed light on the peculiarities in the diseases, remedies, and modes of practice of our own country. To restrict them within such narrow bounds, would be equally selfish and degrading. The authors or editors of them ought to aspire to loftier pretensions, and aim at nobler and more liberal ends. The healing art is still in a state of minority, and the physicians of the United States would subject themselves not only to censure, but contempt, were they ever to resign their part in the honourable work of rearing it to manhood. On the score of medical knowledge, we have long been borrowers from the schools and presses of Europe. As the terms granted us have been liberal and advantageous, it is time for us to think of reimbursing the loan. Nor are we altogether unprepared to cancel, in part, our weighty obligations. Already have certain new lights in medicine broken forth in the United States, and extended their lustre across the Atlantic. Already have the writings and teachings of some of our countrymen obtained the approbation, and even commanded the respect and admiration, of the physicians of Europe. Nor is this all: on various points of medical science, they have conveyed to them new and important instruction. This appears to be particularly the case with respect to the philosophy and treatment of febrile diseases, more especially those of a malignant and pestilential nature. If I am not greatly misled by that partiality which every man feels, and perhaps ought to feel, for his native country, these subjects are better understood in the United States, than in any other quarter of the globe. To embody, therefore, the collective knowledge and experience of our country, on these and other points intimately connected with the health and welfare of mankind, and communicate them to distant nations, should constitute part of the intention of all our periodical publications in medicine. To this we are urged no less by a sense of duty and humanity, than by our pride and love of fame, as a people.

The second division of our subject, namely, to give a statement of the matter which the proposed publication is intended to embrace, and of the manner in which it is to be conducted, comes now to be considered. Under this head it will be impossible to speak otherwise than in very general terms. It will be sufficient, briefly to mention, that all the subjects treated of, shall belong either strictly to the science of medicine, or to some one of its collateral and subservient branches. I need not mention that the most distinguished of these branches are, Chemistry, Botany, and Natural History. Whatever may appear calculated to throw useful light on the Materia Medica of our country, shall receive particular attention. From the extensive range of objects which this division of medical science embraces, it is reasonable to suppose, that in the cultivation of it, discoveries will be made, applicable to some of the subordinate arts. Though our primary aim, therefore, will be the improvement of medicine, we may with much certainty calculate on the accomplishment of certain secondary purposes of general utility, by contributing to the improvement of the arts and manufactures of the United States. Should biographical sketches of distinguished physicians be occasionally admitted, it is hoped that they will not be deemed altogether foreign from the design of the work.

In selecting matter for the proposed publication, though plausible hypotheses and ingenious speculations will not be unconditionally rejected, yet a preference will be always given to inquiries and dissertations of practical utility. It would be as unreasonable and unnatural to renounce entirely the pleasures of theory, as to endeavour in the spring, to bar the senses against the beauty and fragrance of reviving nature. On subjects, however, where experiment and observation can be had recourse to, as tests of truth, mere hypothesis will be admitted with caution. As the Editor will probably annex to each volume, in the form of an appendix, one or more memoirs written by himself, he thinks it necessary to remark, that these are the only papers for the truth of which he will hold himself responsible. The authenticity of all others must rest entirely on their own internal evidence, and the reputation of their authors.

It remains for the Editor to give some account of the resources, by the aid of which he expects to be able to render a periodical publication interesting and useful. From the title of the work it must have been already perceived, that his principal reliance will be placed on the Inaugural Dissertations, published under the auspices of the University of Pennsylvania, and other medical schools in the United States. Of those productions, the annual number issuing from the first of these institutions alone, ranges from fifteen to twenty, each one consisting of from thirty to sixty or eighty pages octavo. Out of this number it rarely occurs, that less than six or eight, and sometimes more, are well worthy of being preserved from that fate, which awaits most works that fall from the press, in the form of pamphlets. These, with a short appendix, consisting of original papers, which can be subjoined without any difficulty, will afford matter sufficient for an annual volume. The Editor flatters himself, that it is unnecessary for him to observe, that, as far as his judgment may be relied on, the merit of each Thesis, in whatever part of the United States it may have been published, shall be the only ground of its admission into the work. In making a selection, no personal or local considerations, shall have any part in determining his choice.

To such persons as may be strangers to the University of Pennsylvania, and therefore, not qualified to judge what anticipations are to be indulged respecting a work consisting in part of Theses, published under its auspices, the following observations may not be unacceptable.

That celebrated School of Medicine, emphatically the Leyden of the West, is annually resorted to by pupils from all parts of the United States, as well as occasionally from the West-India islands. As these gentlemen have all studied the rudiments of their profession, under the direction of private preceptors, each one of them brings along with him some share of medical knowledge. Many of them have even treasured up original and useful facts, collected by personal observation, respecting the soil, climate, vegetables, minerals, states of society, and diseases, of their respective places of abode. So constant, liberal, and familiar is the intercourse between them, during their continuance in the University, by means of private conversation, and public debates in societies, to which they attach themselves, that their medical acquirements are converted into what may be regarded as a common stock. The knowledge of each individual is surrendered up as the property of the whole, while the knowledge of the whole becomes in return the property of the individual. In this way, may each student acquire important information respecting the diseases, remedies, and modes of practice, that prevail in every part of the United States, from the province of Maine to the island of New Orleans, and from the shores of the Atlantic to the banks of the Mississippi. On this common stock, which includes also the knowledge and experience of the different professors, as detailed in their public lectures, the candidates for medical honours are privileged to draw for materials to compose their Inaugural Dissertations. The doctrines and opinions, therefore, contained in these dissertations, are not, as might be supposed, the crude offspring of juvenile minds. They are to be regarded, for the most part, as the opinions and doctrines of the University, or some of its professors, illustrated and amplified by the graduates themselves.

Such is the general outline of the work, to which the attention of the public is solicited. Composed of materials derived from a source so extensive, distinguished, and respectable, the editor cannot, for a moment, doubt of his fellow-citizens concurring with him in opinion, that, if judiciously conducted, it will prove both useful and honourable to our country. As medical characters from all parts of the United States, will occasionally contribute to enrich its pages, it may be viewed in the light of a national production. Though it is not to issue from the press under the immediate sanction of the University of Pennsylvania, yet it will constitute a "*Thesaurus Medicus*" of that institution. It is the present determination of those concerned in the work, to continue it annually, as long as the public may deem it worthy of patronage.

The Editor will only add, that although he has promised to annex to each volume, an appendix written by himself, yet this engagement shall not operate to the exclusion of any valuable papers, with which he may be favoured by his correspondents, either at home or abroad.

Philadelphia, January 1st, 1805.

AN INAUGURAL DISSERTATION:

BEING

AN ATTEMPT

TO DISPROVE THE DOCTRINE OF THE

PUTREFACTION OF THE BLOOD

OF

LIVING ANIMALS.

SUBMITTED TO THE EXAMINATION OF THE

REVEREND JOHN EWING, S. T. P. PROVOST;

THE TRUSTEES, AND MEDICAL PROFESSORS OF THE UNIVERSITY
OF PENNSYLVANIA,

FOR THE DEGREE OF DOCTOR OF MEDICINE;

ON THE EIGHTH DAY OF MAY, A. D. ONE THOUSAND SEVEN HUNDRED AND NINETY-THREE.

BY ADAM SEYBERT, OF PHILADELPHIA;

HONORARY MEMBER OF THE PHILADELPHIA, AND MEMBER
OF THE AMERICAN, MEDICAL SOCIETIES.

"FOR THE LIFE OF THE FLESH IS IN THE BLOOD."——LEVITICUS.

INTRODUCTION.

THE opinion, that the blood became putrid in many diseases, is of ancient date. It has been embraced, with various modifications by most of the sects in Medicine. It particularly engaged the attention and belief of the learned Boerhaave, who publicly taught it in the celebrated school of Leyden. Under the imposing authority of his name, and by means of his industry, it was diffused throughout almost the whole globe. It became the theme of the vulgar; and at this day has many great and illustrious names to support it.

There have been, however, at different times, a few medical philosophers, who, although they adopted it in some instances, seem to have had doubts respecting the truth of the doctrine, and to have rejected it in their explanations of many diseases. In this class may be reckoned the celebrated Hoffmann, who explained the phænomena of many disorders without any regard to the state of the blood. He was followed in this by the great Cullen, who, in his system, attends less to the state of the fluids in diseases than any of his predecessors. The doctrine has of late been wholly denied by the very ingenious Dr. Milman, in his Treatise on the Scurvy; and entirely rejected by the late sagacious Dr. Brown, of Edinburgh, from his System of Medicine. Many physicians appear to have adopted the sentiments of these gentlemen; so that at present there is a diversity of opinion among medical philosophers respecting the putrefaction of the blood of *Living Animals*.

On examining the subject, I found that it had never been put to the test of fair experiment; but, that speculative reasoning (too often delusive at best) and a few indecisive facts, were the chief supports of the argument on both sides. With these data, the result was neither satisfactory nor convincing. I then determined to contribute my mite towards investigating the matter by experiment. In the prosecution of this attempt, my attention has been directed to the advancement of science; and, in the detailing of the experiments which I have made with this view, truth shall be my polar star.

I have been induced, from reflection and experiment, to adopt a positive belief on this subject, and to deny the truth of the doctrine which I have just noticed. I am, therefore, necessarily led into an opposition to the opinions of many celebrated men. But, I trust, I have observed a decent regard and veneration for those from whom I dissent, without being at all depressed by the weight of their authority. Having viewed nature *attentively*, I shall endeavour to communicate *accurately* what I have observed. I will not sacrifice truth to the lustre of great names, but, with confidence adopt the sentiment of Cicero—"Mea fuit semper hæc in hac re voluntas et sententia, quemvis ut hoc mallem de iis, qui essent idonei, suscipere quam me: me, ut mallem quam neminem."

INAUGURAL DISSERTATION.

IT will be proper, before I enter upon the consideration of the Putrefaction of the Blood of Living Animals, to take a brief view of *Putrefaction in general;* for, this is, indeed, the only method by which we can be prepared for an examination of the circumstances necessary to induce that state of the blood in living animals.

Though chemistry is much improved, and numerous discoveries are made almost every day by different philosophers, *Putrefaction* is, at this moment considered as the same difficult subject, that it was in the days of the celebrated Lord Bacon.

This last stage of fermentation, in the days of Stahl, was supposed to be a mere consequence of the *vinous* and *acetous* stages; but, modern discoveries teach us the contrary; for we find, that while some substances undergo only the last stage, others suffer the three successive changes in a regular manner: thus mucilages, &c. become acid without undergoing the vinous fermentation, and the glutinous matter of vegetables will putrefy before it undergoes either of the other changes.

Observation, the grand parent of discovery, has taught us, that no substance is capable of undergoing a change by the putrefactive fermentation, except it be *animal* or *vegetable;* and that the numerous classes of the productions of nature, comprehended under the title of the Mineral Kingdom, are excluded. It is also an opinion, generally established, and proved by experiment, that the fluid and softer parts of those bodies, putrefy much sooner than the harder and more solid parts. It has likewise been observed, "That the flesh of younger animals is somewhat more prone to putridity, than that of older animals."*

Animal and vegetable matters cannot putrefy in every situation or condition in which they may exist; for it is necessary that a living animal or vegetable should undergo a considerable change, before it can be rendered capable thereof: it must even be deprived of life, or the vital principle. No one has ever seen an entire animal or vegetable putrefy whilst

* Medical Commentaries, vol. ii. p. 142.

alive; and Beccher, on this subject, beautifully observes: "Causa putrefactionis primaria defectus spiritus vitalis balsamini est." And, indeed, in all researches into those kingdoms which are the subjects of *fermentation*, it is of so great consequence to keep this universal actuating principle in view, that by neglecting it, we may commit great mistakes, and look to other causes than the true ones for its palpable effects; insomuch that the learned Chaptal, when regretting the imperfect success which chemistry has met with, in the analysis of animal matters, cannot help observing it. "All (says he) have mistaken or overlooked that principle of life which incessantly acts upon the solids and fluids, modifies, without ceasing, the impression of external objects; impedes the degenerations which depend on the constitution itself; and presents to us phænomena which chemistry never could have known or predicted by attending to the invariable laws observed in inanimate bodies."*

The presence of that invisible elastic fluid, which we term *vital air*, is so necessary to putrefaction, that a body cannot putrefy without being in contact with it; and may be preserved sound and pure for years, if the communication between them be destroyed. It is a well known fact that a body will not putrefy *in vacuo*. This has been noticed by an ingenious author, in the following words: "How much the air contributes to putrefaction, is evident hence, that bodies buried deep under the earth, or in water, out of the reach of air, shall remain for ages entire; which yet, being exposed to the open air, shall soon rot and moulder away."†

It appears that too great a degree of moisture, or a total want of it, retards the process of putrefaction. In order, therefore, that a body may putrefy, it is necessary that it be only duly moistened. Thus it happens, that after an animal or vegetable substance has been made perfectly dry, it may be preserved, in that state, for many years after. It has been observed by the immortal Beccher, that too great a degree of moisture prevents putrefaction: These are his words: "Nimia quoque humiditas a putrefactione impedit, prout nimius calor; nam corpora in aqua potius gradatim consumi quam putrescere, si nova semper affluens sit, experientia docet: unde longo tempore integra interdum submersa prorsus a putrefactione immunia vidimus; adeo ut nobis aliquando speculatio occurreret, tractando tali modo cadavera anatomiæ subjicienda, quo diutius a fœtore et putrefactione immunia forent."‡

That *all* enlivening principle, *heat*, which, in a certain degree, is necessary to life, is no less necessary to the bringing on of the dissolution of a body. Temperature has been found to have great influence in promoting and retarding putrefaction.

* Chaptal's Chemistry, vol. iii. p. 280.

† Frewen's Physiologia, p. 128.

‡ Phys. Sul. lib. 1. s. 5. cap. 1. p 277.

I have now mentioned the most essential circumstances necessary to promote the inception of putrefaction; to which we may subjoin *Rest;* for bodies do not putrefy while in continual motion. In proportion as these circumstances take place the process will advance with greater or lesser rapidity.

Certain substances, as well known to the vulgar as to the philosopher, by the name of Ferments, when added to a fermentable mass, are found to hasten the process in a manner truly astonishing; though both the peasant and the philosopher stand on an equal footing with regard to a knowledge of the principle by which their application produces a specific operation. "We are told indeed (says the ingenious Mr. Henry) that a vinous ferment induces the vinous, that a ferment of an acetous kind brings on the acetous fermentation, and a putrid one, that fermentation which ends in putrefaction. But we receive no more information, relative to the manner in which they produce those effects, than we do with regard to fermentation itself."*

I will now proceed to examine, whether in becoming putrid, a body undergoes any considerable change; and, whether it be possible to remove putrescency after it has taken place.

The very meaning of the word *putrefaction* conveys the idea that an essential change must have taken place in any substance which has acquired a putrid state.

Putrefaction reduces both animals and vegetables to the same principles, for, it causes an entire and complete decomposition of them, insomuch, that it is difficult, and indeed impossible, to distinguish between a putrefied mass of animal and one of vegetable matter. The former characteristics of each are at an end. Colour, texture, and every sensible quality of the body, are thereby destroyed. It reduces the animate part of the creation to an indiscriminate level with inanimate matter. There is abundance of truth in the observation, that bodies, in this process, undergo a new combination, as well as separation of their constituent parts. Putrefaction causes the sweetest substances to become the most offensive and disagreeable to the smell; and, instead of a vegetable acid, at length produces a volatile alkali—bodies possessing very opposite principles.

Many philosophers have gone so far as to say, that, by certain chemical processes, they were able not only to render putrid substances sweet again, but restore them to the condition they were in, previously to their undergoing this peculiar change. This, according to them, is to be effected by surrounding the putrefied body with an atmosphere of fixed air; to the loss of which principle, an enlightened philosopher wholly attributed the changes produced in a body by putrefaction; though the opinion has been

* Manchester Memoirs, vol. ii. p. 259.

ingeniously refuted, and therefore needs not much consideration in this place.*

It is true, that by surrounding a putrefied body with fixed air, we shall prevent the advancement of the process; yet the fixed air has no specific operation in this respect, for other substances possess a similar property. After I had suffered several pieces of highly putrid beef to remain completely covered with fresh pump water for several hours, and then washed them frequently in water several times renewed, I found, that by this operation the water gained a highly offensive and putrid smell, and that the meat had lost a great degree of its own; but its solidity was not in the least restored. It is a common and well-known fact in domestic economy, that meat, which has become tainted, is very frequently washed in fresh water before it is cooked; and the reason assigned for this process is, that the meat is thereby rendered sweeter. Thus do house-keepers reason from a knowledge of the fact without any theory to bias them.

Hence it appears very clear, that to explain this fact no specific operation is necessary to be recurred to, since water, free from the combination of fixed air, had effects similar to those produced by fixed air itself. Mr. Chaptal, in my opinion, explains the operation of this, and of the other substances which are said to have the like effect, upon very simple and philosophical principles. When speaking of the necessity of the presence of vital air to putrefaction, he says, "We shall observe, on this subject, that the effects observed in flesh exposed to the Carbonic acid, Nitrogene gas, &c. are referable to a similar cause; and it appears to me that it is without sufficient proof that a conclusion has been drawn, that these same gases, internally taken, ought to be considered as antiseptic; because, in the cases we have mentioned, they act only by defending the bodies they surround from the contact of vital air, which is the principle of putrefaction."†

To me it appears plain, that, so great is the chemical change and decomposition which a putrid body has undergone, that no means can remedy it. On this point Fontana truly observes, "We do not know any power, nature herself does not disclose any, that can recompose an organ that is destroyed, and entirely decomposed by putrefaction, or by the concussions of external bodies. This is what has never yet either been accomplished or seen. We have, therefore, every possible reason not only to believe an animal that is reduced to this state dead, but likewise to believe it dead for ever."‡

From the above consideration of putrefaction, as going on out of the body, it appears, that a certain degree of heat, the presence of vital air, a

* Medical Commentaries, vol. ii. p. 150.

† Chaptal's Chemistry, vol. iii. p. 398.

‡ Fontana on Poisons, vol. i. p. 406.

certain degree of moisture, together with rest, are indispensably necessary to putrefaction; and that without their presence no body can putrefy. We also know that by adding certain substances to bodies which are to undergo the change, the process is hastened in a manner surprising to every one who has ever had occasion to notice it.

Having considered the circumstances in which fermentation, in general, takes place, as it is conceivable that if the above circumstances be present in the living animal, the process might readily go on in it—let us examine if these necessary requisites be present, so as to act in a due and proper manner upon that vital fluid, *the blood*.

Several questions occur here.—Is there any difference between dead and living matter?—Is there not in living animals a positive power of resisting putrefaction?—Whether the nice and inexplicable operation which is employed in converting the dull mass of the motionless creation into the peculiar condition of organic sensibility, acts to no purpose?—In fact, whether the principal characteristic of death, has not been determined by physiologists to be the beginning of putrefaction in the body?

The principle we denominate *vital*, with whose effects we begin to be acquainted, though its nature and origin will perhaps ever be hidden from us, is found to be the chief impediment to the putrefaction of a living animal. As soon as the animal is deprived of it, putrefaction is the inevitable consequence, except the above precautions be well observed. This may be fairly demonstrated. All the circumstances necessary to putrefaction take place with respect to the external surface of our bodies, yet they do not putrefy while alive; but if life be by any means destroyed, they will as readily undergo the process as other inanimate matter.

It is to be observed, that by the living principle is understood that power which in an animal actuates its whole system, or from which is derived sensation, motion, and the other qualities of life. It is the cause of the preservation of the body from dissolution, and is capable of existing, for sometime, under a suspension of all its actions.*

We will now proceed to consider the presence of *heat*, pure *air*, *moisture*, and *rest*, with regard to the blood; in order to determine whether we might expect a putrefaction of that fluid, if the principle of life were not alone sufficient to obviate it.

1st. Of Heat. Though different degrees of heat are found sufficient to maintain life, in different animals, yet no animal while alive has ever been found devoid of a considerable quantity of it; indeed, so careful was Nature in this respect, that she has endowed man, and the inferior animals, with a power, whereby they are capable of generating heat;—a process, the investigation of which has of late much engaged the attention of philosophers, and upon which considerable light has been thrown. It is pre-

* Gardiner's Animal Economy, p. 3.

bable that the blood of every living animal contains a degree of heat sufficient to support the process of putrefaction.

2dly. Pure Air. That element so necessary to our existence, and which we receive into our bodies, by means of those vital viscera, the lungs, is no less necessary to the maintaining of animal life, than to the process of putrefaction. A man will no more live, than a dead body will putrefy, in vacuo.*

It was for a long time supposed that elastic air existed in the blood-vessels of living animals; but, the experiments of the ingenious Darwin clearly prove the contrary; the following is one of them. "Part of the jugular vein of a sheep, full of blood, was included between two tight ligatures, and cut out while the animal was yet alive. It was immediately put into a glass of warm water, and placed in the receiver of an air pump. It sunk at first to the bottom of the water, and did not rise again, although the air was carefully exhausted. After this, it was wiped dry, and laid on the brass floor of the receiver. The air was again exhausted, but there was not the least visible expansion of the vein or its contents."†

By the experiments above referred to it is reduced to a certainty, that no air exists, formally in the blood, while enclosed in the blood-vessels. And it is evident, that it was without sufficient grounds, that philosophers inferred, that air existed in the blood, while enclosed in the blood-vessels; because they perceived it in blood drawn from a vein, and placed in the receiver of an air pump; for during its exposure it must have had time to absorb air from the atmosphere.

The celebrated Huxham was of opinion, that "elastic air is probably generated in the arterial and venous systems, in putrid fevers,"‡ though he has no experiments to prove it. Let us then examine, whether air can exist in the blood-vessels of living animals.

With this view the ingenious Luzuriaga tried many experiments on living dogs. He injected several different kinds of air into the blood-vessels, and in every instance the animals were killed, in a very short time. I will only mention that he twice injected inflammable air; once phlogisticated air; once fixed air; once nitrous air. I will state the particular circumstances that happened in the instance, when dephlogisticated or pure air, which is the chief agent of putrefaction, was injected.—It was forced into the jugular vein of a dog, and in three minutes he died. On dissection,

* That air is a very active and powerful agent in putrefaction, is evident from the following fact, viz. Whilst pus remains shut up in a perfectly close cavity it will keep sweet and inodorous, but on exposure to the atmosphere, it contracts in a very short time a putrid smell; the same circumstance takes place with regard to extravasated blood.

† Medical Commentaries, vol. vi. p. 35.

‡ Medical Observations, vol. iii. p. 36.

the blood appeared of a lively red colour, and frothy, but not grumous nor coagulated.*

Thus it appears, that air does not, and cannot exist, formally, in the blood-vessels of a living animal; a circumstance which refutes the opinion, that the blood is capable of undergoing the process of putrefaction, during the life of the animal.

3dly. Moisture. No person in his senses dares to deny the presence of this third circumstance, as essential to putrefaction, and therefore it needs no further consideration.

4thly. We have mentioned that it was necessary for a body, which is to undergo the putrefactive process, to be at rest. The continual motion of the blood of living animals, must certainly be a great impediment to the process; and indeed motion has been found to be a very effectual means of preventing it; even a brisk wind has been known to retard it.†

We have found that the life of the animal, the want of air in the blood-vessels, and its circulatory motion, were great impediments to the putrefaction of the blood; we shall now proceed to consider, whether the principle of animal life is contained in the blood.

The situation of that principle, to which we owe our existence as living beings, is at present much disputed. While some physiologists will have it to exist wholly in the nervous system, in the form of a subtle fluid, whose presence the best microscopes have not been able to discover; there are others, whose authority is by no means inferior, who strenuously oppose the doctrine, and persist that blood partakes of it also. In this latter class we may rank the celebrated Hunter and Fontana.

That the life of the animal exists in the blood, is an opinion of as ancient a date as Holy Writ itself: it was the favourite sentiment of many ancient philosophers; and the great Harvey, to whom we are so much indebted, says, "the blood is the primum vivens, and the ultimum moriens, of the animal."

This hypothesis was for sometime sunk into oblivion, but was again revived and placed on a firm basis by the able Mr. John Hunter, of London; who supports the doctrine by reasoning truly philosophical, and by experiments, in my opinion, incontrovertibly convincing. It would be needless for me to repeat all the ingenious arguments which are made use of by him on this occasion, as they may be easily seen, by referring to the Encyclopœdia, vol. iii. p. 313.

His 5th argument alone would almost suffice to convince me of the truth of his opinion. "The blood (says he) preserves life in the different parts of the body. When the nerves going to a part are tied or cut, the part becomes paralytic, and looses all power of motion; but it does not

* Luzuriaga's Inaug. Dissertat. Eden. 1786, p. 26.

† Medical Commentaries, vol. ii. p. 146.

mortify; if the artery be cut, the part dies, and mortification ensues." What keeps it alive in the first place? Mr. Hunter believes it is the living principle which alone can keep it alive; and he thinks that this phænomenon is inexplicable on any other supposition, than that life is supported by the blood.*

This doctrine is every day gaining ground; and appears to be further supported by the opinions and experiments, of the celebrated Fontana; who observes, "What may lead one to suspect, that a very active and volatile principle does really exist in the blood, is, that the viper's venom prevents its coagulation when it is drawn from the vessels, and on the contrary, produces it in the vessels themselves. One would suppose (says he) in the first case, that something had flown off from the blood, which exists in it when it is enclosed by the vessels."†

"My experiments on animals (continues this last mentioned enlightened philosopher) in which the nerves were bit by vipers, shew that the venom is a substance perfectly innocent to these organs, that it does not occasion in them any sensible change, and that they are not even a means or vehicle of conveying it to the animal. In a word, it appears that the nervous system does not concur more to the production of the diseases of the venom, than does the tendon, or any other insensible part of the animal: on the other hand, all the experiments on the blood, the injection of venom into the vessels, and so on, constantly evince that the action of the venom of the viper is on the blood itself. This fluid is alone changed by the venom, and this fluid conveys the venom to the animal, and distributes it to its whole body. The action of the venom, and its effects on the blood, are almost instantaneous."‡

His experiments, in my opinion, allow this inference, that since life was not destroyed by the immediate application of the poison to the bare nerves, and that the loss of it was almost instantaneous when the poison was applied to the blood; there exists some quality in the blood, that does not exist in the nerves; and as death can only be produced by destroying the vital principle, this principle must consequently exist in the blood, and in a state different from that in which it exists in the nervous system.

I will now proceed to give a connected view of the reasons, which, together with the experiments hereafter to be mentioned, induce me to deny the putrefaction of the blood to have ever taken place, and which confirm me in believing, that no such change can happen in the blood of living animals.

* Encyclopædia Loc. citat.
† Fontana on Poisons, vol. ii. p. 135.
‡ Ibid. vol. i. p. 396.

sected immediately after death. It has been fairly proved that air, on being injected into the vessels, kills in a very short time.*

8thly. According to the ideas of the supporters of the putrefaction of the blood in diseases, the process must be different in typhus, &c. from what it is in scurvy; since, in the former case, bark and wine, together with other stimuli, are the only true remedies; whereas, according to them, these remedies are found to be of little or no service in the latter, and fresh vegetables are its only remedies. For the blood, they say, is putrid in both instances; and since putrefaction going on in the same substance would appear to be always the same, we might, with propriety, suppose, that what retards and corrects it in the one instance, would do it in the other. Who has thought of exhibiting such vegetables, in typhus, yellow fever, &c. and neglecting those powerful remedies, bark and wine?

9thly. It is incompatible with sound reasoning to suppose that putrefaction of the blood takes place in the plague, &c. since we find that persons sometimes fall down dead, immediately on being exposed to the effluvia, that have been said to give origin to this and other diseases of the same class; as the porters, who opened bales of goods in the lazarettos of Marseilles. Mead relates, that, "Upon opening one of the bales of wool in a field, two Turks employed in the work were immediately killed, and some birds which happened to fly over the place, dropped down dead."† How is it possible that the mass of blood, or any part of it, could become in the least degree putrid in those instances? For the space of time, before death was produced, was too short to allow of it. Since the poison kills so quickly, can we with reason suppose that an animal can live while the fountain of life is impregnated with it? If a small portion of such effluvia produces disease, when its action may, exclusively, be supposed to be merely on the external surface of the body, is there not strong ground for believing that instant death would follow if the blood were in the least tainted by it?

10thly. Experiments seem to prove, that the blood is peculiarly qualified to excite motion in the heart and arteries, and that no other fluid will answer the same purpose.—That even milk, and other bland liquors, when injected into the vessels of living animals, kill. We know it is an established truth, that by putrefaction bodies undergo a complete decomposition and dissolution of their constituent parts, and are rendered totally different from what they were before. How then can we imagine that life can be kept up whilst the blood is in a putrid state in the vessels?

For these reasons I concluded that a putrefaction of the blood could not take place in the living body; but the importance of the subject induced me to engage in a series of experiments which might enable me,

* Vide ante, page 10.

† Mead's Works, p. 198.

with more indisputable certainty, to decide the question, Whether the blood of living animals can be rendered putrid?

And this I supposed, if possible, would be ascertained by the following circumstances, viz.

I. By Starvation.

II. By a putrid Diet. And,

III. By injecting putrid and other substances into blood-vessels.

1st. Of Starvation. When we consider the many and various actions performed by man and other animals, we plainly see why a certain quantity of aliment is necessary to be taken by them. Physiology teaches, that by every action man suffers a loss of the solids, and that by every secretion the blood becomes less in quantity.

The blood being the fountain from whence all the solid parts derive their support, and the fluids their origin, it must naturally have been supposed to be affected by starvation, as it is immediately prepared from the aliment we take in.

If the case be as above stated, is it not surprising that man can live for a length of time without taking any nourishment at all? There are instances related where men have lived for six, twelve, and even fourteen days, or longer, without receiving any aliment.*

Drink is found to be no less necessary to the maintaining of animal life than the solid matters of our food. The necessity of this article was supposed to be merely in recruiting, blunting, and correcting the acrimony of the blood which would take place if we did not use them. And it has been found by experiment, that life may be supported a much longer time by water than it could without it. Rhedi, who made experiments to ascertain how much longer life might be supported by the use of drink, without the use of any other article of diet, than when the animal was totally deprived of all food, found, that fowls to whom he gave no drink lived until the ninth day; whereas one who was allowed water, lived more than twenty days.†

Though man may live for a considerable length of time without taking in any nourishment, yet his situation is not agreeable; for, he not only suffers pain, but all the functions, as well bodily as mental, are in consequence affected; his sensations are strange, his ideas confused, his sight deceptive and deranged, his countenance becomes pale and sallow, his weight decreases considerably, though the excretions are sparing or none at all, the respiration suffers, the action of the heart decreases in proportion, till at last no pulsation can be felt, debility by degrees overpowering life, death approaches and puts an end to his sufferings.

* Manchester Memoirs, vol. ii. p. 467. and seq.

† Ibid, vol. ii. p. 575.

freely, but by misfortune an artery was opened instead of a vein. The colour and smell of the blood were quite natural, being highly florid like arterial blood. It was coagulated at 3 minutes after 12—separation into serum and crassamentum was evident at 13 minutes after 12. The serum and crassamentum appeared perfectly natural. A piece of the blue stained paper was dipped into the serum, and no change of colour was produced. To prevent a further hæmorrhage, as he was much weakened by the bleeding, the wound was well closed, and a piece of spunge bound tightly over it. The leg became paralytic.

August 13th. The leg was much swollen below the ligature, and when handled did not appear to give him any pain. The ligature was somewhat loosened.

August 14th. Weighed 4 1-2lb.

August 15th. This morning at 8 o'clock I found him dead.

During the above management the dog had very few evacuations by stool or urine, till the two last days of his life, when the urine dropped continually from the urethra.

His eyes were several times examined, and no alteration in them was evident.

He did not appear to suffer pain till the 3d of August, at which time he cried very much, gaped frequently, and appeared very weak; weakness continuing daily to increase till his death.

I did not perceive that respiration was much affected; but the action of the heart became so feeble, that I could scarcely perceive its beating for some days previous to his death, though I applied my hand to the part where its stroke is generally felt.

The abdomen was much contracted, and the fore-part of it was drawn up to the spine.

The body was examined immediately on finding him dead, when appearances were as follow:

The stomach contained a considerable quantity of a whitish fluid, its texture was perfectly natural. The intestines were of a greenish cast. The other viscera appeared in a sound and natural state.

At 39 minutes after 8 o'clock, blood was caught in a tumbler from an opening made into the heart; colour and smell natural. It was coagulated at 45 minutes after 8. It began to separate into serum and crassamentum at 51 minutes after 8. Coagulum and serum perfectly natural. Serum did not change the paper stained blue.

EXPERIMENT III.

April 10th, 1793, a dog was kept for the purpose of *starvation.*

April 20th. Blood was drawn from him at 15 minutes past 3 o'clock; the colour and smell were natural—it coagulated at 20 minutes after 3, and separated into serum and crassamentum at 35 minutes after 3. The serum was not in the least acrid to the taste.

April 26th. Blood was drawn at 33 minutes past 3 o'clock. The colour and smell were natural—it coagulated at 38 minutes past 3, and separated into serum and crassamentum at 53 minutes past 3. The coagulum and serum were natural. The serum was not in the least acrid to the taste.

2dly. *Of Putrid Diet.* The necessity of our taking aliment being fully established, Nature, with her usual wisdom, has given man and the other animals an appetite for it. She has also provided different kinds of aliment in the different climates, in a proper quantity, so that each might have what was most suitable and agreeable to his particular condition.

To this purpose the appetites of the inhabitants of the different regions vary; while some prefer a vegetable, others according to the situation, with more pleasure and benefit to their health, derive their chief support from the same class of beings to which they themselves belong, though of different genera and species.

Animals are styled herbivorous, carnivorous, and omnivorous, from the particular kind of aliment they make use of: thus sheep live wholly on a vegetable diet, while the wolf and other animals of the same species are found to support themselves best by a diet which is entirely animal; and man, from the peculiar structure of his teeth, appears to be destined to take in all kinds of alimentary matter, and thus he is found to live best on a diet, composed of animal and vegetable substances. Experiments would seem to prove that an animal, who is naturally herbivorous, may be made to live entirely on flesh, although not so conveniently. A mixed diet, as already mentioned, best suits the appetite and peculiar state of man, but there are instances where he entirely lives on vegetables; and on the contrary, he has, in other situations, been found to live wholly on the inferior animals. While the mixed diet renders him placid and fit for every purpose, for which nature formed him, one entirely animal renders him ferocious as the brute, and that altogether vegetable renders him weak and feeble.

Among civilized nations, aliment, before it is eaten, always undergoes some kind of preparation, whereby it is rendered more or less fit to be the subject of digestion.

Aliment, after being received into the stomach, remains there for some time, and undergoes a considerable change, before it quits that viscus; for it thereby becomes fitted for the forming of the chyle, from which the blood itself is immediately prepared.

The state of the chyle, and consequently that of the blood, is by many supposed to be much influenced by the aliment we take; thus a putrid and bad diet is supposed to produce vitiated and putrescent chyle, and consequently, in their opinion, the blood is tainted in the same manner. This opinion probably took its rise from the flesh of certain animals tasting similar to the food they eat; thus sea-fowls, we know, have a fishy taste; pidgeons who have fed on poke-berries for sometime have their flesh tinged with the colouring matter of that vegetable; and the flesh of the pheasant who has lived upon laurel-berries is capable of communicating the deadly effects of that active poison to the human system. These, and perhaps other like circumstances, were the means of giving rise to the opinion, that the blood of animals must be affected differently by different articles of diet, and that corrupted or putrid food would be the means of producing a putrefaction of the blood. As long since as the days of Hippocrates, physicians have been of opinion that water and corrupted meat were the true causes of putrid diseases. In this manner was the plague, scurvy, &c. thought to have been produced; but the sentiment of many moderns is, "That scurvy arises from the want of a due quantity of alimentary matter in the food of those who are afflicted with the disease." This opinion is supported by the two cases of scurvy related in the Medical Transactions by Dr. Milman, and also by Dr. Stark's Dietetic Experiments.*

To convince myself whether, the state of the blood was affected by a putrid diet, the following experiments were performed.

EXPERIMENT IV.

July 29th, 1792. A dog was put upon a diet of putrid meat and putrid water, and continued to feed thereon till the 27th day of August following. The meat was beef, and never given to him till it was highly putrid: the drink was clear pump water, rendered putrid by suspending a piece of putrid beef in it, and exposing it to the action of the sun.

August the 12th his eyes were examined, and the pupils of both appeared much contracted. They were watery. At 20 minutes past 12 o'clock an ounce of blood was drawn from one of the crural veins Its colour and smell were perfectly natural—it was coagulated at 25 minutes after 12—separation into serum and crassamentum was evident at 30 minutes after 12; the coagulum and serum were perfectly natural—the serum did not change the colour of a piece of paper, stained with a vegetable blue.

August 19th. He appears weak and very sick. For these few days past he has had a disrelish for the putrid meat. His eyes appear sore and inflamed. I washed them with cool pump water. The action of the heart was regular though feeble. Blood was drawn at 4 minutes before 11 o'clock; the colour and smell were perfectly natural—it was coagulated at 11—

* Vide Blane on Seamen's Diseases—Medical Transactions—and Stark's Works.

separation into serum and crassamentum was evident at 6 minutes after 11. The serum and coagulum were perfectly natural—the serum did not change the colour of the paper stained blue.

August 20th. His eyes appear much better—washing them with cold water continued. He appears weak.

August 22d. His eyes appear quite well. Strength somewhat recovered. Appetite appears also to be increased.

August 24th. Blood was drawn at 20 minutes after 5 o'clock. Its colour and smell were perfectly natural. It was coagulated at 24 minutes after 5; separation into serum and crassamentum was evident at 31 minutes after 5. The coagulum and serum were natural. The serum did not change the colour of the paper stained with the vegetable blue.

He in general ate and drank a sufficient quantity; but ate more in the beginning than towards the latter end of the experiment. He was reduced by the diet. The excretions were not evidently affected. The action of the heart was regular as has been mentioned, and respiration appeared to be performed in a natural and easy manner.

EXPERIMENT V.

A dog was kept fasting from September the 10th, 1792, till the 14th of the same month.

During his fasting he did not appear uneasy till the evening of the 13th, when he cried. He continued in this state till the morning of the 14th, when at 15 minutes after 9 o'clock highly putrid broth was given him: he swallowed it greedily, belched several times afterwards, but did not vomit. The action of the heart, which before was frequent and feeble, now became slower and fuller; he appeared very lively and full of play. At 3 o'clock, P. M. he was fed again with putrid broth.

September 15th. At 2 o'clock he received some more of the putrid broth. The action of the heart in every respect natural, except that it was a little feeble.

September 16th. At 3 o'clock he was fed again with putrid broth, which he took in heartily.

September 17th. At 33 minutes after 9 o'clock blood was drawn from one of the crural vessels. Its colour and smell were natural. It was coagulated at 39 minutes after 9. It began to separate into serum and crassamentum at 50 minutes after 9. The serum and coagulum were perfectly natural. The serum had no effect in changing the colour of the paper stained blue.

EXPERIMENT VI.

A bitch was kept under the same circumstances as the dog last mentioned, and the phænomena were alike in both instances.

September 17th. At 12 minutes before 10 o'clock, blood was drawn. Its colour and smell were natural. It was coagulated at 6 minutes before 10. It began to separate into serum and crassamentum at 3 minutes after 10. The serum and coagulum were perfectly natural. The serum did not change the colour of the paper stained blue.

From these experiments, it is evident, that the sensible qualities of the blood are not, in the least, affected by a putrid diet. The question then must certainly be, *How does a putrid diet operate upon the system?* I answer, by inducing debility from the little nourishment it contains; and that from hence the solids suffer. This I hope to prove by an experiment performed on the dog, the subject of the 19th experiment, which will be related under the head of injections of putrid matters into the blood-vessels.

The process of *digestion* is one of the most curious operations of nature. It acts wonderfully and powerfully upon our aliment, changes the properties of animal and vegetable matters, reduces them both to a substance possessing like properties, and operates upon the hardest as well as the most fluid substances—All this is said to be performed by a fluid we call the gastric juice.

If such are the effects of digestion on our aliment, is it not reasonable to suppose, that the properties of putrid matters taken into the stomach may be also changed? Let us examine whether putrid food undergoes a change of properties in the stomach.

That great Italian philosopher, Spallanzani, was I believe, the first who made experiments to this purpose. He performed them on birds, cats, dogs, and even went so far as to swallow putrid meat himself. He found that the meat, in every instance, lost its putrid smell.*

I repeated his experiments on dogs, with the like success, and shall now relate them.

EXPERIMENT VII.

September 17th, 1792. Three ounces of highly putrid beef were given to a bitch. She retained it. Three hours and a half after, the contents of the stomach were examined. The meat was found surrounded by the gastric-fluid; its putrid smell was entirely destroyed, and its colour appeared more natural than it was before the animal had swallowed it.

EXPERIMENT VIII.

September 17th, 1792. Highly putrid broth was given to a dog. The stomach retained it; and three hours after, its contents were examined, some of the broth was remaining together with some of the solid matters that were mixed with it. The putrid smell was found to be abundantly diminished.

* Spallanzani's Dissertations, vol. 1. p. 284, and seq.

I shall now proceed to the consideration of my 3d head, which has for its object of inquiry, Whether the blood be materially changed and rendered putrid, by the injection of putrid and other substances into the blood-vessels?

Many centuries have elapsed since physicians first began to be afraid of the access of the least particle of contagious matter into the blood-vessels, for, that it excited a fermentation therein, contaminated the whole mass of blood, and caused it to partake of its contagious properties.

If (say they) by adding a small quantity of a ferment to a mass of flour, or other fermentable matter, we are able to bring on a fermentation in it, and convert it into the nature of the ferment, why should not the blood, in like manner, be affected, if by chance or otherwise a putrid or other ferment should get into the vessels and mix with it? Thus they supposed the small-pox, measles, and other eruptive diseases to be produced.

These men always overlooked the vital principle, which exists in the one substance, and not in the other; and here in my opinion the material difference lies; for we know that if the other requisite circumstances be duly observed, it is only necessary to destroy life, in order to bring on a putrefaction in the animal. I think, from what I have stated, there is ground for believing that the blood cannot putrefy in the living animal; but the deductions of reason, however just and true, the inferences are not to be solely relied upon, when the better evidence of substantial facts—and of facts more immediately in point than the preceding ones, can be had.

To ascertain beyond a doubt whether the blood could be excited to a putrid fermentation, by injecting putrid matters into the blood-vessels, became an important and essential desideratum. With this view I made the following experiments, which were performed *on healthy dogs*.

The operation of injection was always executed on one or the other of the extremities of the animal, An incision was made, and the vein was laid bare, which was for some distance dissected free from connection with the adjacent parts. An opening was then made into its cavity, sufficiently large to admit a curved tube, which was retained in it by means of a ligature passed round the vessel. The tube was made so as to screw on the mouth of a common pewter syringe.

EXPERIMENT IX.

Was performed July 29th, 1792, on a bitch, two months old, in good health.

About 5 drachms of putrid serum were injected into one of her femoral veins. The serum was obtained from the blood of a healthy dog; and exposed in an open phial, for one week; and had a smell similar to that of rotten eggs.

Some difficulty attended the introduction of the pipe, by which the matter was injected, owing to its large size, and the smallness of the vein—

the vein, when laid bare and touched with any instrument, contracted considerably, its diameter being thereby much diminished. During the operation, she lost but a very inconsiderable quantity of blood, yet she seemed very weak and languid, probably owing to the pain she endured. Whilst I was injecting the serum into the vein, she had two convulsive paroxysms, and appeared to be in great pain, which she expressed by several loud cries. The muscles of her whole body seemed affected with these convulsive motions; that they were not owing to the irritation produced by introducing the pipe into the vein is certain, because no such symptoms appeared before the injection of the serum, though it was introduced twice or thrice. After the injection was finished, she was placed on the floor, and attempted to walk, but was so weak that she fell down. The action of the heart was very frequent, though weak and feeble; respiration was anxious and difficult. She seemed not at all inclined to move; and was quiet, except that at different intervals, she groaned and sighed, and afterwards vomited some of the food she ate at noon, in an indigested state, together with about two ounces of a yellowish green coloured fluid. Continuing in this situation, she was frequently affected with convulsive motions of the abdominal muscles and lower jaw, she became weaker and weaker. At 15 minutes past 7 o'clock, about an hour and an half after the experiment was performed, all her muscles were in a relaxed state, and she appeared motionless. At half past 7 she was dead.

Dissection. I did not examine the body till the 30th, at half past 8 o'clock; so that she was 13 hours dead, when the examination took place.

The abdomen was much distended; upon cutting into its cavity, a quantity of putrid air, of a smell like that of rotten eggs, rushed out. There was an effusion of a lympid fluid. The intestines were distended with air, and contained but a small quantity of liquid fæces. The stomach contained little else, but a large quantity of the before mentioned flatus, and a fluid of a yellowish colour. At its lower orifice it appeared rather preternaturally red. All the other viscera were in a sound and natural state. The veins appeared much distended; and air, intermixed with blood, of a dark venous colour, appeared through their coats. When I cut into the vessels, air came out in bubbles, together with the blood. The blood did not appear to be very firmly coagulated. The heart was much distended with blood, particularly the right auricle, which contained some air also. In the heart the coagulation was more perfect than it was in the veins. The blood did not smell in the least putrid.

EXPERIMENT X.

Was also performed July 29th, 1792, on a bitch in good health. About a drachm of pus, diluted with a small quantity of clear pump water, was injected into one of her femoral veins. The pus was obtained on the 27th inst. from an abscess of the intercostal muscles. This day it possesses a putrid smell.

During the injection of the matter, she had two very violent fits of convulsion, the last of which continued for some time, and she appeared to be in great pain; respiration was quick and irregular; the heart beat frequently though with some force. The pulsations were small and irregular, accompanied with frequent intermissions, and to all appearance she was dying. When she was placed in a cool situation, the respiration became less difficult, and the action of the heart more regular. She continued in this state and lay quiet for some time, when a mercurial thermometer was applied to the axilla, and the mercury rose to 100°, as it did when it was applied before she underwent the experiment. After lying about twenty minutes in a cool place, she got up and attempted to walk, but was very weak, and did not go far before she fell down; after which she seemed again to recover. At 30 minutes past 7 o'clock she was seized with twitchings of the muscles of her abdomen and lower jaw; the weakness increased; at length she became motionless; and at fifteen minutes before 8 o'clock, about an hour and an half after the injection, she was dead.

Dissection.—The body was not examined till Monday morning the 30th, about 14 hours and an half after she died. The abdomen was considerably distended; I made an incision into it, but found that no air escaped; about 2 oz. of a lympid fluid was found effused in the cavity. The intestines were much distended, and when an incision was made into them, air and fæces came out. The stomach was likewise much distended with air and half digested food. The same preternatural redness about the lower orifice, which occured in the former case, appeared also in this. The other abdominal viscera appeared perfectly natural and sound. The heart was much distended with blood, but no air was found in it or the blood-vessels. The blood was more firmly coagulated than that mentioned in the last experiment, and it had no unnatural smell.

EXPERIMENT XI.

Auguust 6th, 1792. A drachm of fluid matter, produced by highly putrid beef, was diluted with four drachms of putrid pump water, and injected into one of the femoral veins of a healthy dog.

The matter was injected at half an hour after 11 o'clock. During the injection he was much convulsed, and appeared to be in great pain. The pupils of the eyes were somewhat dilated. The beating of the heart was now very frequent, though feeble, as it was during the injection of the matter At fifteen minutes before 12 o'clock the breathing became very frequent, accompanied with great difficulty and sighing. At this time he vomited a quantity of half digested food, and appeared to be greatly relieved by it; at 12 o'clock he vomited again, which relieved him so that he rose, but in one or two minutes laid down again. At 4 minutes past 12 he had an evacuation of fæces—at 7 minutes after 12 he attempted to lay down, but fell, and laid in an unnatural posture—he appeared to be very weak—his breathing became more difficult and laborious—his eyes appeared watery and sunk in the orbits. At 10 minutes past 12 he had another evacuation of fæces. At 15 minutes after 12 he was raised upon his feet—he stood, though with difficulty, and his left hind leg became paralytic.* In a short time all his hind parts appeared to be more or less affected in this manner. At 40 minutes after 12 he became very restless, turning and twisting his body every way. At 45 minutes after 12 he was again raised upon his feet, but could not stand, for all his muscles appeared greatly relaxed. At 10 o'clock the pupils of the eyes were much dilated, and his sight seemed to be greatly diminished—he had an evacuation of urine. He began to groan, and the abdominal muscles were convulsed. The convulsions, after continuing some time in those parts, extended to the muscles of his head and neck. Respiration, and the action of the heart ceasing, he died at 12 minutes past 10 o'clock.

Dissection. The body was examined immediately after death, and nothing unusual was observed. The blood was quite natural in every respect.

EXPERIMENT XII.

August 10th, 1792. Twelve grains of putrid blood, diluted with a drachm of clear pump water, were injected into the femoral veins of a healthy bitch, at thirty-five minutes past 10 o'clock. During the injection she seemed uneasy, and had an evacuation of urine. The action of the heart became much slower, and very feeble. Respiration was somewhat difficult. She was then placed on the floor, and continued standing—12 minutes after she had a natural motion from the rectum, and then laid down. At 11 o'clock her eyes were examined, and they did not appear any way preternatural. She seemed dull, heavy, and much inclined to sleep. At 12 o'clock I perceived that she had had another evacuation of urine. The eyes were now examined again, and no alteration appeared to have taken place in them. She laid quiet till half past one o'clock, when violent efforts to vomit came on, and she brought up a quantity of the food she had eaten previously to the performing of the experiment. The heart beat very frequent and feeble—respiration was not greatly affected, but she continued dull and heavy. At 2 o'clock meat and drink were offered her—she would not even bear the smell of meat, but rose and drank some water. She laid down. At half past 2 o'clock she got up again, walked about, and evacuated urine; then laid down again, and

* The incision in this experiment was made on the *right* thigh.

was dull and heavy as before. At 5 minutes before 5 o'clock she had another evacuation of urine. At 3 minutes before 5 she had a very copious evacuation of extremely fluid fæces, of the colour of coffee-grounds, and of a very putrid smell. She now appeared weaker than she was before the evacuation, though not quite so dull. At 6 o'clock she evacuated urine again. I left her at 25 minutes after 6, when circumstances did not appear much altered. At 8 o'clock I saw her again, meat and drink were offered—she drank, but would not eat. The heart beat very frequent and feeble—in other respects as before.

August 11th. This morning, at 8 o'clock, I found her dead. A disagreeable odour arose from her body. The abdomen appeared somewhat distended. I perceived she had had another loose evacuation of the description above mentioned, though not so copious as the former one. There was a great deal of saliva about the mouth, and the tongue protruded through the teeth. On

Dissection, the following appearances were observed. Upon cutting into the cavity of the abdomen, no air escaped, but I experienced a very disagreeable fœtid smell. The superior parts of the intestines appeared in several places of a dark green colour, spotted with small white specks, while the lower portions appeared natural. When an incision was made into the intestines, a quantity of putrid air rushed out, together with liquid dark green coloured fæces. The stomach appeared to be rather small—I made an incision into it, when some of the like liquor flowed out. The liver was in many places of a preternatural colour, and adhered to almost all the other abdominal viscera, particularly to the stomach and right kidney. The gall-bladder was much distended with a light green coloured bile; the lungs were collapsed; the right lobes were of a blackish colour, intermixed with red; the left lobes appeared natural. The red appearance of the lower orifice of the stomach, mentioned in the other cases, was not apparent in this. The veins and heart were much distended with blood, which was not very firmly coagulated, but its smell was perfectly natural.

EXPERIMENT XIII.

August 14th, 1792. At 40 minutes after 10 o'clock six grains of putrid blood, mixed with a drachm and a half of clear pump water, were injected into one of the femoral veins of a bitch. During the injection she appeared very uneasy—the action of the heart was slow and feeble—respiration slow, and performed with difficulty. She was placed on the floor, appeared dull, and laid down. At 10 minutes before 11 o'clock her abdominal muscles were violently convulsed. At 7 minutes before 11 she was seized with violent retchings and efforts to vomit, but did not evacuate. At 15 minutes before 3 o'clock meat and drink were offered her—she ate, but would not drink. At 23 minutes before 7 she had a copious evacuation of urine. At 20 minutes before 7 she ate and drank. I left her at 15 minutes before 7, when she seemed easy. I saw her again at 8 o'clock and no alteration was evident.

August 15. This morning at 8 o'clock, I saw her—she appeared perfectly easy—action of the heart nearly natural, though rather frequent. She was now fed, and ate as before. I perceived that she had had a natural evacuation of fæces. At 21 minutes after 5 blood was drawn—its colour and smell were natural—it was coagulated at 24 minutes after 5—the separation into serum and crassamentum was evident at 30 minutes after 5; a piece of the paper stained blue was dipped into the serum, and no change of colour took place—the serum and coagulum were quite natural. I saw her again at 8 o'clock. She appeared as she did when in health.

August 16th. She was perfectly well.

EXPERIMENT XIV.

August 17th, 1792. At 15 minutes after 12 o'clock twelve grains of putrid blood, mixed with a drachm and a half of clear pump water, were injected into one of the humeral veins of the bitch last mentioned. During the injection, she cried violently, and appeared to be in great pain. Respiration became very frequent—action of the heart frequent and feeble. The eyes were examined, and the pupils were found to be much contracted. She was placed on the floor, walked a few steps, leaned against the wall, in a standing posture, and appeared very sick. At 26 minutes after 12 she had an evacuation of fæces, which was rather loose. At half past 12 she laid down, appeared dull, and much inclined to sleep. At half past 2 the abdominal muscles were affected with convulsive contractions; they did not continue long. The eyes were again examined, and the pupils appeared natural. At 3 minutes before 5 she had a copious evacuation of urine. At 10 minutes after 6 I left her eating, and to appearance easy, though dull and languid. I saw her again at 8 o'clock, when I perceived she had had another loose evacuation of fæces, which was copious. Pulsation of the heart frequent and feeble—she appeared easy.

August 18th. Blood was drawn at 5 minutes before 6 o'clock P. M. its colour and smell were natural—it was coagulated at 6—at 5 minutes after 6 it separated into serum and crassamentum—coagulum and serum were quite natural.—Serum did not change the paper stained blue. At 8 she appeared perfectly well.

August 19th. She was perfectly well. The blood, which was drawn yesterday, had a natural odour, when it was examined this morning at 11 o'clock.

EXPERIMENT XV.

August 20th, 1792. At 4 minutes before 11 o'clock, half a drachm of putrid blood and a drachm of clear pump water, were injected into one of the humeral veins of the last mentioned bitch. During the injection she was very uneasy, and gave several loud shrieks. The heart beat frequent and feeble. Respiration became very difficult. She was placed on the floor, and immediately after had a copious evacuation from the stomach. She appeared very weak, and leaned against the wall. At 7 minutes after 11, she had retchings and violent efforts to vomit, but no evacuation ensued. At 32 minutes after 11, she began to groan and sigh. At 5 minutes after 12, she had efforts to vomit, but nothing was thrown up. I left her at 15 minutes before 1; she drank, and appeared easy, though she was very weak. I saw her again at 2 o'clock, the heart beat very frequent and feeble. I perceived she had had an evacuation of fæces while I was absent—it was rather loose. Meat and drink were offered her; she drank, but did not eat. She appeared very sick. I saw her again at 8 o'clock, when she appeared just as she was at 2 o'clock.

August 21st. Blood was drawn at 2 minutes before 5 o'clock P. M. its colour and smell were natural—it was coagulated at 3 minutes after 5—separation into serum and crassamentum was evident at 9 minutes after 5—the coagulum and serum were perfectly natural—the serum did not change the colour of the paper stained blue. At 8 o'clock she appeared very well.

August 22d. Serum of the blood drawn yesterday, did not change the colour of the paper stained blue.

EXPERIMENT XVI.

August 22d, 1792. At 11 o'clock, a drachm of putrid blood, mixed with half a drachm of clear pump water, was injected into one of the crural veins of the bitch last mentioned. During the injection, the action of the heart became very frequent and feeble. Respiration very laborious. She vomited. After this she was placed on the floor, and appeared very weak, yet stood for some minutes, and then fell down gently. At 8 minutes after 11, she had an evacuation of urine and of fæces. At 14 minutes after 11, the eyes were examined; the pupils were found to be very much contracted. At 16 minutes after 11, she had another evacuation of urine. At 19 minutes after 11, she began to cry violently, and appeared to be in very great pain. The respiration became very laborious, and the action of the heart increased in frequency and feebleness. At 23 minutes after 11, she became convulsed. At 25 minutes after 11, a finger was drawn over the eye, and no contraction of the eye-lids took place. At 28 minutes after 11, she was dead.

Dissection. The examination of the body took place at 8 minutes after 12 o'clock. All the viscera were found in a sound and natural state, except the lungs—there a bloody effusion was discovered, particularly in the inferior parts of the left lobes. At 20 minutes after 12, blood was obtained by opening one of the large veins; the colour and smell were natural. When I left the blood, it was not so perfectly coagulated, as in the other instances, owing to its having been by accident much agitated. I returned in the afternoon, and found the coagulation complete—the coagulum and serum were natural—the serum did not change a piece of paper stained blue.

EXPERIMENT XVII.

August 14th, 1792. At 16 minutes after 3 o'clock, six grains of putrid blood, diluted with a drachm of clear pump water, were injected into one of the femoral veins of a healthy dog. During the injection, he was very uneasy and gave several loud shrieks. The action of the heart became so feeble, that it could scarcely be felt, and was also frequent. He was placed on the floor, when he lay down. In a short time he rose up, stood sometime, and appeared very weak. Respiration became laborious, and he lay down again. At 15 minutes before 4 o'clock, he was seized with twitchings about the bottom of, and across the thorax, in a great degree resembling a hiccup. At 4 minutes after 4, meat and drink were offered him; he would not drink, and seemed as if desirous to eat; but when he approached near the meat, he drew his head from it, as if the smell of it were offensive to him, though the meat was fresh killed this morning. At 2 minutes before 5, he had an evacuation of urine, and rather a loose evacuation from the rectum. At 4 minutes after 5, he had a copious evacuation from the stomach, when the action of the heart became perceptible to the touch. At 20 minutes after 5, he had violent efforts to vomit, and brought up a quantity of greenish coloured fluid. At 34 minutes after 5, they recurred again, with the like effect. They attacked him a third time at 3 minutes before 6; the consequence was the same as in both the former instances. I left him at 15 minutes before 7 o'clock, when he was eating and drinking. He seemed easy.

I saw him again at 8 o'clock, when he appeared heavy. I perceived that since I had left him, he had had a sparing evacuation of fæces, rather loose.

August 15th. This morning at 8 o'clock, he appeared dull, heavy and weak. The action of the heart was frequent and feeble. He was now fed. At 2 o'clock, I saw him again; he was as described in the morning.

At 5 o'clock, blood was drawn—the colour and smell were natural—it coagulated at 4 minutes after 5—separation into serum and crassamentum took place at 12 minutes after 5—the colour of the paper stained blue was not changed by the serum—The coagulum and serum were perfectly natural—I saw him again at 8 o'clock, and he appeared perfectly well.

August 16th. He was perfectly well to-day.

EXPERIMENT XVIII.

August 17th, 1792. At 15 minutes after 4 o'clock, ten grains of putrid blood, mixed with a drachm of clear pump water, were injected into one of the femoral veins of the dog last mentioned. During the injection he appeared to be in much pain, and had an evacuation of urine ; the action of the heart became frequent and feeble, but respiration was not much altered. At half past 4, the abdominal muscles were convulsed; the convulsions did not last any length of time. At 20 minutes before 5, he was seized with tremors over his whole body; they lasted about two minutes, and then went off. At 16 minutes before 5, he had an evacuation of urine. At 15 minutes after 6, I left him eating, when he appeared pretty easy. I saw him again at 8 o'clock, and perceived he had had a motion since I left him. The action of the heart was frequent and feeble. While I was with him he had an evacuation of urine, and appeared easy.

August 18th. Blood was drawn at 10 minutes after 6 o'clock this morning—its colour and smell were natural—it was coagulated at 14 minutes after 6—separation into serum and crassamentum began to take place at 23 minutes after 6. The coagulum and serum were natural. The serum did not change the colour of the paper stained blue. At 8 he appeared to be perfectly well.

August 19th. He was perfectly well to-day. The blood that was drawn yesterday was examined this morning, and possessed no unnatural smell.

EXPERIMENT XIX.

August 27th, 1792. At 40 minutes before 10 o'clock, a drachm of putrid blood, mixed with half a drachm of clear pump water, was injected into one of the femoral veins of the dog, the subject of the 4th experiment. During the injection, he was very uneasy, and evacuated urine. The action of the heart became very frequent and feeble. He was placed on the floor, and immediately after vomited. At 3 minutes before 10, he had an evacuation of natural fæces. At 2 minutes before 10, he vomited again. At 10 his breathing became very laborious, and he had a loose and small evacuation of natural coloured fæces. At 3 minutes after 10, he lay down. The heart beat so feeble, that it could scarcely be felt. At 10 minutes after 10, he rose, vomited again, and then lay down. At 15 minutes after 10, the eyes were examined and no alteration in them was apparent. At 21 minutes after 10, he rose again, walked about the room, stood for sometime, and then again lay down. The action of the heart became more evident. At 11 o'clock, he groaned very much. At 6 minutes after 11 the action of the heart became more frequent and feeble. The eyes were now again examined, but presented no unnatural appearance. At 20 minutes after 11, he rose and walked a few steps, had a sparing evacuation of chocolate-coloured, liquid fæces, then fell down, and appeared as if stimulated to evacuate again; he rose and had an evacuation of urine, and again fell down. At 18 minutes before 12, his abdominal muscles became convulsed. At 4 minutes before 12, he vomited again. At 28 minutes after 12, he rose, walked a few steps, had an evacuation of very thin chocolate-coloured fæces, and appeared to be very weak; after this he walked a few steps again, and then lay down. I left him at 10 minutes before one o'clock, when he appeared easy, though very weak and sick. When I returned, at 12 minutes before 3, I perceived, that during my absence, he had had two or three evacuations of urine. The heart beat frequently and feebly. At 10 minutes before three, he had an evacuation of urine, and afterwards vomited. At 8 minutes before 3, he had an evacuation of very liquid fæces, intermixed with mucus. At 5 minutes before 3, he had retchings and efforts to vomit, but did not evacuate. At 16 minutes before 3, he rose, and had another evacuation of fæces similar to the one last mentioned. At ten minutes after 4, he had an evacuation from the rectum, of mucus intermixed with blood. At 25 minutes after 4, putrid meat and putrid water were offered him, he drank plentifully, but did not eat. I left himat half past 4, and saw him again at half past 7, when I perceived he had had a very copious evacuation of urine, but had not eaten. The heart beat frequently, though not so feebly as before. Respiration was pretty free. He seemed easy, and appeared much better than when I left him the last time.

August 28th. When I saw him this morning at 8 o'clock, marks of an evacuation of fæces and urine were evident. I perceived he had eaten nothing. Putrid water was given him and he drank of it. The heart beat frequently and feebly. He appeared very weak; and the wound put on a bad appearance. At 2 o'clock I saw him again, when I perceived that he had had two or three evacuations of urine, but that he had not eaten. I now offered him some fresh meat; he held it in his mouth, but did not swallow any of it, and let it drop. Putrid water was again offered to him and he drank it. Respiration did not appear to be much affected. The action of the heart was frequent, and so feeble as scarcely to be felt. In my presence he had an evacuation of urine; I caught some of it in an earthen vessel, dipped a piece of the blue coloured paper in it, but no change of the colour was evident. The wound appeared in a gangrenous state. He seemed very weak, and it was with difficulty that he stood. At 8 minutes before 5, blood was drawn; the colour and smell were natural. It coagulated at 4 minutes before 5; separation into serum and crassamentum was evident at 7 minutes after 5; the serum and crassamentum were perfectly natural; the serum did not change the colour of the paper stained blue. The wound appeared much worse. In every other case, yet mentioned, it invariably put on a good appearance and healed readily. After bleeding he appeared exceedingly weak. At 35 minutes after 5 I left him very uneasy. I saw him again at 8 o'clock, and he appeared nearly in the same condition as when I left him last.

August 29th. This morning at 8 o'clock, I found him dead. The blood drawn yesterday, was now examined. No unnatural smell was evident. The serum did not change the colour of the paper stained blue. The coagulum was so firm, that when thrown out of the tumbler on the floor, it did not break. A very disagreeable and fœtid odour arose from the body.

Dissection. The body was examined at half past 9 o'clock, when I cut through the skin covering the lower ribs, the flesh below appeared gangrenous. The liver in some places was of rather a lighter colour than natural. The inferior part of the small intestines appeared inflamed. The stomach was perfectly natural, both internally and externally; it contained mucus and a whitish fluid. A bloody effusion was found in the lungs, particularly in the right lobes. The bladder was quite natural. The heart and veins were much distended with blood, which was firmly coagulated in both; the colour and smell of it were perfectly natural.

As yeast is well known to be a powerful ferment, and the volatile alkali a great chemical agent, and a dissolver of the blood when out of the body, the following experiments were made, to ascertain what effect they would have on the blood, when injected into the blood-vessels.

EXPERIMENT XX.

August 14th, 1792. At 20 minutes after 11 o'clock, three drachms of stock-yeast were injected into one of the femoral veins of a dog. During the injection he appeared somewhat uneasy. Respiration became very difficult and laborious; the action of the heart irregular, intermitting, and somewhat increased in force. He was placed on the floor but was not able to stand. At 30 minutes after 11, he had an evacuation of urine; at 33 minutes after 11, he had an evacuation of natural fæces. The eyes being examined, did not appear evidently altered. The muscles of his whole body now seemed to be in a relaxed state. At 40 minutes after 11, his eye-sight appeared to be much diminished; at 5 minutes before 12, he was dead.

Dissection. The body was examined immediately after death. Nothing preternatural was observed, except a bloody effusion in the lungs. The blood was in every respect natural.

EXPERIMENT XXI.

August 7th, 1792. At 20 minutes before 11 o'clock, 15 grains of mild volatile alkali, dissolved in 2 drachms of clear pump water, were injected into one of the femoral veins of a healthy bitch. During the injection, she gave three or four loud cries, and seemed to be in great pain. She was placed on the floor, walked three or four steps, and then lay down. The action of the heart was increased in frequency, and respiration was performed with difficulty. In other respects she seemed easy and quiet. At 11 o'clock, meat and drink were offered to her; she would not take of either. Respiration now seemed to be performed with no great difficulty. The action of the heart was as before mentioned. At 35 minutes past 4, the eyes were examined, and they appeared no ways altered. At 42 minutes past 11, she began to be restless. The action of the heart seemed to have recovered some degree of quickness; the frequency of it still continued. Respiration appeared pretty easy, though short. At 40 minutes past 12, her whole body was seized with a tremulous motion, when a thermometer was applied to the axilla, and the temperature was as when in health. These tremors came on in paroxysms, each of which lasted but a little time, and they recurred frequently. The heart, during a paroxysm of trembling, beat frequently and feebly. The tremors increased in violence, and in frequency of recurrence. At 14 minutes past 1, she was affected with several violent convulsive contractions across her abdomen. At half past 1, the eyes were again examined, and the pupils were observed alternately to dilate and contract frequently and considerably. At 40 minutes after 1, she rose, looked about; meat and drink were again offered her, but she refused both, and lay down again. At 10 minutes before 2, the tremors attacked her again, her breathing became more difficult and laborious, the tremors continued more or less violent for 5 minutes, then went off, and she appeared easy. At 15 minutes after 2, the tremors and laborious breathing came on again; they were of short duration; when they went off she appeared easy, and continued so till 5 minutes after 4, when they occurred again. At 10 minutes after 4, she gave three or four loud cries, as if affected with much pain, then was easy again, and remained so till 40 minutes after 5, when I left her. At 8 o'clock, I saw her again; she seemed perfectly easy, the action of the heart was frequent and feeble.

August 8th. At 8 o'clock this morning she appeared easy, but was very weak; and her heart beat frequently and feebly. She now ate and drank; at 2 o'clock she appeared as in the morning: I also perceived that she had had a natural evacuation from the rectum since I saw her in the morning, which was the first that occurred since the operation was performed. At 7 o'clock I saw her again; no alteration was evident. I found that she had had a natural evacuation of fæces this afternoon; but I did not perceive any marks of a discharge of urine.

August 9th. She seemed quite well, but drank more than dogs usually do in health.

EXPERIMENT XXII.

August 10th, 1792. At 40 minutes after 11 o'clock, 25 grains of mild volatile alkali, dissolved in two drachms of clear pump water, were injected into the bitch last mentioned. During the injection, she was violently convulsed, and gave several loud cries; she also had an evacuation of urine and vomited. The heart beat very frequently, and breathing was laborious. She was placed on the floor, ran about the room, and then lay down. Her aspect was very wild. She did not lay long before she got up again; but in a few minutes lay down. At half past one she rose, and immediately afterwards lay down again. The breathing was very frequent. At 2, meat and drink were offered her, but she did not take of either. I left her at 25 minutes after 6, she continuing to be in the same condition. At 8 o'clock I saw her again; meat and drink were then offered her, she drank, but would not eat. Her heart beat very frequently, though weak; she seemed in other respects as she was before.

August 11th. This morning at 8 o'clock she appeared to be easy, was quiet, and ate and drank. Her heart beat frequently and feebly. I saw her again at 8 in the evening; she was in the same condition as in the morning. At 26 minutes after 8 blood was drawn—its colour and smell were quite natural—it coagulated at 32 minutes after 8 o'clock—separation into serum and crassamentum was evident at 38 minutes after 8 o'clock—the coagulum and serum were every way natural—the serum did not change the colour of the paper stained blue.

August 12th. To-day she appeared perfectly well.

EXPERIMENT XXIII.

August 13th, 1792. At 15 minutes before 11 o'clock 45 grains of mild volatile alkali, dissolved in two drachms and a half of clear pump water, were injected into the same bitch. During the injection she appeared to be in great pain, and gave several loud cries. She was placed on the floor, ran about the room, then lay down, and was very restless. The action of the heart was extremely frequent, and her aspect was wild. After lying a few minutes she became easy. At 35 minutes after 2, meat and drink were offered her; she ate, but would not drink. At 30 minutes after 4 o'clock, blood was drawn—its colour and smell were natural—it coagulated at 35 minutes after 4—separation into serum and crassamentum was evident at 39 minutes after 4—the crassamentum and serum were perfectly natural—the serum did not change the colour of the paper stained blue. At 39 minutes after 4 more blood was drawn from the same vein—its colour and smell were natural: as soon as it was drawn, I added 45 grains of mild volatile alkali, dissolved in ʒij of clear pump water, to it, when the colour became a very deep brown, nearly black. At 8 minutes before 5 o'clock it appeared to have a tendency to coagulate. At 10 minutes before 6 it was of the consistence of mucus. At 10 minutes after 6 she seemed perfectly easy, and was eating meat. At 8 o'clock she appeared in the same easy situation.

August 14th. At 8 o'clock this morning she appeared perfectly easy. The blood last mentioned was examined, and its consistence was much as before.

From the above experiments the following inferences result:

1st. That, contrary to the generally received opinion, the blood is neither rendered alkaline, acrid nor putrid by starvation. This I think is evinced by these circumstances—that the blood went through its spontaneous changes in the regular and usual manner—that the serum had no effect in changing the colour of my test—and that no sense of acrimony was perceptible to the taste.

2dly. That a putrid diet does not operate upon the blood so as to change its sensible properties. This is a material part of the inquiry, since physicians, from Hippocrates down to the present day, have supposed various and wonderful changes to be produced in the animal economy by such food. From the above experiments, it appears very clear, that a putrid diet had no effect whatever in changing the qualities of the blood, although the animals were strictly confined to it. We are also led to conclude from them, that a putrid diet does not change the sensible qualities of the excretions; since, as I have related, the urine did not alter the colour of the vegetable blue. Here it may be objected to me, that I ought merely to infer from these experiments, that the blood of dogs only is not affected by this treatment. I answer, that the inference may, probably, be extended to the whole animal creation; for, we cannot but suppose that nature is, in this respect, as beneficent to the one species as she is to the other; and to substantiate this opinion, I avail myself of the result of the experiments of the celebrated Spallanzani, as above mentioned, who not only operated upon the brute creation, but went so far as to risk his own life for the benefit of science; and found, contrary to the received opinion, that vitiated food, previous to its leaving the stomach, is converted into a matter capable of furnishing good blood. "Men, such as the inhabitants about the mouth of the Orange river, in Africa, live always on animal food, such as whales, seals, limpets, and what fish they can catch; that many times their food has entered into a great degree of putrefaction, and there is no vegetable food whatever employed at the same time; probably most of them never tasted any vegetable substance in their lives, excepting aromatics for seasoning; yet they are perfectly healthy and free from all putrefaction in their fluids or solids, though they are not very careful of avoiding it in the exterior parts of the body. We see likewise maggots live in and upon putrid masses, while they themselves, and all their fluids, are perfectly sweet and free from all appearance of putrefaction."* There appears to be an intimate connexion between the fluid and solid parts of our bodies; but how far this connexion between them subsists, I will not undertake to assert. Perhaps I might, in truth, have said that a putrid diet acts upon the solids of our machine, by its not containing alimentary matter in a proper quantity, and therefore not affording a sufficiency of blood to support the animal. We know that the solids derive their nourishment from the blood; and, in my opinion, it is sufficiently evidenced, from the mortification of the muscular parts which ensued upon making an incision into the thigh, as has been already stated, that a putrid diet does operate upon the solids.

3dly. That although the blood, or other animal matter, may be excited into a putrid fermentation out of the body, by the addition of a putrid ferment, yet that such a process cannot be excited in the living body. We have found that many grains of putrid matter existed in the blood-vessels for some days without changing the blood. Is it probable that a fermentation can be induced in the blood when the ferment enters the system by absorption, and no such process take place by introducing a putrid ferment immediately into the blood-vessels?

4thly. That by the introduction of putrid matter into the blood-vessels, very violent symptoms were produced, although the quantity of the matter was small, and of the same nature as the blood, except that it had become putrid.† Is it probable, then, that the animal could exist with the whole mass of blood in a state of putrefaction, when such violent symptoms were the consequence of the introduction of so small a quantity of putrid matter as was injected in the foregoing experiments?

And, 5thly. That though the volatile alkali may operate upon the blood in a wonderful manner out of the body, it does not evidently affect the blood when injected into the blood-vessels.

From these experiments, it is also evident, that cathartic and emetic medicines, when injected into the blood-vessels, cannot operate in a specific manner; for in almost every instance evacuations were the consequence of the experiments, when neither cathartics nor emetics were injected. Probably any matter capable of producing a sufficient irritation, will produce those effects, when thus thrown into the animal.

I attribute the presence of air in the vessels and heart, which occurred in one of the experiments, to the imperfect state of the syringe;

* Fordyce on Digestion, p. 155.

† My reason for making use of putrid serum and putrid blood, is, that they are the same substance, only altered by putrefaction. We know, from experiments of transfusion, that blood may be passed from the vessels of one animal, into those of another, without any evident injury. From this circumstance we are led to infer, that it, in these instances, acted as putrid matter.

for it consisted of one whole piece, when the two first experiments were performed.

Before I leave this part of the inquiry, I beg leave to state the following questions, viz.

Is there a power in the blood-vessels, or in the blood, capable of assimilating to the blood matters which are injected into the vessels? Do not the following circumstances make it probable? I could not by a minute examination distinguish between the blood and the matter injected. The serum of the blood, where the volatile alkali was injected, did not possess the odour of that salt, neither did it change the colour of the paper stained with a vegetable blue. Does not the doctrine of secretion, which is now gaining ground, favour such an idea? I own that there are certain exceptions to this opinion.

Do not the loss of vision, the dilated and contracted pupil, together with the convulsions which happened, denote the nervous system to be affected by the contents of the blood-vessels?

I shall now proceed to inquire, whether there is any reason to believe that blood becomes putrid in any disease?

This is a question of considerable importance. Extensive experience is necessary to decide it; and as my own has been very limited with regard to diseases supposed to be putrid, I must beg leave to refer the unprejudiced reader to books, which treat at large of them. Perhaps upon thorough consideration, he will believe the matter to be at least doubtful.

All I can do, will be to state some objections to the inferences that have been drawn from the symptoms and circumstances attending these diseases.

A symptom on which great stress has been laid, is, the appearance of petechiæ, vibices, or effusions of blood, which takes place in the last stage of typhus, yellow fever, scurvy, &c. These appearances are generally supposed to be indicative of a dissolved state of the blood in those diseases, though in my opinion without a sufficient reason. It is conceded by all that the system in those diseases is very much debilitated, and of consequence the system of blood-vessels, and these particularly at their ultimate terminations. The effect of this debility is a relaxation of the solids; the fibres of the blood-vessels will not now be in as close contact as they were in health, and the mouths of the exhalents will not be as narrowly contracted. This particular state of the vessels at their terminations, will allow the blood to transude, and be effused in the cellular membrane; or the exhalents, which in health pour out a lympid fluid, may now be capable of forwarding the red blood itself. Analogy supports the opinion. Do not the vessels of the eye in ophthalmia, through which a colourless lymph is circulated in health, receive in this diseased state, red globules? Do we not find in dropsy, that the vessels allow of a greater exhalation

disposition to coagulate; and do we not find the blood in several other diseases besides the scurvy, &c. to be in a more fluid state than it is in health? Certainly we do.

We are informed by Dr. Lind, that the blood of scorbutic persons did not impart the least sense of acrimony to the tongue, any more than the white of an egg; and that the blood of scorbutics does not become putrid sooner than other blood, which it certainly ought, cæteris paribus, if it had already begun to putrefy in the body. Nay, we are moreover informed by this celebrated writer, that the serum of the blood of such patients, is not septic but antiseptic; and would it not be absurd and inconsistent with the facts of chemistry to say, that a portion of matter which had already began to putrefy on being added to an unputrefied mass should retard the process?

The benefit derived from the use of fresh vegetables and their acids in curing scurvy, is, in my opinion, by no means to be admitted as proof of the bloods being in a putrid state.

We must in this place notice, that it is a well established opinion, that the powers of digestion are such, as to be capable of converting all matters into one and the same kind of chyle, whether animal or vegetable, though the one may not afford it in as great proportion as the other.

The above circumstance being admitted, and it cannot be denied, we at the same time must allow, that those matters undergo a considerable change in the stomach before they go on to the formation of the blood. Consequently, if they suffer a change, they possess no longer the properties of a vegetable, or a vegetable acid, and therefore a tertium quid must be formed. What its nature and properties are, I will not pretend to say, but leave the candid to judge for themselves; though we might equally well suppose it to be septic or antiseptic.*

There are many physicians, who supposed fixed air to be chiefly useful when injected into the intestines in those diseases suspected to be putrid, by acting as an antiseptic on the putrid blood; but, when thus applied, does it not rather act on the contents of the intestines, and destroy the bad effects produced by their offensive smell, &c. since it has been well observed, "that, any thing putrid is totally incompatible with the perfect well being of the animal?"

It is generally allowed, that putrid effluvia act as debilitating powers on the system. Indeed putrid matters, lying for a time in the intestines,

* I think that the following circumstance justifies my assertion, that vegetable aliment after it has suffered the changes produced on food in the stomach, &c. when it is sent on to form the chyle, has more of a séptic than antiseptic quality. Fordyce on Digestion, p. 164, mentions, that by distillation in a retort we obtain an empyreumatic oil, volatile alkali and water, and charcoal remains in the retort, whether the substance distilled be chyle, a piece of flesh, or other animal substance.

would seem to be debilitating, as in the instance of the dysentery. It appears also that fixed air acts as a stimulant: Mr. Henry found that it inflamed an ulcer.* And Dr. Dobson says, that when received by the mouth into the stomach, in ten minutes it raised the pulse from 71 to 77 strokes.†

It was objected to me, that the urine, breath, and other excretions of persons labouring under diseases of the putrid class, were highly fœtid and obnoxious. We grant this may be the case; though a question will then arise, whether these excretions contract this fœtor from the blood, or become fœtid after they are separated from the general mass? I am of the latter opinion, and think it highly probable that they become fœtid in their respective reservoirs, or in some other manner not yet explained. We know that the excretions do not by any means possess the properties belonging to the blood, and we also know that they may be very different in disease, though the blood remain the same as when in perfect health. To this purpose, I will quote Dr. Home's observation: he mentions that the blood of diabetic persons appeared perfectly natural; that the serum of the same possesses no more sweetness than that of other blood; though the urine of the same patients tasted very sweet, and upon evaporation afforded a large quantity of saccharine matter.‡

The case of a patient, who was lately under the care of my worthy preceptor, Dr. Wistar, may also be mentioned in confirmation of these sentiments. He was affected with pneumonia, and had all the usual symptoms; but in addition to them, an odour proceeded from him so putrid and offensive that no one could remain long in his room without great inconvenience; it even extended its offensive smell into the rooms a story below him. His urine had also an intolerable fœtor. The symptoms of pneumonia required bleeding, and this remedy was used with great caution; but notwithstanding the above circumstances, the blood coagulated very firmly, and had some inflammatory appearances on its surface.

The arguments taken from Dedier's, Couzier's, and Homes's experiments, employed by Dr. Ferris to prove, that the bile and blood undergo a change in the plague and measles, are by no means conclusive. We know the plague and measles to be highly contagious diseases, and easily communicated to those who are in a state of predisposition to receive the infection. Dr. Ferris supposes, that as the blood or bile of one who died of the plague, when injected into the veins of a dog, produced symptoms of that disease; and as the measles ensue after inoculating with the blood of those who labour under it, the blood and bile must have undergone changes in those diseases. In my opinion these facts prove nothing

* Henry's Exper. p. 127.

† Dobson's Commentary on Fixed Air.

‡ Homes's Clinical Experiments, p. 332.

that favours such a conclusion; and all that we can infer from them is, that the contagious or infectious matter was diffused through the blood, or adhered to it in those instances, as it does to old buildings, cloathing, &c. Facts prove that it does so with respect to these subjects, for months, nay even years, and then is as effectual as ever in its deadly operation. We are uninformed of the appearance or state of the blood in those instances. In the measles, the blood appears to be no more changed or altered than in other inflammatory diseases. How it is in the plague, I know not. Dedier and Couzier have taken no notice of any evident change—gentlemen whose accuracy would not have allowed them to have neglected such a circumstance if it had occurred.

What confirms me in the opinion, that the contagious matter was only diffused through the blood, or adhered to it in the same manner as to old buildings, &c. is, that in the one instance the plague, in the other the measles, was produced. If the contagious matter had mixed with the blood so as to produce a chemical change therein, neither the plague nor the measles would have been the result of the experiments; as by this mixture, they would both have lost their former properties, and a new compound would have been produced by their union, not possessing the properties of the contagious matter or of the blood. The sixth law of the affinity of composition, reads thus: "Two or more bodies, united by the affinity of composition, form a substance, whose properties are very different from those of any one of the bodies before their combination."* Consequently a disease very different from the plague or measles must be produced, if a chemical union had been formed.†

With respect to Dr. Home's Experiment, I beg leave to make one or two observations. He says, "I thought that I should get the blood more fully saturated with what I wanted, if it was taken from the cutaneous veins amongst the measles, than if I took it from a large vein, where there was a much greater proportion of blood from the more internal parts than from the skin. I therefore ordered a very superficial incision to be made amongst the thickest of the measles, and the blood, which came slowly away, was received upon some cotton."‡

The doctor appears to be of the opinion that a fermentation of the blood, produced by the introduction of contagious matter into the system, was not the cause of the disease; for, he says, the blood taken from the

* Fourcroy's Chemistry, vol. i. p. 64.

† We have already proved that the blood in the vessels of living animals was not subject to the laws of fermentation, and that no fermentation could be excited in it by the introduction of ferments; therefore, if a change had been produced in the blood in the above instances, it must have been by a chemical union, and consequently must be subject to the laws of chemical affinity.

‡ Home's Medical Facts, &c. p. 268.

more internal parts was not as plentifully saturated with the morbillous matter as that flowing in the cutaneous vessels. Is not this inconsistent with the true and well understood course of the blood? For, is not the blood of the cutaneous vessels at one moment in one part, and at another in a very different part of the system? Consequently the whole mass must have been equally affected. Again, if the motion of the blood were not itself sufficient to produce the change throughout the whole mass, this would have happened from the well known laws of fermentation; for we know that a very small portion of a ferment is sufficient to assimilate a very large mass of fermentable matter. Further, this experiment is not conclusive, since the blood was obtained by a very superficial incision made amongst the thickest of the measles, and the blood also flowed slowly. Here certainly in making the incision, the lancet or instrument used must have pierced some of the pustules, from the situation in which it was made; and as the blood flowed slowly it had time to entangle or mix with a quantity of the matter contained in the pustules; so that this experiment is a very indecisive one. Moreover I have been told by a gentleman who sometime since attended the lectures of the celebrated John Hunter, that Mr. Hunter informed his pupils, that he had made frequent attempts to inoculate with the blood of those who had the small-pox, and lues venerea, but never succeeded in imparting the infection.

Lind's observations on the blood in the yellow fever, support the opinion, that substances different from the blood may be diffused through it without changing it. He relates that the serum was of a deep yellow tinge. A person by curiosity tasted it, and found it bitter.*

From these facts we learn, that the bile may exist in the blood-vessels, without producing a change in the blood, or suffering one itself, since its sensible properties were in these cases evident. Who will assert that the properties of the blood are changed in jaundice?

We might proceed to a much greater length in proving that certain substances may exist in the blood-vessels of living animals unchanged themselves, and without producing any change in the blood, as turpentine, &c. This, I believe, is a position at present generally allowed by physicians; I shall therefore pass it over in silence.

Eruptive diseases were, for ages, supposed to be owing to changes in the blood; but the following experiment would seem to operate against the doctrine. Dr. Coxe transfused between 14 and 16 ounces of blood from the jugular vein of an old dog, who laboured under an eruptive disease, into the jugular vein of another dog, who was in health. This being done, the diseased dog got well; and to the other, who received the

* Lind's 1st Paper, p. 13.

blood which might probably have been supposed to be affected, nothing amiss happened.*

This experiment proves clearly that the blood did not suffer a change from the disease under which the dog at that time laboured. Eruptive diseases are, I believe, for the greatest part, contagious; and as the blood did not, in this instance, communicate the disease, we cannot suppose that the contagious matter adhered to it; this, therefore, strengthens my opinion, and leaves room for the supposition, that contagion may adhere to different parts of the body in different subjects.

If contagious diseases are produced by the contagion operating upon the blood as a ferment, whence is it that the blood of brute animals is not susceptible of being excited into this fermentation, since, from experiments, it appears to be much of the same nature with that of the human species?

Five months after the foregoing sheets were written, I had the good fortune to obtain the reading of Dr. Milman's very ingenious " Inquiry into the source from whence the symptoms of the scurvy, and of putrid fevers, arise, &c." wherein the author displays a great deal of ingenious reasoning. I was happy to find that we agreed in sentiment respecting the general opinion, though we have treated of the subject in a very different manner. For particulars I refer the reader to the book itself.

Thus I have sought in nature the phænomena of my doctrine. In my own estimation, facts respond to theory, and the inferences of my experiments to speculation. When I first contemplated this subject, I was not particularly attached to any opinion respecting it; and, from this circumstance, I gained the advantage of an impartial and unprejudiced examination of facts. After collating and considering the experiments I had made, I drew my conclusions with a deference to reason. If, in the event, I shall have contributed to advance the interests of medicine, and furnished any principles to aid the labours of practice—to lessen the horrors of putrid diseases—and to arrest, for a moment, the dreadful arm of death, I shall be more than compensated for performing my duty, and paying this tribute to humanity.

* Etmulleri Opera, tom. 3tio, p. 1619.

AN ESSAY

ON THE

MEDICINAL PROPERTIES

AND

DELETERIOUS QUALITIES.

OF

ARSENIC.

SUBMITTED TO THE EXAMINATION OF THE

REVEREND JOHN EWING, S. T. P. PROVOST;

THE TRUSTEES, AND MEDICAL FACULTY OF THE UNIVERSITY OF PENNSYLVANIA,

ON THE SEVENTEENTH DAY OF MAY, A. D. ONE THOUSAND SEVEN HUNDRED AND NINETY-SIX.

FOR THE DEGREE OF DOCTOR OF MEDICINE.

BY NATHANIEL POTTER, OF PHILADELPHIA.

For nought so vile that on the earth doth live,
But to the earth some special good doth give;
Nor aught so good, but, strain'd from that fair use,
Revolts from true birth, stumbling on abuse:
Virtue itself turns vice, being misapplied;
And vice sometime's by action dignified.——SHAKSPEARE.

PREFACE.

IN the subsequent investigation little aid can be derived from the opinions of preceding writers. The few lights that have appeared on the subject, have only served to conduct me into a solitary and uncultivated desert. The ingenious Dr. Fowler, of Staffordshire, in England, is the only author who has treated systematically of the virtues of Arsenic. Although his observations have been confined to intermittent fevers and periodical head-achs, he has done much toward establishing the reputation of this valuable acquisition to the materia medica. It may be thought by some, superfluous to increase the catalogue of medicine, with which the materia medica already superabounds; it must nevertheless be acknowledged, that medicine to fulfil certain indications with promptitude and energy, are the grand desiderata of the materia medica. The man who shall discover a solvent for the urinary calculus, or a cure for the epilepsy, will, in my opinion, better merit immortality than all the astronomers from Sir Isaac Newton to the present time.

INTRODUCTION.

THE man who presumes to recommend to the world a medicine which has ever been deemed a most virulent poison, ought seriously to contemplate that mixture of admonition and consolation, which the poet offers to the juvenile satyrist:

> But tread with cautious steps the dang'rous ground
> Beset with faithless precipices round,
> Truth be your guide; disdain ambition's call;
> And if you fall with truth, you greatly fall.

Although eight hundred years have elapsed since a celebrated Arabian physician* prescribed Arsenic as an internal medicine, and although its success has been attested by the most respectable authorities; yet, even at this enlightened period, it is scarcely known as a medicine. The frequent attempts that have been made to introduce this mineral into the practice of medicine have either perished with their authors, or soon followed them in silence and oblivion. To determine whether the fate of this medicine has been the just reward of its pernicious effects, or the inevitable consequence of scepticism and timidity, shall be the business of the subsequent disquisition.

The idea of a poison associates itself so intimately with death, that the thought of 'separating them would at first view seem to offer violence to the understanding. The epithet, poisonous, has generally been made to designate some enigmatical or inexplicable quality; and a poison has accordingly been defined, "whatever by its action upon the body produces death by its quality, without respect to its quantity." The reverse of this definition is perhaps the most accurate idea of poison that can possibly be given. Little sagacity will be required to perceive, that poisons are, in their own nature, as relative as heat and cold, and that the same sub-

* Avicenna. lib. 2. tract. 2.

stance may be either a medicine or a poison, according to its strength and quantity. However repugnant this opinion may have been to the feelings of physicians, it was familiar to the penetrating eye of the philosophic Shakspeare; who makes friar Lawrence in his soliloquy exclaim:

> Within the infant rind of this small flower
> Poison hath residence, and med'cine power:
> For this, being smelt, with that sense cheers each part;
> Being tasted, slays all senses with the heart.*

An ingenious living author† has expressed the same thought, with a stricter application to the practice of physic. "Poisons in small doses are the best medicines; and the best medicines in too large doses, are poisonous." There is no substance so replete with death as not to admit of a safe application to the body, both externally and internally, by a certain degree of division or dilution; neither is there any so innocent as not to be capable of abuse by excess in quantity, or by long continued application. Opium, in portions accommodated to the excitability of the system, proves a most exhilirating cordial, and by its universal stimulus imparts tone to every fibre of the body; but increased to the quantity of a few grains, it dissipates life like a vapour. The oxygenated muriate of mercury,‡ in doses of the fourth or sixth part of a grain, is an inoffensive and efficacious medicine, but ten grains of the same salt will annihilate life with irresistible impetuosity. Stramonium,|| digitalis purpurea,§ the cicuta virosa,¶ and many of the metallic preparations are invaluable medicines while they are confined within their proper limits, but in large doses derange and disorganize the most vital functions of the animal economy. The virus of the spider, which has so often extinguished the lamp of life, when taken into the stomach diffuses the animating sensation of the mildest stimulant, and is often used in the cure of intermittents with the most propitious effect. The carbonic acid,** in combination with wine, cyder, and malt liquors, gives to them the most delicious flavour, but is so poisonous to the lungs as to be totally unfit for respiration. Nitrogene gas,†† which in combination with oxygene, composes $\frac{72}{100}$ of the atmosphere we breathe, is incapable of supporting respiration. Bile, by its excess in quantity, or by a morbid quality, revolts with deleterious malignity against the system in which it was generated. Even the direful contagion of the plague and yellow fever, when diluted by water,

* Romeo and Juliet, act ii. scene 3.

† Withering's Botanical Arrangement.

‡ Corrosive sublimate of mercury.

|| Jamestown weed. § Foxglove.

¶ Hemlock. ** Fixed air.

†† Impure air, or atmospherical mephitis.

ceases to propagate infection. Ardent spirits are as certainly poisonous as arsenic, but they differ in this, the former requiring a series of time to accomplish what the latter effects in a moment; the one, like an enemy in ambush, insinuates itself into the most vital parts, while the latter surprises the mind by an instantaneous operation. Every substance that can possibly destroy life by being taken into the stomach, must accomplish this end, either by its excess of force dissipating the excitability and running the excitement to its highest pitch, terminating in death, or by gradually wasting the excitability by feeble but repeated efforts of the same nature. In the first way, such substances as are denominated poisonous act; in the last all the ordinary stimuli of life produce their effect. It is almost an invariable law of the animal economy, that impressions by repetition become more feeble, until at last no sensation is produced: it is in this way that the lives of old persons not diseased go out like a taper. The same catastrophe which is effected by a poison in a moment, an hour, or a day, is brought about, in process of time, by aliments, drinks, and all the stimuli that ever have been the most friendly auxiliaries of life, and by a perfect similarity of operation. It is therefore as unphilosophical to say, a man was poisoned by arsenic, as to say of him who died of old age, that he was poisoned by his drinks or aliments. To condemn a medicine because it has been abused by the injudicious, is as illiberal as it would be to stigmatize the inebriating qualities of wine because drunkards have sometimes expired in a paroxysm of intoxication.

Other reasons no less formidable, and equally unjust, have co-operated to prevent the introduction of arsenic into the practice of physic. An ignorance of its true operation, as well as of the pathology of the diseases in which it has been prescribed, have rendered its exhibition hazardous and uncertain. Without previous information on these two particulars, it must be impossible to adapt any medicine to those states of disease which it is intrinsically calculated to relieve. Without the minutest attention to the condition of the system at the moment the medicine is taken, how vague and uncertain must be the prospect of relief? The neglect of this particular has induced some practitioners to assert, that blisters are useless in the scarlatina; yet, what physician that has applied them in the typhiod state of that disease cannot testify in their favour? The same cause has produced such repugnant accounts of the digitalis in the treatment of dropsies; a medicine which I am confident never cured that disease, unless it was accompanied by some degree of inflammatory diathesis. The want of attention to the same circumstance, I have no doubt, caused that collision of opinion between Mr. Chaptal and Doctor Beddoes on the subject of vital air in phthisis pulmonalis; yet, as in other dissensions, both may have been right or wrong. In the inflammatory state of phthisis, so stimulating a power as oxygene cannot rationally be expected to harmonize with the tender vessels of the lungs, although it might prove the most salutary cordial, at a more advanced period of the same disease. We shall see in the sequel, whether or not this important circumstance has led Doctor Clark to deny the efficacy of arsenic in the cure of intermittents.

However much physicians may felicitate themselves on the boasted advantages of experience, without just principles to direct them in deducing their indications of cure, they must ever float upon an ocean of doubt and uncertainty. If the materia medica contained the principles upon which medicines produce their effects upon the different departments* of the system, the practitioner would then possess a luminary to conduct him; but how little better is the materia medica in its present state, than a compages of empyricism and contradiction? There is no such thing in nature as a specific; no remedy will infallibly cure any disease. No disease appears invariably with the same series, degree, succession and continuation of symptoms, even in those bordering nearest on uniformity. No two persons of a hundred will be affected with the same symptoms, alike in number, force, and order. The physician who prescribes for the picture of a disease as it is delineated on paper in a systematical nosology, acts as scientifically as he would do, were he to write a prescription for the day of the month, or for the colour of his patient's skin. If all the wisdom from Hippocrates to the immortal Cullen could be concentrated in one man, he could not, after defining a disease, say with what symptoms it would appear the next year. He therefore, who spends his time in searching after specifics, will find his scheme as visionary as the pursuit after the philosopher's stone, or as that of the alchemists, who imagined themselves capable of forming a substance that, like the hand of Midas, could metamorphose every thing into gold.

The exhibition of such a heroic medicine as arsenic, renders the strictest attention to the preceding observations indispensably necessary; for, he who expects it always to succeed in diseases of the same name which it has occasionally cured, will soon have reason to lament the fallacy of his own errors.

* See page 53.

INAUGURAL ESSAY.

OF THE INTERNAL OPERATION OF ARSENIC.

PREVIOUS to treating of the therapeutic virtues of Arsenic, it may be useful at least to attempt an explanation of its operation; especially as its internal effects, so far as I can inform myself, have never publicly been the subject of a conjecture. No branch of medicine is veiled in more mystery than the true operation of medicines. The following division of the human body into different systems by a late writer,* opens the fairest prospect of improvement in this department of science. 1. The brain and nerves. 2. The liver, lungs, and alimentary canal. 3. The sanguiferous system. 4. The muscular. 5. The glands and lymphatics. 6. The cutaneous. 7. The secretory and excretory organs. 8. The blood. 9. The senses and appetite.

As health consists in a due proportion of excitement being kept up between these different systems, so disease consists in divided excitement, or a loss of that equilibrium which constituted health. To cure diseases, it therefore becomes necessary to find out what system or systems are more immediately the seat of the disease, and to accommodate the remedies accordingly. There is so intimate a connexion between these several systems, that the action of a medicine upon one, may bring another or more into sympathy; yet it is sufficiently demonstrable, that some medicines possess a greater affinity to one system than another—The fœtid gums have a greater attraction for the nerves—mercury acts more particularly upon the lymphatics—cantharides seem to stimulate the urinary organs specifically—the oil of amber excites most particularly the muscular fibre—antimony is determined to the skin—stramonium affects the brain violently; and columbo root does not extend its influence far beyond the alimentary canal. I am so sensible of the propriety of considering the operation of medicines in this view, that were it not for the

* Rush on the Yellow Fever.

the modes in which they are generally exhibited, we shall find it more difficult to draw a line of distinction between their tonic powers, than we might on a superficial view, be induced to suppose. Arsenic is not generally prescribed more than three times a day, by which means the system relapses into its former atony; whereas bark is given at least every two hours in the same diseases: the former, from an ignorance of its immense power, has been prescribed in doses so large as seldom to be accommodated to the excitability of the system, while the latter acts more feebly, and requires but little judgment to manage it to the greatest advantage. By the stimulus of arsenic the system is irresistibly precipitated into indirect debility, and even by a very inconsiderable dose; to obtain its corroborating effects it is therefore necessary to begin with the minutest doses, and to increase them in the most gradual proportions. The Peruvian bark may be compared to a ligature, while arsenic more aptly resembles a two-edged sword. One of the most valuable attributes of this medicine is, its inherent power of accumulating the dormant excitability of a callous system; it would seem to fasten on the stimulability of the stomach, while it was insensible to all other impressions. It may be established as a general position, (and I shall have frequent occasion to repeat it) that arsenic can never be judiciously administered while the smallest degree of inflammatory diathesis is present; it is therefore in a state of fever not to be put in competition with the Peruvian bark, which being less stimulating, may overcome a slight fever by a stronger action, without throwing the system into those turbulent commotions excited by arsenic. The dominion of this medicine will be more conspicuous when we come to apply it to particular states of the disease; and first of those of the

ARTERIAL SYSTEM.

The patent ague drop, formerly so celebrated in England, has been the principal means of calling the attention of physicians to the internal use of arsenic; for, although it had been before recommended by the indefatigable baron Stork, it had not obtained much celebrity. Doctors Fowler,* Arnold and Withering have united their testimony in favour of arsenic in the cure of intermittents. The first of these gentlemen has recorded, that he relieved or suspended two hundred and forty-two cases, out of two hundred and forty-seven, one hundred and seventy-one of which number were radically cured. Dr. Withering informs us, that thirty-three out of forty-eight persons were cured under his care by Dr. Fowler's mineral solution. Dr. Arnold does not mention the number which he either treated or cured, but observes, "that the solution seldom

* See Fowler's works, for the letters of Drs. Arnold and Withering.

failed." Dr. Clark* informs us, that he only cured twelve out of twenty-five patients by the mineral solution, and that another physician of the New-Castle dispensary succeeded in only eight out of eighteen cases, but adds, that he received a communication from an ingenious medical friend which informed him, that only four cases out of a hundred had resisted this remedy. During my residence in a part of the state of Maryland where autumnal fevers prevailed epidemically, I resolved to try the comparative efficacy of arsenic and the Peruvian bark. Many of the fevers at that time assumed the remitting type; to such I gave either emetics or purges of calomel previous to the exhibition of tonics: by this practice I either obtained a complete apyrexy, or so far subjugated the fever as to venture with safety on the use of stimuli. For fifteen cases of this description I prescribed the solution, six of whom were cured, and four suspended: I then gave the bark to an equal number under similar circumstances, thirteen of whom were cured, and the other two suspended. From the unpleasant effects produced by arsenic in these cases, it was obvious, that it was a substance too stimulating for the remaining portion of phlogistic diathesis which still accompanied the remittent state. I likewise administered the bark to twenty persons labouring under tertians, without previous evacuations of any kind, and found sixteen of that number effectually cured: I repeated the experiment with the solution, and found it to succeed equally well. In many other cases I premised evacuations, and found whether I used the bark or the solution, that no advantage resulted from them in purely intermittent fevers. From the fifteenth of October to the fifteenth of November, I perceived that most of the intermittents had degenerated into quartans, and that those recently attacked, now generally wore that type. The two remedies were now again put in competition: the bark cured twelve out of twenty, whereas the arsenic out of an equal number of cases cured nineteen. This experiment was again attended with nearly the same result. In a few cases both remedies failed: I then combined them, and found them more successful, but still some obstinate cases resisted their power. Reflecting on the pathology of this disease, and upon the nature of the remedies upon which I had so repeatedly experimented, it appeared to me, that the insensibility of the system was obviously the cause of failure in the few cases unsuccessfully treated: I therefore administered the arsenic three times a day, in doses so large as to produce some sensible effect: by thus increasing the excitability, I found, that the bark given in small doses, and gradually increased according to the state of the system, proved almost universally successful; for I do not

* See Observations on the Diseases which prevail in long voyages to hot climates, and on the same diseases as they appear in Great-Britain, by John Clark, M. D.

remember more than two who returned for cure after I instituted this method.

Although it may be reasonably inferred that arsenic and the Peruvian bark, are nearly equal in point of efficacy in the cure of intermittents, as they occur in their variety of types in different seasons, yet the former possesses some advantages over the latter with respect to the facility of its administration to all ages and conditions. Patients (more especially children) can seldom be prevailed on to persevere in the use of the bark until a complete cure is obtained. In an economical point of view, arsenic has a still stronger claim upon the attention of physicians. An ounce of the best Peruvian bark, at this time, costs two shillings and six pence, and will seldom cure more than one person when it may be proper to exhibit it; whereas the same money will buy as much arsenic as will make as much of the mineral solution, as will, at the most moderate calculation, cure ten thousand persons of the same disease.

I attempted the cure of intermittents by arsenic, in Philadelphia, during the autumns of 1794 and 1795, but was compelled to desist from its use. It occasioned those unwelcome symptoms, which I shall hereafter notice, so frequently, and in doses so inconsiderable, that independent of its inefficacy, it became at best a very disagreeable remedy. It only succeeded in a few quartans; for so great was the inflammatory diathesis of the intermittents of those seasons, that their cure was most commonly effected by remedies of a very opposite character. From a retrospect of the preceding facts and observations, it will not be difficult to account for Dr. Clark's unfavourable deductions from the exhibition of arsenic, especially as he concludes his paragraph on that subject, with this observation: "When the continuation of the ague had brought on much weakness, I seldom in such cases tried the solution."

To elucidate more satisfactorily the operation of arsenic, in the cure of intermittents, it may be useful to reason on the condition of the system under the influence of that disease. Whatever may be the specific nature of miasmata, or of other remote causes of this state of fever, they all give a predisposition to disease, by inducing a state of either direct, or indirect debility, which when not induced by causes of a long continued or slow operation, leave the system more sensible to impressions. In this defenceless capacity of the system, every stimulus acts with double force, and the arterial system, which would always seem to be the first to take the alarm whenever the harmony of the animal economy is molested, is thrown into convulsive motions.* In this situation of things, the excitement is divided,

* These irregular motions have generally been attributed to the friendly interposition of an imaginary vis medicatrix; but they seem to be nothing more than the effect of stimulus disproportioned to excitability, or impulse disproportioned to resistance: we might as well say, that a ship under sail was actuated

and that portion which belongs to the others is concentrated in the arterial system. This state, which is a *fever*, continues to agitate the system, until it depletes from the arteries so much as to leave them upon a level with the other systems. With this picture of an intermittent before us, let us attempt its removal. In that preternatural state of excitement which constitutes a paroxysm, the disease is confined exclusively to the arterial system, and to cure it, the remedy should be directed specifically to that system. The use of stimuli will ever be an unavailing mode of practice, unless their power is so great as to overcome the diseased action* of the heart and arteries, to attempt which might often prove dangerous; we therefore leave this state to be treated by other remedies. Where the fever is almost in an evanescent state, and the action so feeble as not to call for a treatment in some measure antiphlogistic, the presumption then may be, that it can be subdued by arsenic, bark, or other stimulating powers: but that state of atony in which a paroxysm leaves the system, is what more particularly demands our attention in the cure. In this state, the excitement, although at a very low ebb, is presumed to be perfectly equal; but the same remote causes continuing to operate, reproduce the same coincidence of excitement and excitability, from which the disorder first originated, and thereby invite the same repetition of motions, with which it was before associated.

The excitability of the system, after the paroxysm of an intermittent, is, whether in a state of direct or indirect debility,† easily excited by the action of stimuli, unless the disease has become chronic by a long continued operation of causes, or a reiterated repetition of paroxysms. Arsenic may be advantageously used to prevent the recurrence of that condition from which the paroxysm first originated: this indication will be most effectually fulfilled, by administering it as nearly preceding the accession as possible. If the disease shall have become chronic, although the debility be ever so great, a powerful stimulus will generally be requisite to excite

by a vis medicatrix, because she did not stand still; for it is a universal law, that force unequal to resistance shall produce irregular motions or a deviation from order; nor will the difference between animate and inanimate matter afford us the means of discrimination.

* No two unequal actions can exist in the same system, at the same time; the stronger always deposes the weaker. I would as soon suppose, that two cubic inches of a solid, could each occupy the same cubic inch of space at the same instant: yet the arterial, lymphatic, and nervous systems, may each labour under a distinct disease at the same time.

† Dr. Brown has told us, that in cases of indirect debility the most powerful stimuli are necessary; but independent of the impropriety of stimuli, in many cases of indirect debility recently induced, this theory is erroneous; because, whether the debility be direct or indirect, the excitability is equally susceptible of the action of stimuli.

the system above the danger of a relapse. Although the versatility of the excitement and the excitability of the human frame is so great as often to render it difficult to account for all the phænomena of its morbid states; yet, from what we have already seen, we may presume with some confidence on the operation of arsenic in this and other states of disease, and perhaps distinguish some of those to which it is more particularly adapted. As we have already learned that arsenic acts powerfully upon the skin, some may be led to attribute the cure of intermittents to such an operation. If any benefit is derived from this part of its operation, it consists, like other stimuli, in restoring the excitement of the languid extremities; thereby equalizing it throughout the whole system. It is probable, that arsenic frequently excites some degree of inflammation in the stomach, which communicates a temporary artificial inflammatory diathesis to the whole system, and may thereby occasionally supersede the diseased action.

The epithet intermittent, has generally been intended to convey the idea of a disease of extreme debility, so great as always to require stimuli; but as diseases of a very malignant nature sometimes assume that type, and exhibit the specious aspect of a mild disease, when the patient is in the most imminent danger, particular care is necessary to discriminate: the causes producing such a disease, often prostrate the system suddenly into the most abject state of indirect debility, rendering the pulse almost imperceptible, with many other spurious symptoms of a common intermittent. Should a physician unfortunately attempt the cure of such an intermittent by arsenic, he would prescribe as judiciously as he would do, were he to administer liquid laudanum to a patient poisoned by opium.

IN PERIODICAL HEAD-ACHS.

Doctor Fowler, to whom we are already so much indebted, has recorded seven cases of *periodical head-ach* successfully treated by the mineral solution of arsenic, without the exception of a failure I do not know of a more vague appellation than that of HEAD-ACH; we hear of nervous head-achs, hysteric head-achs, and a variety of others; yet as we presume them all to be diseases of the arterial system, we shall arrange them accordingly. As the words periodical head-ach, are an indefinite mode of expression, it will be necessary to affix some determinate state of the system to the terms, before we can deduce our indications of cure with any degree of certainty. All the cases of periodical head-ach which have occurred to my observation, appeared to have been no more than intermittent fevers under a concealed form,* appearing at the same sea-

* "Febres intermittentes sub forma larvata" of Dr. Senac.

sons with other intermittents, and yielding to the same remedies. The Peruvian bark often succeeds in the cure of this disease, but so far as I have been able to ascertain, is far inferior to the solution; nor need this surprise us, for this affection generally exhibits a more chronic appearance, is not attended by much inflammatory diathesis, and is consequently admirably adapted to the operation of arsenic. There is no reason why this medicine should cure a periodical head-ach more certainly than others, provided they are constituted by the same degree of action; and from the general operation of arsenic we should be induced to expect the happiest effects from its use, in all cases not the consequence of considerable inflammatory diathesis. The head-ach is often a chronic disease; the effect of a feeble morbid action in the vessels of the brain, while the other parts of the system possess their usual powers. In all such cases as cannot be traced to some offending matter in the stomach, or to some local cause, arsenic may be recommended with the most flattering prospect of success. Although the action in this disease is so inconsiderable as to be easily overcome by arsenic, it is nevertheless indisputably the effect of excessive morbid excitement, and the pain is incontrovertibly the effect of inflammation,* without which there can be no pain. The proximate cause of all fever consists in an irregular action of the arterial system, and the most abject state of typhus is as essentially a fever, and as certainly the effect of some degree of inflammation, however insignificant, as a phrenitis or a pneumony. As the proximate cause of all fever is the same, there cannot possibly exist any other just distinction between them, than what arises from their different degrees of inflammation. The cure, with this view of fever, divides itself into two parts; such as require depletion to subdue them, and such as are capable of being overcome by the action of stimuli. Arsenic may be used advantageously in all such cases as require a new action to be suddenly excited, unless so great a degree of debility exists as to render other tonics, more certainly invigorating, indispensable.

IN DISEASES OF THE ALIMENTARY CANAL.

Among the variety of cases in which I had occasion to prescribe this medicine in the diseases of children, I sometimes observed, that such as were affected with symptoms usually judged to be most characteristic of worms, recovered under its use. The worms were in some cases discharged, but this occurred in a very small proportion of cases, in which the most prominent symptoms were effectually removed. In many of these cases the symptoms were so ambiguous, that I found it difficult to

* See Dr. Alexander's ingenious dissertation on the effect of one disease curing another, p. 20.

determine whether the arsenic acted by obviating a state of debility, with which worms were accidentally connected, or by destroying the worms acting as the cause of an idiopathic disease. In two cases, where the most unexpected cures were obtained, the symptoms rather indicated the phænomena of an atrophy from lymphatic obstructions, than of any other disease; and whoever reflects on the insinuating properties of arsenic, will not think it irrational to conjecture that it might have operated as a deobstruent upon the mesenteric glands. The pulvis stanni has formerly been a remedy much celebrated as a vermifuge, and has by some been supposed to act mechanically: although it is possible that it may produce some effect in this way, it would appear to me more philosophical to ascribe its virtues to the arsenic it contains. As this remedy, which suggested itself fortuitously, is yet problematical, with respect to its operation as an anthelmintic, we shall not presume to recommend it in preference to the more ordinary remedies; nevertheless, as they are all occasionally fallible, it will be laudable in such cases, to experiment with judicious caution on this new remedy.

IN DISEASES OF THE SKIN.

Were we to speak strictly anatomical, we might have classed these diseases with those of the arterial system, and indeed some of those under this head invade likewise the glandular and lymphatic system; but as they appear more conspicuously on the superficies, and affect more particularly this part, we have judged it most convenient to arrange them with the cutaneous affections. Whoever observes the force which this medicine exerts upon the skin, will find the transition to its use in cutaneous diseases natural, and the prospect of its advantages plausible. I received the first information of its success internally in cutaneous diseases, from the judicious Dr. Martin, of Maryland, who had witnessed its victory over an obstinate case of leprosy. Soon after I received this useful instruction, I was appealed to, to decide on the nature of a case which had run the ordinary routine of remedies generally resorted to in such cases, and which so strongly resembled the description the doctor had given of the case which he had cured, that I did not hesitate to denominate it the lepra Græcorum of medical writers. As this case had already become one of the opprobria of medicine, I undertook it with much diffidence, rather to gratify the solicitations of the patient's friends, than because I expected to perform a cure. I directed ten drops of the mineral solution to be taken morning, noon, and night: distance prevented my seeing the patient for the space of two weeks, but the medicine was assiduously persevered in. I now had the pleasure to observe, the hard white surfaces of the ulcers which had usurped almost the whole superficies of the body, beginning to moulder away in white pulverulent sloughs, and the bottoms of many of the sores, before of a phagedenic appearance, assuming a more salutary complexion. At the end of six weeks the disease was completely eradicated, and had not returned two years after. On my return to Philadelphia in the summer of 1792, I informed my preceptor, Dr. Rush, of what I had heard and seen concerning the internal use of arsenic; he soon had an opportunity of trying its efficacy in an obstinate herpetic eruption which had resisted the usual remedies; it succeeded, but as it had been a tedious chronic case, and the predisposition not completely removed, some symptoms of the disease have since returned.

I have repeatedly seen this remedy tried in various anomalous cutaneous affections, and find it a powerful medicine where neither fulness nor inflammatory diathesis exist; in which cases we shall soon see, previous depletion is necessary. In cutaneous diseases we may observe two states of the excitement diametrically opposite to each other: some cases depend upon a loss of tone in the extreme vessels, by which means the excitement is divided. The cure of this state consists in restoring the excitement, either by raising it by external applications, or by such internal powers as exert their influence principally upon that system of vessels. To prevent the vessels upon the surface from relapsing into their former atony, it will be found requisite in this state of the skin, to raise a higher tone than that which is the state of the same vessels in a state of health; an indication which may often be effectually fulfilled by arsenic. But, one stimulus will not always answer the desired indication, although it may be sufficiently strong; it will therefore often be prudent to use them in succession. The want of a certain knowledge of the particular system, which a stimulus specifically affects, often renders its exhibition problematical.

The remaining state of cutaneous disease, is in every respect the reverse of that already delineated. In both the excitement is divided and unequal; but in the latter its morbid force is concentrated in the skin, and may be denominated a *febris extroversa* with a propriety as strictly physiological, as the dysentery has been a febris introversa by Dr. Sydenham. This state is truly a local fever, and the indication of cure is to diminish the excitement in the extreme vessels, until it shall be reduced to an equality with the other parts of the system. It would be superfluous, and perhaps hazardous, in this state of the system, to prescribe astringent topics, or to administer stimuli internally, unless some can be found sufficiently powerful to raise a superior action in the vessels, a thing not easily accomplished. As there is no medicine more stimulating than arsenic, there can consequently be none more improper in this state of the system. Agreeably to this idea of the inflammatory nature of some cutaneous diseases, we find a fact recorded by Sir William Jones,* who observes, "The natives (of Indostan) cure the elephantiasis by one part of white arsenic united to six parts of black pepper; but the remedy is more certain when gentle cathartics and bleeding are previously used." By this practice two advantages are gained; first, the tone which prevented a more healthy action from being excited is removed; secondly, the excitability of the whole system is accumulated, by which means stimuli act with more force and certainty. The same author relates the case of a gentleman "so affected with the confirmed lues, (called in Asia, the Persian fire) with his hands and feet entirely ulcerated and almost corroded, that he became an object of disgust and abhorrence. Some blood was taken from his arm, and a cathartic administered on the next day; in a fortnight his recovery was complete." He farther adds, "But the power of this medicine has been chiefly tried in the cure of what has been called the Juzam, a disease affecting the whole mass of blood, attended in the last stage with an erosion of the fingers: it is also hereditary, and in that respect has been classed by medical writers with the gout, consumption, and white leprosy." This learned author assures us, that this preparation of arsenic and black pepper was successful in every case in which it was used, and relates a great variety of cases; but from his having placed the cause of the disease in a contamination of the fluids, he has not developed the disease so clearly as to enable us to follow him in its investigation. The pathology of the fluids is so obscure a corner in the field of science, that I would not presume to determine what share they may have in the causes of diseases. Whatever might have been the precise meaning of physicians by that class of medicines called *alteratives*, I am persuaded, that arsenic merits that character to a very eminent degree; perhaps not by a direct operation upon the blood, but by changing the state of the excitement, by its sudden and energetic action on the several systems it affects. I should have suspected the accuracy of the author's observation on this solitary case of syphylis, had it not been corroborated by a case which lately came within my own notice. A medical gentleman from the West-Indies, having been baffled in his efforts to cure a cutaneous affection of a syphilitic origin, consulted Dr. Rush, who advised the internal use of arsenic with the most propitious result. Arsenic in such inveterate chronic cases, seems to act by its power in exciting the system insensible to other stimuli, for in this case, even mercury had been used in vain. In buboes, that after ulceration have become callous, and not disposed to heal, but put on a cancerous appearance, I can say, both from my own observation and that of others, that no remedy with which we are acquainted, is so powerful as the internal use of arsenic.

* Jones's Asia, p. 479—80. Published 1793.

IN DISEASES OF THE GLANDULAR AND LYMPHATIC SYSTEM.

Much has been said concerning the use of arsenic in cancers, but from having seen it fairly tried in only two cases of ulcerated cancer, I cannot say much of its virtues.* It has been dogmatically asserted that arsenic is competent to the cure of every condition of cancer; an opinion which, so far as I can inform myself, is extremely presumptuous. Cancer, like all other diseases, is attended with different states of action, no one remedy can therefore cure every case. There is no disease whose pathology is involved in more obscurity than that of cancer, and every internal remedy that can be prescribed for it must be in some measure empyrical, until its causes are better understood. If there should ever be a radical cure for cancer found, and no doubt there will, it will probably be one that acts specifically powerful upon that system principally occupied by the disease, or by altering the condition of the whole system. The chimeras of fancy have often constituted a part of the theory of diseases, but none which I have ever read, are more visionary than the phantoms of Mr. Justamond's† imagination. This gentleman has by his boldness contributed in some measure to lessen the prejudice of physicians against the internal use of arsenic; but from his ludicrous hypothesis, of the disease depending upon insects in the cancer, he has detracted from the weight his observations might have carried with them; for his animalcules, like those of Liewenhoech, have either never been demonstrated, nor ever seen by any but their authors. In both those cases in which I have seen this medicine tried in cancer, the disease had so totally contaminated the whole system, that little hope could be reasonably entertained from any remedy; for if even in such cases the ulcer should be healed, unless the predisposition could be eradicated, a return of the disease would still await the unfortunate patient. It must be acknowledged, that in those cases wherein I attempted the use of arsenic in this disease, it was not altogether an inert medicine; the excruciating pains were mitigated, and the intolerable fœtor of the ulcers entirely corrected. These temporary alleviations, were of a very transitory duration, for those symptoms returned with their usual virulence as soon as the remedy was withheld. This medicine was persevered in six months in one case, and four in the other, yet no advantage was gained as to the healing or diminution of the ulcer. What might have been the issue of these cases under the use of arsenic at a more early period of the disease, I cannot venture to conjecture, but

* See Medical Commentaries, vol. ii. p. 304, &c.

† Justamond's Tracts.

esteem it some consolation to possess a medicine that can, when death is inevitable, strew flowers on the borders of the tomb. From the penetrating nature of arsenic, it would seem to promise the most beneficial effects in all cases of schirrous and obstruction in the glandular system; and from this property it has obtained the reputation of having cured the more inveterate states of cancer. It is often a desirable indication to rouse the torpid vessels of a part into vigorous action; under such circumstances, arsenic may be advised with more plausibility than any other remedy with which we are acquainted, provided it affect that system of vessels occupied by the disease.

As this mineral operates forcibly upon the lymphatic system, what would be its effects in scrophula? Although I have not seen it experimented upon in this disease, I should, a priori, be induced to think favourably of its powers, more especially where the system had been sufficiently reduced to admit of its most extensive influence. Wherever the same indications of cure are to be fulfilled, in the cancerous, callous, or fungous state of ulcers, the internal use of arsenic in moderate doses, affords the fairest prospect of success. In many cases where its external application is proper, it may be found advantageous to conjoin its internal use. There is no fact in the science of medicine, of which I am more decidedly convinced, than that arsenic is an improper medicine in all cases where ulcers are accompanied with an inflammatory diathesis.

The following letter from Dr. Martin may tend to corroborate some of the preceding observations, especially as it was written by a gentleman whose authority in medicine is inferior to none.

EASTON, FEBRUARY 1st, 1796.

DEAR SIR,

I HAVE long promised you some observations on the use of arsenic, which I shall confine within the bounds of my own experience. I have been in the practice of prescribing arsenic, about five years, in cutaneous diseases and intermittents only. My friend Dr. Birchhead first recommended the use of arsenic, and referred me to the Dispensatory lately published, for some hints under the head of mineral solution. Here I found that arsenic was recommended in cutaneous diseases. As I had been baffled in a case, which I had called lepra Græcorum, after using every remedy which could be thought of by myself or others, I resolved to try the mineral solution of arsenic on my old patient, Thomas M'Namara, who had wearied out every physician and others, who would administer any thing for his relief. As he had retired to some unknown part of the country, I mentioned my intention to Dr. Johnson, who had witnessed his deplorable situation in the poor-house, while a student with me, and desired that he would administer the solution whenever he should find him, engaging that I would do the same if I should see him first. The Doctor shortly after this fell in with him, and told him what we had agreed on. Poor Thomas was always eager to catch at any thing for relief. He was desired to take twelve or fifteen drops three times a day, but in a week or two the poor fellow had increased the dose to thirty drops, because he found his sores healing in a manner he had never before experienced, for at least five years, and I think, in less than four weeks, every sore or part affected, was healed up. Thus relieved, contrary to all expectation, Thomas began to make free with ardent spirits, when some appearance of the disease was again discovered, which was a second time relieved; but his intemperance soon brought on the disease with worse appearances, when he was once more admitted into the poor-house. I was astonished to observe how soon his sores began to heal, and to vanish entirely, except one up his nostrils, and even this to appearance was cured, when Thomas begged to be discharged. This winter he is a third time admitted, and it yet remains to be tried, whether he can be again relieved by arsenic. Quere: If arsenic had been used earlier, and this patient had been a temperate one, whether the predisposition to this disease might not have been entirely eradicated? A mulatto man in this county, (Talbot) aged about forty years, had symptoms of the lepra Græcorum before he was twenty-one; and I am well satisfied, when I saw him fifteen months ago, he had this disease with every characteristic symptom. The mineral solution of arsenic had a most astonishing effect in this case, for every symptom vanished in the course of a few weeks, except one sore on his leg or foot, and I have not seen him since last March. I once thought the mineral solution had a wonderful effect in a schirrous breast, but the predisposition still remained: the woman was of a bad habit of body, and some hardness continued to her death.

In agues and fevers I am sometimes induced to think this a valuable remedy, but like every other it is fallible, and I am frequently disappointed in its effects, yet I have known it to succeed, when the bark has failed. In some children in the ague and fever, it has an immediate effect. That it is a safe and useful remedy I am well convinced, and therefore give it to my own children without scruple. In my son it seemed to have no effect, good or bad, but has greatly relieved my little daughter in the ague and fever.

In the periodical head-ach, I have sometimes thought arsenic better entitled to infallibility than any other remedy in the materia medica.

Thus, my dear sir, I have summed up all that I can say about this safe, agreeable, and valuable acquisition to the materia medica. You are at liberty to make any use you may think proper, of the above observavations. With my best wishes for your success in life, I am your friend,

ENNALLS MARTIN.

OF THE EXTERNAL USE OF ARSENIC.

HOWEVER much timidity and scepticism may have influenced the minds of practitioners, respecting the internal use of arsenic, both empyrics and theorists have been less scrupulous in its external application. Two opinions have divided practitioners concerning its application to cancerous and other ulcers. Dr. Mosely* condemns its use unequivocally, and observes, "that it will not produce the salutary effects obtained by corrosive sublimate. It rots indiscriminately the sound and unsound flesh wherever it comes in contact. Corrosive sublimate is bounded in its corrosive action by healthy flesh, or acts but slightly as a destroyer. Arsenic has a tendency to destroy or deaden the functions of organized parts; corrosive sublimate to inflame those parts." In the introduction to this essay, I adverted to the disastrous consequences of not attending particularly to the doses of medicine, and the state of the part to which they may be applied; here we see it exemplified in a peculiar manner: we see prejudice co-operating with sophistry, producing a conclusion from false premises equally ridiculous and absurd. Every practitioner who has seen arsenic applied externally in different degrees of strength, must testify against the injustice of Dr. Mosely's criticisms. The principle upon which the operation of arsenic essentially depends, is the same that actuates corrosive sublimate, of which he has spoken so extravagantly. Arsenic, in the state it is used externally, is a true metallic oxyde, or the semi-metal united to vital air. Corrosive sublimate is composed of the oxygenated muriatic acid and mercury, and owes its activity to the oxygene it contains, otherwise calomel, which is composed of the same metal, united to the common muriatic acid, would prove equally corrosive. Red precipitate, which is a calx of mercury, or that metal united to vital air, is likewise a caustic of considerable power, but if it be subjected to a strong heat, the oxygene will be dissipated, the metal will resume its native state, and is as innocent in actual contact with the most irritable surface as so much water. It would therefore appear that the operation of these medicines depend upon the oxygene they contain, and that their powers are accurately apportioned to their relative degrees of fixity and concentration, Nothing can illustrate this idea more clearly than what we must have often observed; that caustics and escharotics in a state of dilution or division act as the most certain astringents. In this state arsenic and corrosive sublimate, produce the most salutary effects, and are nearly entitled to infallibility, in all cases of tenia, herpes, and other states of the skin, not supported by fulness or inflammation. The particular condition

* See Treatise on Tropical Diseases, by Benjamin Mosely, M. D. p. 521, 2, 3.

of the part to which arsenic is to be applied, should be carefully observed; if much inflammation attend, it should be used in a very diluted state, and may then be advantageously applied to any part. At the request of Dr. Rush, I applied a solution of arsenic to a cancerous inflammation in the internal canthus of the eye, where the rapid progress of the disease menaced the erosion of the lachrymal sack, and probably the patient's life: we had the satisfaction of seeing the disease completely extirpated, and the man soon restored to health.

In the year 1784 Dr. Rush* detected the presence of arsenic in the celebrated cancer powder, so successfully administered by Dr. Hugh Martin; and has favoured us with observations on the use of this caustic. He remarks, "I should suppose from the examination of the powder I made with the eye, that the proportion of arsenic to the vegetable powder could not be more than $\frac{1}{40}$ part of the whole compound. The great art of applying arsenic successfully is, to dilute and mix it in such a manner as to mitigate the violence of its action. Dr. Martin's preparation was happily calculated for this purpose. It excited a moderate inflammation, which separated the morbid from the sound parts, and promoted a plentiful afflux of humours to the sore during its application. It seldom produced an eschar; hence it insinuated itself into the deepest recesses of the cancers, and frequently separated those fibres in an unbroken state, which are generally called the roots of the cancer." Thus we see, that however useful it may be to attend minutely to the state of the system in the internal use of arsenic, that it is equally indispensable in its external application. Its operation depends upon the same principles of excitability and excitement, and it must be obvious to all who are the least conversant in the practice, that so acrimonious a substance must require the most cautious attention. Upon a review of all the cases of the external use of arsenic that have been recorded, we find no discrimination of the different states of the parts to which it has been applied; but from all that we can learn on this subject, we are authorised to say, that its beneficial effects are principally confined to such as have been already enumerated.

In the year 1783, an itinerant practitioner, who called himself Lafferti, travelled through the state of Maryland: he astonished the practitioners of that country by curing ulcers long deemed beyond the reach of the surgical art, and it is not to be controverted that his success was unparalleled. It was observable, that he refused to undertake the cure of recent ulcers, and, unlike most of his empyrical brethren, candidly acknowledged that he had no skill in such cases as others considered most curable. The author's deceased father who at that time practiced physic and surgery in

* See Rush's Medical Inquiries and Observations, vol. i. p. 235, or, Philosophical Transactions.

that country, left the following account of this practitioner, in a letter which he intended to have sent to a medical friend. "We have in this county (Caroline) a man who does wonders in the cure of obstinate old sores; but he uses so much mystery, and applies his powder with so much secrecy, that he does not seem to intend to let us into the secret. However, I have just procured a small parcel of his medicine; at first I thought it looked like corrosive sublimate, but upon trial found myself mistaken: I put some of it on the fire, which soon perfumed the room with the smell of garlic, from which it must be arsenic." Mr. Justamond, who has recommended arsenic so strenuously in cancer, mentions the authority of Sir Hans Sloane for its good effects in scrophulous ulcers, and thinks it a valuable medicine in such cases. I have no doubt but that arsenic will remove the state of atony often attending such sores, or that cancerous state of callosity into which they sometimes degenerate; but to cure them radically, the diathesis on which they depend should be removed. The muriate of barytes* is said to have proved useful in schrophula, as it often contains a small portion of arsenic, it is not improbable that it may owe its virtues to this active mineral. What might be the effect of the arsenical acid, as a medicine, I leave future researches to instruct us.

PHARMACEUTIC TREATMENT OF ARSENIC.

It may perhaps be thought necessary to investigate the chemical properties of arsenic, but as all I could say on that head would be no more than plagiarism from authors already in general circulation, I shall content myself with as laconic an explanation as possible of the pharmaceutic treatment of that preparation, which appears to me to possess some advantages over all others, and which so far as I am capable of judging, admits of no improvement.

The following receipt for making the mineral solution is translated from the Latin of Dr. Fowler, and the table of doses which we shall have occasion to mention, is taken from the same author; both of which it may not be improper to insert, as arsenic is a medicine so little known.

"Take of the powder of white arsenic, and of the purest vegetable alkali, each sixty-four grains, of distilled water half a pound, apothecaries' weight: put them into a vessel and submit them to a sand heat; let them boil moderately until the arsenic shall be perfectly dissolved; then add to the cold solution half a pound of the compound spirit of lavender, and so much distilled spring water as will make the whole accurately fifteen ounces."

* See Bell on the Venereal, and Med. Com. vol. 5.

The simplicity of this chemical preparation renders it preferable to more complex forms. The vegetable alkali has not the smallest effect in diminishing its virtues, for that proportion of the solution which we know to contain any given quantity of arsenic, will act as forcibly upon the system, as the same quantity in pills, or even in a state of pulverization. The almost infinite divisibility of this form renders its doses variable to the exigencies of all possible cases. The small proportion of compound spirit of lavender is added to give it a more medicinal appearance; not with a view of captivating the eye, by drawing the veil of mystery over the composition, but lest from its being colourless and insipid, those who may be intrusted with its exhibition should be tempted to use it with too much liberality, the consequences of which might prove troublesome, if not dangerous. To a pound of the solution, sixty-four grains are added for the purpose of a more accurate calculation, by which means the precise quantity of arsenic contained in any given number of drops may be ascertained. If the alkali should not be perfectly pure, it will be found inadequate to the production of a perfect solution; a circumstance which might occasion great confusion and uncertainty in the doses of the medicine. If therefore, the alkali cannot be obtained pure, a double proportion of purified nitre may be substituted, for there is a stronger attraction between the arsenic and the vegetable alkali, than between the same alkali and the nitric acid, which last is therefore disengaged. The two solutions do not differ in point of efficacy, and by attending to the preceding directions, they will be found to possess a uniform degeee of strength; a circumstance of importance in the use of such a heroic medicine.

Although we cannot altogether approve Dr. Fowler's mode of administering the solution, and must therefore observe, that his doses are rather larger than we prefer, (at least in this country) his table may be useful in graduating the doses for different ages.

Patients are to take, according to their ages, the following doses of the solution:

YEARS.	DROPS.
From 2 to 4	from 2 or 3 to 5
—— 5 — 7	—— 5 — 7
—— 8 — 12	—— 7 — 10
—— 13 — 18	—— 10 — 12
—— 18 and upwards	12

Thus from five to seven years the dose may be apportioned by allowing a drop for each year, but a drop for each year under that period will be insufficient, and soon becomes too much beyond it, as twelve drops are a medium dose for an adult.

This medicine may be administered with considerable latitude to adults, but a very general rule may be established which will often prevent most of those unwelcome consequences which follow the use of large doses. It will generally be found most advantageous in the end, to begin with an under dose, and to increase it until it shall affect the stomach slightly, unless a cure be obtained. If the system should be much disordered by the solution, it will be proper to discontinue it a day or two, and instead of ten drops three times a day, five drops six times a day may be administered, which will often agree with the stomach, and perform a cure as certainly, though not as expeditiously, as a larger dose. It will often be necessary to continue the medicine for some time after the cure is apparently complete; by this practice a relapse may often be effectually prevented. Where nausea, vomiting, or pains in the bowels arise from the solution, they may not only be mitigated, but often prevented, by combining a few drops of laudanum with the solution, and this will seldom interfere with the virtues of the medicine, as the former is generally admissible where the latter is proper. A combination of their stimulant effects will sometimes be found more powerful than either of them alone, especially in curing intermittents. In some cases instead of diluting the dose by a tea-cup full of cold water (the usual vehicle) in case of turbulent symptoms supervening, a larger proportion of water will be a successful method of obviating them. This observation applies more particularly to the use of the solution among children,* whose tender organs are often molested by a very small dose of the solution; instead therefore of giving the medicine in a tea-spoon or table-spoon full of water, double the quantity may be used to advantage. The disease for which the medicine is prescribed, will likewise require to be noticed, both with respect to the quantity of the dose, and the time of administering it. In intermittent fevers and periodical head-achs most advantage wiil result from the administration of as large a dose as the system can conveniently bear, as nearly preceding the paroxysm as possible. In the treatment of cancers and many diseases of chronic debility, it may be necessary to continue the use of the solution for weeks, and even months, to obtain all its advantages. Under such circumstances, the solution must be frequently gradually increased, for the system becomes so habituated to its stimulus, that an ordinary dose will be altogether inert. I have gone as high as thirty drops three times a day, in a case of cancer, without producing one disagreeable sensation. In the exhibition of this medicine, little is to be learned from an apparent delicacy of constitution, for women whose appearance would lead us most to expect irritable frames, often bear the medium dose of an adult with the greatest composure; whereas the most robust men frequently feel very sensibly the commotions excited

* It is nevertheless worthy of observation, that children often bear larger doses, in proportion to age and other circumstances, than adults.

by a smaller dose. I have given this medicine to pregnant women labouring under intermittents, with safety, in very considerable doses, but cannot avoid observing, that although these cases were such as in every respect (their gravid state excepted) might from common experience be supposed most easily cured, I was less fortunate than in any equal number of cases that came within the sphere of my notice. The tension imparted to the arterial system by the stimulus of distension, or that artificial inflammatory diathesis which accompanies a state of pregnancy, must have prevented the medicine from exciting an action sufficient to cure the disease.

It has been alleged against the internal use of arsenic, that it destroys the tone of the stomach, thereby laying a foundation of dyspepsia and general debility. If this objection should be founded on truth, it will alone be sufficient to exclude arsenic from the materia medica, and to banish from the mind of every reasonable physician all thoughts of advocating its character. To determine this important question beyond the possibility of a controversy, I examined all those cases wherein I was under the necessity of persisting long in the use of arsenic. Out of forty persons whom I interrogated touching this point, I found but two who discovered dyspeptic symptoms, both of whom were notorious for their attachment to ardent spirits, by which the disease had been produced years before they had taken arsenic. Even dogs that had been poisoned by it and recovered, exhibited no marks of indigestion.

It has moreover been objected to arsenic, that both from its internal and external use, it has sometimes produced paralytic symptoms and a vertiginous disposition in the brain; but in all the cases where I have seen it used, even where, from a long protracted external application, an absorption might have been thought probable, no such consequence followed. Such effects have doubtless followed the poisonous influence of arsenic, but those who cannot draw the line of distinction between its medicinal and poisonous degrees, would do well not to interfere with the feelings of mankind. Mr. William Gaskill,* an ingenious surgeon, at Rotherhythe, instituted a series of experiments upon the external absorption of arsenic, from which he proved decisively that no symptoms of a disordered economy were even perceptible. He has not taken notice of its diuretic qualities, although they are dwelt upon with so much emphasis by Mr. John Sherwin,† surgeon, who performed the same experiments with tartarized arsenic, and attributes the diuretic effects it produced upon himself and four others, exclusively to the operation of arsenic. But the conclusion he has drawn is by no means just, and a

* See a pamphlet entitled, Experiments on the external absorption of arsenic and emetic tartar.

† Medical Commentaries, vol. xv. p. 220, et seq.

very superficial knowledge of chemistry will be required to detect its fallacy. By the union of the chrystals of tartar and arsenic, the tartarized arsenic is formed; a substance which, although it partakes in some measure of an arsenical nature, is yet widely different from the pure semi-metallic oxyde, and possesses properties peculiar to itself.

Dr. Fowler has recorded many unpleasant effects of the solution; such as nausea, vomiting, swellings of the face, and sometimes of the abdomen, all which, he says, vanish from the use of gentle aperients, and most of them by a temporary omission of the medicine. Doctors Arnold and Withering, although much in the habit of using the solution, have not mentioned such effects; and even Dr. Clark, who inveighs with so much acrimony against arsenic, has been silent on this particular. I first began the use of the solution in the doses prescribed by Dr. Fowler, and experienced many of the painful sensations which he has ascribed to it. I afterwards found it easy to prevent them in most cases, by diminishing the dose, and observing the precautions already mentioned.

One of the most characteristic properties which we have attributed to arsenic, is, its power of accumulating the stimulability of the system; a circumstance of much importance to be attended to in practice, especially in a state of convalescence; inasmuch as both medicine and diet are to be regulated accordingly, either of which, in over doses, might be productive of the most alarming consequences. This caution cannot be better enforced than in the emphatical words of the elegant Dr. Armstrong.

. . . . When the vital fire
Burns feebly, heap not the green fuel on;
But prudently foment the wand'ring spark
With what the soonest feels its kindred touch:
Be frugal even of that; a little give
At first; that kindled, add a little more,
'Till by deliberate nourishing, the flame
Reviv'd, with all its wonted vigour glows.

OF THE DELETERIOUS QUALITIES OF ARSENIC.

Hitherto we have viewed arsenic as a remedy, capable of abstracting from that portion of pain and disease to which the frailty of human nature is subjected. But as this mineral, like all other things destined for the use of man, is liable to abuse, and subject to a deviation from that order which was originally imposed upon it; we are constrained to the melancholy necessity of reversing the picture, and of contemplating human nature in the most deplorable state that the imagination can possibly conceive. Happily for mankind, such catastrophes as this poison is capable of producing do not often occur; but as physicians are sometimes summoned to arrest the progress of death from this cause, the most effectual antidote becomes a desideratum of the highest importance.

It would far exceed the limits of this essay to detail all the experiments from which we have deduced the following conclusions; we shall therefore give the result of the most important, in as concentrated a form as the nature of the subject will admit.

We have already hinted at some of the formidable consequences of this poison; and where its medical effects end, we may date the commencement of its deleterious qualities. This stygian draught when taken into the stomach in quantities disproportioned to the excitability of the system is productive of nausea, vomiting, purging, hiccough, gastrodynia, convulsions, subsultus tendinum, increased flow of saliva, hematuria, thirst, gnashing of the teeth, syncope, asphyxia, and death, unless a speedy remedy is administered. As some peculiarities attend the operation of this poison, it may be useful to trace them to their remotest consequences. A gentleman whom I saw, and with whom I conversed, soon after he had been nearly deprived of his life by this poison, exhibited the following phænomena. From having been remarkable for his althletic powers, he became sallow, emaciated, and enervated. Previous to this accident he had enjoyed an uninterrupted series of good health for tèn years. In the autumn after this misfortune, he was attacked by an obstruction of his liver, which left him in a state of paralysis, from which he with difficulty recovered during the winter. He has been subject to jaundice three or four times every year since that memorable event, and his teeth, before remarkable for their whiteness, became incrusted with a black scale, and some of them have decayed without pain. It has sometimes been observable that arsenic, even in small quantities, like the vegetable and other acids, has set the teeth on edge; and some of the dogs who recovered from the poisonous effects of arsenic lost their teeth, an occurrence I apprehend to be very unusual in these animals. Whether arsenic produces this effect by a general operation, inducing a general debility in consequence of which the teeth decay, or whether its peculiar acid acts specifically upon the calcarious earth of the teeth, I shall not presume to determine.

Mr. William Lempriere,* an intelligent English surgeon, has drawn the picture of a case from the poisonous effects of arsenic, which is sufficient to demonstrate, that even in some cases where a recovery is obtained, life is under such circumstances the most intolerable of human burdens. He observes, "I was desired to visit the emperor's favourite wife, who had been poisoned by arsenic, conveyed into her food by the machinations of her rivals. After a tedious conflict between life and death, the effects

* Tour to Morocco.

of the poison in part abated, but the unhappy lady was left in a dreadful state of debility and irritation. Her beauty, the fatal cause of her misfortune, was completely destroyed, and her enemies, though disappointed in their aim at destroying her life, yet enjoyed the malignant triumph of seeing those charms which had excited their jealousy, reduced below the standard of other women. Her digestion was so weak, that every species of food, after remaining a few hours on her stomach, was returned perfectly crude and undigested. Her body was reduced to a shadow, and her strength so far exhausted that she could not walk without assistance. Her skin, from being naturally clear and fair, was changed to a sickly brown, which joined to a ruined set of teeth and ghastly countenance, effaced every trace of that beauty which she might once have possest."

Various antidotes have been proposed to counteract the poisonous effects of arsenic. Oils and such other substances as seemed best calculated to obtund the acrimony of the metallic particles, have been supposed adequate to the relief of the pernicious effects of all saline poisons. Sceptical on this subject, I instituted a series of experiments upon dogs, the result of which clearly demonstrated, that no oleaginous substance is equal to the prevention of evil from such causes. In cases where the portion of poison was inconsiderable, mucilaginous and oily matters seemed in some measure to protract and mitigate the symptoms, but never afforded entire relief; for such as recovered had taken so small a quantity as scarely to be capable of doing mischief if no remedy had been attempted. I used for these experiments the oil of almonds, train oil, linseed and castor oils, all of which proved insufficient. The castor oil, where it was given in quantities so large as to operate speedily, seemed to procrastinate life, by translating the seat of the disease from the stomach to a less vital part, the lower intestines. When the arsenic was given mixt with the oils, its virulent effects were not obviated, unless the quantity of the poison was so small as to remain suspended, and not to come in contact with the stomach and intestines. Conformable to this idea of the insufficiency of oils to prevent the effects of arsenic on the stomach, we may regard a custom authorised by the superstition of the Hindoos. One of the nine modes of trial by the ordeal consists in compelling the accused to eat from the hand of a Brachman a preparation composed of sixty-four parts of clarified butter, mixt with two parts and a half of pure arsenic; if the poison produce no visible effect, he is absolved, otherwise condemned.*

Milk has been proposed as an antidote against arsenic and other poisons, but proved inert in every instance, although I gave it the fairest trials, and never produced the smallest benefit, only in proportion as it

* History of Indostan.

diluted or washed off the poison concentrated in the stomach. Whoever will observe the specific gravity of arsenic, must readily conceive the difficulty of defending the stomach against its corrosive qualities. The impracticability of such a hypothesis is farther augmented by reflecting, that the arsenic is always in actual contact with the stomach before the antidote can possibly be administered, and that all attempts for relief must be superfluous, unless the poison be instantaneously removed. The premature exhibition of viscid substances may moreover interfere with the operation of that remedy which we shall see hereafter affords the only rational prospect of relief. Where the poison has been evacuated, and a slight inflammation still remains, oils may prove useful by their lubricating quality.

I attempted the relief of those devoted victims by a variety of diluents, given copiously immediately after the poison; but they all proved futile, and their synchronous exhibition was attended with the same fate.

Chemistry has furnished a variety of substances which have been thought equal to the neutralization of arsenic. Vinegar has been extolled by Mr. Sage; but whatever appearances the combination of these agents may exhibit to the eye, I can assert, from repeated experiments, that when they meet in the stomach they do not rescue the body from destruction.

Mr. Navier, an ingenious French physician and chemist, has proposed to decompose arsenic by a direct combination of the liver of sulphur. We know that orpiment, although it contains a large proportion of arsenic united to sulphur, may be taken into the stomach in a considerable quantity with impunity. This would at first sight seem to favour the idea that sulphur alone might neutralize arsenic, but from a variety of experiments we can assert the contrary. The hepar sulphuris is a most rapacious solvent of some metals, and might therefore aptly obtrude itself upon the prolific imagination of a speculative chemist. Whoever will be at the trouble of mixing arsenic and the liver of sulphur, will be amply satisfied of their slow and feeble influence upon each other. Their action is so slow, even out of the body, that a man might die a thousand deaths before a single particle of arsenic could be neutralized. Every experiment tended to confirm me in the opinion, that chemistry has yet invented no power capable of neutralizing arsenic. The animals upon which the experiments were made individually died, although in many cases a few grains only had been taken, and the proportion of hepar, mentioned by the fanciful Mr. Navier, immediately administered. The proportions of each were varied, and the experiments repeated, with a result equally unpropitious. Whatever effect the alkali may have in the formation of the hepar, I am satisfied that sulphur alone will do as much towards effecting a cure as in their combined state; for death will ever be the inevitable consequence of the arsenic, although they may both be used with ever so much liberality and expedition. No remedy can ever alleviate these melancholy preludes of death, unless it operate with the velocity of light as a solvent, or evacuate the poison from its contact with the stomach. I defy the imagination to conceive of a poison more irresistible in its operation than a large dose of arsenic; it will therefore be irrational to indulge a hope of averting its instantaneous effects, unless a remedy can be invented that shall equal it in the rapidity of its operation. I will not dogmatically affirm that chemistry does not possess a substance adequate to the instantaneous neutralization of arsenic, but can safely say, that none of the great variety upon which I have so repeatedly experimented, is equal to this important indication. History has recorded an antidote for this herculean poison, which we are *seriously* informed is infallible. "The best antidote against the poisonous effects of arsenic are the scrapings of leather reduced to ashes: if the quantity taken be accurately known, four times as much of these ashes mixt with water, and drunk by the patient, will sheathe and counteract the poison."*

Baffled in every attempt to prevent the fatal effects of arsenic taken into the body, I attempted it by the use of the most powerful emetics that could be obtained. In every case where the quantity was not so great as to destroy life suddenly, or to render the stomach altogether insensible, an effectual relief was obtained. If the length of time between the taking the poison and the exhibition of the emetic shall be considerable, all attempts for relief will be in vain. The strongest emetics that can be obtained should be administered as soon as the accident shall be discovered, and however copious their effects may be, large quantities of warm water should be immediately taken, and persisted in, until it may be supposed that the whole is evacuated. The warm water not only washes off the acrimonious particles of the poison, but accelerates the operation of the emetic. Amongst the variety of emetics of which I made trial, I found the vitriol of zinc the most certain: indeed I can say with certainty that it afforded complete relief in all cases where the excitability was not nearly extinguished. After a partial evacuation of the stomach, I endeavoured to finish the cure by such substances as might be judged most powerful on account of their inviscating qualities; the uniform consequence of which was, to retain the poisonous particles in closer contact with the stomach, and to expedite the approach of death.

* History of Indostan, p. 481.

TESTS FOR DISCOVERING THE PRESENCE OF ARSENIC.

It may often be a desirable thing to determine satisfactorily what poison has been the cause of such distressing symptoms. The presence of arsenic may be detected in two ways. 1. If the least particle can be perceived and burnt, it will emit white fumes, and an evident smell of garlic. 2. Confine a small quantity of arsenic between two plates of copper, and subject them to a strong heat, a white appearance will be communicated to the copper. These methods are sufficient to detect the presence of arsenic, even where it may be diffused among the contents of the stomach, or present in a very minute proportion; but whoever shall feel himself dissatisfied on this point, may use a method communicated by Mr. Bergman. Infuse a small portion of the powder in a solution of vegetable alkali in water; after standing an hour or two, pour upon it a solution of the sulphate of copper in water; the colour of the vitriol will be immediately converted into an elegant green, and will soon be precipitated. The same experiment may be used to detect its presence in water.

Arsenic is a substance which is copiously diffused through the bowels of the earth: it is a component part of many metallic products, and may, by its latent distribution among them, become the unsuspected cause of the most serious calamities. Tin, as we have already observed, contains a considerable portion of arsenic; we ought therefore to be cautious in admitting it into the composition of culinary utensils, especially such as may be intended to contain acids, or to be much exposed to great heat.

Pewter likewise sometimes contains a small portion of arsenic, but the quantity is so insignificant as not to be justly an object of terror: it is nevertheless, a duty incumbent on the manufacturers of these metals, to ascertain with precision what proportion of arsenic their materials contain. If we dissolve tin which contains this substance, in the muriatic acid, the solution will exhibit a black powder, which consists of the arsenic separated from the tin. This experiment renders the smallest particle conspicuous.

The property which arsenic possesses of being soluble in water, multiplies and facilitates its destructive powers; springs and rivulets are sometimes impregnated by flowing over this noxious mineral, and those who inhabit their vicinity may fall victims to their insidious influence before a suspicion of the fatal cause shall arise. Besides the method we have already described, for discovering the presence of arsenic in water, it may be accomplished with more simplicity and equal certainty, by evaporating the water in a clean iron vessel: a portion of the arsenic will be deposited on the sides and bottom of the vessel, and when thrown upon burning coals, will emit the well known garlic-like odour. If a copper vessel be used, the inside of the vessel will become white.

The same end may be attained more expeditiously by evaporating the water rapidly from an ignited iron; but this method is liable to a deception, for no odour will be emitted unless the water be strongly impregnated by the semi-metal. These methods may, however, prove less accurate than others devised by the ingenuity of chemists. The most infallible with which we are acquainted, are the following:

1. If a solution of the hepar sulphuris be poured into water adulterated with arsenic, a colour more or less yellow will be produced, and if the sulphur superabound orpiment will be deposited.

2. If boiling lime water be poured upon water holding arsenic in solution, a white precipitate of difficult solubility in water, will fall down. This precipitate is soluble in the acetous acid, and in a solution of arsenic; when mixt with oil and laid upon the fire, it yields the garlic-like smell peculiar to arsenic.*

3. Cuprum ammoniacum affords an excellent means of detecting the presence of arsenic in any liquid; it produces with it a yellowish green precipitate, which, if separated from the superincumbent liquor, dried and put upon ignited coals, manifests the same garlic-like odour.

Other tests might be devised to ascertain the presence of arsenic by different re-agents, but it would be a work of supererogation, as those already described are deemed amply sufficient to detect it in its almost infinite variety of combinations.

It has been the object of the preceding essay to collect such information as could in any wise tend to illustrate a subject as yet in its infancy. As an impartial investigator, the author has, unbiassed by prepossession or prejudice, extolled or condemned it agreeably to the suggestions of his own judgment. It may perhaps be observed, by those who have experienced the difficulties of adapting medicines to particular exigencies and to the various conditions of disease, that more minuteness and precision are necessary in the use of so active a medicine: but whoever will attend to the principles that govern the operations of this medicine, as they are laid down in the progress of the subject, will find its management both practicable and easy. It would have been easy to have decorated every page with the tinsel phantoms of the imagination, but as the theory which has been contemplated, is the inevitable consequence of the phænomena of diseases, or the obvious operation of the medicine, the author ought in justice to be rescued from the implication of vanity or presumption. Should any thing have escaped him which shall be hereafter found erro-

* Chemical tests, invented by J. F. A. Gotling, Professor of Chemistry at Jena, in Saxony.

PREFACE.

IN treating of the Nitric and Oxygenated Muriatic Acids, it may be necessary to premise, that it is not my intention to speak of their chemical affinities to the substances which surround us. As this is very accurately and minutely taken notice of, in every system of chemistry, I mean to confine myself to their medical properties, and their effects on the human body, as this part is that with which physicians are, as yet, least acquainted.

In pursuing this subject then, I shall first, in as concise a manner as I am able, give the history of the discovery of the *use* of the Nitric Acid in diseases: next relate some cases in which it was exhibited, and draw such conclusions from them as they justly warrant: I shall then treat of the Oxygenated Muriatic Acid in the same manner, and endeavour to prove that these acids act on the same principle with mercury in the cure of the venereal disease.

ON THE NITRIC ACID.

THE history of the important discovery of the use of the Nitric Acid in some diseases, is as follows:

Mr. William Scott, surgeon in the service of the East India Company, (in August, 1793) imagined that the obstructions of the liver were occasioned by the bile depositing its resin; and desirous of becoming acquainted with the modus operandi of the calces of mercury, which seem so peculiarly qualified for removing those obstructions, he instituted a series of experiments on the bile.

By mixing a quantity of the resin of the bile, (carefully separated from its soda and lymphatic matter with which it is united) and half its weight of the red calx of mercury with ten or twelve ounces of water, and exposing them to heat, he found, that part of the oxygen of the mercurial calx, had combined with the resin, an dmade it surprisingly more soluble in water. By his experiments on the base of the bile, having thus found that oxygen made it more soluble in water, and being at that time afflicted with chronic hepatitis, he resolved to take a quantity of oxygen united to some substance, for which it has no great attraction. After some reflection on the subject, nothing appeared to him so well calculated for the purpose as the nitric acid, which is known to consist of about four parts of oxgyen, united to one part of nitrogene, with a certain portion of water.

In September, 1793, says Mr. Scott, I began to take the nitric acid. I mixed about a drachm of the strongest I could procure, with a sufficient quantity of water; and was happy to find that I could finish that quantity, in the course of a few hours without any disagreeable effects from it: the following is the journal I kept of myself at that time.

September 11th, 1793, 1st day. Took at different times about a drachm of the strong nitric acid, diluted with water. Soon after drinking it, I felt a sense of warmth in my stomach and chest; but no disagreeable sensation from it, nor any other material effect.

2d. I have taken to-day a considerable quantity of acid, diluted with water, as much as I could easily drink during the forenoon.

3d. I have continued the acid. I feel my gums affected from it, and they are somewhat red, and enlarged between the teeth; I slept ill, but could lie for a length of time on my left side, which from some disease in my liver, had not been the case for many months before. I perceive a pain in the back of my head, resembling what I have commonly felt when taking mercury.

4th. My gums are a little tender: I continue the acid as before, I still find a pain in my head, and about my jaws, like what arises from mercury. I perceive no symptoms of my liver complaint.

5th. I have taken the acid, and always feel an agreeable sense of heat after drinking it. I spit more than usual.

6th. I continue the acid. I observe my mouth sorer to-day, and spit more.

7th. I think I am now sufficiently oxygenated. I feel my mouth so troublesome, that I shall take no more acid.

From this time my mouth got gradually well, and I found my health considerably improved.

Mr. Scott administered the nitric acid in several cases of tedious intermittents, in two cases of diabetes, in each of which the subjects were in the decline of life, and in a number of syphilitic cases with the happiest effects.

This short account of the discovery of the use of the nitric acid in diseases, I have thought it my duty to give, in honour of the discoverer: and it is done with more pleasure, as it was the result of reflexion and well directed experiments; and not, as is frequently the case, stumbled upon by accident.

Mr. Scott's account of the nitrous acid, was first published in the Bombay Courier, of April 30th, 1796. It has since been republished in the first volume of Dr. Duncan's Medical Annals, and also in the first volume of the New-York Medical Repository.

In the latter end of August, 1797, I met with Mr. Scott's account of the nitric acid, in Duncan's Medical Annals: and have since that time given it in a number of syphilitic cases, with the happiest effects.

As the subject is new, and nothing is wanting to bring this invaluable medicine into general use, but facts in its support, I will relate some of the cases which came under my own observation.

CASE I.

September 1st, 1797. F. M'C——r, came into the alms-house as a pauper. She had four chancres on her labia pudenda and nymphæ, which she had contracted three weeks before. I directed her to take five drops of the nitric acid every hour in a little sweetened water, which she continued for a fortnight; her mouth was slightly affected, three of her chancres had entirely healed and the other greatly diminished. She was at this time attacked with pleurisy, so violent as to make me neglect the syphilitic complaint. Her pleurisy was cured by four bleedings and cathartics of glauber's salts and emetic tartar. I thought no more of her syphilis for six weeks after this, when she informed me that her old complaint had returned. Upon examination I found that two of the chancres had reappeared and were nearly of the size of a small button. She recommenced taking the nitric acid, and continued its use three weeks longer, at which time she left the alms-house. But previous to her departure she informed me, that her sores were entirely well, except (to use her own expression) "a small spot about the size of a pin's head."

December 27th. After being out of this institution three weeks, she this day called on me to request more acid; telling me at the same time, that the sore which had not been entirely healed, had enlarged to a size that would admit the end of her little finger.

I gave her the nitric acid as before, and desired her to call on me every three or four days. She faithfully took the acid for four weeks more, when the chancre appeared to be perfectly well. But by way of insurance I desired her to take the acid two weeks longer, which she did.

I saw her the other day; it is upwards of a month since she has omitted the acid, and she informed me, that she has been perfectly well ever since.

CASE II.

October 8th, 1797. T. D. aged twenty-three years, was received a pauper in the alms-house: he was afflicted with pains and ulcers in his limbs. The history of his case is as follows: In January last he was infected with the venereal disease, he had a gonorrhœa, chancres, and an enlargement of the glands in his groins, which had been discussed by mercury, and he considered himself as cured. Fourteen weeks ago, he took the small pox (and had the disease mildly) the natural way; shortly after the small pox had run its usual course and disappeared, small pustules arose on his legs which degenerated into ulcers, in their appearance resembling those of a venereal nature; from which time to the present they have continued to increase, and he now has on his lower extremities six and twenty ulcers: he also complains of pains in his limbs.

I gave him the following mixture:

℞ Gum: Arab: iv drach: iv.
Aquæ menth: vi unc: vi.
Acid: nit: ii drach: ii F. M.

With directions to take a table spoonful every hour, mixed with sweetened water.

October 11th. The third day after he began with the acid, and when he had taken but four drachms of it, I found him lying on his bed, with his hand supporting his head, and the saliva driveling from his mouth into a cup. His mouth was very sore; his gums had a similar appearance with those of persons slightly affected with mercury. The salivary glands were much enlarged. His breath, at present, has no offensive odour, though he says that when he first began to spit, it was very disagreeable. The ulcers on his legs look cleaner. His mouth is so sore that the acid is omitted.

October 16th. His mouth is much better: his ulcers are in a healing state, but the pains in his bones still continue. He was desired to take the acid again.

November 1st. Has continued to take the acid so as to keep his mouth slightly affected. His ulcers have entirely healed, but the pains in his limbs still continue.

November 16th. He has continued the acid and is well in every respect, except the pains in his limbs, which remain the same.

Finding the acid after so fair a trial incompetent to the removal of the pain, and believing the rheumatism to be syphilitic, I thought it my duty to give him mercury. He took, from the 16th of November to the 18th of December, calomel, so as to keep up the affection of his mouth, and a pretty considerable ptyalism, without producing the least alteration of the pain. He then left off the use of mercury and used friction with the flesh brush, and in the course of three weeks found himself almost free from pain. He continued the friction, and in a short time after was perfectly well.

CASE III.

W. L. aged thirty-nine, was admitted in the alms-house for a venereal complaint. He has several times before had syphilis, has at present nocturnal pains in his bones, nodes on his shins, and an ill-conditioned ulcer on the calf of his right leg.

October 16th, 1797. He was ordered the following mixture:

℞ Gum: Arab: drach: iv.
Aquæ menth: unc: viii.
Acid: nitros: drach: ii. F. M.

With directions to take a table spoonful every hour, mixed with sweetened water, and the ulcer to be dressed with the ung: merc: precip: rub:

October 21st. He has taken four drachms of the acid; he complains of soreness of his gums: the medicine he thinks has caused a griping and looseness in his bowels: his ulcer looks cleaner and his nocturnal pains are less violent.

October 25th. He has continued the acid; the ulcer is clean and in a healing state; the nodes are less painful and begin to decrease.

October 30th. Has continued to take from one to two drachms of the nitric acid daily. His mouth is but slightly affected; he has no fœtor of the breath, and continues to mend in every respect.

November 4th. Has continued the acid; his teeth are loose, and he has a moderate ptyalism.

November 20th. He has continued to take the acid. His ulcers have entirely healed, his nodes have disappeared, and he appears in every respect to be perfectly well.

It is now four months since he left off the use of nitric acid, and he has never had the return of a single symptom of his old complaint.

N. B. He has taken no mercury for upwards of twelve months.

CASE IV.

February 7th, 1798. M. M'G——n, aged thirty-four, was admitted into the alms-house for a venereal complaint. She had a deep ill-conditioned syphilitic ulcer on the calf of her right leg; together with a syphilitic discolouration of the skin on the face, neck, breast, and arms. The following mixture was prescribed for her.

℞ Gum: Arabic: drach: iv.
Aquæ menthæ unc: iv.
Acid: nitros: drach: iii. F. M.

With directions to take a table spoonful every hour, mixed with some sweetened water. She was directed to dress the ulcer with the ung: ex ærugine.

February 9th. She has taken the acid. No alteration in her disease. She complains of a little griping in her bowels.

February 10th. She has continued to take three drachms of the acid daily. She complains of a swelling in the submaxillary glands; and her gums begin to swell. The ulcer is less painful, and looks cleaner. The discolouration of the skin remains the same.

February 13th. Has continued the acid. She complains of her mouth being sore and her teeth loose. No fœtor of the breath is observable. Her ulcer is contracting, and the discolouration of the skin she thinks is lessening.

February 16th. Has continued the acid. Her mouth is very sore and she spits near a quart a day. The ulcer is healing and the discolouration of the skin diminishing. On account of the soreness of her mouth the acid was omitted.

February 24th. Her mouth is much better, and the ptyalism has ceased. The ulcer is nearly healed and the skin resuming its natural appearance. She was directed to recommence the use of the acid

N

February 27th. Has taken the acid. Her mouth is slightly affected, and she continues to mend.

March 2d. Has continued the acid. The ulcer has healed, and the discolouration of the skin is fast disappearing.

March 6th. Has taken the acid. Her mouth is slightly affected. She continues to mend.

March 12th. Has continued the acid. She is in a profuse ptyalism. Her skin is of its natural colour, and she appears perfectly free from her complaint. The acid was omitted. Her mouth in a week after the omission of the acid got well, and she was discharged as cured.

N. B. Not a single grain of mercury had been given in this case.

As my present object is to shew the efficacy of the nitric acid in the venereal disease; and as nothing will have a greater tendency to its establishment, than its utility having been experienced by medical gentlemen of unblemished reputation, in different quarters of the globe; and especially as the result of their experience is in the hands of but a few, I think it my duty to avail myself of some of their cases. And in order to make this paper less prolix, I will omit the recitation of several other cases, which came under my immediate inspection.

Within these few days I have had the good fortune to meet with part of a small pamphlet, entitled, "Reports of the effects of the Nitrous Acid in the Venereal Disease." It contains twelve cases by Mr. Hammick, jun. one of the surgeons of the royal hospital, at Plymouth (England) which were selected from upwards of fifty cases in which the nitric medicine had been found efficacious, and sent to Dr. Beddoes for publication. As the one now in my possession is the only copy in Philadelphia, and I have some reason to believe it is the only copy as yet in America, and as it contains some very decided cases, which corroborate my experience on this subject, I will take the liberty of relating some of them.

CASE V.

George Hall, a marine, thirty-nine years of age, was received into the royal hospital, at Plymouth, on the 17th day of April, 1797, for a venereal complaint which he had contracted about three weeks before. At this time he had a large, irregular, foul chancre on the lower part of the penis near the scrotum, with an enlargement in the right groin; had never taken any medicine, or applied any thing to the chancre itself: the next day, the 18th of April, he was ordered the following drink:

℞ Acidi nitrosi diluti drachmas jj.
Succi limonis uncias j.
Aquæ fontanæ libras jj.
M. bibat quotidie.

The ulcer was dressed with the simple white ointment: he continued this drink daily to the 3d of May, when the diluted nitric acid was changed for the same quantity undiluted, which he took, with the addition of syrup till the 11th day of May, when the chancre was healed, the enlargement in the groin could not be felt, and in every respect he became well. He was discharged to quarters on that day to go on duty.

N. B. This man never took a grain of mercury.

CASE VI.

Thomas Plangett, marine, twenty years of age, was received into the royal hospital on the 17th day of April, 1797, for a venereal complaint, which he had contracted about sixteen days before; had not used any medicine for it: he had now two large indurated glands in the right, and a large one still in the left groin, and a venereal eruption on the pubis. The following day he was ordered;

℞ Acidi nitrosi drachmas 1 ss.
Syrupi simplicis uncias vj.
Aquæ fontanæ libras jj.
M. Bibat quotidie.

He took it that day, and continued it in the same proportion daily to 15th day of June, when the swellings in his groins being gone, and the eruption having entirely disappeared, he was discharged on that day, in order to go to quarters.

N. B. This man had not used any mercurial preparation.

CASE VII.

John Burr, seaman, twenty-seven years of age, was received into this hospital, on the 5th day of June, 1797, for a venereal complaint, contracted about a month before: this man had not taken any thing for it. Its appearance at this time was a large bubo in the right groin, which had suppurated two days before; two chancres appeared on the lower part of the penis; he had a phymosis with great inflammation, and an appearance tending to gangrene; and an ulcer on the scrotum: the same day he was ordered the following drink:

℞ Acidi nitrosi drachmas ii.
Syrupi simplicis uncias viii.
Decoct: lignorum libras ii.
M. capiat quotidie.

The penis and bubo were poulticed, and the chancres dressed with the simple white ointment. He took his drink that night, and before he had taken it six days, there was an apparent alteration for the better. He continued it to the 10th day of July, when his bubo, chancres, &c. being healed, the phymosis entirely removed, and the man in perfect heath, he was ordered from the venereal ward to another surgical ward, as he had a hernia.

N. B. This man had never used mercury.

CASE VIII.

Samuel Pope, seaman, twenty years of age, was received into the hospital on the 4th day of June, 1797, for a venereal complaint, which he had contracted about ten days before. The account he gave me was, that about six days before his arrival here, he found great pain and difficulty in passing his urine, attended with a phymosis, and a discharge of matter from the urethra; that three days after that, he perceived a black spot on the prepuce, which continued spreading till the day of his arrival here, when a profuse hæmorrhage taking place from the dorsum penis, it alarmed him and he then applied to the surgeon for the first time, who immediately sent him here. I found the whole prepuce entirely mortified, and the mortification had seized the upper part of the glans penis, from whence the prepuce had, from its weight in hanging down, been detached; he had also much symptomatic fever: he was ordered to be well fomented twice a day, and the yeast poultice to be applied, and to take the following drink:

℞ Acidi nitrosi drachmas ii.
Syrupi simplicis uncias viii.
Decocti lignorum libras ii.
M. capiat quotidie.
Capiat hora somni opii grana ii.

June 5th. Has bled somewhat during the night, and the sphacelus on the glands seemed to have spread; the nitric drink, &c. continued as yesterday.

June 6th. Nearly the same as yesterday, only appears to have less fever; drink, &c. continued.

June 7th. The whole of the prepuce sloughed off this morning; the mortification on the glans had not spread: nitric medicine, &c. continued.

June 8th. There was a detachment of the sloughs: drink, &c. ordered as usual.

June 9th. The sloughs came entirely off this morning from the glans, so deep as to occasion some alarm that the urine would find its way out through the side of the urethra.

June 10th. Appeared to be better, and the nitric drink, &c. were continued without any alteration, (except the ulcer being dressed with ointment on the 13th, in lieu of poultice) to the 17th of July, when the wounds round the glans from whence the prepuce had sloughed off and elsewhere, were healed; and he being in all respects perfectly cured of the venereal disease, he was sent from the venereal ward to another surgical ward, as he had a lame arm from a hurt he had received on board some time before.

N. B. This man had never used either mercury or bark.

The preceding cases in my opinion justly warrant us in saying not only that the nitric acid is useful in the venereal disease, but that it is at least equal to mercury. But there is hardly a practitioner, who has not met with some cases of syphilis, in which he has had too just cause to lament the inefficacy of mercury, where either owing to idiosyncracy, or to the constitution not having sufficient stamina to contend both with the disease and remedy, or rather to the constitution being so irritable, as to be unable to bear the action of mercury, for a sufficient length of time, to eradicate the disease; in which cases the physician is reduced to the extreme mortification of being a spectator to the sinking of his unfortunate patient out of his miserable existence, with a disease, which besides being the most painful, is, by the world, esteemed the most loathsome and detestable.

With what honours then, should not science crown the man, who discovered a remedy capable of snatching a fellow creature in so deplorable a situation, from the jaws of death. The nitric acid, as will appear by the following cases, is sufficient to accomplish so desirable an end. And the gratitude of mankind in general, should pay the tribute justly due to the ingenious Mr. Scott, whom reason, reflection, and experiment, has conducted with so much honour to the discovery.

In the pamphlet from which some of the preceding cases were taken, we find also the following, which are much to our present purpose.

CASE IX.

Thomas Homewood, seaman, twenty-six years of age, was received into this hospital on the 29th day of March, 1797, for a venereal complaint, which he contracted about a month before. He had taken different medicines without effect, for it, on board; the appearance of the disease, was, a very large and deep chancre, extending all the length of the penis on the back part; a large and extremely inflamed bubo in each groin, with a profuse discharge of matter from the urethra, accompanied with great heat in passing his urine. His buboes were ordered to be fomented and poulticed twice a day, and two drachms of the strong mercurial ointment to be well rubbed in on the thighs at night; which applications were continued to the 2d of April, when his buboes (now ready to suppurate) were touched with the lapis infernalis, and the eschars were thrown off on the 5th. The next night he used the friction again, and continued it to the 10th, when I found him very weak and low, with a violent cough and much expectoration of thick phlegm streaked with blood, profuse sweats, and such extreme debility, as not to be able to raise himself from his pillow, attended with diarrhœa. The chancre and buboes continuing very foul, the friction was discontinued, and he was the next day ordered the following drink:

℞ Acidi nitrosi drachmas ii.
Syrupi simplicis uncias viii.
Aquæ fontanæ libras ii.
M. capiat quotidie.

The day after, finding himself somewhat better, the drink was continued, and from the above alarming symptoms yielding, and an entire alteration taking place both in his health and appearance of the ulcers, he took it to the 10th of May; when he complained the drink vomited him. The nitric acid was then reduced to one drachm daily, which he drank without any uneasy sensation in his stomach, and continued it in that proportion to the 30th day of May; when his buboes and chancre being healed, and all the venereal complaints entirely gone, the drink was no longer used, and he was discharged from the royal hospital on the 5th day of June, in order to join his ship.

CASE X.

Thomas Edmed, seaman, aged twenty-four, was received into this hospital on the 9th day of February, 1797, for a venereal complaint, contracted about three weeks before, and had taken mercury on board for it. At this time he had a very bad phymosis; the prepuce being very thick and hard, with a profuse discharge appearing to come from chancres situated behind the glans penis: he had also great difficulty in passing his urine, accompanied with a chordee. This man was very delicate, with fair complexion: he was ordered the next day to rub well into his thighs at night, two drachms of the strong mercurial ointment, and to apply twice a day, a poultice of linseed meal, &c. to the penis: he rubbed, between the 10th of February and 12th of March, twenty-three times, and fumigated the part night and morning with the cinnab: factit: on this day, viz. 12th of March, he was ordered to forbear the friction, as he was low and weak, and his mouth very sore and swollen: he spat in the four and twenty hours nearly three pints; diarrhœa, cough, pain of breast, and his venereal complaint appeared much aggravated. He was put on a nutritive diet, with wine and cordials; he was ordered an infusion of bark in lime water; two grains of opium every six hours, and a quart of the decoction of woods, to be taken daily. He was kept on that course without gaining ground in any respect, to the 12th day of April: he was then ordered mercurials in small doses, combined in different forms; various fumigations and poultices without the least success, until the 14th day of May, when he complained of his being a great deal worse; so weak that he could not quit his bed, or only be helped out to have it made once a day; his diarrhœa still continued, his cough and sweats increased, his appetite was entirely gone; he had nausea, excruciating pains in his legs and arms at night, and was exceedingly reduced; in this state he was ordered to leave off his bark, decoction of woods, opiates, &c. and to trust entirely to the following drink:

℞ Acidi nitrosi drachmam i ss.
Syrupi simplicis uncias viii.
Aquæ fontanæ libras ii.
M. bibat quotidie.

Before he had taken this nitric medicine twelve days, he thought his pains were less severe, and his diarrhœa somewhat abated; on the seventeenth day, the venereal complaint appeared better; he could get the prepuce a little way back, and he thought himself in every respect better. On the twenty-sixth day of taking this drink, he was able to walk about his ward with some assistance, and on the 16th day of June could get the prepuce entirely back over the glans penis. He continued his nitric drink to the 10th of July, when his venereal complaint was entirely well: diarrhœa, pains, cough, and sweats had left him, his appetite good, and grown plump and strong; he was discharged that day at his own desire, to join his ship.

I am informed that the ingenious Dr. Beddoes, in part the fifth, of his considerations on factitious airs, has collected some valuable information on the use of the nitric acid, in diseases. I regret exceedingly, that though I have made every exertion in my power, I have not been able even to procure a perusal of it. But I find in the New-York Medical Repository, a very interesting and satisfactory case extracted from it, which I beg leave to repeat. The case above alluded to is in the words of Mr. Thomas Baynton, who gave it to Dr. Beddoes for publication.

CASE XI.

On the 8th of February, 1795, says Mr. Baynton, I was requested to visit Mr. ——, with pains of the limbs, and ulcers of the throat and tonsils, which, from appearances, suspecting to be a case of cynanche maligna, induced me to prescribe bark, wine, and gargles. That plan was persevered in until the 19th, without any advantage: from that circumstance, conjoined with the situation of his pains, and the times of their aggravation, I began, (though my patient was married, and the father of healthy, fine children) to suspect the case to be venereal; and after expressing my suspicions, I learnt from him, that he had contracted such a disease some years before, and had at that time an ulcer on his penis; but that he supposed himself perfectly cured, having passed through a regular course of medicine for that purpose: it was with difficulty that I convinced him that his present sufferings were referable to such a cause. However, possessing his confidence, I prevailed upon him to commence a mercurial course, and he continued to take from that time to the 8th of March, a grain of calomel, with an equal quantity of the ext. papav. three times a day, in the third part of a pint of the decoct. lignor. cum rad. mezerii. On that day, in consequence of the appearance of some eruptions, the form of the medicine was exchanged for the following:

℞ Hydrarg: muriat: gr. iv.
Aq: cinnam: unc. iv.
M. cap. coch. larg. noct. maneque.

This was continued until the 27th of April, and then a drachm of the ung: hydrarg: fort: was ordered to be rubbed into the thighs every night at bed time, and the former medicine omitted on account of the eruption having yielded, though the pains had increased; this with the decoction of the woods and mizereon, was continued till the middle of June. On the 20th of October, he began to take four grains of the blue pill, with a quarter of a grain of opium three times a day; and it was continued without intermission, until the middle of January, 1795. On the 14th of that month, calomel, with opium, was again used in its stead, and continued until the middle of February. On the 12th of July, he again commenced its use, and continued to take it until the middle of August last, when I was obliged by the fullest experience of the inefficacy of all the mercurial preparations that had been tried, to again request him to desist from the use of all medicines, except occasional opiates, to mitigate his pain, which it had been necessary for him to use with the greatest freedom during the whole of the mercurial course. It will here be necessary to remind you, that when I first was called to his assistance I found him labouring under only the constitutional or secondary symptoms of the disease, and as I did not keep any minutes of the case I cannot speak with that exactness I would wish of the particular effects of the different preparations of mercury that were exhibited. But I have the fullest recollection, that the ulcers of the throat and the affection of the skin, were removed in due time by the means that were adopted; and although there was a complete failure in my attempts to dislodge the poison from the bones, I am not convinced that the failure resulted from the incompetency of the mercury to produce such an effect, as the constitution of this patient was rendered so irritable by the disease, or the means made use of, (or perhaps both) that I was never able to impregnate the system with a sufficient quantity to produce the desired effect; though it was exhibited with every precaution and united with every corrective that my judgment could suggest, conjoined with the advantages of country air, bark, milk diet, and the occasional omission of all medicines, which for a time, answered so well, as to make my disappointment the greater at each time of the disease returning. At last, medicine of almost every kind failed to afford even relief, and I was reduced to the necessity of being content with the mere palliative effect of opium given in large doses. The peculiarities of this case are however too common; and every practitioner that is much engaged in a large city, especially if it be a sea-port, must have had to deplore such occurrences. It was my good fortune to meet with Mr. Scott's communication, in the way before described, just at a time when I had exhausted my endeavours to cure this patient, and when I say with him, that he was indeed a "rueful spectacle," with little more than diseased bones remaining, when he began the use of the nitric acid; and that he now appears in good health, I contemplate with astonishment the change that has been produced; more especially as he was always labouring under profuse sweats, diarrhœa, and ptyalism, from the mercurials that were exhibited, though they were so guarded, and has now obtained a cure by the use of the most powerful acid, without experiencing even momentary inconvenience, and in less time than would have been requisite to remove even the mildest symptom of the disease by any other known method.

The gentleman who was the subject of the above case, at the request of Mr. Baynton, drew up a very excellent account of his sufferings, and as in it he has given a particular account of the effects of the nitric acid, I think it may not be improper to insert his letter, which is as follows.

BRISTOL, NOVEMBER 25, 1796.

DEAR SIR,

PURSUANT to your request at your last visit, I take up my pen to describe, if possible, the deplorable condition, and sufferings I have endured for near two years last past, and the almost miraculous delivery therefrom, by your care and unwearied attention thereto.

I was first seized with ulcers of my throat and violent pain in my shins at night, that threw me into such perspiration, that for nine or ten mornings, I was under the necessity of changing my linen before I could possibly get up; which was soon followed by, or with excrescences or nodes from my knees almost down to my insteps, attended with violent pains in in my head. My arms also were attacked with excruciating pains, where swellings of a considerable size made their appearance. My knees also swelled, and the pain so acute, that I durst not move them the least aside: sleep fled, nor did it return for ten weeks, and for twenty-two weeks I could not bear to be moved without suffering the most extreme torture, notwithstanding your tender care to administer every thing you could devise and prescribe for my relief. I knew you perfectly understood my case; but my disease seemed to baffle the power of medicine and every effort. Having for the last thirteen weeks lived wholly upon milk, you advised me to discontinue the medicine, in hopes I might soon be able to make use of stronger food, and recover a little strength. This treatment had the desired effect; and my pains for some time seemed to abate; but alas! they soon returned again, when you advised another course of medicine, which operated more powerfully than it had hitherto done, and in a few months restored me so as to enable me to walk from my lodgings in the country to town. The satisfaction you expressed on the occasion I shall never forget, and with myself was in hopes of a radical cure. But at the end of three months, my hopes were destroyed by a violent relapse, which soon confined me to my bed. My legs, if possible, were worse than before, for not only my shins, but the main bones pained me dreadfully. One node formed a little below my right knee in a short time almost as large as a hen's egg. The pains from my shoulders to my finger ends I can scarcely describe. The sinews of my arms, thighs and legs, stiff and contracted; my fingers I could by no means bend; they were fixed by disease and every joint swelled. The bones of my head shared equally with the parts I have described, and nothing but death was expected to put a period to such a scene of misery. Added to this, my body was a rueful spectacle, a mere skeleton; so that disease had nothing left but my vitals for its prey.

This, sir, is a faint description of the state and condition you found me in about three weeks ago, when you visited me, and with joy in your countenance told me, a new discovery had been made of a medicine that you had great hopes would reach my case; and with your wonted goodness of heart cheered up my drooping spirits, by describing to me its mildness and efficacy in several cases similar to my own. Encouraged by this information, and relying upon your judgment, I was determined to give it a fair trial. I began, and continued to take the quantity as prescribed. At the expiration of seven days I found it began to operate, as you had before described, by creating a saliva in my mouth. On that day I had a desire to be lifted from my bed, and to sit up a little, which was done with some difficulty; but could not bear my feet on the ground, my knees being also in a very debilitated state, but found my pains greatly abated. I spat a great deal the next night, which was very thin, and not disagreeable. On the eighth day my pain seemed quite gone, and I requested again to get up; when to my great surprize I found myself capable of bearing the weight of my body on my legs. On the ninth day I was capable and absolutely walked from my bed to my chair, the distance of six feet without assistance. I bespoke a pair of crutches; but, thank God, I never used them, nor have had occasion for them; for on the tenth day I walked several times backwards and forwards in my room without crutch or stick, or any other assistance whatever. On the eleventh day I walked from one room to another, and finding it attended with no extra pain, but stiffness and weekness in the shins, I absolutely walked up a pair of stairs of fourteen or sixteen steps, and down again. My appetite was now restored to an amazing degree, insomuch that I found I could not continue the usual quantity of medicine, which in fact seemed to have operated more like a charm than a medicine, but I continued taking about three parts in four thereof daily. And I have the pleasure further to inform you, that I have walked out several times, and yesterday in particular I walked more than a mile, and was in hopes to have surprized you, which I know would have been an agreeable one, by paying you a visit at your own house, but was informed you were from home. Be assured I shall always esteem it a pleasure to answer any queries respecting my case, and the efficacious operation of the acid in so wonderful a cure. Believe me to be, dear sir, &c.

To Mr. Baynton.

The utility of the nitric acid is not confined to the venereal disease alone. Mr. Scott, who gave it a fair and ample trial in the chronic hepatitis, thinks it even preferable to mercury in that disease. Two cases of diabetes which came under his care, in both of which the patients were in the decline of life, yielded to the nitric medicine.

In the little pamphlet above spoken of, and from which I have extracted some of the preceding cases, a very deplorable case of lumbar abscess is related, by Mr. Sanford, surgeon of the Worcester infirmary, which was successfully treated by the nitric acid.

CASE XII.

On the 25th of March, says Mr. Sandford, R. H. aged twenty-two, a farmer's labourer, was sent to the Worcester infirmary with a lumbar abscess, that for some weeks past had suppurated, and discharged itself through a small opening above Poupart's ligament in the right groin; the thigh on that side was considerably enlarged, inflamed, and painful on pressure. Upon his admission I thought his case hopeless, and was fearful that little could be done to benefit him by medicine or surgery. I was only anxious to have him sent back into the country as soon as possible, being apprehensive that confinement in the hospital would only hasten his death, which at this time seemed inevitable.

Before he quitted the infirmary and was made an out-patient, it occurred to me, that it was probable the nitric acid might be tried to advantage in this case. I accordingly began by giving him one drachm mixed with a pint of water which he took in the course of four and twenty hours. He said it felt warm at his stomach, and at first taking flushed his face; he continued it however with little interruption, from April 4th, to June 10th, a week after which time, he walked from his house (eight miles from Worcester) and called upon me.

The wound in his groin was only dressed superficially, with a simple cerat of wax and oil. The discharge he informed me, had gradually lessened, till it ceased entirely. The swelling and inflammation of the thigh had gradually subsided, and assisted by the moderate pressure of a calico roller, was finally restored to its usual size and action.

As a proof that this unexpected cure was principally effected by the means of the nitric acid, the poor fellow had a relapse once or twice during his confinement at home, for want of his medicine, the quantity dispensed to him from the infirmary having been all taken before he had an opportunity of sending for more.

I have had an opportunity of trying the nitric acid but in one case of scrophula, the result of which I will now lay before the reader.

CASE XIII.

December 27th, 1797. F. D—th, aged twenty, a pauper in the alms-house, was afflicted with scrophula, which she said she had had for two months previous to this time. She also informed me that her brother had some years ago suffered with a similar affection, and though she could not inform me whether her parents ever had the disease, yet I think it probable, from her brother being affected with it, that it was hereditary. At present the glands on the right side of the neck are very much enlarged and very painful. The tumours have opened and discharged from three small ulcers a great deal of matter.

On this day (December 27th) I gave her the nitric medicine.

December 30th. She had taken daily two drachms of the acid diluted with water. There is no alteration.

January 1st, 1798. Has continued the nitric medicine as before; she says her gums feel sore, and she spits a little.

January 3d. Has continued the medicine. She complains of her mouth being sore and teeth loose; her ptyalism is increased. The pain in her neck is grealy diminished, the ulcers look better, and discharge less.

January 6th. She complains much of her mouth, and discharges about a pint of saliva in the day. She has very little pain in her neck, and the ulcers continue to mend.

January 10th. Has continued to take the medicine as usual. Her mouth continues in the same situation. She has now no pain in her neck, and the ulcers continue to mend.

She continued the nitric medicine from this time to the 28th day of January, in such manner as to keep her mouth slightly affected. At which time two of the ulcers had healed, the other was nearly well, and the tumour somewhat diminished. During the whole time of her ptyalism she never had a fœtor of the breath.

It must here be observed, this unfortunate girl had from her infancy, been afflicted with epilepsy, and generally had three or four fits a week. But during the salivation, which continued nearly four weeks, she had but five fits. The epilepsy has almost converted her into an idiot; and she never from this time could be prevailed upon either by entreaty or threats to take any more of the medicine.

The affection of the neck continued for some time, after the omission of the medicine, without alteration. But at the time of my writing this, (which is upwards of two months since the medicine was left off) the tumour is of the same size, and the ulcer, which was nearly healed, and had ceased to discharge, has now enlarged its limits and runs much more.

The very beneficial effects of the nitric acid in this case, and the relief received during its exhibition, certainly warrants me to conclude, that if it had been persisted in, it would in a short time have eradicated the disease for which it was given.

I have seen the nitric acid given with advantage in several cases of chronic rheumatism; and in one case in particular it produced the happiest effects.

CASE XIV.

March 20th, 1798. J——s S——th, aged forty-one, was admitted into the alms-house for a rheumatic complaint. He informed me that twelve weeks previous to his admission into this institution, and immediately after a recovery from a pleurisy, he was attacked with pain in the joints of his arms, ancles, and knees, which always became easier after being warm in bed. This complaint continued to increase until the present, and he is now confined to his bed, his knees are swollen, and he is incapable of the slightest motion without severe torture.

He was ordered to take the nitric medicine; and for five days took daily three drachms of the acid. At this time he found himself so much better as to be able to move his limbs, the pain and swelling had considerably abated. The acid soon after being taken produced a sense of warmth in the stomach, which he compared to the sensation he had often experienced after a *dram* of ardent spirits. He continued from this time to take from one drachm of the acid to two drachms daily. On the eighth day of the exhibition of the medicine, his mouth was sore, his teeth so loose that he was apprehensive they would drop out, and he spat about a pint in the day. On the 15th of April, to which time he had continued the acid, he was able to walk about, free from pain, and said he was as well as he ever was in his life. The acid appeared to increase both his appetite and strength. He was on this day at his own request discharged from this institution.

I have in two instances given the nitrate of alumine: one patient took a drachm of it daily for five days, and it produced a salivation and sore mouth, that could be distinguished from the same effects produced by mercury, only by there being no fœtor in the breath. The ptyalism continued very profuse for six weeks after she omitted the medicine.

The secretion of urine was so much diminished by it, that without making inquiries to ascertain that effect, she told me, she made little or no water, and was apprehensive that some bad consequences would arise on that account.

In the other case, it was taken but for two days, and the only effect it produced, was, in some degree lessening the secretion of urine.

If on further trial it should be found pretty generally to produce this effect, it will suggest the propriety of giving it, even in preference to the nitric acid, in cases of diabetes.

The nitrate of alumine which was used, I made by precipitating the clay from the common alum or sulphate of alumine, by the vegetable alkali, and washing the precipitate a number of times, to free it from the salt formed by the vegetable alkali and vitriolic acid, then adding the nitric acid and evaporating with a very gentle heat. The salt thus formed is more astringent than common salt.

From the preceding cases it is evident that the first effect the nitric acid produces on the body, is a sensation of heat at the stomach, soon after it is swallowed, which for the most part is agreeable. This, though most frequently the case, is by no means a universal occurrence. For in some of the patients to whom I gave the medicine no such sensation was experienced. I have myself, in order to ascertain the effect it would have upon my pulse, taken at one dose forty drops; and a fellow student of mine, at my request, took sixty, and neither of us experienced that sensation of heat in the stomach. Nor had it, in an hour, (which was the time we attended to the pulse) the least effect on the pulse.

When taken in any considerable quantity, viz. from two drachms and upwards daily, we find that from three to fifteen and twenty days, according to the idiosyncrasy of the patient, it produces a soreness of the gums, looseness of the teeth, ptyalism, increased heat of body, and in every respect increases the combustion of life. This increased heat and action in the arterial system, does not arise to such a height, as to wear down the system and bring on indirect debility; on the contrary, it appears to give strength and vigour to the body. We have even seen a case where the patient has been so much debilitated as to be unable to raise his head from his pillow, and after the exhibition of this medicine for a short time, he regained his strength in a most rapid and astonishing manner.

There is no medicine which physicians have more reason to regret the want of, than a tonic which will act on every part of the system, and the exhibition of which can be continued for a sufficient length of time to produce durable effects.

I shall not I hope be considered as chimerical, nor as giving virtues to medicines which they do not posses, when I say, that I have no doubt but that the nitric acid will hereafter be acknowledged to possess these two properties. The effect it produced, in some of the preceding cases, in restoring tone and strength to every part of the system, justifies the opinion of its being a universal tonic. And that it can be given, if proper care is taken in the exhibition, for a sufficient length of time to produce durable effects, I think is evident from the cases above alluded to.

We have seen every stage and form of syphilis cured by this medicine, and even in habits broken down by the antecedent use of mercury, under which the disorder gained ground. The patients recovered their health and strength in a short time, without the use of diet drinks, bark, or any other tonic medicine whatever.

The nitric acid is well known to have a strong affinity to calcareous earth, which earth is a component part of the teeth, and from a supposition of a decomposition of the teeth, an objection to its use may arise.

That the acid in an undiluted state will have this effect, no doubt can be entertained. But this objection can have no weight, when applied to the diluted acid, especially if it is combined with sugar, syrup, or mucilage of any kind. For in none of the cases in which I have seen it exhibited, have I heard the least complaint of this kind, nor did the teeth in a single instance appear to have suffered the least injury. Yet I think it adviseable and prudent, always to wash the mouth immediately after every dose of the acid. If after what has been said, any person should be timid on this account, his fears may be done away, and the objection obviated, by taking it through a glass funnel, in which manner it does not come in contact with the teeth.

ON THE OXYGENATED MURIATIC ACID.

THE nitric acid is known to be composed of an active principle, oxygen, and an inert one, nitrogene. Its beneficial effects in diseases, have by Mr. Scott been attributed to the active principle alone.

In contemplating the subject, it occurred to me that if the cure of syphilis and some other diseases, was to be effected by oxygen, other substances might be found, which contained a larger proportion of it, than the nitric acid, and which would, with greater facility part with it.

The muriatic acid is capable of combining with a very large quantity of oxygen, forming then, what by chemists is called the oxygenated muriatic acid, or the dephlogisticated marine acid. From this oxygenated muriatic acid containing a very large proportion of oxygen, and from the facility with which it is decomposed, it appeared to me to be well calculated for the oxygenation of the system.

As reason dictated, and my situation gave me the opportunity, I thought myself justifiable in trying the experiment. I have never in the course of my reading, or in the conversation of my medical friends, met with, or heard of, the oxygenated muriatic acid being given in diseases; and as from my experiments, it appears to be a very valuable medicine, I think it my duty in this place, to lay before the reader the result of my inquiries on this subject.

CASE I.

February 12th, 1798. M. S. aged eighteen, was admitted into the alms-house with a venereal complaint. She had a syphilitic discolouration of the skin, eruptions on almost every part of the body and particularly on her head, forming a true venereal tenia capitis, and an ill-conditioned ulcer on the upper and back part of the œsophagus. The soreness of her throat and the eruption on the skin, commenced as nearly as she could recollect, about six weeks ago. Upon inquiry I found that six months previous to this, she had chancres, and a bubo which suppurated, for which at that time she took some medicine which did not produce a ptyalism or soreness of the mouth.

She took the following mixture:

℞ Oxyg: muriatic acid: drach: iv.
Gum: Arab: unc: ss.
Aquæ menthæ unc: vi. M. F.
Cap: coch. mag. q. q. hora.

February 13th. She had taken four drachms of the acid; the gums between the teeth were a little swollen, and there was a slight ulceration, or rather a diminution of gum at the roots of the teeth, and she made more water in the night than she had been accustomed to do.

February 15th. Has continued to take about half an ounce of the acid daily. Her mouth remains in the same situation as on the 13th. The pain in her throat is considerably less, and the urinary discharge is considerably increased.

February 20th. Has continued the medicine as usual. She complains of her teeth being loose, but has no ptyalism or fœtor of the breath. Has no pain in the throat and the ulcer there is nearly well, many of the eruptions have disappeared, and the others are drying up fast. The secretion of urine is so great that she is obliged to rise three, four, and five times in the night to discharge it.

February 26th. Has continued the medicine. Her teeth are loose, and she spits a little, but has no fœtor of the breath. The ulcer in her throat has healed, the eruption on her body has entirely disappeared and those on her head are nearly well, but the discolouration of the skin still continues. Her urinary discharge still continues increased.

March 1st. Has continued the medicine. Her mouth is in the same state as on the 26th of February. The eruption is entirely well, and the skin is resuming its natural colour; she continues to discharge a large quantity of urine.

March 12th. Has continued the medicine. Her skin is perfectly clear and of the natural colour; and she appears to be well. She was therefore discharged from the venereal ward.

CASE II.

February 2d, 1798. C——e N——e, aged twenty years, applied to me for assistance. She had four small chancres, two about the clitoris and the others on the nymphæ, which she had contracted two weeks before. I desired her to take the following:

℞ Gum: Arab: unc: ss.
Aquæ menth: unc: vi.
Oxyg: muriat: acid: drach: iv.
M. capiat, coch. mag. q. q. hora.

February 6th. She had taken the above mixture daily. Her chancres were less painful. She discharges more urine than customary,

difficult to restrain, the unhappy patient was very much emaciated, and so debilitated as to be unable to rise from her bed. She had in addition to the symptoms just now related, a diarrhœa, night sweats, and cough.

The mercury was omitted, as the disease under its use was rapidly gaining ground.

On the 19th of February, the patient, in the above situation was directed to take ten grains of the nitrate of alumine every two hours. This medicine for two days agreed with her. It checked the diarrhœa, and she thought it diminished her flow of urine. But on the third day her stomach rejected it, and it was omitted. She took for some days opium to allay her pains and the irritation of the stomach. On the 30th of February, she commenced taking the oxygenated muriatic acid. For the first three days after her commencement of this medicine, she took three drachms of the acid per day, in which time she found considerable benefit from it. Her pains were less violent, and she was in better spirits. The medicine had no other sensible effect except proving diuretic in a considerable degree. From this time she took from a drachm and a half to two drachms of the acid daily. On the tenth day of taking this medicine, she found that her pains had almost left her, and she had so far recovered her strength, as to be able to get out of bed and walk across the floor. Her nodes were lessening. The only objection she had to the medicine was its proving powerfully diuretic. She complained that in the night she could not remain long enough in bed to get warm, on account of the frequent calls to evacuate her urine. In three weeks she was able to make her own bed, her nodes were fast disappearing, and she could bend her elbow joints with ease. The acid continues to prove most powerfully diuretic.

She took the acid constantly till the 1st of April, (one month) when it was discontinued on account of her mouth becoming very sore, and a profuse ptyalism taking place, Her pains have entirely left her, and very little appearance of her nodes remain: on an average she discharges three large potfuls of urine a day. On the 16th of April her mouth was well, her pains did not return, her nodes have all disappeared but one which is on the malar bone, and that one was on the decline. She thinks herself perfectly well. I advised her to take the acid three weeks longer by way of insurance.

The preceding cases certainly prove,

1st. That the oxygenated muriatic acid is competent to the removal of syphilis in any of its forms, and in as short a time, at least, as could be done by mercury.

2dly. That a salivation by it, is not necessary in the cure of the disease, for in Case 2d and 3d, no ptyalism was induced.

3dly. That it acts as a tonic. And

4thly. That it proves powerfully diuretic.

From the oxygenated muriatic acid, in every instance in which it was exhibited, proving powerfully diuretic, I flattered myself that it would be found an invaluable medicine in dropsy. And a case of hydrothorax, which I shall proceed immediately to relate, (and which was the only case of dropsy, in which I had an opportunity of giving it a trial,) tends not a little to corroborate the opinion

March 28th, 1798. E. S. a pauper in the alms-house, complained to me, of an anxiety about her breast, a difficulty of breathing, which was increased when she attempted to walk fast, and more especially when she ascended the stairs: she had frequent startings in her sleep and palpitations of her heart; she had also with these symptoms œdematous legs, and scantiness of urine. The dyspnea, she told me, she had had for a fortnight, and the swelling of the legs she discovered a week ago. She took the following medicine.

℞ Aquæ menth: unc: vi.
Oxyg: muriat: acid: drach: iv. M. capiat coch:
Mag: q. q. hora.

March 29th, 2d day. She has taken the acid as directed without the least alteration.

3d day. Has continued the acid; has made a great deal of water, and thinks her difficulty of breathing is somewhat less.

4th day. Has continued the medicine. The secretion of urine is so much increased that she was obliged to rise five times last night to discharge it. She thinks too, that she discharges much more at a time, than she was accustomed to do before the use of this medicine. The affection of her breast and difficulty of breathing are much less troublesome and her legs are not so much swollen.

5th day. She is better in every respect. She continued from this time to the 10th of April, (the 13th day since her commencement with this medicine) from three to four drachms of the acid per day. The affection of the breast, has several days since disappeared; she is able now to ascend the stairs without the least inconvenience, which at the commencement with this medicine she was not able to do without sitting down to rest herself. The œdema of her legs are no more to be seen, and she appears in every respect perfectly restored to health.

In the above case it must be observed, that although the patient was ordered to use diluents freely, yet from an idea that it would increase her swelling, she drank as little as possible; yet notwithstanding this it proved powerfully diuretic.

Now almost all diuretics, and even digitalis itself, which is justly esteemed one of the most powerful we are acquainted with, produce little or no effect that way, without the use of diluents.

I would not from this, be imagined to recommend the prohibition of diluents, when the oxygenated muriatic acid is exhibited as a diuretic. So far from it, that I am persuaded that the free use of drinks increases the action of *this*, as well as all the medicines of the diuretic class. My only intention in mentioning this, was to establish the fact, that the oxygenated muriatic acid proved powerfully diuretic, (at least in this case) without the assistance of diluents.

The oxygenated muriatic acid which was exhibited in the preceding cases, contained the largest possible quantity of *vital air*. I made it by distilling the common muriatic acid on manganese.

That the utility of the nitric and oxygenated muriatic acids, in curing diseases is owing entirely to the oxygen they contain, will, I am persuaded, from a knowledge of their composition, be controverted but by few; and in my mind there exists not a doubt, but that mercury acts on the same principle with them, in the cure of lues venerea and some other diseases.

I am well aware, that it is the almost universal opinion among physicians, that the anti-venereal properties of the different preparations of mercury, belong exclusively to the semi-metal, and that the different preparations only serve the purpose of disposing it, to be taken into the system.

It may not therefore be improper in this place to take some notice of this opinion.

It is a fact well ascertained, and familiar to every practitioner, that mercury in its crude state, has no effect on the human body. Instances have been known where it has been taken from one to two ounces daily, for several years without producing the least evident effect on the constitution.*

It is also a fact well ascertained, that those preparations of mercury, which have the greatest quantity of oxygen in their composition, are the most active.

A very striking example of the activity of the different preparations of mercury being owing to the quantity of oxygen they contain, appears on comparing the effects of calomel and corrosive sublimate. The calomel is a very mild preparation of mercury, and the corrosive sublimate is a very active one; and the only difference in their preparation is, that calomel is made with the common muriatic acid, and the corrosive sublimate with the oxygenated muriatic acid. In this case, the greater activity of the corrosive sublimate, can be attributed to nothing else than its superabundant oxygen.

In the mercurial ointment, it is generally supposed, that the mercury is merely in a state of extreme division, and that the lard made use of serves no other purpose than to keep the particles of quicksilver separate.

* Gertanner on the principle of irritability.

In answer to this opinion, I will observe, that mercury, like all other metals, is capable of *oxydation;* this has been proved by a number of experiments. I have at present lying on the table before me, near a drachm of a dark coloured oxyde, which was taken from a phial containing mercury, which an old woman, now in the alms-house, from some superstitious notions, had worn next her skin for thirty years. The oxydation here, must have been effected by the almost incessant agitation of the mercury by her motions, in this great length of time.

The lard, then, in making mercurial ointment, serves the purpose of dividing the mercury, and thus exposes a larger surface of the metal to the air for oxydation. Every man who has ever rubbed down mercury for his own use, must know, that the longer the trituration is continued, and of course the greater the surface exposed to the air, the more powerful will be its effects, and the deeper colour will it assume.

Now, both lard and mercury are white, and if their mixture in making an ointment, was merely a mechanical diffusion, and no chemical combination, the ointment should be white also; but this is not the case, and as the oxyde of mercury is of a black or dark gray colour, is it not more reasonable to suppose that the mercury is oxyded, and this oxyde gives its colour to the lard. The same arguments will apply to the blue pill and other preparations of the same kind.

As there is very litttle doubt but that all the preparations of mercury contain oxygen, a question may arise, are there any proofs of a decomposition of the different preparations of mercury in the body? The answer is, yes; numberless facts prove it. Dr. Garthshore mentions an instance of a gentleman, who took corrosive sublimate only, and who had those parts of his flute, which were silver, on which he played, evidently tarnished with mercury.

Mr. Clare, in an essay styled a new mode of curing lues venerea, says, that he has repeatedly seen gold rings on the fingers, gold watches, and money in the pocket, become white and black from corrosive sublimate, calomel or *mercurius calcinatus* exhibited by the mouth. Now, neither corrosive sublimate, calomel nor mercurius calcinatus, either in their own form or mixed with water, have the least effect in amalgamating either gold or silver; and of course these preparations must have been decomposed and the mercury reduced to its pristine state, before the above amalgamation could take place. I could quote numberless instances, from authors, of mercury being found in its revived state in different parts of the body after death. But nothing can tend more to the establishment of this doctrine, than some experiments made by Dr. Thomas Kirkland. "Many years ago," says the doctor in his Child-Bed Fevers, page 114, "I gave a scruple of calomel to a dog that was very ill. It did not purge him, and he died in the night. Next morning we opened him, when I found, to my great surprize, the mercury returned to its pristine state, and globules of quicksilver adhering to the coats of the stomach." He likewise mentions that several other similar experiments were followed by the same results.

Mercury and nitrogen in their simple state, are known to be substances that produce no effect on the constitution, and it is more than probable that the base which with oxygen forms the muriatic acid, is likewise inert. Now as these three inert substances, which in their nature are perfectly dissimilar, are each united to oxygen, a substance of active properties, and we find that the different preparations formed by their union, are all easy of decomposition, and all produce the same effect in some diseases, and as the effect produced is in proportion to the quantity of oxygen contained in the preparation, I think we may with justice conclude, that the power of these medicines depends on the oxygen alone, and that the inert substances act only as vehicles to conduct it into the system.

I have, in the preceding pages, given, in as concise a manner as I was able, the result of my inquiries on this subject.

It was my intention when I first undertook it, to have ascertained the effects of a number of other substances, which contain a large proportion of oxygen, and are easy of decomposition: as oxygenated vinegar, oxalic acid; oxygenated muriate of potash, and many other substances,* all of which I am persuaded will be found to possess nearly the same medicinal properties. But the shortness of time (six weeks) which was allotted for the preparation of this essay; and a large portion of that time being necessarily occupied in my duty as an apothecary to an institution in which there are a hundred patients, and only another gentleman and myself to attend them, obliged me, for the present, to abandon this very interesting subject.

It would be injustice in me to close these pages, without paying the debt of gratitude, due the physicians and surgeons of the alms-house and house of employment, of Philadelphia, Drs. Samuel Duffield, William Boys, John Church, and Thomas C. James, who have generously permitted me to exhibit the acids in some of the preceding cases. And I now beg leave to return them my sincere thanks both for this, and other favours conferred, since I have had the honour of being a pupil in that institution.

* Since this essay has been in the hands of the printer, I have seen the fourth number of the first volume of the Medical Repository, which is just published. In it I have the happiness to find that Mr Cruikshanks has given the oxygenated muriatic, and the nitric acids, as well as the oxygenated muriate of potash, in syphilis with success. In the same number we also find that Mr. Alyon used the superoxygenated muriate of potash in cases of chancre and syphilitic ulcers, and found the good effects from it, more expeditious and more certain than those of any mercurial preparation.

AN EXPERIMENTAL DISSERTATION

ON THE

RHUS VERNIX, RHUS RADICANS,

AND

RHUS GLABRUM:

COMMONLY KNOWN IN PENNSYLVANIA BY THE NAMES OF

POISON-ASH, POISON-VINE, AND COMMON SUMACH.

SUBMITTED TO THE EXAMINATION OF THE

REVEREND JOHN EWING, S. T. P. PROVOST;

THE TRUSTEES, AND MEDICAL FACULTY OF THE UNIVERSITY OF PENNSYLVANIA,

ON THE TWENTY-SECOND DAY OF MAY, ONE THOUSAND SEVEN HUNDRED AND NINETY-EIGHT.

FOR THE DEGREE OF DOCTOR OF MEDICINE.

BY THOMAS HORSFIELD, OF BETHLEHEM, PENN:
MEMBER OF THE MEDICAL AND CHEMICAL SOCIETIES OF PHILADELPHIA.

INTRODUCTION.

EVERY enlightened citizen should be interested in the examination of the natural productions of his country. This observation applies with particular force to the United States of America, whose extensive limits afford numerous opportunities, for discoveries and improvements in every branch of natural science. A certain patriotic zeal ought to animate every individual of this republic. As it is indispensably requisite to the man, whose business it is to defend its political rights from infringement, so it should stimulate that man likewise, whose time and talents are devoted, for the general interest of his country, to the more humble employment of investigating its natural productions.

In the choice of a subject for an Inaugural Dissertation, my attention was early directed to the indigenous vegetables of the United States. After several months laborious research, I regret that I have been able to add but little to our knowledge of those vegetables, which were the objects of my attention.

I wish those experiments which are laid before the public in their present unfinished state, to be considered as an introduction to a series of experiments, on the American species of the genus Rhus. Unacquainted with the extensive nature of the subjects which are considered in the following pages, I found, after having made some progress in their examination, that the complete analysis of each, individually, would afford more than ample employment for all the time that was at my command.

Persons who have been engaged in similar pursuits, are acquainted with the numerous hindrances which retard the progress of the inexperienced experimenter. Every candid suggestion, from these, will be attended to with care, and acknowledged with gratitude.

To the managers and physicians of the Pennsylvania hospital, under whose parental patronage the author commenced and continues his medical studies, the following dissertation is dedicated, as a small tribute of the esteem and gratitude of an obliged pupil. This excellent institution, while it perpetuates the benevolence of its original founders, reflects great honour on its present administrators. It is not erected, it is true, on the basis of wealth or grandeur, but it is supported by the more solid and durable pillars of science and humanity. It affords to numerous pupils, extensive opportunities for improvement in the practical part of medical science, and it is happily conducive to the relief of much human misery

INAUGURAL DISSERTATION.

DESCRIPTION OF THE RHUS VERNIX, RADICANS, AND GLABRUM.

THE plants which are the object of the succeeding experiments and reflections, belong to a numerous and extensive family of vegetables. Different species of the genus Rhus are found in every quarter of the globe, and inhabit almost every climate.* Seven species of this genus have been discovered in Pennsylvania: the rhus typhinum, rhus copallinum, rhus glabrum, rhus canadense, rhus vernix, rhus toxicodendron, and rhus radicans.†

The three last species are most numerous in the southern parts of the United States: the more temperate climate of the northern states appears best adapted to the four species first enumerated.

Three other species of rhus are found in the southern regions of North America: the rhus elegans, aromaticum, and suaveolens.

My design, at present, is to decribe those species, which in the subsequent pages, are the more immediate objects of my investigation: the rhus vernix, the rhus radicans, and the rhus glabrum.

In the Linnæan system of vegetables, the genus rhus is arranged under the class Pentandria, and order Trigynia. Dr. Jussieu, in his very learned and interesting work, entitled "Genera plantarum secundum ordines naturales disposita," has placed it in the fourteenth class of plantæ dicotyledones polypetalæ, and twelfth order Terebintaceæ.

The following are the characteristic marks of this genus. The perianth‡ is five-parted, beneath, erect and permanent. The corol consists of five petals, which are egg'd and a little spreading. The filaments are five, very short. The anthers are small, shorter than the corol. The

* Thirty-two species are enumerated in the last edition of the Systema Vegetabilium Linnæi, published by Gmelin.

† Clayton, in his Flora Virginiana, published 1762, describes seven species.

‡ Most of the botanical terms, which occur in the description of these plants, are taken from the translation of the Systema Vegetabilium of Linnæus, by a botanical society of Litchfield.

germ is above, roundish, and of the size of the corol. The styles are scarcely evident. The pericarp a berry of one cell. The seed roundish and bony.

Most of our native species of rhus, are not simply hermaphrodite: in several, the male and female flowers are found on different plants; others have male, female, and hermaphrodite flowers on the same plant.*

I. THE RHUS VERNIX.

This tree is distinguished in Pennsylvania by the different names of, poison-tree, poison-wood, varnish-tree, poison-ash, swamp sumach, and white sumach.†

The rhus vernix is the largest of all our native species of rhus; it grows in some instances to the height of twenty-five or thirty feet; its average height, however, may be estimated at twelve or fifteen feet. It seldom exceeds five inches in diameter.

It delights in a fat soil, in low, marshy places. It is generally found near creeks or rivulets, in dark and shaded situations. Two swamps, which I several times visited, during the course of last summer, are the most desert and gloomy places, which are to be found in the vicinity of Philadelphia. They are accessible with difficulty; and appear devoted by nature, to be the habitation of injurious plants and venomous reptiles.

The common trunk of the rhus vernix, after arriving at the height of four or five feet, generally divides into two branches, which pursue for some distance, a regular oblique course. In some instances each of these branches is again divided in a two-forked manner; but in many cases they divide into four or five smaller branches, which are sent off near each other circularly from the circumference of the parent branch, forming a regular verticill. These smaller branches pursue the same oblique course, and are again subdivided, into a number of verticills (or umbells) of branches, which finally send off long, sappy, luxuriant shoots, and are terminated by a thick circular cluster of leaves.

The singular disposition of the branches, gives the tree a very elegant appearance. In some instances the subdivisions are perfectly regular;

* Walther, from this circumstance, in his Flora Caroliniana, has placed the whole of this genus in Linnæus's class of Polygamia: he describes five species.

† This is the toxicodendron foliis alatis, fructu rhomboide of Dillenius, Hort. Elth.

Arbor americana, alatis foliis, succo lacteo venenato; Plukenetii Almagest.

Sitz sive sitz-dsju; vulgo urus seu urus no ki.

Arbor vernicifera legitima, folio pinnato juglandis, fructu racemoso ciceris facie. Kæmpferi amœn. exoticæ, p. 791, &c. vide appendix.

Several varieties of the rhus vernix are found in Pennsylvania.

all the smaller branches proceed from a main branch in an oblique direction; and give the tree a circularly spreading appearance, highly pleasing to the eye.

The trunk is frequently erect; in some instances it proceeds from the ground in an oblique inclining direction; three or four trunks sometimes arise from one root. The bark of the trunk is of a dark gray colour; in the upper branches the colour is lighter. The bark of the young trees is generally smooth, it becomes rough and furrowed as they advance in age. The wood is light, brittle, and of a spongy texture; the younger branches are tubular and contain much pith. They can easily be made hollow, and have been used by the natives, in making flutes or whistles;* they are also employed in the preparation of their calumet or pipe of peace.

It has been an opinion, which at one time was pretty generally received, and which has been copied from one description of the tree into another, that the wood when touched, imparted to the hand an evident sensation of cold; and that by this circumstance alone, it might be distinguished from every other tree of the forest. This appears to be a vulgar prejudice, originating from a certain degree of terror, which is always associated with this tree.† I have frequently touched it, with a view to be made sensible of this coldness; but I never found it to differ in the smallest degree in temperature, from the trees in its vicinity.

The leaves break forth about the beginning of May; at their first appearance they are of a cupreous, or of a dark yellow colour; after being fully expanded, their superior assumes a lively deep green colour, while their inferior surface is considerably paler. The leaves are compound: each leaf consists of four or five pair of pinnæ (is five-feathered or winged) which are placed opposite, on very short petioles, and are terminated by an odd one.‡ The separate leaflets are smooth, intire, egg'd, and terminate in an acute somewhat elongated point. They are divided into two equal parts by a nerve, which is continued from their separate petiole in a straight course through the leaflet, and from which a number of smaller nerves run to its margin. The common petioles and the nerves of the leaves, are frequently of an elegant red colour. In some instances the

* Kalm.

† "My neighbour (says Dudley, Philos. Transact. 1720) that was so sadly poisoned with handling it, told me one thing very remarkable of the wood; (of rhus vernix) and that is, that when he touched it, he plainly perceived it to differ from the other wood, that he was throwing up into his cart; for it was as cold as a piece of ice; and withal assured me, he could distinguish it blindfold, or in the dark, from any other wood in the world, by its coldness; *but the poor man is as much afraid of it*, when he goes into the woods, *as of a rattle snake*."

‡ The number of wings varies considerably; in some instances I have found three, in others six or seven.

leaves are slightly sinuated near their points: and in a few cases I have observed their inferior surface, covered with a slight down.

They are of a strong tough consistence. Linnæus's specific character of the vernix is, "leaves feather'd, most intire, annual, opake, petiole intire, equal."

The flowers make their appearance, in the neighbourhood of Philadelphia, during the first or second week of June; in the more northern parts of Pennsylvania, I have found the tree in blossom during the first days of July. The flowers are arranged in small separate racemes, which form, at the termination of a long common peduncle, an interrupted panicle (of flowers) of considerable length. The peduncles with the terminating panicles, frequently extend to the length of six or seven inches. The peduncles always originate from the smaller branches, at the basis of a common petiole of the leaves, by which they are surrounded and supported. The flowers are chiefly produced near the end of the branches, where they exist in great abundance; they are arranged in common with the leaves that support them, circularly around the lesser branches, forming, when the tree is in blossom, a beautiful globular cluster of leaves and flowers. They are very small and of a yellowish or herbaceous colour. Their odour is very agreeable, and during the flowering of the tree, they attract innumerable swarms of bees: I have never heard that the honey collected from them, is supposed to possess any deleterious quality.

The male and female flowers of the rhus vernix (like those of several other species of rhus) are in most cases produced on different plants: I have not perceived any difference in the growth of the male and female trees.

The flowers are succeeded, on the female plants, by seeds which are arranged on their panicles, in the same manner as the blossoms; they somewhat resemble bunches of small grapes, and are ripe about the middle of October. The seeds are not perfectly globular, but a little compressed. They are surrounded in their dry state by a thin light green cortical covering, which before they become dry, contains a pulpy substance; when this exterior thin shell is removed, the compression is more evident, and the seeds appear striated. They are of a yellowish colour, and about one fourth part of the size of one of our common peas. They are very hard, and when broken are found to contain a small yellow kernel of an oleaginous nature.*

* "Candles, which are burned on particular festival days in Japan, are prepared of an oil procured by expression or decoction from the seed of the rhus vernix and rhus succedanea; the latter of which grows in many districts of Japan, and produces a great quantity of seed. The expressed oil of the seeds of the rhus succedanea is also used by the Japanese in preparing their victuals." Thunberg's travels.

The roots of the rhus vernix are large, irregular and knotty; they generally run some distance near the surface of the ground.

To determine the question whether the tree which has been described in the preceding pages, be the same with that from which the famous varnish of several oriental countries is obtained, has at different times, been an object of attention among botanists of the first reputation. The oriental varnish-tree has been described with great accuracy by Kæmpfer, in his Amœnitates Exoticæ. Of this description an extract is given in the appendix.

From a very accurate comparison of Kæmpfer's description of the varnish-tree of the Japanese, with the rhus vernix of our country, and from frequent comparisons of the leaves of our rhus vernix, which I procured from different parts of Pennsylvania, with Kæmpfer's figure of the vernix of Japan, I am fully convinced that they are identically the same.

This opinion was also entertained by Linnæus, Miller, Dillenius, Clayton and Thunberg.

To trace the similarity of our varnish-tree, with that described by Kæmpfer, in all their different parts, would lead to a tedious prolixity.*

A long and acrimonious controversy, concerning this subject, was carried on about forty years ago, between Mr. Philip Miller and Mr. John Ellis.† The former contended for the sameness of the American varnish-tree with that of Japan; which the latter positively denied. To relate, in this place, the different opinions and arguments, which were adduced on both sides of the question, would be neither interesting nor instructive. I have perused their different papers with much attention, but I find nothing in them to induce me to change the opinion above advanced. The dispute certainly was carried on under very unfavourable circumstances: the difficulty of procuring specimens from foreign countries; the imperfection or fallacy of specimens when procured; and the variations, to which vegetables naturally are subject, when removed from their native soil to distant and unaccommodated climates, has led them, on both sides, into many inaccuracies and mistakes. Every person, acquainted with botany, who has seen the trees in their native countries, will readily discover the sources of their deception. It is to be regretted, that while the plants concerning which they differed in opinion, were natives of Japan

* Dillenius, in his description of the rhus vernix, remarks: "It should not seem strange, that the varnish-tree should be found in America near the same latitude with Japan; since the ginseng, the bignonia, commonly called catalpa, with many other plants, are found to be natives of these countries. And I question, if the tea-tree might not be discovered in America, if persons of skill were to search for it."

† See London Philosophical Transactions, vols. xlix and l.

and North-America, their controversy was unfortunately carried on in Great-Britain.

One of my chief objects, in investigating the properties of our rhus vernix, was to determine whether it contained the same singular varnish-like juice, which is obtained from this tree in Japan; and whether this juice might be employed with advantage, by our artists, in the preparation of varnishes.

My first trials were instituted at that season of the year, when in trees of a similar growth, the sap or juice is found in greatest abundance. During the first week of May, on a warm day, I made a number of circular incisions into the bark of a tree of moderate size; these incisions were immediately followed by a very copious flow of a viscid fluid, which at first was of a white or light yellow colour, but which upon a short exposure to the air, became brown, and in some instances nearly black.* I was able in the course of about half an hour, to collect from this tree half an ounce of its juice. I made similar incisions into several other trees, in the neighbourhood of the first; and by repeated collections on different days, I obtained several ounces of this varnish.

If the vessel in which this juice has been collected, is immediately closed, it retains its original white or yellowish colour, and becomes brown only on that surface, which is in contact with the air. The juice has a sweet, but somewhat disagreeable odour; it is viscid,† glistering and pellucid; its taste is acrid; it imparts, when fresh, a slight sensation of heat to the tongue, and when swallowed irritates the fauces and excites coughing. It appears to agree in every particular with that described by Kæmpfer, as flowing from the varnish-tree of Japan.

About the middle of May, when the leaves are fully expanded, I perceived that the juice flowed less copiously out of incisions into the bark; its quantity gradually diminished, and during the first week of June, the decrease was very evident. I found, however, by frequent trials, that at every period of the year, from May till September, some juice always followed an incision into the bark.‡

My next object should have been to determine the application of this juice, to the art of varnishing: this I have not been able to accomplish. The investigation of some of the properties of several other species of rhus, engaged all the time I could devote to these pursuits. Perhaps I

* The juice which accidentally exsudes from the tree and dries on its bark, always assumes a jet black colour.

† "From the toxicodendron, when wounded, issues a great quantity of juice, which when exposed to the heat of the sun, turns so very clammy, that it proves a good birdlime, and is with great success made use of for that purpose." Hughes's Natural History of Barbadoes.

‡ The juice flows more copiously after wet weather.

would have been more usefully employed in determining the properties of this varnish. From its very great analogy to the oriental varnish described by Kæmpfer, it is evident that it promises considerable utility in the art of varnishing: it is a matter of importance, and will be the object of some of my future researches.

II. THE RHUS RADICANS.

This plant is generally known in the United States, by the name of poison-vine; in some parts of Pennsylvania it is called poison-creeper.* It grows only in a fertile soil; but is capable of existing in different situations; sometimes it is found near rivulets, at others in dry and elevated places.

The rhus radicans has a slender ascending stem, which supports itself on the bodies in its vicinity. Like various species of ivy, it frequently climbs up to the top of our tallest trees, arriving at the height of forty or fifty feet. It appears to delight in ascending the lofty oak trees, which surround the fields of our farmers. Very often it is met with along the sides of fences, which serve it as a very convenient support. Its stem rarely exceeds two or three inches in thickness; in a few plants which were growing in a very fertile soil, I have seen it nearly five inches in diameter. The stem in many cases is compressed on the sides; sometimes it is interrupted by small tubercles. It is covered with a gray bark, which in young plants is of a lighter colour.

If the rhus radicans happens to grow in situations where it meets not with a support, it never exceeds four or five feet in height; in these instances its growth is always winding or oblique; its branches frequently return and creep along the ground. In some cases it is forced to pursue this procumbent course for a considerable distance, without exceeding two or three feet in height.

In order to attach itself in its ascent, to trees and other substances, the stem and branches of the rhus radicans are furnished with a great number of long, thin, thread-form radicles or tendrils, which proceed in abundance from almost every part of the stem and branches. It is from these numerous radicles, that it has obtained its specific name of *radicans* or rooting.† By means of these radicles it sometimes adheres with so much

* This is the toxicodendron triphyllon glabrum. Tournefort. Institut.
Hedera trifolia canadensis. Cornuti.
Dillenius's toxicodendron rectum, foliis minoribus glabris, is a variety of our rhus glabrum.
The Delaware Indian name is Pu-tschis-ktey. D. Zeisberger's spelling book.
I have observed several varieties of this species of rhus.

† In very young plants these radicles frequently are of a beautiful crimson colour.

force to the neighbouring bodies, that it is difficult, and not unfrequently impossible to detach, without breaking it. I have frequently seen large plants, so completely enveloped with a thick coat of these tendrils, that no part of their bark was visible.

From the bark of the rhus radicans, when it is wounded, exsudes plentifully a milky juice: in proportion to the size of the plants, it appears to flow as copiously from the rhus radicans as it does from the vernix. After a few hours exposure to the air, it takes on an intensely *black* colour; in its other properties it resembles the juice obtained from the rhus vernix. Its taste is acrid and somewhat aromatic. A similar juice exsudes from the leaves, immediately after being detached from the plant: this juice has been employed in a few experiments, which will be related in a subsequent part of this dissertation.*

The trunk of the rhus radicans, almost during its whole course sends off lateral branches in an oblique or rectangular direction; which frequently proceed to the distance of two or three feet, and in their course again send off a number of lesser branches. The smaller branches are very slender and frequently pendulous. This arrangement of the branches gives the plant, where it stands exposed to view, an irregular and bushy appearance.

If it grows in situations, where it is supported for a small distance only, it sends off, near the top, numerous branches, which spread irregularly in various directions.

The rhus radicans frequently pursues a spiral course around its neighbouring trees; it is frequently intwined in a beautiful manner with our hedera quinquefolia.

Its wood is brittle, but more firm than the wood of the vernix.

The leaves of the rhus radicans are trifoliate, (three'd) the common petiole, on the larger plants, is several inches in length. The two lateral leaflets are supported by very short petioles; while that of the terminating leaflet is somewhat longer. The leaves generally are intire, egg'd, smooth, and terminated by an acute point; they are divided in the middle by a prominent nerve. But I know few plants, the form of whose leaves is so various: sometimes they are pretty regularly saw'd, at others irregularly sinuous; in some instances they are nearly oval, in others they are lance-shaped. According to Linnæus's specific character the leaves are: "three'd: leaflets petioled, egg'd, naked, most intire." When they first appear in the spring,† they are of shining red or of a copper colour; after having arrived at perfection, their superior surface is of a light green

* When the bark of the rhus radicans is burned, it emits a smell resembling that of burning chestnuts or cashew-nuts.

† In the vicinity of Philadelphia about the latter end of April; in the back parts of Pennsylvania, later.

colour, which is paler beneath. They are thin and of a weak texture. When dried, and preserved in a box, they have a very agreeable odour, very much resembling that of bohea tea.

I have been informed by Dr. Barton, that the leaves are eaten by horses with impunity; they are also eaten by cows; and sheep (according to professor Thunberg, in his travels) eat the leaves of a similar species, the rhus lucidum. Horses, Mr. William Bartram informs me, are very fond of the leaves of the rhus toxicodendron.

The flowers are produced along the whole course of the lesser branches; they mostly originate at the basis, and just within the common petiole of a leaf, though many of their peduncles are sent off seaprately. Their peduncles are very short, and their panicles are less complex than those of the rhus vernix; about fifteen or twenty flowers are generally supported by one common peduncle. The flowers are small, and their petals of a light yellow colour.* They have a most delightful odour, which resembles, but far exceeds in suavity, the odour of the reseda odorata.

The male and female flowers are generally produced on different plants.

Irregularly round, striated berries, of a green colour, succeed the female flowers; they contain a small hard seed which is laterally compressed. They ripen about the beginning of October. The seeds are very permanent, and frequently adhere to the branches, in a dry state, during the course of a whole year.

The roots of the rhus radicans are slender; they run near the surface of the ground, and in their course, send forth a number of young plants.

III. THE RHUS GLABRUM, COMMON SUMACH OR SMOOTH PENNSYLVANIAN SUMACH.

This plant is found abundantly in almost every part of Pennsylvania. It grows in a loose, fertile soil; frequently upon vacant or uncultivated fields, and along the sides of roads and fences. It sometimes rises to the height of eight or ten feet; in many instances it does not exceed four or five feet.

The trunk of the rhus glabrum is seldom erect; after rising in an incurvated or oblique direction, to the height of two or three feet, it divides into several large branches.

The small branches which they send off, are disposed in such a manner as to form numerous irregular, oblique angles. They are covered with a smooth light gray or reddish bark, containing a viscid, glossy, yellow

* They appear about the beginning of June.

juice, which is not changed upon exposure to the air. The wood is very light. The leaves, like those of the vernix, are compound; each leaf consists of nine or ten pair of leaflets or wings, which are placed opposite, and are terminated by an odd one.

The leaflets are smooth, lanced, sawed and acute; the Linnæan specific character is "leaves feather'd, saw'd, lanced, naked on both sides." It is said, the leaves when dried, are mixed by the Indians with the tobacco they use in smoking.

The leaves which in summer are of a deep green colour, change to a beautiful red, in autumn; hence, what Kæmpfer elegantly remarks of the spurious varnish-tree of Japan, may with strict propriety be applied to the rhus glabrum: "Rubore suo autumnali, quo viridantes sylvas suaviter interpolat, intuentium oculos elonginquo in se convertit."

The flowers are produced in large, erect, compound thyrses, at the termination of the superior branches. They are of a herbaceous colour. They appear about the beginning of July.

The seeds are arranged, like the flowers, in large conical thyrses. They are of a dark red colour; and after their arrival to maturity, early in autumn, covered with a white tenacious powder, of an agreeable acid taste. The berries with their saline covering, have been subjected to several experiments, which will be related in a subsequent part of this dissertation. From this white, saline powder, which is found on the berries, the rhus glabrum has been called, in various parts of Pennsylvania, Indian salt.*

OF THE POISONOUS QUALITIES OF SEVERAL SPECIES OF RHUS.

My observations under this head, apply chiefly to the rhus radicans and the rhus vernix. These two plants in consequence of the deleterious effect they produce on the skin, and by means of it on other parts of the human body, have been called *poisons;* the former, as if it claimed that name in preference to all other substances, is called in some parts of Pennsylvania, *the* poison; and both formerly belonged to a genus denominated in systems of botany, Toxicodendron.

As I shall have frequent occasion, in the course of the following observations, to use the term poison, it may be proper to remark, that with

* "Indigenæ hac substantia ad carnes condiendas utuntur." Schœpf.

"The seed of a certain species of rhus, was formerly used, according to Pliny, for seasoning meat, instead of salt; and was thought to render all flesh-meat more savoury and grateful to the palate." James's Med. Dict.

"The berries are used as a mordent, or fixer, for the red colour with which the Indians dye their porcupine quills."

Dr. Barton's Collect. towards a M. M. of the U. S.

1. A warm or cold climate. "In the southern climates these plants are more active than in the northern."*

2. Different seasons of the year: this I have very strikingly experienced in my own case. The rhus vernix never affects me in the smallest degree, except on very hot days in summer.†

3. Infancy or manhood. Dr. Barton has remarked, that children are more readily poisoned than adults: several instances in proof of this, have fallen under my own observation.‡

4. Exposure before or after a meal. "These plants more readily poison immediately after than before a full meal."||

5. The presence of moisture. It is very generally believed, by inhabitants of the country, that the effluvia of the plants, when combined with moisture, are most apt to produce the eruption. According to several facts which I have collected, this opinion deserves credit. It requires to be further investigated.§

6. A state of increased perspiration, at the time of exposure to the poison, has a most powerful influence, in rendering persons more susceptible of the eruption. Of this I had several unequivocal proofs during the course of last summer: in collecting the juice of the rhus vernix, if I was in a state of high perspiration at the time, I never failed to be affected, more or less with the eruption; if my skin was perfectly dry, the poison produced not the least effect upon me. Professor Kalm remarked the same during his travels; speaking of the rhus vernix, he says: "I found, however, that it could not exert its power upon me, when I was not perspiring."

Females, Dr. Barton has observed, are more easily affected than males.

Of the different species of rhus, which are natives of the United States, three are considered poisonous. They vary in their degree of

* Dr. Barton.

† "They are more active in spring and summer, than in autumn and winter." Wangenheim, von den Nord Amerikanischen Holzarten. Gœttingen, 1787.

‡ This I believe is the case with most other cutaneous diseases. An intelligent person informed me, that during his infancy he was very easily infected by the itch; but, that since his arrival to manhood, he is, upon exposure, much less readily affected by that disease. I also met with a person, who in early life was readily poisoned by the common rue of our gardens, in whom this plant produced no effect, after having advanced in age.

|| Dr. Barton.

§ That moisture does not destroy their poisonous quality, appears by the following quotation from Fontana on Poisons, vol. ii. Fontana, in order to examine the air of the leaves of the toxicodendron, as he knew that he was easily poisoned, observes: "I caused them to be got ready by another person, but I touched a few of the leaves when *under water*. In four days my face and eyes swelled, &c."

activity. The rhus vernix is the most violent, and the most universal in its effects: the toxicodendron and radicans, though they resemble it, possess the poisonous property in an inferior degree; many persons who are never poisoned by the radicans, are easily affected by the vernix. From a number of observations and facts, I am inclined to believe, that with the existence of certain circumstances, such as warm weather, and a state of high perspiration, no person would be found entirely unsusceptible of the poisonous influence of the rhus vernix.

The manner in which the eruption is excited, like its degrees of violence, is subject to many variations. It is mostly produced by one of the following circumstances:

1. By the exhalation or effluvium of the plants. Although this may be questioned by persons, who have not had an opportunity of observing their effects, yet, the facts in proof of it are so numerous, and many of them so decided and unequivocal, that not the least doubt is to be entertained on the subject. Dr. Barton and Dr. Woodhouse both informed me, that they had been poisoned in this manner. In these cases the first symptoms frequently shew themselves on the face and hands; but, though the poison appears to be applied principally to these, yet its effects in most instances soon become general. To what distance the exhalation is capable of extending its influence, I have not been able accurately to determine. My observations lead me to believe, that in very excitable habits, it extends at least to fifteen or twenty feet.*

A second method is, the *smoke* of the burning wood. This circumstance is pretty generally known, and I believe generally admitted. Several facts in proof of it, were communicated to the royal society of London, by Dr. Sherard and Mr. Dudley, before the year 1720.†

It is also noticed by Kalm in his travels, and by Wangenheim.

From several observations, I am induced to believe, that the effect in these instances, does not depend simply upon the smoke of these plants, but, that the particular part, in which the poisonous quality consists, is volatilized by the application of heat, and spreads its influence in every direction. Persons frequently are poisoned by sitting near a fire, in which some of the wood of either species is burning, without the least particle of smoke coming in contact with any part of their body.

Dr. Cooper was poisoned by the steam arising from a decoction of the rhus radicans.

* Some authors ascribe the poisonous effect to the *scent* of the plants; this appears to be a mistake; the odour, especially of the radicans, is by no means strong; it is scarcely perceptible when a person is in contact with the plant, nor is any specific effect produced on the schneiderian membrane.

† Philos. Trans. Abr. vol. vi. p. 307 and 308.

3. By the actual *contact* of the leaves, stem, or branches of the plants. In this manner, I believe, the eruption is most generally excited.*

4. By the immediate application of the *juice* of the plants, to the external surface of the skin, or by introducing it by inoculation, within the cuticle. In some persons, who can handle the leaves and branches without injury, the eruption is excited by the application of the juice to any part of the skin.† That the poison can be communicated by inoculation, was first demonstrated by an experiment made by Dr. Barton, during the last spring. I was since led by accident, to observe the production of the disease, by the insertion of a very small quantity of the juice, between the cuticle and cutis. In two or three instances I observed a slight eruption, after phlebotomy, with a lancet, the quantity of juice communicated by which must have been so very minute, as to render it scarcely conceivable how the eruption was excited. The peculiar symptoms, produced by inoculation, will be detailed hereafter.

In illustration of the foregoing positions, many facts might be related, but this would lead to a prolixity, perfectly inconsistent with the design of this dissertation.

The particular part of these plants, in which this deleterious property resides, appears to be their *juice*, which as was observed in the history of their growth, exsudes plentifully out of incisions made into the bark. This juice is highly acrid and corrosive; which is evident from its application to the skin of persons, who are not liable to be affected by the plants; in these cases it uniformly produces, like nitrous acid, or lunar caustic, a destruction and desquamation of the cuticle. It is capable of being volatilized by heat; and during the life and vigour of the plant, it appears to be exhaled, in form of a peculiar gaseous fluid possessing its specific qualities, surrounding and defending the plant by an atmosphere of poison.

The manner in which those different symptoms are produced, which will soon be described, I have not had an opportunity of examining with sufficient accuracy. It will appear evident from a review of these symptoms, that the poison induces in the vessels of the skin a peculiar morbid action; an inflammation *sui generis*. This inflammatory action exists in different degrees: in some instances the poison produces merely an erysipelatous eruption, which in others advances to effusion, to suppuration, and to ulceration.

* Mr. N. Jones informed me, that he knew a numerous company of boys to be poisoned by their using a stick, cut from the rhus vernix, in playing at ball.

† Its application to the cuticle in the palms of the hands, is in some cases, an exception to this rule; in one instance, the juice being applied to these parts did not produce any effect, till it was communicated by contact to other parts of the body.

This I acknowledge is speaking only in very general terms; it is repeating what must be obvious to every person who examines the disease with attention. At present, however, I have neither time nor inclination to speculate on the pathology of this singular cutaneous affection. The subject is open for further investigation.

I proceed now to the description of the symptoms which occur, upon exposure to the poison, in any of the methods above enumerated; and, in the first place, I shall describe them, as they appear in persons but moderately disposed to the disease. The interval of time, between the exposure and the appearance of the poisonous eruption, is various. It depends in a great measure upon the disposition of the person to be acted upon by the poison. In certain habits the effects appear in a few hours, in others not till in as many days.

A slight degree of itching, or a sensation of heat, is the first harbinger of the approach of the eruption; this itching gradually increases, and is followed by redness and inflammation of the skin, which in some instances is very extensive, and in others is confined to round circumscribed spots, or to longitudinal streaks. The inflamed parts now become somewhat elevated and tumefied; small vesicles appear on their surface, containing a pellucid fluid, which gradually increases in size. The fluid soon becomes yellow, and after some time, takes on the colour and consistence of pus. The vesications in some cases are found separate, but where the inflammation has been violent, their number is mostly so considerable, that they come into contact, run into one another, and cover a considerable space. After the vesications are completely distended, they break, and some of their pus being discharged, by drying on their surface, forms a yellow incrustation.* In this state, if the vesicle is single, it somewhat resembles a pustule of the small pox; but where the vesications have been numerous, their purulent surface is proportionably extensive; so that towards the end of the disease, large surfaces are covered with a yellow incrustation, which in a short time becomes brown. The disease generally terminates by a desquamation of this crust, which leaves the skin very tender and of a florid red colour.† A very troublesome itching accompanies the whole course of the eruption. It is very seldom that scars remain after its disappearance. Four or five days are generally required for the eruption to pass through these different stages.

Somewhat like this is the common course of the disease when left to itself; but, like all other diseases, it is in its progress subject to innumer-

* If the vesications be irritated by scratching or by friction, large quantities of serum or pus are discharged from them; the incrustations in these cases are formed sooner, but are less regular on their surface.

† If the eruption has been very slight, or if it has been stopt in its progress by the early application of proper remedies, still the cuticle of the affected parts is universally thrown off.

able variations. These depend, in a very great degree, on the habit of the person affected. By the application of the remedies which are to be mentioned hereafter, its progress in moderate cases, can be prevented; and in violent cases the symptoms may be much relieved and their period shortened.

I now proceed in tracing the history and symptoms of this disease; and in the first place, I shall mention several deviations from its usual course.

1. In many cases, although the disease does not exist in a violent degree, yet it is not terminated by passing through the several stages above enumerated. The eruption being excited in one part, is often propagated to the part lying contiguous to it, from whence by slow degrees, it passes in some instances through the whole cutaneous system. Thus there exists in different parts of the body, a constant succession of separate eruptions passing through their several stages.

2. The itching and the vesications which take place in the incipient stage of the eruption, frequently disappear and return several times successively. In some instances pustules of considerable size, are entirely reabsorbed.

3. The poison appears to have a peculiar capricious disposition, to attack particular parts; in most persons the eyes are specifically affected; in some the body, the legs or thighs. A peculiar and distressing itching of the scrotum and of the præputium penis, is one of the most general and characteristic symptoms of the disease, which in males, I have found to take place in every instance that has fallen under my notice, with but one exception. In quibusdam occurrit erectio penis. An fœminis labia pudendæ similiter affectantur? Its peculiar tendency to affect the eyes, is most strikingly observed in persons that are readily susceptible of the eruption; in most of these, if its progress is not opposed by proper remedies, blindness of a longer or shorter continuance, is the certain consequence.*

It has been observed that the eruption, when it is re-excietd, has a particular disposition to affect again the same parts, that were formerly affected.

In habits which are very susceptible of the poison, the same symptoms occur which were above enumerated; but they make their appearance

* Although I am but little disposed to the eruption, yet whenever the smallest degree of it is excited in me, my eyes are primarily affected: a slight tumefaction of the eye-lids and of the skin immediately below the eyes, takes place; this is accompanied by a sensation of fulness, by heat and itching, and followed by a scarlet eruption, extending circularly several inches around the eyes.

Not only in this, but in several other cutaneous diseases, the eyes are specifically affected: blindness of a short continuance, frequently occurs in violent cases of the small-pox.

5. By a burning of the palms of the hands and soles of the feet. 6. By head-ach. 7. By a throbbing of the temporal arteries; and 8. By delirium.*

Most of these symptoms I have observed in several cases that have come under my notice.

The time of the appearance of the fever, after the exposure to the cause of the disease, is various; in cases of a less degree of violence, it is not excited till the eruption has made considerable progress, till the second, third or fourth day; in violent cases it takes place very early in the disease.

After the detail of all these symptoms, it might properly be asked, whether or not, in some instances, death is produced by these injurious plants. Several cases were communicated to me, in which the symptoms were so violent, that it was more probable that the disease would terminate in death, than in a recovery; and it is very reasonable to suppose, that the extensive ulcerations might in some instances terminate in gangrene, or that the sympathetic fever might proceed so far, as to prove destructive to the life of the person affected. Although I never met with a case, in which this disease proved mortal, yet its possibility is rendered very evident by the violence of the symptoms in some cases; and that it is capable of producing death, appears likewise to be proved by the following quotation from a writer of credit. "In Pennsylvania I was assured by a number of reputable persons, that there were instances of persons having died in consequence of the bad effects of the rhus vernix; and probably by neglecting the use of proper remedies." Wangenheim.

There are several other symptoms of this disease, which on account of their singularity deserve to be described, though they appear less frequently, than most of those which have already been enumerated; they might be called with propriety anomalous symptoms.

1. A periodical return of most, sometimes of all the symptoms of the disease, about a year after its first appearance, without fresh exposure to the cause in any manner. This periodical return occurs annually in some instances, for the course of four, five, or even ten years.

A number of cases of this periodical return have been communicated to me. Several I have observed myself. But no case establishes this singular occurrence in so unequivocal a manner, as the case of Dr. Barton. He was poisoned in the year 1785, near the Ohio; about a year after this, he went to Europe. In the year 1786, nearly at the same time of the year when he was first affected, most of the symptoms of the eruption returned: here there could not be the most distant suspicion of the poison being communicated to him by means of the atmosphere, or in any other

* Several other symptoms of fever occurred in a case communicated to me by Dr. C. Caldwell. See the appendix.

manner. The eruption returned annually till the year 1790; at every successive attack it was less violent in degree.*

To account for this periodical return of the eruption, might furnish a subject for interesting speculation. But to speculate is at present not my intention. The poison generally is excited during the hot summer months. It is only during these months that the eruption re-appears spontaneously. I have never heard of its recurrence at any other period. During these months, from various causes, the irritability of the skin is considerably increased; and I believe, on inquiry of persons thus affected, we should find, that before the re-appearance of the eruption, they have by means of exercise, or by other causes, produced a certain degree of increased action in the vessels of the surface of the body. May not then, with this increase of irritability, or of action, in the cutaneous vessels, a morbid action, similar to that which existed about an annual period before, (under analogous circumstances of every kind) be *associated;* and by this association, an inflammation of the skin, resembling the poisonous eruption, be re-excited? Dr. Rush ascribes, with much ingenuity, the recurrence of the paroxysms of the intermitting fever to a certain association of ideas and motions. This eruption appearing periodically, might be called an annual fever of the cutaneous system.

* There exists in many respects, a striking analogy between the rhus vernix and the anacardium occidentale. The juice of the anacardium, like that of the rhus vernix, produces an indelible black stain on linen. From its stem exsudes a black liquor, which may be used for the purposes of varnish. Like the rhus vernix it produces an eruption on the skin, which to complete our analogy, in some cases returns every year periodically. A remarkable instance of this was communicated to me by Mr. Josiah Coates, of Philadelphia, where the eruption returned for four or five years successively. The physician who had the care of it employed with success in its cure, a salivation. A similar case came under the notice of Dr. Physick, in which the eruption returned periodically, for several years. In another case of eruption, excited by the oil of cashew-nuts, in a young woman, who was lately attended by Dr. Physick, violent febrile symptoms supervened, which were relieved by bleeding; the blood drawn was sizy.

The hippomane mancinella, a celebrated poison tree of tropical countries, is also in many respects similar to the rhus vernix. The milky juice contained in its bark, upon touching the skin, immediately raises watery vesicles. (Bankrofft's History of Guiana.) And dew drops falling from it are so caustic, as to blister the skin and produce dangerous ulcers. Darwin's Botanic Garden.

Several vegetables which are familiar to most persons, produce effects analogous to the rhus radicans and the vernix; though in an inferior degree. The effluvia of the nerium oleander, produced in a case which came under Dr. Barton's notice, an extensive erysipelatous affection of the skin; the same has also followed the contact of the flowers of our broad leafed laurel (kalmia latifol.) That an eruption of the skin is produced by the common rue of our gardens, is almost generally known; and one case was communicated to me, in which a universal eruption was produced, by the berries of the rhus glabrum.

appeared above the place where the vein had been opened by the lancet; these upon examination were found to be the poisonous eruption. This eruption rapidly extended over the whole arm, and some of it appeared on the arm of the opposite side. The scrotum was also covered by the eruption and by vesications. The most remarkable phenomenon of this case was, that during the exacerbation of the fever, the eruption and vesications very sensibly increased, and the scrotum was perceptibly tumefied, while they almost entirely subsided during the intervals between the paroxysms.*

The different methods in which the poison is generally communicated, were formerly mentioned; there remains, to enumerate a few symptoms, which are produced by several of the different methods of communication.

1. When the smoke of the rhus vernix is the exciting cause, its effects generally resemble those produced by other causes; but when it has been very powerfully applied, symptoms of a different nature have been produced: this appears from the following fact, communicated to the philosophical society of London, by Dr. Sherard. "Some people had cut some of the rhus vernix for fuel, which they were burning; in a short time they lost the use of their limbs, and became stupid; so that if a neighbour had not accidentally opened the door, and saw them in that condition, it is generally believed, they would soon have perished."

2. Desirous of knowing the effects of the juice of the rhus vernix on myself, I applied a considerable quantity of it to the back of my left hand. This was done while I was standing near a tree, from which the juice flowed after making an incision through the bark. A slight pungent pain was felt immediately after the application, resembling that produced by the emplastrum cantharidum. The spot soon assumed a black colour, and became somewhat elevated; this elevation gradually increased; it was accompanied by a constant sensation of fulness and heat. In twenty-four hours, the whole spot was covered by a large vesicle, which on being punctured discharged a viscid, serous fluid: a scar is yet to be seen where the juice was applied.

3. It was mentioned, in a preceding part of this dissertation, that Dr. Barton had excited the eruption by inoculation: a very small quantity of the juice of the rhus vernix was introduced by a slight puncture, between the cuticle and skin of his left hand. In thirty-six hours, an itching and tumor were perceived in that spot, and soon afterwards symptoms of the eruption and slight vesications appeared on different parts of the body;

* This case would serve to prove, if any proof were required, that the poison acts in the first place locally on the cutaneous system; the patient at the time of its application laboured under a violent affection of the arterial system: that two general affections of one system (for instance, the arterial, nervous, &c.) cannot exist at the same time, is I believe, at present almost universally admitted.

it produced likewise, a slight swelling of the axillary gland of the left arm, the usual affection of the scrotum, and a universal itching. Its progress was checked by a remedy, which will be mentioned in its proper place.*

The following case was communicated to me by my very ingenious friend Dr. S. Cooper. "On Monday, about twelve o'clock, I was inoculated on my wrist, with some of the juice of the poison-vine. The next morning when I awoke, my eye-lids itched and were redder than natural. In the afternoon this symptom became worse, my face began to itch, swell and look red, and my scrotum and prepuce were affected in a similar manner. The part where I was inoculated, now began to inflame. During the existence of the preceding symptoms, I experienced frequent flushings of heat through my whole body; and my pulse was unusually quick and frequent. Vesicles were hindered from forming about my eyes and face, by often applying ice to these parts. Vesicles arose on the scrotum, on each wrist and hand, and on my feet and legs. In about eight days time the whole affection disappeared. Much cuticle came away from my face."

CONCERNING THE CURE OF THE DISEASE, PRODUCED BY THE RHUS RADICANS AND VERNIX.

From a review of the causes and symptoms of this disease, we shall easily be led to a proper method of cure. It is produced by a highly inciting cause; in every instance an inflammatory affection of the skin exists, and in many cases it is attended by an inflammatory fever of the whole system. Reasoning therefore from its causes and symptoms, I shall not hesitate to recommend such remedies, as are generally known to relieve inflammatory affections. These are chiefly evacuants. But another class of remedies, which act locally on the part affected, is necessary; these remove the existing morbid action, by exciting a stronger action in the diseased part. The application of these remedies to this disease, and to many others, has been rendered very obvious, by the clear and comprehensive therapeutical rules, which are taught in this university, by the excellent professor of the institutes and clinical medicine.

The remedies are naturally divided into such as act *generally* in the whole system, and such as are applied *locally* to the parts affected. Of the former I shall recommend blood-letting, purging, cold in the form of ice, cold water or cold air, and mercury given with the view of exciting a salivation.

* I have in several instances, and in habits which were very susceptible of the poison, attempted to propagate it, by inoculation with the serous fluid contained in the vesicles; but in no instance was I able to excite the infection.

1. Blood-letting. This remedy has lately been used, with singular success, by Dr. Barton and Dr. Cooper, of this city, and by Dr. Stocket, of Maryland.* The appearance of the blood drawn, was mentioned in a former part of this essay. In violent cases it should never be omitted, and in moderate cases it will shorten the course of the disease, and render the symptoms less distressing.

2. Purging. This remedy, especially in cases accompanied by much inflammation, should always be used in conjunction with the last remedy: It may be particularly useful in such cases, where from the extent of the tumefaction or ulceration, blood-letting is rendered impracticable. In these cases this remedy will be one of our chief resources.

Under this head is to be ranked, copious drinking of sea-water. This was used in a case communicated to me by Mr. Samuel Coates, in which the disease returned several years periodically, and resisted all the remedies that were applied, until the patient was sent to the sea-shore, where it soon yielded to sea-bathing and plentiful potations of sea-water, which produced very copious and continued evacuations from his bowels. Wherever this is convenient it may be an excellent remedy. The analogy between these poisons and the mancinella, was mentioned in a former part of this essay. It is confirmed by the following circumstance: Bancrofft, in his History of Guiana, tells us, that in the bad effects produced by the mancinella, sea-water, and the juice of limes, &c. are effectual remedies. A remedy which is used in the cure of the eruption, occasioned by the rhus vernix of Japan and China, produces violent purging.†

3. Cold, whether applied in the form of ice, of cold water or cold air, is an excellent palliative of all the symptoms, in the early stage of this disease. It particularly relieves the itching, which in some cases is very distressing, and it often retards or entirely prevents the tumefaction. A person who is very susceptible of the poison, informed me, that a very cold bath relieved all the symptoms and entirely checked the progress of the disorder. Dr. Cooper derived great benefit from the application of ice to his face; and in my own case every symptom was soon relieved, by the use of this excellent remedy. Kalm, likewise used this remedy with advantage. It was remarked formerly, that the eruption is less easily excited in cold than in warm weather.

4. In violent cases, where the symptoms of the disease run high, and where the patient's life may be in danger, I recommend with great confidence, the internal use of mercury, with the view of exciting a salivation. From its almost specific effect in all cutaneous diseases, we may also in

* It was also employed, with good effect, in a case communicated to me by Dr. C. Caldwell.

† Duhalde's History of China.

this instance, expect much from this powerful remedy. I formerly mentioned a case of erysipelas produced by the anacardium occidentale, in the cure of which it was used with success.

2. Topical remedies.

The first and most powerful of these, is a solution of corrosive sublimate in water. This remedy was first employed by Dr. Barton in his own case, and by him it has been recommended to several other persons: from the success which has uniformly attended this remedy, in every case in which it was used, he is led to believe, that it may be employed in the cure of this eruption, with as much certainty and probability of success, as it is in many other diseases. The application of the solution, very soon excites a specific inflammation of the skin in the parts affected, which at this time are in a state of great excitability; this inflammation is of greater force than that produced by the rhus, which it counteracts or destroys.* It soon allays the itching, accelerates the suppuration of the vesicles, if any have formed, and brings on a desquamation of the purulent crust, or in moderate cases, of the cuticle. But, its effects are not topical alone; in all persons that are easily acted upon by mercury, it produces a salivation; this happened in Dr. Barton's case.†

2. In moderate cases I have applied with very considerable advantage, an ointment prepared by mixing one drachm of saccharum saturni with one ounce of unguentum simplex. My friend and fellow graduate, Mr. P. G. Prioleau, informed me, that upon being poisoned by the rhus radicans, particularly on his hands and arms, after using several remedies with little effect, he was cured by the application of a solution of saccharum saturni. It was also used in conjunction with the ungt. mercur. precip. rubr. in the case related p. 136, with considerable advantage.

3. The unguentum simplex of the Dispensatories, sweet oil, and emollient cataplasms, should be applied in all cases, where in consequence of neglect in the beginning, tedious and ill-conditioned ulcers remain. Whenever their cure is much protracted, mercury should be exhibited internally.

With these remedies, general and topical, regulated according to the existing stage of the disease, and accommodated to the constitution of the

* Dr. J. Otto informed me, that a friend of his had in several instances, removed the eruption in a short time, by the application of the spiritus sal. ammon. diluted with water: this acts in a manner similar to the solution of corrosive sublimate.

† The ungt. mercur. precip. rubr. is used in some parts of Pennsylvania in the cure of this eruption; it is called by the inhabitants of the country, poison-salve. In the case which occurred in the alms-house of Philadelphia (see p. 136) it assisted in the cure of a number of chronic ulcers produced by the rhus radicans. It is an excellent remedy in these cases.

appeared very analogous to crystals of tartar; and the following circumstances, tend to prove their complete identity with that saline substance.

1. Their taste very much resembled the taste of cream of tartar; it it was however more truly acid, devoid of the earthy taste which always accompanies that salt, and which may be owing to the aluminous earth by which it is clarified, or to calcareous earth with which it appears sometimes to be adulterated. Perhaps the crystals obtained from the rhus contain a greater proportion of acid. 2. The superabundance of tartareous acid was rendered very evident by a violent effervescence, which took place on adding a small quantity of potash to a solution of these crystals. Their analogy to chrystals of tartar is further proved: 3. By their difficult solubility in water: 4. By being rendered much more soluble by the addition of a little borax; and 5. By a slight red colour, which they produce on blue vegetables.*

The gallic acid has been found to exist in every species of rhus, which has hitherto been examined by the chemist. I have met with it in different proportions, in several of our native species of this genus, in the rhus vernix, rhus radicans, rhus typhinum, rhus copallinum, and especially in the rhus glabrum. And we may very probably suppose from analogy, that it will be found in a greater or less degree, in every species of this genus.† It appears from the following experiments, that this acid exists in different proportions in different parts of the same species: no part of the plant, however, is entirely destitute of it; I have found it in the flowers, the leaves, the bark, the seeds, and even the root. The relative quantity contained in the different species, is very various: from the few trials I have made, the result of which requires much further confirmation, I suppose they may be ranked in the following order: the rhus glabrum contains most, next the typhinum, copallinum, vernix, and radicans.

As the rhus glabrum appeared to contain the gallic acid in the largest proportion, and as it was procured with more facility than any of the other species, I subjected it to the following experiments.

EXPERIMENT II. The infusion of the leaves, after a short exposure to the air, is of a light brown colour; the infusion of the berries is of a beautiful crimson colour.

EXPERIMENT III. To a pint of pure water, in which one grain of sulphate of iron had been dissolved, one drop of an infusion of one ounce of the leaves in a pint of water was added: a slight grayish blue discolouration soon took place, which in twenty-four hours became more perceptible; if to the same solution three drops were added, the water soon assumed a purple colour.

* A solution of these crystals, produced in lime-water and in a solution of the acetite of lead, a white precipitation.

† It exists plentifully in the rhus coriaria and cotinus of Europe.

Experiment IV. If to half an ounce of the infusion of one ounce of the fresh berries in a pint of water, one drop of a solution of sulphate of iron is added, a black precipitation takes place, which is immediately re-dissolved; if even three or four drops are added, still the liquor remains transparent; but if now a few drops of a solution of potash are added, a deep black colour is instantly produced.

Experiment V. If to half an ounce of the infusion of the leaves, of equal strength of that of experiment the third, one drop of a solution of sulphate of iron is added, the liquor immediately assumes a black colour: but, if to the same quantity, previously one drop of diluted sulphuric acid is added, the black precipitation, as in the last experiment, is again re-dissolved, until the acid is neutralized by the addition of a few drops of a solution of potash. This experiment was repeated with the same result, with an infusion of galls.

Experiment VI. To determine the relative quantity of gallic acid contained in the leaves and berries of this species of rhus, I infused one ounce of each, in half a pint of water, and to equal quantities of both infusions, I added a scruple of sulphate of iron. Characters written with the infusion of the leaves were nearly black, while such as were written with the infusion of the berries were only brown. With a view to neutralize the tartareous acid of the berries, I added a few drops of a solution of potash; an effervescence took place, but the blackness of the infusion was not increased.*

Experiment VII. To determine the relative quantity of gallic acid contained in the rhus glabrum, and in galls, I infused an ounce of the leaves, berries, and galls, each separately in a pint of water; to equal quantities of these infusions, I added a scruple of the sulphate of iron. Characters written with the infusion of the berries, were as in the last experiment, considerably paler than those written with an infusion of the leaves; while between such, as were written with the infusion of the leaves and of galls, I could not discover the least difference in point of blackness; nor could several of my friends to whom they were shewn.

Observing the blackness produced in the last experiment by the sulphate of iron in an infusion of the leaves, I was induced to believe, that by a due proportion of the relative quantity of the leaves of the rhus glabrum and of sulphate of iron, they might be used advantageously in the preparation of ink; to determine this, the following experiments were instituted.

Experiment VIII. My first trials were made with the berries; six drachms of these, two of sulphate of iron, and one of gum arabic, were

* Small quantities were first added, and these were gradually increased; but though by a very small quantity the colour of the infusion was not impaired, yet, when it was increased, a dirty brown colour was always produced.

infused in half a pint of water. The ink obtained in this manner, as might readily be expected from the result of the sixth experiment, was of a brown colour. It was not improved by the addition of potash. By taking a larger proportion of berries the blackness was somewhat increased.

Experiment IX. I infused six drachms of the leaves, two drachms of sulphate of iron, and one of gum arabic in half a pint of water;* by this infusion I obtained very good ink, equal in point of blackness to our common writing ink.

Experiment X. To determine more accurately the degree of blackness of this ink, I made a relative trial with exactly the same quantity of galls. The blackness of characters written with both, after having been kept for four months, was so nearly alike, that by the nicest examination I could scarcely perceive any difference: they were shewn to, and examined by several persons, who were of opinion, that no difference existed between them.† If a greater proportion of leaves to the other ingredients was used, the intensity of the blackness of the ink was considerably increased.

The preceding experiments indicate the presence of a considerable quantity of gallic acid; to establish more fully the identity of the rhus glabrum, with those vegetables, which are commonly called astringent vegetables, I made the experiments which immediately follow.

Experiment XI. To a very strong filtered infusion of the leaves, a small quantity of a solution of potash was added; a very copious yellowish precipitation soon took place; the addition of potash was continued till no more precipitation ensued: after some time the supernatant liquor was poured off, and a few drachms of sulphuric acid were added to the precipitation, by which it was instantaneously dissolved.‡

Experiment XII. If to an infusion of the berries, the solution of potash was added, a violent effervescence took place, which was owing to the tartareous acid of the berries (exper. 1.); by increasing the quantity of potash, a precipitation ensued, as in the last experiment, which was very soluble in sulphuric acid.

Experiment XIII. After saturating about two pints of the infusion of the berries with potash, half an ounce of diluted sulphuric acid was added to it; the liquor then was suffered to evaporate spontaneously. In about two months a number of crystals were formed, resembling those of sulphate of potash.

* These are the proportions recommended by Mr. Lewis. Phil. Com. of Arts.

† When this ink was first prepared (with the rhus glabrum) and a few days afterwards, it had a slightly brownish hue; this disappeared in my specimens, after being kept a few weeks; the ink prepared with galls, as is well known, has when recent, a bluish cast.

‡ The precipitation was insoluble in water and in alcohol.

Experiment XIV. To the clear supernatant liquor, remaining in the twelfth experiment, small quantities of sulphuric acid were added; a precipitation took place, which being dried, by forming a dark brown tincture with alcohol, exhibited evident signs of the presence of a resin, which however exists only in small quantity.

Experiment XV. In the supernatant liquor of the twelfth experiment, a solution of sulphate of iron produced a black precipation; the oxygenated muriate of mercury, and the muriate of barytes, produced a precipitation of a brown colour.

Experiment XVI. The precipitation produced by potash in the infusion of the leaves, is readily soluble in the sulphuric acid, as appears from the eleventh experiment; but, by the addition of potash, ammoniac, or a solution of borax, it is again precipitated. By spontaneous evaporation of this solution, a highly astringent, gummy substance is obtained, which being dissolved in water, is readily decomposed by potash or by muriate of barytes.

Experiment XVII. The infusion of the berries concentrated by evaporation, produced in a solution of silver in the nitrous acid, a white, and in a solution of mercury, a yellow precipitation; in a solution of the acetite of lead it produced a white precipitate; in lime-water and the oxygenated muriate of mercury a gray precipitate; and in a solution of sulphate of potash a brown precipitate.

Experiment XVIII. About two pounds of the berries, recently collected, were digested several days in a gallon of water; the liquor was then filtered, evaporated to a pint, and one ounce of quick-lime gradually added to it; having digested twenty-four hours, the calcareous sediment was collected, and frequently washed with warm water; one ounce of diluted sulphuric acid was now poured upon it, and after some time the liquor was poured off, evaporated to a pellicle, and exposed to crystallize. About two drachms of long, slender, four-sided crystals were obtained.* These crystals did not effloresce in the air; dissolved in water they produced a copious milky precipitation in lime-water and in a solution of the acetite of lead.

Experiment XIX. On half an ounce of the berries of the rhus glabrum, I poured five ounces of strong nitrous acid; a large quantity of nitrous gas was disengaged; a moderate degree of heat was applied for several hours; the remaining fluid was then poured into a plate, and in a short time a large number of delicate, needle-form crystals were produced. A solution of these crystals caused an instantaneous milky precipitation in lime-water and in a solution of the acetite of lead; in a solution

* I obtained at the same time, a few very minute crystals of the sulphate of potash.

of sulphate of zinc or copper, a white precipitate was produced. These crystals effloresced when they were exposed to an air of a moderate temperature.

On a repetition of this experiment, I obtained some crystals of the same form with those just described, and others, of a quadrilateral form, terminated by dihedral summits; the former of which effloresced in the air.

Experiment XX. By treating half an ounce of the extract of rhus glabrum, with five ounces of nitrous acid, I obtained lamellated and oblong crystals, which possessed all the properties of those described in the last experiment.

When these crystals are thrown into water, an evident crackling noise is heard; this is peculiar to this salt, and is ascribed by Fourcroy to a sudden breaking of its particles.

Experiment XXI. About three pounds of the leaves of the rhus radicans were infused in brandy; after being boiled a few minutes, they were exposed in a moderate temperature. At the end of several months, this infusion was found converted into very strong vinegar; it resembled that which is obtained from cyder, exceeding it however in strength. Its taste was very pungent, and it retained some of the peculiar odour of the plant. On adding a little potash to it, a violent effervescence took place.

Experiment XXII. To two pints of a strong infusion of the berries of the rhus glabrum, some chalk, and a small quantity of weak spirit of wine was added; the mixture was then exposed in a vessel slightly stopped, in a moderate temperature. In the course of three or four months, vinegar was produced, which resembled that generally used for culinary purposes; some of it being added to a little potash, produced considerable effervescence.

OBSERVATIONS ON THE PRECEDING EXPERIMENTS.

By the first experiment a considerable quantity of semisaturated tartrite of potash was obtained. In order to succeed with this experiment, it is necessary to collect the berries while they are in a state of entire perfection, and covered with the white saline substance, which was described in the history of the growth of the plant; if this is not attended to, the salt is dissolved by the rain or moisture, to which the plant is frequently exposed. I have endeavoured to establish the identity of this salt with crystals of tartar, by a few simple but apparently conclusive experiments and analogies. Mr. Hermbstædt obtained a similar salt from the berries of the rhus coriaria,* which has been analysed by Mr. Tromsdorf:† I

* Physikalisch-chemische Beobachtungen. p. 210.

† Crell's Annals, 1787.

regret that I had not an opportunity of seeing this analysis. This salt, Mr. William Bartram informed me, exists in much larger quantity, on the berries of the rhus copallinum; it is used in the preparation of punch, and in his opinion, is nearly equal to the juice of lemons, for that purpose.

To determine, with the utmost degree of precision, the relative quantity of gallic acid, contained in different parts of the rhus glabrum and in galls, was the object of the third, the sixth, and seventh experiments; the result is obvious, and will be applied more particularly under the subject of ink.

In the fourth experiment, we observe the black precipitation produced by sulphate of iron in a solution of the berries, to be immediately re-dissolved; from a review of the first experiment, this circumstance admits of very easy explanation; the tartareous acid, which exists in the berries in large proportion, re-dissolves the iron and holds it in solution, by which means the effect of the gallic acid is directly counteracted. This is rendered still more evident, by the immediate black precipitation which ensues, when the tartareous acid is neutralized by potash.

In the fifth experiment, the action of the tartareous acid is imitated by the addition of a little sulphuric acid; the result of this confirms the observations that were made on the last experiment.

From the result of these experiments, we can draw an important practical inference, with regard to the use of the rhus glabrum in the art of dyeing. It is very common in many parts of this country, to employ the berries in preparing a black dye for hats and for other substances; from a comparison of the fourth and fifth, with the sixth and seventh experiments, it appears, that they are much less adapted to this purpose than the leaves, which I shall recommend hereafter. Although the tartareous acid be neutralized by potash, yet, as it is impossible to regulate the proportion of potash so exactly, as not to saturate at least a part of the gallic acid also, the colour in this case is by no means so black, as that produced by the leaves; but it is generally of a rusty brown hue.

In the eighth, ninth and tenth experiments, my object was to determine the probable utility of the rhus glabrum in the preparation of ink. In the eighth, the berries alone were used; the ink produced by these (according to the fourth and sixth experiments) was of a brown colour. The result of the ninth and tenth experiments, not only fully answered, but even in some degree, exceeded my expectations. The quality of the ink which they afforded, and the relation they bear to that prepared with nut-galls, has been mentioned in the account of those experiments. The qualities, and the principles of the formation of ink, have always been considered as a matter of importance. Chemists of the first reputation, have at different times been engaged in the investigation of this subject. Among the number of those who have cultivated it with most success,

rhus coriaria in Turkey, Germany, Spain and France; the rhus cotinus in Italy.* Our own forests are sensibly consumed; our oak-trees will soon be exhausted. To what vegetable then, for a substitute, can we so readily resort, as the rhus glabrum? It grows plentifully in most parts of the United States, and where it is deficient, it may easily be cultivated; its growth is rapid, and while in a very young state,† it answers all the purposes of this art.

I have thus endeavoured to point out the probable utility of the rhus glabrum in several of the most useful of our arts. The further investigation of this subject, especially on a larger scale, is requisite. I wish it to be considered, not merely as a matter of philosophical curiosity. It deserves attention not only in a local or individual point of view, but its application may become of a more extensive nature. I have no doubt, that at a future period, the cultivation or preservation of this plant, will be considered an object of general, of national policy.

The analysis of astringent vegetables has of late been effected with great accuracy: in the eleventh and several of the succeeding experiments, (12, 14, 15 and 16) it was my design to shew the similarity of the rhus glabrum to several of the most powerful of this class. The reader will perceive, that most of them were suggested by the experiments of Dr. Woodhouse;‡ whose accurate analysis of the persimon has added much to our knowledge of the subject of vegetable astringency.

In the eleventh experiment, the alumine, one of the constituent parts of astringent vegetables, was precipitated from a strong infusion of the leaves, by a solution of potash: one of the chief characteristics of this precipitate, is its ready solubility in sulphuric acid. The result of the twelfth experiment concurred with that of the last, after the tartareous acid of the berries was saturated. The quantity of resin contained in the rhus glabrum, appears to be very minute, (exp. 14.) In the sixteenth experiment, by the re-union of the alumine, precipitated from an infusion of the leaves, and sulphuric acid, a liquor was produced, which possessed all the properties of a solution of the common sulphate of alumine. In reviewing these and the preceding experiments, we find in the rhus glabrum all the component parts of astringent vegetables; a small proportion of resin, and a considerable quantity of gallic acid and of alumine.

In the thirteenth experiment, the sulphuric acid, in consequence of its greater affinity, appeared to unite with the potash of the mixture, while the tartareous acid was separated. The precipitations produced in

* Smith's Tour.

† The shoots of the rhus coriaria, are cut down every year, quite to the root; and after being dried, they are reduced to powder by a mill, and thus prepared for the purposes of tanning. Nichol. Chem. Dict.

‡ Inaugural Dissertation, 1792.

the seventeenth experiment, resemble those that are produced in the same solutions, by the crystallized acid obtained from the nut-gall.

The design of the eighteenth and several of the succeeding experiments, was different from those we have already discussed. In the eighteenth, by the process invented by Scheele for obtaining the pure tartareous acid from crystals of tartar, I obtained an acid, which appeared to possess all the properties of the tartareous acid. I found it of importance to employ quick-lime in this process; with common chalk the experiment did not succeed. This experiment affords additional proof, to what was said in considering the first experiment, of the nature of the salt obtained by lixiviation from the berries.

By subjecting the berries and extract of the rhus glabrum to the action of the nitrous acid, according to the process generally employed for procuring the oxalic acid, I obtained crystals of different kinds: some upon examination appeared to possess the properties and form of the oxalic, while others more exactly resembled the tartareous acid. The affinities of these two acids to earthy and to other substances are so nearly alike, that I have found it somewhat difficult to distinguish between them. One of the most evident marks of distinction appeared to be the circumstance, that the oxalic acid generally effloresced in a warm temperature, while the tartareous did not. It is not difficult to conceive, that by the same process these two acids should be procured. Hermbstædt obtained from sugar by weak nitrous acid, tartareous; by strong, oxalic acid. The oxalic differs from the tartareous, merely in being more highly oxygenated; it is in fact the most oxygenated of all vegetable acids. This is one instance among innumerable others, of the ready convertibility of one vegetable acid into another; the analogy between many of these acids is so great, that they are considered by several chemists as fundamentally the same. Among the most respectable characters who entertain this opinion, are Westrumb* and Hermbstædt,† two German chemists; the former contends that the acetous is the only elementary vegetable acid, while the latter considers as such exclusively the tartareous. On reviewing their labours, every person will admire their perseverance and ingenuity; but neither of them has yet been able so far to extend or generalize his experiments, as to establish his hypothesis.

In the twenty-first experiment, by the infusion of the leaves of the rhus radicans in brandy, after exposure for several months, an excellent vinegar was produced. Different species of rhus have long been employed in the preparation of vinegar. I have been informed by the gardener of the Pennsylvania hospital, who has several years resided in

* Keir's Chemical Dictionary.

† Physikalisch-chemische Beobachtungen.

Canada, that the French inhabitants of that country about Montreal, prepare a very good vinegar from the berries of the rhus glabrum. The berries are boiled in water, and afterwards exposed in a cask to the heat of the sun, in order to ferment. "The young germen of the fruit of the rhus typhinum, when fermented, produces vinegar; hence it is called vinegar-tree. (Encyclopædia.) Boththese species grow abundantly in many parts of Pennsylvania, where they might be collected, fermented, and afford cheap and excellent vinegar.

By the twenty-second experiment, vinegar was likewise produced; in this instance was not the tartareous acid of the berries gradually converted into the acetous?

CONCERNING THE USE OF SEVERAL SPECIES OF RHUS, IN THE ART OF DYEING.

Experiment I. The fresh leaves of the rhus radicans, broken off from the plant, exsude a liquor of a white milky appearance; this liquor applied to flannel, to muslin or linen, imparts to them, after being a short time exposed to the light, and especially to sunshine, an intensely black colour. Applied to paper it stains it black in the same manner. The liquor when first applied, is scarcely visible; it resembles a spot of oil, but gradually becomes yellow, red, brown, and finally black. This black stain cannot be washed out by means of alkali or soap. It does not fade on exposure to the air. Alcohol appears to have no action upon it. If leaves or branches are broken off and exposed to the air, the juice exsuding from their ends, likewise becomes black; in this case the colour has a more glossy appearance.

Experiment II. In order to obtain this elegant black juice, for the purpose of fixing it on flannel or linen, I pounded some of the stalks which appeared to abound with it, and expressed their juice; but I obtained merely a greenish liquor; the black colouring matter appeared to be absorbed by the pith of the stalks. I have tried a variety of methods, to extract or separate this black colouring matter, which exsudes from every part of the plant, but with none have I, as yet, been able to succeed.

Experiment III. In a strong filtered decoction of the fresh leaves of the rhus radicans, I boiled for the space of an hour, small pieces of flannel, muslin, linen, and silk. It imparted to the flannel and silk a dark fawn colour, which after the stuffs were once washed in soap and water, was permanently fixed. The linen and muslin had scarcely taken up any of the colouring matter.

Experiment IV. In a decoction prepared in the same manner as the last experiment, I boiled small pieces of flannel, muslin, and linen, after having steeped them in a strong solution of sulphate of alumine.

The flannel was dyed of a dark yellow colour, which was permanently fixed. With the linen and muslin the success was better than in the last experiment.

Experiment V. In a decoction similar to that used in the third experiment, after adding a little vinegar, I boiled small pieces of flannel, muslin, and linen; they acquired a dirty brown colour: with small quantities of the nitrous or sulphuric acid, the result was nearly the same.

Experiment VI. To separate portions of the decoction of the third experiment, I added small quantities of nitrate of potash, muriate of soda, potash and urine: the stuffs were impregnated with a brown or grayish colour; but neither of these promises the least utility in dyeing.

Experiment VII. In a decoction of the leaves of the rhus radicans, prepared with the addition of one fourth part of spirit of wine, I boiled a piece of flannel which had been soaked in a solution of alum; it acquired an elegant pale yellow colour.

Experiment VIII. In the simple decoction of the leaves and bark of the rhus vernix, a piece of flannel acquired a pale fawn colour; and

Experiment IX. A piece of flannel, having been steeped in the solution of alum, acquired a very brilliant yellow colour.

Experiment X. With the fresh bark of the rhus glabrum, a strong decoction was prepared, in which small pieces of silk and flannel were dyed of a yellowish fawn colour, which on the silk appeared more elegant than that of any of the former simple decoctions.

Experiment XI. In the same decoction, a piece of flannel and silk, after being impregnated with a solution of alum, acquired a very handsome permanent yellow colour.

Experiment XII. In a decoction of the leaves and stalks of the rhus glabrum, a piece of flannel and silk having been steeped in a solution of alum, acquired a yellow colour, resembling that of the last experiment.

Experiment XIII. In a strong decoction of the rhus radicans, in which a certain quantity of sulphate of iron was dissolved, I boiled repeatedly a piece of blue woollen cloth; it acquired a deep black colour.

REMARKS ON THE PRECEDING EXPERIMENTS.

The black juice which flows from several species of rhus, on wounding their stem or detaching their leaves, (exp. 1.) has long attracted the notice of naturalists. Kæmpfer appears to be the first who described the juice distilling from incisions into the rhus vernix.* Lawson, in his natural history of Carolina, published in the year 1709, has the following

* Amœnitates Exoticæ.

passage: "The juice of the poison-vine will stain linen, never to wash out. It marks a blackish blue colour, which is done only by breaking a bit of the vine off, and writing what you please therewith."* In 1755 the Abbé Mazeas communicated to the philosophical society of London several experiments, which he made on suggestion of the Abbé Sauvages, with three different species of rhus.† He mentions having produced a brown stain on linen with the juice of the rhus vernix, while with the juice of another species, which he calls hedera trifolia canadensis, he produced a deep and permanent black. The latter of these plants, from his description, evidently appears to be the rhus radicans.‡ This supposed discovery of the Abbé Mazeas, was the cause of the controversy between Mr. Miller and Mr. Ellis, which was mentioned in the description of the rhus vernix.

Since the time of the Abbé Mazeas, this subject has been attended to by several persons, and among others, by the ingenious Mr. Lewis: hitherto their labours have not been successful in extracting or separating the black juice, for the purpose of fixing it on linen. The subject deserves to be further investigated.

It is supposed by some persons, that the Indians of North America make use of the rhus radicans, in staining different substances of a permanent black colour.

The circumstance, of the immediate change of the colour of the juice from white to black, upon exposure to atmospheric air is very singular. What would be the effect of an atmosphere of pure azote, or oxygen?

By the simple decoction of the rhus radicans, vernix and glabrum, a fawn colour of a darker or paler hue, is produced on flannel and silk, which appears to best advantage on the latter. By none of my experiments, was I able to impart to linen or muslin, the same colour that was easily imparted to flannel and silk: it appears necessary that they should previously undergo some process, to fit them for the reception of the colouring matter.

If the flannel or silk, was steeped in a solution of alum, before being boiled in the decoction of the different species, it acquired a very good yellow colour.

Every one of these colours was permanently fixed.||

* He adds: "I have thought, that the East-India natives set their colours, by some such means, into their finest calicoes."

† London Philosophical Transactions, v. xlix.

‡ The stain produced by the rhus vernix, I have always observed, to be less black than that produced with the rhus radicans.

|| The result of these experiments agrees with those of Berthollet on the rhus coriaria. "The rhus coriaria alone (he says) gives a fawn colour inclining to green; but stuffs that have been impregnated with aceteus alum, take a very good and durable yellow."

It appears to me, from a review of my specimens, that several species of rhus, and especially the glabrum, might be employed with considerable advantage, in imparting a yellow colour to flannels.

Its cheapness, and the facility with which it is procured, ought certainly to recommend it to our dyers.

To the decoction of the seventh experiment, a small portion of alcohol was added; this appeared to increase the brilliancy of the colour: it is probable that the colouring matter of these plants is contained chiefly in their resinous part, which is rendered soluble by the extractive matter they possess in considerable proportion.*

CHEMICAL ANALYSIS OF THE RHUS VERNIX AND RHUS RADICANS.

Experiment I. I distilled about two pounds of the flowers and leaves of the rhus vernix, after adding to them a few quarts of water, in a small copper still; a moderate degree of heat was applied; the fluid which came over, was slightly impregnated with the odour of the plant, but did not appear to possess any active properties. From the liquor remaining after distillation, I obtained by evaporation one ounce of extract, of a dark brown colour.

Experiment II. About two pounds of the leaves and flowers of the rhus radicans, were distilled, like those of the vernix in the last experiment; and with a similar result. I obtained no essential oil, either by this or by the last experiment; although the odour of the flowers of both species was highly aromatic, and appeared to indicate its presence. It is probable, that by employing very large quantities of the flowers, a small portion of essential oil might be obtained. I had not an opportunity of determining with accuracy, the properties of the distilled water; I have no doubt, but that in persons who are easily affected by the poison of these plants, it would produce a cutaneous eruption. Du Fresnoy† observes, "the leaves inflamed and swelled the hands and arms of those that took them out of the still, and brought on an itching, which remained for several days." No effect was produced on my hands by handling the leaves, remaining after distillation; the cause of which was mentioned in a former part of this essay.

* A young man, a native of the United States, is said to have prepared a kind of ink, resembling China ink, from the rhus radicans; for the exclusive privilege of making which, he procured about thirty years ago a patent from the British parliament. It appears probable to me, that it was prepared with the charcoal of the bark of the rhus radicans, finely levigated and conglutinated by means of gum or glue.

† Des propriétés de la plante appellée rhus radicans, p. 46.

The liquor remaining after distillation, and a solution of the extracts, of both species, produced a copious black precipitation in a solution of sulphate of iron.* Might not an extract of the rhus glabrum, be advantageously employed for the purpose of dyeing black? It would contain the gallic acid in a very concentrated state, a small quantity only would be requisite, and it might be very conveniently preserved for use.

Experiment III. Several ounces of the berries of the rhus vernix, radicans and glabrum, were separately exposed in a retort, to an intense degree of heat. A pellucid fluid, of an empyreumatic smell, first passed into the receiver; the degree of heat being increased, a black empyreumatic oil was obtained, resembling that which by this process is procured from most vegetables. The result was the same with the three different species. In the pellucid fluid which first came over, a slight precipitation was produced by potash, sulphuric acid and oxygenated muriate of mercury. This distillation, with a violent degree of heat, is at present generally omitted in the analysis of vegetables; from plants which possess the most opposite properties, from "cabbage and hemlock," the same products are obtained. I was convinced of its fallacy, by the result of these experiments: the presence of the gallic acid, one of the chief component parts of these vegetables, was scarcely discoverable in the products of this distillation.

Experiment IV. From one ounce of the dried bark of the rhus radicans, I obtained by frequent infusions with rain water, thirty-five grains of pure extract.

Experiment V. One ounce of the powdered bark of the rhus radicans, was repeatedly infused in highly rectified alcohol; by spontaneous evaporation, after carefully filtering this infusion, I obtained fifty-five grains of an elegant shining, deep brown resin.

Experiment VI. Four drachms of the powdered bark of the rhus vernix, afforded two scruples of resin, resembling that obtained from the rhus radicans. A solution of the resin of both species in alcohol, produced in a solution of the sulphate of iron, a copious black precipitation. This resin very much resembles the juice exsuding from the bark of these plants. It is probable that it might be collected in this manner, for the purposes of varnish.

Experiment VII. By infusing the bark of both species, in common spirit, a considerable quantity of gum-resinous substance, of a brown glossy appearance, was obtained.

Experiment VIII. A quantity of the leaves of the rhus radicans and glabrum, being infused in boiling water, and exposed to a warm tempera-

* The quantity of extract which I obtained after the distillation of the rhus radicans, amounted to six drachms.

ture, (in the month of July) soon passed into a state of fermentation, and emitted a smell resembling that of small-beer. Whether upon distillation spirit of wine might be obtained from them, I have not yet determined.

OF THE MEDICAL PROPERTIES OF THE RHUS RADICANS.

LITTLE is at present known, concerning the internal effects of the rhus radicans on the human body, or concerning its qualities as an article of the materia medica. To determine its probable utility or inefficacy in the cure of diseases, was one of the objects, which at the commencement of the examination of the properties of this vegetable, I had particularly in view. A defect of proper cases for its exhibition, and the interference of a variety of other pursuits, have prevented me from collecting the necessary materials for this purpose. A concise statement of what has been written on this subject by others, and a few facts and observations, which I have had an opportunity of collecting, will be related in this section: in how far future experiments will confirm or refute these observations, I am not able even to conjecture.

To a man, who had laboured under pulmonary consumption nearly two years, and who was in a very debilitated state, I gave two ounces of a strong infusion of the leaves of the rhus radicans; it excited some fever, increased the frequency and hardness of his pulse, produced pain in his bowels, and an increased flow of urine; after the disappearance of these symptoms, his pulmonary complaint was considerably relieved. His wife, invited by the agreeable odour of the infusion, drank a tea-cup full; it produced an unusual degree of cheerfulness, and a copius discharge of urine.

A person who had been afflicted above a year, with a very obstinate head-ach, took four ounces of a strong infusion of the leaves of the rhus radicans. In consequence of this, he discharged, by his own account, in one night nearly as much urine, as he was accustomed to do in a week. He had been in the habit of discharging very small quantities.

In the case of a woman, who had universal anasarca, the infusion produced a copius perspiration, by which she was considerably relieved.

In two cases of pulmonary consumption, in which the infusion of the leaves, and small quantities of the extract were exhibited, they appeared to produce an increase of the symptoms of the disease and were soon omitted. The subject of the first of these cases was a man in the early stage of the disease; the other, a woman about twenty-five years of age, originally of a plethoric habit: the extract produced a pain in her stomach. In another case of the same disease, small quantities of the extract evidently relieved the symptoms; the person was aged; the medicine, in her opinion, relieved her by keeping the bowels open.

From the foregoing cases it appears, that the rhus radicans acts slightly as an incitant and diuretic.

It is a very general opinion, among the inhabitants of the western parts of Pennsylvania, especially about Carlisle, that the rhus radicans is an effectual remedy in the cure of phthisis pulmonalis. Dr. Woodhouse, in his inquiries concerning this subject, met with a young man, who lived in the vicinity of that place, and who had been relieved of a pulmonic affection, by an extract prepared from the bark of this vegetable. The young man informed him, that it relieved the pain in the breast, produced an eruption on the skin, and a slight salivation; and that it opened the bowels. He remarked likewise, that in order to derive benefit from it, it is necessary to take large quantities. Schœpf, in his mat. med. Americana, has the following observation: " Radices (rhus radicant.) a nonnullis in asthmate chronico bono cum successu adhibebantur in Lancaster."

If it should be found, by further trials, that the rhus radicans internally exhibited, is not an active medicine; the effects which it produces on the external surface of the body, may still render it highly valuable, in the cure of many diseases. Its application for evident reasons, will be confined to particular habits and constitutions. A determination to the skin produces relief in many acute diseases; and the production of violent cutaneous affections, has cured or relieved many diseases of a chronic nature. Eruptive diseases have in several instances removed mania. Melancholia and epilepsy have been cured by inoculation for the itch.* Eruptions and burns have frequently suspended epilepsy, and relieved fatuity.† A case of herpes, cured by the eruption excited by the rhus radicans, is related by Du Fresnoy. (See the appendix.)

In certain stages of mania, melancholia, and pulmonary consumption; in epilepsy, palsy, and other chronic disease, which for a long time had resisted the effects of powerful remedies; in habits that are acted upon by the rhus radicans or vernix, I should not hesitate to excite a universal cutaneous eruption, by means of the poison of these plants, There appears to be considerable ground, for a reasonable supposition, that at least in some instances, this treatment would remove a disease or palliate its symptoms.

A dissertation has lately been published by Dr. I. Alderson, of England, on the rhus toxicodendron, which he particularly recommends in paralytic affections; as appears from Darwin's Zoonomia.

From one to four grains of the powder of the dried leaves, are to be taken three or four times a day. I have not had an opportunity of seeing this dissertation.

* Instances of this are related by Drs. Muntzell and Zimmermann.

† A case of epilepsy, cured by the small-pox, is related by Dr. S. Cooper, in his Inaugural Dissertation on Stramonium.

One treatise only has hitherto been published on the medical properties of the rhus radicans. It is entitled: "Des propriétés de la plante appellée Rhus Radicans. De son utilité et des succes, qu'on en a obtenu pour la guerison des Dartres, des affections Dartreuses, et de la Paralysie des parties, inférieures. Par M. Du Fresnoy, M. D. 1788. Dr. Du Fresnoy was led by accident to the employment of this vegetable in herpetic eruptions. The case which first suggested to him its use, is curious: it is related in the appendix. Before he exhibited the rhus radicans to other persons, Du Fresnoy ascertained its dose by experiments made upon himself. Perceiving no effect from taking an infusion of one of the leaves of the plant, he gradually increased the number of leaves for each infusion, to twelve. "At this dose, (he says) I observed a slight pain in my stomach, and my perspiration and urine were increased in quantity."

He relates seven cases of obstinate herpetic eruptions, which had resisted several powerful remedies, but which were cured by the infusion or the distilled water of the rhus radicans. After ascertaining the good effects of this vegetable, in the cure of herpes, he met with a case of palsy, which was ascribed to the suppression of a herpetic eruption;* he exhibited the extract of the rhus radicans, and by this remedy, continued about two months, succeeded in curing his patient.

The successful issue of this case, suggested the employment of the extract of the rhus radicans in several other cases of palsy, four of which were cured, by this remedy. These cases are related in his treatise, with great accuracy and minuteness.

Du Fresnoy, from the fortunate event of the cases just mentioned, is led to consider the rhus radicans as a specific for those paralytic affections of the inferior extremities, which succeed convulsions. "Je ne crois pas devoir hésiter a présenter le rhus radicans, comme un spécifique pour la paraplexie, ou paralysie, des extrémités inférieures, lorsqu'elle est la suitte des mouvements convulsifs." He mentions in his treatise three species of this disease: palsies of the upper extremities; palsies in consequence of apoplexy, and palsies which follow convulsions. In the two former species he did not succed with this remedy.†

It is somewhat difficult to conceive, according to our present ideas of this disease, that in palsies which affect different parts of the body, the

* Soupçonnant donc que l'humeur Dartreuse pouvoit bien être la cause de sa paralysie, je crus devoir saisir cette occasion d'essayer l'extrait de rhus radicans.

† Il y a des paralytiques qui ont eu le courage de prendre de l'extrait de rhus radicans, jusq'à la dose d'une once, trois fois le jour, sans en ressentir le plus léger effet.

The usual dose of the extract, will appear from a case related in the appendix. It is generally requisite, in order to derive benefit from it, to exhibit it in large doses. From two grains to several drachms, three or four times a day, have frequently been given by Du Fresnoy.

same remedy should produce such different effects. It would have been desirable, if Du Fresnoy had related more definitely the symptoms and habits of those patients in whom the remedy failed. Does he consider paralytic affections of different parts of the body as essentially different, and depending on different causes? Many trials are yet necessary, before we can draw a certain conclusion concerning the use of his remedy in palsies. Much credit, however, is due to Dr. Du Fresnoy for this dissertation; he appears to be candid in the relation of his cases, his remarks are made with caution, and indicate great accuracy of observation.

A more effectual remedy than either of those recommended by the ingenious author of the preceding dissertation, for the eruptive disease produced by the rhus vernix, rhus radicans, &c. will be found in the use of blisters. While the cutaneous affection resulting from these poisons is yet circumscribed, its further progress may be effectually arrested by the application of one or more blistering plasters, of such dimensions as not only to cover the whole diseased surface, but to extend about an inch beyond its circumference. Those persons acquainted with the principles inculcated in the writings of the late celebrated John Hunter, will readily comprehend the modus operandi of the remedy here recommended. It induces in the skin a new disposition and state of action, which destroy the existing diseased ones, continue for a few days, and then terminate in health.

EDITOR.

APPENDIX.

A.

DESCRIPTION OF THE RHUS VERNIX OF JAPAN: EXTRACTED FROM KÆMPFER'S AMOENITATES EXOTICÆ, P. 791.

Sitz, vel *Sitzdsju*, i. e. *Sitz* planta, vulgo *Urùs* seu *Urus no ki*. Arbor vernicifera legitima, folio pinnato Juglandis, fructu racemoso ciceris facie.

Arbor paucis ramis brachiata, salicis ad altidudinem luxuriose exsurgit. Cortice donatur incano, ex verruculis scabro, facile abscedente; *ligno* salignio fragillimo; *medulla* copiosa, ligno adnata; *Surculis* longis crassis in extremitate inordinate foliosis. *Folium* est impariter pennatum, spithamale vel longius, Juglandis folio æmulum, costa tereti, leviter lanuginosa; quam a semipalmari nuditate stipant lobi sive folia simplicia, pediculo perbrevi nixa, tenuia, plana, ovata, trium vel quatuor unciarum longitudinis, basi inæqualiter rotunda, mucrone brevi angusto, margine integro, suprema facie obscure viridi, lævi, et ex nervis lacunosa, dorso incano et molliter lanuginoso. Nervus medius in mucronem terminans subinde multos a latere demittit nervos minores, citra marginem deficientes. Sapor folio sylvestris inest, cum sensibili calore; humor affrictus extemplo chartam ferrugineo colore imbuit. In surculis quibusdam ex foliorum axillis singuli surgunt *Racemi* laxe ramosi, palmares, tenues, qui, petiolis in calyculos rotundos desinentibus, *Flosculos* continent pumilos, et citra Coriandri seminis magnitudinem radiantes, in luteum herbaceos, pentapetalos, petalis carnosis nonnihil oblongis et repandis; staminibus ad petalorum interstitia singulis, apicatis, brevissimis, stylo perbrevi tricipite, floris turbini insidente. Odorem spirant dulcem, Aurantio flori affinem et pergratum. *Fructus* flosculum excipit gibbosus, utcumque in rhomboidis figuram compressus, bifidus, facie ac magnitudine ciceris, membranula tenui micante vestitus, per maturitatem durissimus et obsoleti coloris.

Cortex arboris cultro crenatus lacteum fundit lentorem, humore crystallino (ex aliis ductibus stillante) permixtum, qui ad aëris contactum nigrescit. Eundem surculi divulsi, foliorum pediculi, et nervi produnt, nullius gustabilis qualitatis participem, nisi calefacientis sine acredine.

Venenatos tamen spiritus hæc arbor exhalare dicitur, vehementes adeo, ut pueris circa eandem commorantibus exanthemata in corpore pariant: qualia etiam lignum tractantes alii (non omnes) experiuntur. Collectio *Urusj*, sive Vernicis, ut instituatur, caudices præcipue triennes, paucis crenis vulnerandæ sunt, ex quibus stillans liquor subinde excipitur, iterata in recente loco sectione, donec exsucci marcescant. Emulsi atque omni succo orbati, illico amputandi sunt; sic nova e radice provenit soboles, quæ triennis facta, collectioni denuo subjicitur.

B.

THE FOLLOWING INTERESTING CASE WAS OBLIGINGLY COMMUNICATED TO ME, BY DR. CHARLES CALDWELL.

PHILADELPHIA, APRIL 16, 1798.

DEAR SIR,

In the summer of ninety-seven, T. L. a boy of about twelve years old, and possessing a very high degree of what is denominated a melancholic temperament, was exposed to the action of the rhus radicans. I am unable to say whether or not he handled the leaves of the plant, or was only subject to the action of the effluvia which it emits.

As circumstances render it probable that our patient was exposed, for several days in succession to the influence of this poison, it is impossible to tell what length of time elapsed after he had actually received the infection, (for so I shall term it) previously to the commencement of the disease which it produced. The following is a brief sketch of the first appearance and subsequent progress of the symptoms of his illness.

The disease was ushered in by an attack on his hands and face, which were affected with an eruption accompanied by some degree of redness and swelling. A fever succeeded, attended by unusual drowsiness, some thirst, and considerable constipation of the bowels. The fever continued for several days, during which time the eruption spread gradually over his whole body, in a manner as well as I recollect, much resembling the progress of the inoculated small-pox. A swelling and very troublesome itchiness accompanied the eruption through the whole of its course.

The exact term during which our patient's febrile symptoms continued, I do not now recollect. In about ten or twelve days from the commencement of his attack, the eruption had, in a great measure, disappeared from his hands and face, and continued to die away on the other parts of his body in the same gradual manner in which it first made its appearance. So severe and painful was the affection on his lower extremities, that for several days he was unable either to sit, walk, or stand, without experiencing much distress.

During the course of his illness he was bled twice, kept cool and quiet, and took two or three doses of purgative medicines. Of the exact appearance of his blood I am not able to inform you, having had no opportunity of seeing it after it was drawn. By those who examined it, it was said to be *very bad*, from which I think it probable that it was marked with an appearance of size. This, however, I advance only as a matter of supposition. What, or whether any external applications were used, I do not now with certainty remember; for I was not the attending physician, but only an occasional visitant in the family.

From the foregoing statement it would appear, that the poison of the rhus radicans acts in a manner somewhat similar to the contagion of the small-pox when communicated to the system through the medium of inoculation. In the case of T. L. the hands and face having been exposed naked to the poisonous effluvia, may be considered as the immediate seat of inoculation. These accordingly became first disordered, by a topical eruption. In a short time a fever supervened, and the eruption extended to the other parts of the body, in a manner analogous to the inoculated small-pox.

If the foregoing communication can avail you any thing in your present interesting investigation, I beg you to consider it entirely at your command, and to give me credit for the sincerity with which I have the pleasure to be,

Your friend and very humble servant,

CHARLES CALDWELL.

C.

AN ACCOUNT OF THE CASE WHICH SUGGESTED TO DR. DU FRESNOY THE EXHIBITION OF THE RHUS RADICANS IN HERPETIC AFFECTIONS.

A YOUNG man, he first informs us, had voluntarily rubbed his hands with the leaves of the rhus radicans: the succeeding day he perceived an eruption on his hands, which he mistook for the itch. He then proceeds: "Le lendemain les mains et les poignets, dont le gonflement avoit augmenté la nuit, étoient couverts d'une grande quantité de petites vésicules, qui se remplirent en grossissant de plus en plus pendant sept ou huit jours, d'une férosité jaunâtre, qui annoncoit un érésipèle fâcheux. Malgré les saignées, les bains, les fomentations émollientes et les boissons délayantes, la tête s'enfla si fort, qu'il fut aveugle, par le gonflement prodigieux des paupières, pendant plus de vingt-quatre heures. Les démangeaisons se portèrent ensuite sur toutes les parties du corps, principalement les chevelues et celles de la génération qui'l se mit en pièces à force de se gratter. Au bout de dix jours, les accidens cessèrent, les poignets,

A DISSERTATION

ON THE

PROPERTIES AND EFFECTS

OF THE

DATURA STRAMONIUM,

OR

COMMON THORN-APPLE;

AND

ON ITS USE IN MEDICINE.

SUBMITTED TO THE EXAMINATION OF THE

REVEREND JOHN EWING, S. T. P. PROVOST;

THE TRUSTEES, AND MEDICAL FACULTY OF THE UNIVERSITY OF PENNSYLVANIA,

ON THE TWELFTH DAY OF MAY, A. D. ONE THOUSAND SEVEN HUNDRED AND NINETY-SEVEN.

FOR THE DEGREE OF DOCTOR OF MEDICINE.

BY SAMUEL COOPER;

MEMBER OF THE CHEMICAL AND MEDICAL SOCIETIES OF PHILADELPHIA.

PREFACE.

IN the following pages we have ventured to bring into view some experiments and observations, relative to the properties and effects of the Datura Stramonium, or common Thorn-apple. It was thought that, as this active plant has not received much attention from the medical experimenter, it was worthy of further investigation. It furnishes a subject extensive and important. He, who would do justice to such a subject, should possess talents, leisure, and industry. To a deficiency in the two former points, many of the imperfections of the present work may be justly ascribed. Instead of some months, or rather weeks, could a few years have been devoted to its execution, it might have been less imperfect, and consequently less unworthy of the acceptance of the reader. With great diffidence it is submitted to his inspection.

INAUGURAL DISSERTATION.

THE vegetable world is highly tributary to the existence and happiness of man. From hence he derives many articles of food, and many remedies for disease; and it is probable that his happiness will increase in proportion, as ingenuity or accident shall give rise to useful discoveries in this department of nature. Many unknown plants doubtless inhabit our globe, some of which may possess valuable alimentary properties; and others again, may be imbued with energies capable of obviating some of the present incurable diseases, which invade the animal system. These plants and their uses, will, sooner or later, be discovered. In the mean while, any effort to extend our knowledge of such, as are imperfectly known, may be attended with advantage. Influenced by this belief, we have been led to devote some time to the consideration of the Datura Stramonium. We shall premise some remarks on its natural history, and afterwards attempt an experimental investigation of its component parts, of its effects on the animal body, and of its use in medicine.

The genus Datura is arranged by the celebrated Linnæus, in the class Pentandria, and order Monogynia; and is thus described: "Datura. Cor. funnel-form. Cal. tubular; angled, deciduous. Caps. 4—valved."* This genus includes seven species; that of which we are about to treat, is the second, with "pericarps thorny erect egged, leaves egged smooth."* In respect to the leaves this description is erroneous, if we may be guided by observation, and by the authority of Dr. Haller and Dr. Woodville. The leaves are not egged; but the plant when it is very young, has for the most part egged, or ovate leaves; and Linnæus might have drawn its specific character from an imperfect specimen. According to Dr. Haller they are angled; and according to Dr. Woodville, they are pointed at the extremity, indented, and formed into several obtuse angles.

This species of thorn-apple very generally grows throughout the United States. Whether it be a native of these states, or an exotic, is a

* System of Vegetables of Linnæus, translated by a botanical society at Litchfield. London, 1783.

question, which has not yet been determined by botanists. It is an annual plant. It grows upon the borders of roads, around houses, in gardens, and upon vacant lots. It delights in a nitrous soil, and seems to love the abodes of man. It not only flourishes in the neighbourhood of Philadelphia, but by the sides of some of the streets, in some yards, and in great abundance, particularly in the vacant northern grounds of the city. Its general height is from two to four, but it sometimes reaches to ten feet. It springs from the earth about the latter end of May, from which time it continues to spring, till vegetation be checked by the cold of autumn. Its flowers are produced in the two last summer and the first autumnal months. They appear in succession, and may be seen on the extreme branches when the lower ones possess thorny seed-vessels. These observations, relative to the first appearance, duration, and flowering of the plant, will chiefly apply to the neighbourhood of Philadelphia.

In its progress to maturity the face of the stramonium considerably alters. The stalk at first acquires two opposite lanced, and afterwards several ovate leaves, arranged in alternate order. After some time these opposite and alternate leaves decay; the stalk divides into branches; these branches divide into others, and leaves of a different form are produced. The leaves are now irregularly set, and are angled and indented.* The flowers are most frequently white: they are placed on short peduncles at the junction of the branches and leaves. Some plants possess purple flowers, and purple stalks, while others again have these parts tinged of a blue colour. Hence it appears, that three varieties belong to the datura stramonium. The stamina are generally five, at times six, and at other times, one of them is cleft supporting two anthers. The thorny capsules are replete with kidney-form seeds, of a dark brown colour. A capsule of the common size contained seven hundred and fifty seeds. The root is white and fibrous.

Many caterpillars feed upon the leaves, and especially one, which is a species of sphinx, and is that which feeds upon the tobacco. A small beetle eats the leaves into numerous perforations. The leaves are sometimes infested by the puceron or louse of plants. Professor Barton has been well informed, that the goat eats both the leaf, and the seed-vessel. A similar occurrence fell beneath my own observation, about the commencement of last fall. A female goat daily devoured large quantities of the leaves, and at the same time gave milk, which was used by a man and his wife, and their two children. At this time three of the family were diseased; the man and his wife laboured under intermittents, and one of the children under an affection of the bowels. This milk was probably

* Dr. Woodville, in his elegant work, entitled Medical Botany, makes the leaves of this plant alternate. This I think is an inaccuracy.

imbued with noxious properties; and if it did not cause, might have increased, or disposed to, their complaints.

The odour of the leaves does not appear to be offensive to the bat, or the mouse, as I found by exposing them to it in vessels which had proper air-holes. Upon bringing a large quantity of the boughs of the plant into a room, a dog seemingly in health, ran to them, laid down among them, and slept without appearing to experience any inconvenience from their exhalation. Notwithstanding what has been said, the exhalation of this plant may be offensive to some insects, and to some animals. It is frequently offensive to man, as will appear in the subsequent pages.

EXPERIMENTS ON STRAMONIUM.

Experiment I. A pound and a half of the pounded leaves of stramonium, and a gallon of water were put into an alembic, to which a constant fire was applied. After some time a colourless fluid collected in the receiver; this was tasted and seemed slightly to possess the properties of the plant. Nothing resembling essential oil appeared on the surface of the distilled fluid. Upon continuing the distillation an empyreumatic liquid was obtained.

Experiment II. An ounce of the colourless fluid just mentioned was taken by a healthy person, and its effects were very inconsiderable; it seemed only to induce a slight nausea. This experiment was repeated with a similar result.

Experiment III. By subjecting the dried leaves in a stone retort to a violent heat, a dark oil of a disagreeable odour was obtained. This oil possibly arose from a decomposition of the resin of the leaves; the resin losing its oxygen, and hence becoming an oil.

Experiment IV. A quart of the expressed juice of the leaves was suffered to stand many days in an atmosphere moderately warm. A degree of fermentation seemed to take place in the liquid, and a white oleaginous matter gradually arose to its surface. My ingenious friend, Dr. Woodhouse informed me, that, he once procured a considerable quantity of a substance which resembled oil, by infusing this vegetable in water for some weeks.

Experiment V. A large handful of the leaves were burnt to ashes, upon which water was poured, and suffered to stand a considerable time. The water was then filtered and evaporated. By this process a small quantity of potash was procured.

Experiment VI. Some spirit of wine, and half an ounce of the powdered leaves of stramonium, were rubbed together in a marble mortar. The spirit assumed a deep green colour. After decanting this portion of spirit, other portions were added, as long as they appeared by their colour to extract any thing from the powder. The clear decanted liquid was evaporated, and afforded twelve grains of resin. I am induced to believe that four drachms of the above substance contain a larger portion than twelve grains. Does not a portion of the resin rise into the air with the alcohol? This I think was the case in one of my experiments. Highly rectified spirit of wine was poured, quantity after quantity, on an ounce of the powdered leaves; was frequently shaken with the material, and afterwards decanted. The spirit used in this way amounted to eight pounds. The whole of the decanted liquid, upon evaporation, only yielded eight grains of resin.

Experiment VII. The four drachms, which had been subjected to the action of alcohol, were triturated with repeated quantities of rain water, till the water ceased to be coloured. Upon filtering and evaporating these quantities of coloured water, one scruple of gummy matter was obtained, among which many very minute saline crystals were visible. The matter which the spirit and water did not dissolve was insipid, gray, and earthy. Being dried, it was found to weigh two drachms, eighteen grains.

Experiment VIII. A considerable quantity of the dried leaves were infused in snow water for two or three days. The infusion was exposed to an intense cold; the whole of it became a congealed mass, except a small quantity, which was dark, and which remained beneath the ice. This dark fluid crystallized upon removing it into another vessel. The crystals resembled nitre, in their form, and in their taste. The powdered leaves thrown upon live coals sparkled like small particles of nitre.

Experiment IX. The watery infusion of the leaves is yellow, bitter, and somewhat nauseous. The spiritous tincture of them is green, and astringent; and upon adding some of it to a solution of sulphate of iron, a brown thick fluid was formed.

Experiment X. I put a quantity of the pounded seed with some water into a vessel, which was exposed to a boiling heat. Globules of oil rose to the surface of the fluid, which resembled sweet-oil in colour and in taste. When the pounded seeds are wrapped in paper they stain it like oil; when they are thrown into the fire they flame; and when they are set in a dish, and exposed to heat, an exhalation arises similar to that which emanates from roasting coffee.

Experiment XI. Half an ounce of the sliced root was infused in a pint of water for three days. About four ounces of the strained infusion were swallowed. They produced some fever, which was accompanied by a slight intoxication, and head-ach. This infusion is not disagreeable to the taste.

Experiment XII. I placed a grain of the powdered leaves upon my tongue, and suffered it gradually to mix with the saliva of my mouth. A highly bitter and nauseous taste and an increased flow of saliva ensued. The disagreeable taste remained many minutes after ejecting the material from my mouth.

Experiment XIII. About two grains of the powder was snuffed up the nose; some irritation was immediately felt in the membrane; sneezing occurred, and the mucus of the part was increased in quantity.

Experiment XIV. Two scruples of the powder were put into a phial, and an ounce and a half of warm water were poured upon them. The phial was frequently shaken, and the contents of it in about half an hour were filtered. The filtrated liquor possessed a yellowish colour, and a taste more bitter than nauseous. Of this an ounce was injected into the urethra. A slight heat and pain were the consequence.

Experiment XV. A drop of the above filtrated liquor was let fall into the left eye. An uneasy sensation of the part was immediately experienced. In five minutes the uneasiness seemed to be somewhat increased; in ten it was less; in fifteen a slight pain occurred in the other eye. In half an hour the pupil of the eye on which the fluid was suffered to fall began to enlarge. The pupil previously to its dilation appeared to be more contracted than that of the right eye. It was largest about twelve hours after the experiment; at which time it was viewed in a considerable light, and seemed thrice as large as the other. It continued dilated during two days. In a strong light, objects were seen more distinctly with the right eye, but in a weak light, or in the dark, with the dilated one. This experiment was made upon myself. It was repeated on a boy about fourteen years old; in this case the liquid used, was the expressed juice of the leaves diluted with water. He sat in a pretty strong light, and in about ten minutes after the application, he was of opinion that the vision of the affected eye was increased in energy. The other phænomena were similar to those related in the former experiment. In this instance the dilatation of the pupil continued three days. But my friends Dr. Bache and Thomas Horsfield to whose eyes the infusion was applied, experienced afterwards no increase of sight in the dark. To experience this effect their pupils perhaps were not sufficiently dilated.

Dr. Rush, when treating of vision, informs his class, that a certain philosopher, who lectured in Philadelphia, was accustomed to give the following advice to his pupils. For some time previously to appearing in the company of ladies you should view a piece of black cloth, which will render your eyes soft and languishing. If any of his audience had been disposed to follow this advice, they might have accomplished the desired end, with greater ease, and effect, by letting fall into the eye a drop of the infusion of stramonium.

Experiment XVI. A drop of the expressed juice was let fall into the eye of a cat. An itching or pain of the part appeared to be induced;

the animal making great exertions with her feet as if she wished to rub off the material. The whole of the coloured part of the eye seemed in a short time to be converted into pupil. Inflammation and a puriform discharge, of many days continuance, succeeded.

Experiment XVII. I scraped away a portion of the cuticle from my wrist. Upon applying the fresh leaves pounded into a soft poultice to the part, it swelled and became so painful, that after some time I was induced to change the application for that of a mild cerate.

Experiment XVIII. A quantity of the pounded leaves was bound on the inner surface, and about midway of the thigh, and suffered to remain unremoved during twelve hours. No other effect than a moisture on the skin was apparently produced by the application.

Experiment XIX. About four o'clock in the afternoon six ounces of a warm decoction of the leaves were rubbed on each arm of a boy till the whole disappeared. In a very short time he was affected with febrile symptoms, his head becoming light, his skin warm and sweaty, and his mouth dry. He was troubled with a slight head-ach during the whole of the next day.

Experiment XX. A rat, so young that it was nearly devoid of hair, was treated in the following manner. A decoction of the leaves, the same in strength as in the former experiment, was liberally applied to the surface of its body. It was quickly roused out of its quiet state, and made violent, but irregular exertions to move away. These exertions seemed to be of the convulsive kind. They would suddenly cease, and the animal would fall on its side, and lie as if asleep; and their return would be equally sudden. This alternate activity and torpor of the animal frequently occurred, but gradually diminished, and entirely disappeared in a few hours. To another equally young, water similar in quantity and temperature to the decoction, was in like manner applied. The water excited it into considerable exertion, but its motions were more natural than those of the former.

Experiment XXI. At night I strewed fresh branches of the plant on my bed, on each side of the pillow, and about the room. I afterwards went to bed, slept, and awoke in the morning with a slight fever and head-ach.

Experiment XXII. To I. L. twenty-six years old, healthy, his pulse beating seventy-two strokes in a minute, I gave one grain of the powdered leaves of stramonium diffused in about an ounce of temperate water.

In	2	5	10	15	20	25	30	35	40	45	55	65 minutes,
The pulse beat	72	73	72	74	73	73	73	71	71	70	69	89 strokes.

In about ten minutes his pulse seemed to be somewhat increased in force. In twenty minutes he became dull; and in thirty began to doze. In thirty-four minutes the pulse was full and unequal, and in forty-seven somewhat tense, and less unequal. He continued to doze till about the fifty-fifth minute; at this time some tea was handed to him, which he drank, and which raised his pulse to eighty-nine quick strokes. This experiment was made in the evening; and next day he informed me, that he had experienced during the night much heat in his hands and feet.

EXPERIMENT XXIII. To P. M. healthy, thirty-two years old, his pulse soft and full, and beating seventy-four strokes in a minute, I gave two grains of the leaves diffused in water.

In	5	10	15	20	25	30	35	40	45	50	55	60	65	minutes,
The pulse beat	80	76	78	78	79	82	83	82	84	79	80	84	71	strokes.

When ten minutes had elapsed he found his head somewhat giddy: in twenty he was drowsy, and his pulse was quick. In thirty minutes his hands sweated: in forty his tongue acquired some whiteness, and the pupil of the eye was somewhat dilated. In forty-five minutes his pulse beat fuller and quicker strokes; his hands were warm, and had become more sweaty; his cheeks were redder, and his drowsiness had increased. In fifty minutes he complained of pain in his head, and his whole body was unusually warm: in sixty his eyes became misty; his stomach sick. Desirous of sleeping he laid his body in a horizontal position. In sixty-five minutes his pulse evidenced great tension and was still fuller. I was now obliged to leave him, but returned to him, about the eightieth minute, and found him asleep; his eye-lids were not completely closed; his face was red, and moist; he resembled a person labouring under the effects of too much spirituous potation.

EXPERIMENT XXIV. M. F. aged forty, healthy, but having an ulcer on the leg, and his pulse beating seventy-eight strokes in a minute, took in the evening two grains of the powdered leaves suspended in about an ounce of temperate water.

In	5	10	15	20	25	30	40	45	50	55	60	65	70	80 minutes,
Pulse beat	81	83	83	81	79	78	78	76	74	73	72	70	72	68 strokes.

In five minutes his pulse was quicker, and between the tenth and fifteenth minute, he was affected with a slight pain of the head. In sixteen minutes he experienced a heaviness in his eyes; in twenty his hands and feet were warm and sweaty, and his pulse was full and tense. In seventy, his face was flushed; and in eighty, nausea supervened. He now drank some cold water, which relieved the nausea and the other symptoms. On the succeeding morning he informed me, that he felt unusually cheerful, and that his appetite was increased.

EXPERIMENT XXV. P. M. on whom experiment twenty-third was instituted, his pulse beating eighty times in a minute, took four grains of the powdered leaves.

In	2	5	10	15	20	25	35	45	50	60	75 minutes,
Pulse beat	81	76	80	78	76	74	74	68	66	66	66 strokes.

In twelve minutes the pulse was more full and tense, and the heat of his face and hands was increased. In twenty-eight minutes his head became giddy, and his stomach sick. This sickness at stomach was of short duration. In about forty-eight minutes the fulness and tension of his pulse had rather increased. In about seventy minutes evident intoxication was induced, resembling that which arises from drinking alcohol. His cheeks and his eyes were reddened; his pupils were dilated; his voice faultered, and he was hardly capable of sitting erect. About the eightieth minute he took some tea, which obviated in some measure the preceding symptoms. In a short time his pulse was felt, and it beat eighty-four strokes in a minute. This experiment was made in the evening. During the night he experienced much head-ach and thirst. On the succeeding morning his tongue was covered with a white pellicle. He was troubled with head-ach and with pain in his limbs for many subsequent days. These latter symptoms regularly supervened upon the approach of evening, and continued till morning.

Experiment XXVI. I gave to I. C. about fifty years old, and labouring under mania, of diminished violence, five grains of the powdered leaves. Previously to exhibiting the medicine, his pulse beat eighty-three times in a minute.

In	5	10	15	20	26	30	40	45	50	55	70	80 minutes,
Pulse beat	75	73	74	63	61	60	64	66	70	77	75	80 strokes.

In about eight minutes some nausea occurred: his pulse was more tense. In twenty minutes he had a urinary discharge; in thirty his pulse had become full. The Peruvian bark, he said, that he had taken, had made him thirsty. He drank some water. In forty-two minutes the heat of his body was manifestly increased; and in fifty his cheeks were reddened. About the fifty-eighth minute he had an alvine, and a urinary discharge. In seventy minutes he was thirsty again, and drank; in ninety he had another evacuation of urine.

Experiment XXVII. I gave to M. F. on whom experiment twenty-four was made, six grains of the powdered leaves diffused in water. Previously to taking the medicine, his pulse was somewhat full and tense, and beat eighty strokes in a minute.

In	3	6	10	15	20	25	30	35	40	45	50	60	65	70	75	80	88	90	95	100 m.
p. b.	80	84	78	77	75	74	74	74	77	79	80	80	86	87	87	87	100	99	102	105 st.

In five minutes his pulse had undergone some increase in tension, and he thought that his eyes had become slightly dim. Before ten minutes had elapsed his hands were warmer, and he had experienced a slight pain in the back part of his head, which was succeeded by dulness of mind. In thirty minutes this dulness was diminished. About the thirty-sixth minute he took four grains more of the powder. On the forty-second minute his pulse had increased in tension, and in sixty, in quickness. In seventy minutes he was thirsty, dull and sleepy; in seventy-five he experienced some head-ach. About the eightieth minute he was bled to ten ounces. His pulse immediately afterwards was less tense and quick, and his head-ach and disposition to sleepiness, though not his thirst, were removed. In ninety-five minutes his body was warmer, and his thirst greater. I was now obliged to leave him. In three hours I saw him again. During my absence he had taken some drink, slept, and discharged a considerable quantity of urine. He had continued free from any pain in his head; the heat of his body had greatly diminished, and his pulse was full and soft, and only beat seventy-two strokes in a minute. It may be proper to observe, that in the beginning of this experiment my patient was sitting, and that when his pulse was last felt he was in a recumbent posture. The blood which was drawn evidenced a degree of siziness.

Experiment XXVIII. After bringing the heart of a frog into view, a few drops of the expressed juice of the leaves were applied to it. It ceased to move in a few minutes; and it could not be roused into fresh motion by pricking it with the point of a scalpel. The scalpel was applied afterwards to some of its muscles, and they contracted.

Experiment XXIX. Upon bringing into view the heart of a young dog, and the ventricles ceasing to beat, I injected into the vena cava some of the expressed juice. The motion of the heart was renewed; and the heart pulsated many times.

Experiment XXX. Upon bringing into view the heart of a frog, aboutthree drachms of the expressed juice were injected into the intestines. Previously to the injection, the heart beat nineteen strong strokes in a minute. In five minutes it beat nineteen quick and stronger strokes. In ten minutes it beat eighteen strokes; in fifteen, seventeen; in twenty, sixteen; in twenty-five, twelve; and in thirty, eight. After the twentieth minute its motions gradually lessened in strength till they entirely ceased. This frog lived about an hour and a half. Another one treated in the same manner, but without the injection, lived for several hours.

Experiment XXXI. After removing a portion of the cranium in such a manner as to bring the brain into view, some of the expressed juice was applied to the part. Convulsions were the immediate consequence, the frog dying in three minutes.

Experiment XXXII. About two drachms of the juice were injected into the cavity of the abdomen of a dog. Upon killing the animal about two hours afterward and inspecting the abdomen, a portion of the surface of the intestines, and a part of the omentum and peritoneum were preternaturally reddened.

Experiment XXXIII. About half an ounce of the expressed juice of the leaves, was injected into the jugular vein of a bitch. She made one violent struggle and died. She lived about half a minute after injecting the juice. Upon dissection, her blood was found fluid; and a white coagulum had extensively formed in the right auricle and ventricle of the heart. The brain was more watery than natural.

Experiment XXXIV. More than an ounce of the expressed juice was injected into the rectum of a small dog. In a few minutes the pulse became more frequent and quick. In about ten minutes he endeavoured to vomit, but his endeavours were ineffectual. His stomach was oppressed, and something regurgitated into his mouth which was swallowed as often as it regurgitated. In fifteen minutes he had several stools, which consisted of fæces and of the expressed juice. After these discharges, fresh efforts to vomit ensued, but they were as vain as the former. He tottered as he walked. In half an hour he became dull and slept. During his sleep his whole frame was affected with frequent tremulous motions or startings. He did not sleep long, made again efforts to vomit, attempted to walk, but was unable, and laid down. He now fell into a sleep of greater composure and of longer continuance. On the ensuing day he was languid, but able to walk; took some nourishment, and gradually recovered.

Experiment XXXV. To a bitch whose pulse was at one hundred and eleven in a minute, I gave about an ounce and a half of a spirituous tincture of the leaves. The pulse gradually increased in quickness and frequency; and in thirty minutes it beat one hundred and forty strokes. In twenty-three minutes derangement of mind came on; the animal running in circles into the fire, against the furniture of the room, tumbling on her side, rolling on her back, and agitating her feet in the air. She was now bled from one of the jugular veins. Her blood, as it flowed, was florid; upon standing some time the crassamentum floated in the serum, was somewhat loose in its texture, and numerous red particles were deposited at the bottom of the bowl. Upon killing and dissecting the animal, neither her flesh, nor her secretions appeared to be imbued with the odour of the tincture of the leaves. In this experiment I was induced to pay attention to the odour of the flesh and secretions, as I had once made a similar experiment with the tincure of opium, and found that the whole animal upon dissection seemed to be impregnated with its smell. The portion of the tincture of opium used, was about three ounces.

Experiment XXXVI. I poured an ounce and a half of the expressed juice down the throat of the dog, used in the last experiment except one. His heart afterward beat slow and strong strokes. His respiration

OBSERVATIONS ON THE PRECEDING EXPERIMENTS.

It appears that the leaves of stramonium possess no essential oil, as they afford none when distilled with water. The distilled water, taken into the stomach, produces little or no effect. (Exper. 1, 2.)

The oily matter, which rises to the surface of the expressed juice, cannot with strict propriety be called an essential oil, as it does not rise till a degree of fermentation occurs in the juice. This oily matter probably originates from a decomposition of the resin of the fluid.

The dry leaves, when exposed in a retort to a violent heat, yield empyreumatic oil, and when burnt yield potash; products which are afforded by numerous vegetable substances. (Exper. 3, 5.)

From four drachms of the powdered leaves, which had been exposed to repeated quantities of alcohol, twelve grains of resin were obtained. The same leaves subjected to a pure water yielded one scruple of gummy matter, among which many very minute crystals existed. Had these crystals been separated from the gum, they would perhaps have amounted to three or four grains. (Exper. 6, 7.) Three grains of the resin, given to a healthy person, produced much exhilaration of mind, fever, and head-ach, and several alvine discharges. Three grains of the gummy matter given to a healthy person, produced a slight exhiliration of mind, fever and head-ach, and a copious flow of urine. Might not this gummy matter be exhibited with effect in some dropsies? The gray, earthy, insipid substance, which neither the alcohol, nor the water dissolved, weighed two drachms and eighteen grains.

Hence it seems that the constituent parts of the powdered leaves of stramonium are a resin, a gum, an essential salt, and an earthy matter. The resin appears to be the most active part. The gum possesses a bitter, the resin, an astringent principle. (Exper. 9.)

With regard to the other parts of the plant, the honey of the flowers, the seed, and the root, my experiments are extremely deficient. However, I may observe that the honey is bitter, the seed and the root sweet, and that they all in a greater or less degree possess the properties of the leaves. An ingenious botanist, William Bartram, informs me, that he eats this honey with pleasure and avidity, and without experiencing any thing morbid from its use. But my fellow student, Joseph Johnson, assures me, that he once was attacked with head-ach, and with sickness at stomach, after sucking many of the flowers; and professor Barton supposes that this is one of the plants from which the bee, in this country, sometimes extracts a pernicious honey. The sweet taste of the seed has betrayed many a child into great danger. The seed and the root are pulverized with much difficulty. When it is necessary to resort to them, it will perhaps be best to roast, grind, and make the former into coffee; and best to slice, and boil the latter for a decoction. In the following experiments, and cases, I have used the leaves in preference to the other parts of the plant. They may be gathered, dried, and powdered with ease. They may be plucked during the flowering state of the branches, put in an airy and shaded apartment, and pulverized as soon as they become dry. The use of a fine seive will be proper to obtain the powder free from the fibres of the leaves. It may now be kept in close vessels; and if good, it will possess a beautiful green colour.

When this powder is applied to the tender membrane of the nose it produces sneezing, and an increased flow of mucus. When taken into the mouth, it stimulates the salivary glands into great action. (Exper. 12, 13.) It disagreeably affects the senses of taste and smelling; and if the leaves be chewed for some time they induce a degree of intoxication. In these circumstances stramonium resembles nicotiana, or tobacco. And they are similar in other respects; for the same insect, in the caterpillar, and in the fly state, and the same animals feed alike on both plants. Should man not alter the current of his present propensities; should he continue to indulge a relish for unnatural luxuries, and delight in being the ingenious tormentor of himself, it may be predicted that stramonium, like tobacco and alcohol, will become the bane of society. But a hope is entertained that future generations, taught by the experience and misfortunes of their ancestors, will relinquish the general use, and prevent the introduction of such injurious articles.

A drop of an infusion of the powder applied to the eye, dilates the pupil. (Exper. 15, 16.) It is difficult to account for this curious effect produced on the eye. Upon the application of the infusion does an unusual action occur in the external coat of that organ, which is communicated by sympathy to the retina; an action or affection, which to a certain degree deprives the retina of the power of receiving the impressions of objects? Or does the diffusive vapour of stramonium, like heat or electricity, penetrate to the retina, and excite in it a peculiar disease? That fluids or vapours are capable of thus acting, is rendered somewhat probable by the following experiment. About three drachms of the powdered leaves were mixed with a pint of tepid water, and poured into a sound bladder; the neck of the bladder was then tightly tied; and as soon as the bladder became well moist, the odour of the stramonium was perceptible on its surface. And the great Dr. Darwin, in the second volume of his work entitled, Zoonomia, says, "There appear to be three different modes by which extraneous bodies may be introduced into the system besides that of absorption. 1st. By etherial transition, as heat and electricity: 2d. By chemical attraction, as oxygen; and 3d. By *expansive vapour*, as ether, and essential oils." That the infusion applied to the eye is absorbed, and produces its effect through the medium of the circulating fluids, is an opinion, which is highly improbable. If the infusion acted through this medium, both eyes would be alike affected. The following instance of the action of stramonium on the eye is a curious one, and it favours what we said in a former experiment. "The late Dr. Bond had under his care a patient, a young girl, who had put the seeds of this plant into her eye, which dilated the pupil to such a degree, that she could see in the dark, but in the light was almost blind."* Since this vegetable is capable of acting with such energy on the eye, we may expect, that it will sooner or later be applied with success, in some diseases of that exquisite organ.

If the leaves be pounded into a soft poultice, and applied to a part from which the cuticle is removed, they excite heat and pain; if applied to a part, which is shielded by the cuticle, no effect seems to be produced. (Exper. 18.) But if a decoction of the leaves be rubbed for some time on the surface of the body, febrile symptoms will be excited. (Exper. 19.) The decoction externally applied to a very young rat, seemed to induce convulsions. (Exper. 20.)

The exhalation of stramonium excites head-ach and fever in the human system. (Exper. 21.) This exhalation, I endeavoured to collect, by putting many branches of the plant under a large glass vessel full of water, and afterwards exposing it to the sun. But in this I did not succeed; the water imbibed the exhalation. That the vapour emanating from this plant is capable of producing febrile action, is an opinion, which is supported by many facts. The following particulars countenance this opinion. "Below the falls of Ohio, (says a very intelligent gentleman†) I lay some days *in camp*, with general Putnam, and several others. Here the earth was extensively covered with stramonium in full blossom, whose strong odour produced a pain in my head, which continued after leaving that place and till we arrived in the Wabash river. I was now seized with a fever, which had nearly proved fatal, and which was succeeded by a giddiness of the head of many weeks continuance. General Putnam was also attacked with this fever; he recovered, though his recovery was not expected. Although my illness and general Putnam's might have arisen from breathing the fogs of the river, yet as I had been accustomed to these fogs before, without being affected by them, I have ever been of opinion, that my fever originated from the scent of these plants." And professor Barton has been informed, that just before the peace of 1783, it was supposed by the physicians of New-York, that the great abundance of stramonium in the vicinity of that city had produced fevers; and that on this account the plant was cut down. For a similar reason, according to the

* Appendix to Notes on Virginia, written by Thomas Jefferson.

† John Heckewelder, of Bethlehem, Pennsylvania, in a letter.

same excellent professor, a law has been lately passed at St. Vincennes, on the river Wabash, for destroying this plant. The inhabitants of the place assert, that previously to its introduction* among them, they knew not what a remitting fever was. It may be said, that the preceding opinions relative to the production of fever by its exhalation, are founded on deception; and that fever in these instances might have arisen from the common cause, vegetable and animal putrefaction. It cannot perhaps be declared, that no putrid animals and vegetables existed at Fort Vincennes, and the other places, where the vapour of the thorn-apple seemed to produce such morbid effects. Vegetable and animal putrefaction might have existed in these places, and have co-operated with the scent of the plant in producing disease. As alcohol gives origin to a febrile disposition in the system, in like manner, the scent of these plants might have disposed to the production of fever. But their vapour or scent, I should imagine, would be highly equivalent, without the aid of any other cause, to give rise to fevers of a remitting type; especially when we recollect that the leaf can excite head-ach, convulsion, and mania. Alcohol and opium alone have excited the most violent diseases. I saw, I think, towards the close of last autumn, a case of true yellow fever which was induced in a person somewhat depressed in his mind, who for several days had taken nothing except repeated quantities of wine and brandy. And it is well known, that a kind of apoplexy originates from an excessive dose of opium. Thus we see that many morbid stimuli act with equal violence,

* Colonel Winthrop Sargent informed Dr. Barton, that the datura stramonium has been known to exist on the grounds near the river Wabash, above twelve or fourteen years. To these parts it was probably conveyed by accident or curiosity. Some persons much admire its flowers. In a letter already quoted, that I received from J. Heckewelder, of Bethlehem, there is the following information worthy of note. He says, "I met with this plant about the year 1769, for the first time, on the road from this place (Bethlehem) to Philadelphia. Its flower pleased me so well, that after its seeds were ripe, I took some of them home with me, and planted them on a farm near Nazareth. I found the plant at some places on the river Ohio, particularly at Fort Washington. This plant does not grow about Bethlehem, and does not appear in several parts of Pennsylvania." Professor Kalm, who travelled about fifty years ago through this country, from Wilmington, in Delaware state, to Quebec, in Canada, only speaks of the datura stramonium, as growing between Wilmington and Philadelphia. From these and other circumstances it is doubtful, whether the thorn-apple is a native of all of the United States. Perhaps it is not a native of any one of them.

In a former part of this work I might have observed, that the plant is frequently called *James-Town*, or *Jameson weed*, because a number of soldiers were once violently diseased by ignorantly eating the boiled plant at *James-Town*, in Virginia. In like manner I might have observed, that it is called French chestnut in New-Jersey.

and in a way somewhat similar on the human system; hence may we not be led to conjecture that their constituent principles may be the same, at least in some degree, though differently combined?

Might not this active vegetable exhalation* be breathed with advantage in some weak habits, and in some cases in which the intestines and stomach are not able to retain medicine? Would it not make a good addition to the pneumatic materia medica, a science which has been cultivated with considerable success by Dr. Beddoes? Thus, former physicians were in the habit of placing the poppy in the chambers of persons labouring under certain diseases; a practice which might perhaps be revived, and extended with advantage to many plants.

One grain of the powdered leaves taken in a small quantity of temperate water, in the space of fifteen minutes, increased the frequency and force of the pulse; and finally produced thirst and sleepiness. (Exper. 22.)

In five minutes two grains of the same medicine increased the frequency of the pulse. In a longer space of time, they rendered it full and quick, as well as more frequent; and produced the following symptoms: giddiness, warm skin, moist hands and face, intoxication, sleepiness. (Exper. 23.)

The same quantity of medicine, given to an older person than either of the two former, rendered the pulse full and tense; produced cheerfulness, and increased the appetite for food. (Exper. 24.)

The medicine given in larger doses produced fulness, quickness, and tension, rather than frequency in the pulse; produced intoxication, difficulty of speech, and great thirst; dilated the pupils of the eyes; rendered the blood sizy and the stomach sick; opened the bowels; increased the flow of urine, and gave origin to febrile symptoms of some days continuance. (Exper. 25, 26, 27.)

From the preceding symptoms produced by stramonium, we may infer that this substance is a stimulant. The experiments on frogs and dogs tend to confirm this opinion. They moreover serve to point out its higher degrees of action.

Like other stimulants, stramonium exhausts the irritability of the animal body. The expressed juice of the leaves applied to the heart of a dog destroyed its motion in a few minutes. (Exper. 28.) But the same fluid injected into the vena cava of a dog, after the heart had ceased to beat, stimulated it into fresh motion. (Exper. 29.)

* The other species of datura probably exhale a strong odour. In a garden, which Dr. Smith visited on the continent, he says, that the datura arborea, covered with its magnificent and fragrant flowers, was at this time the finest thing, and had crowds of visitors every evening. Few people could support its perfume any length of time. Sketch of a Tour on the Continent, in the years 1786 and 1787, vol i. p. 118.

ing on its local application to the stomach or intestines. We cannot with propriety attribute such affections to its absorption, and consequent diffusion through the body. Some absorption may take place; yet that they are produced in this way, is opposed by their sudden existence upon the application of the article, and their sudden disappearance upon its removal. Stramonium taken into the stomach, in a very few minutes affects the head and the arteries. In the first volume of the American Philosophical Transactions, Dr. Rush has related the case of a child who was much affected in consequence of swallowing the seeds. In this case the symptoms were fever, tremors, blindness, and an eruption on the skin, which immediately disappeared upon the final evacuation of the seeds from the stomach and intestines, by the repeated use of vomits and purges.

The intoxication or mania, which stramonium induces, seems to be analogous to that which arises from the action of opium, alcohol and some other stimulants. Like alcohol it sometimes induces mania of many days continuance. In Beverley's History of Virginia, a remarkable account is related of its effects on several persons, who gathered, boiled, and ate it for greens. In each of these persons it seems, that a mania came on, which lasted about eleven days.* According to some it has induced mania which has continued during life. Do not such facts throw a blaze of light on the nature of the mind? Should we not be induced to attempt the discovery of other articles which affect the mind? May not articles exist which are capable of affecting all its different faculties? May not posterity gain a dominion over it, nearly as complete as that, which we possess over the soil of our gardens?

In two persons† who used the boiled leaves for vegetable food in New-Jersey, violent diseases supervened; mania in one, and tetanus in the other. Two children who had eaten of the seeds, seemed to labour under hydrophobia, according to Dr. Lobstein.‡ They rejected every kind of

* The James-Town weed being an early plant was gathered very young for a boiled salad, by some of the soldiers sent thither; and some of them ate plentifully of it, the effect of which was a very pleasant comedy; for they turned natural fools upon it for several days: one would blow up a feather in the air; another would dart straws at it with much fury; and another stark naked was sitting up in a corner, like a monkey, grinning and making mouths at them; a fourth would fondly kiss and paw his companions, and sneer in their faces, with a countenance more antic than any in a *Dutch* droll. In this frantic condition they were confined, lest they should in their folly destroy themselves; though it was obseved, that all their actions were full of innocence and good nature. Indeed they were not very cleanly; for they would have wallowed in their own excrements, if they had not been prevented. A thousand such simple tricks they played, and after eleven days returned to themselves again, not rembering any thing that had passed. Beverley's History of the present state of Virginia. Book ii. p. 24. London, 1705.

† Dr. Barton's Lect. on the Materia Medica. ‡ Med. Trans. v. 5. p. 83.

hyoscyamus niger: and Dr. Rush has lately applied copious bleedings with success in the disease which arises from swallowing opium. But the following case in the best manner answers my purpose. In a child that had taken into her stomach some of the seeds of stramonium last fall, blood-letting and purges were used with the best effects. The blood which was drawn was cupped. This patient I saw. She was attended by my ingenious friend, Dr. Caldwell. In slight cases, or after the use of more powerful remedies the vegetable acid may prove serviceable. In affections produced by the plant, it has been used with advantage by the late Dr. Bond and others.

But a plant diffusing such a poisonous exhalation should not be suffered to grow upon farms, near roads and houses, or upon vacant grounds in cities. The rich soil, which it infests and impoverishes, should be surrendered to more salutary vegetables. Its abode should be confined to the remotest corners of private medical or botanical gardens. Few or no accidents would then arise to children from swallowing its seeds; foreigners or ignorant persons would not be tempted to use it for food; its morbid exhalation would seldom or never be breathed; and when recourse was had to its aid, as a remedy, it would more readily affect the human system.

From what has been advanced relative to the effects of stramonium on the animal body, we may fairly conclude that it is a stimulant. In this opinion we are not alone. It is held and taught by two of the professors in the university of Pennsylvania; and in the second volume of the Zoonomia, stramonium is enumerated among the inciting articles of the materia medica.

ON THE USE OF STRAMONIUM IN MEDICINE.

To that acute physician Dr. Storck, are we indebted for introducing the datura stramonium in the cure of diseases. He exhibited an extract of the leaves of this plant with advantage, in some cases of mania, in epilepsy, and some other convulsive affections. In similar cases this remedy was afterwards used by Drs. Wedenberg and Odhelius with considerable success. Dr. Greding was then induced to try the stramonium. He gave it in a great number of epileptic cases, and in cases of epilepsy joined with mania, and only found the remedy to be effectual in a single instance. And the late very respectable Dr. Cullen thinks, that it is seldom suited to the cure of such diseases. Yet, as if unwilling to decide on a subject concerning which such different opinions were entertained, he says, "Nevertheless, I have no doubt that narcotics may be a remedy in certain cases of mania and epilepsy; but I have not, and I doubt if any other person has learned to distinguish the cases to which such remedies are properly adapted."

Thus it seems that Drs. Storck, Wedenberg, and Odhelius, gave the medicine with success, while Dr. Greding found it to be nearly useless. The former physicians might have given it in proper cases and doses, while the latter one in these respects might have erred. On the occasion Dr. Cullen deduces nothing from his own experience. He considers stramonium as a narcotic, and therefore a sedative. But the preceding experiments and observations I hope will evince, that it produces stimulating effects on the animal body. The following cases may in some measure point out the conditions of the system in which it may prove useful.

The preparation of the plant which has been chiefly given in these cases is the powder of the leaves. The dose is from half a grain to four grains, mixed with conserve of roses or water, twice or thrice in the day.

IN EPILEPSY.

Dr. Rush has greatly simplified and elucidated the subject of nosology, by considering pleurisy, phrenitis, mania, apoplexy, and numerous other diseases, as febrile states of the system. To the number of these febrile states of the system, I think epilepsy might be added; for it is induced by the same causes which induce other fevers. These causes are alcohol, terror, obstinate costiveness, a cessation of certain discharges, as in the cases of piles, ulcers, &c. It appears to be a fever, because during the existence of a paroxysm the pulse is uncommonly tense and full, the face red, and the pupils of the eyes dilated; symptoms which frequently occur in fevers, Moreover in the intervals between the fits the patient is subject to vertigo, and his tongue is for the most part white. Like other fevers, epilepsy generally attacks the young, and the plethoric, and occurs in the night, and sometimes changes into mania. In a woman, who, during the existence of a paroxysm died of epilepsy in the Pennsylvania hospital, in the spring of 1796, the arteries of the brain seemed to be enlarged to twice their usual size. Other dissections have discovered water, and hydatides inthe brain; effects which probably resulted from morbid action in the arteries of that viscus.* Dr. Rush has effectually cured it once or twice by mercury; but, has not, I believe, succeeded always upon using

* This disease has been cured or suspended by the small-pox, by pulmonic and by intermitting fever. I was informed by a black man, whom I could believe, that he had been once very subject to fits, which according to his description must have been of the epileptic kind, and that his fits had never returned upon his violently having the natural small-pox. There is at this time a patient in the Pennsylvania hospital, who has been long subject to epileptic fits, yet upon his being lately attacked with pulmonic fever, they have entirely ceased to occur. A boy who was much harrassed by fits remained free from them for several months; but during this time he laboured under an intermitting fever, as will hereafter appear. Hence, is it not probable that epilepsy is a fever, as it seems at least sometimes to change into other fevers.

this remedy. In epilepsy, Dr. Darwin has exhibited opium with great success. I suspect that stramonium is a remedy which is equal, if not superior to either of these, in the present disease. It is a less disagreeable remedy to the patient than mercury, as it does not confine him to his bed, by inducing a painful affection; and it seems to answer all the purposes of opium, without constipating the bowels. It was used with considerable success in the case of a boy about twelve years old, in the Pennsylvania hospital, in part of the autumn and winter of 1796. I will briefly relate his case. If the contagion of the yellow fever did not occasion his epilepsy, the cause of it is unknown. I am informed by his friends, that he had the first epileptic fit during the prevalence of the yellow fever in Philadelphia in 1793; that he had another in about two months afterwards; and that the fits in 1794, and in the beginning of 1795 occurred somewhat more frequently, till he at length had one about every two weeks. But I am told that in the summer of 1795 he was sent into the country, where he was attacked by an intermitting fever, under which he laboured for four or five months. During this period, it seems that his fits entirely left him; but that they returned upon the disappearance of the fever. In the winter of 1795, and in the spring and summer of 1796, he took the cuprum ammoniacum and some other remedies, which seemed in some measure to lengthen the intervals between the fits. But not being durably relieved by these remedies, they were discontinued; and he was admitted a patient into the Pennsylvania hospital in the autumn of last year. He now had three or four fits every week. Dr. Shippen who was at that time the attending physician, prescribed for him Fowler's mineral solution, to be given in small quantities. As the fits became more frequent upon using the solution, it was discontinued. Half a grain of the powdered leaves of stramonium was now prescribed; it was to be taken in the morning and evening, and the dose was gradually increased to three grains twice in the day. Upon taking this last medicine, his fits soon began to diminish in force and frequency; and at length he appeared to be cured of them. It is proper to observe that he was bled and purged before taking the stramonium, and was bled again a week or two afterwards; and that at a time when he seemed to be threatened with a paroxysm, Dr. Rush, who succeeded Dr. Shippen in the hospital, had him bled with obvious advantages. Would not the application of pressure to the carotid artery just before its commencement, prevent the fit? This boy was discharged from the hospital as cured, but I am sorry to add that after remaining about five months free from the fits, he has had a return of them About the beginning of this spring after getting his feet wet by long exposure on a rainy day, and coming in the evening into a close warm room, and having to do some work which required stooping, he was seized with a fit. He has had several of them within the two last months; but he has lately been bled and purged, and resumed the pills with much advantage. The last fit and the preceding one have been less violent, and more protracted in their access. By bleeding and stramonium a perfect cure may yet be effected. If they should fail, the age of puberty may make a favourable change in his system.

I gave two grains of the powdered leaves every evening to a woman about thirty years old, having imperfect speech, and a kind of catalepsy, which returned every night, and lasted about an hour. These affections, I believe, arose from frequent spirituous intoxication. Upon taking the medicine about three weeks, she appeared to be cured of the catalepsy, and relieved of the imperfect speech. Might not stramonium be taken as a substitute for alcohol, in persons who wish to relinquish the use, or who suffer great inconvenience from the want of this noxious article?

Two grains of the powdered leaves taken evening and morning, seemed to be of much service to a woman, who had long been subject to tremors of the limbs, and occasional epileptic fits; affections which seemed chiefly to arise from a contortion and ulcer in the foot.

Our remedy was given in some cases of epilepsy in the alms-house of this city with a success not to be regretted. Dr. Hart, who attended to its effects, informed me, that it performed a cure in one of them; and that it relieved the others.

The following valuable observations on the effects of stramonium in epilepsy, I lately received in an obliging letter from Dr. John Archer, of Harford county, Maryland. This letter contains other observations respecting the use of stramonium, which I shall detail in their proper places. "I have administered the stramonium to several, with various effects; to some with great advantage, and to others without any other advantage than prolonging the time of the return of the fit, or only lessening its duration. I observed that those who had regular returns of the fits, or had them at short intervals, received the greatest benefit; because they could take a sufficient quantity of the medicine before the time of a return: by this means the return of the fit was prevented: then attending to the time another fit should return, the medicine was again given, and in general a cure obtained; but those who had irregular returns of the fits, unless of short duration, could not take the stramonium so as to act at the time of the fit, as it came on unexpectedly; these were not much benefited.

"In the epilepsy with regular periods of return, I order two grains of the powdered seeds made into a pill, to be given every four hours, until some sensible effects are produced; and then lengthen the time to six or eight hours, so as to keep up some sensible operation of the medicine, until the time of the fit. The medicine is now to be omitted till three or four days before the next return. It is then to be exhibited as before. To use the stramonium with effect in these cases, and with any benefit in the preceding ones, low diet and occasional blood-letting should be enjoined.

"I need scarcely remark to you that those patients, who have irregular returns of the fits with long intervals, must take the stramonium constantly, else they will miss the proper time of taking it. In such cases, though it may not prevent the return of the fits, it may mitigate their violence. I have also remarked, that it is administered with little or no advantage to those patients who are become simple or foolish by the violence or long continuance of the disease.

"The seeds should be gathered when fully ripe, before the frost and the rains injure them. The best seeds for use are of a blackish colour. Take a scruple of the powdered seeds, half a scruple of flour, and a sufficient quantity of water; mix them well, and divide them into ten pills. These are to be given as already directed.

"I do believe the seeds of stramonium to be a valuable medicine in many diseases; I have tried them in several others with great advantage, as mania, retention of urine, &c. but they must be given with regularity and attention. In my opinion in a regular epilepsy, it is as powerful as the Peruvian bark in intermittents."

In the cure of epilepsy how does stramonium act? Does it act by giving origin to a new disease, or by wasting that general or partial accumulation of excitability or sensorial power, which favours the production of a paroxysm?

IN MANIA.

This disease, which Dr. Rush has proven to be a fever, may in some of its forms be obviated or relieved by stramonium. In J. B. this remedy appeared to effect a cure. Being deranged in his mind, and somewhat outrageous, he was admitted into the hospital during Dr. Parke's attendance last summer. He was bled and purged several times; in consequence of this treatment he became somewhat more rational. After these remedies two grains of the powdered leaves of stramonium were prescribed; these were to be taken every evening and morning. The medicine occasioned whiteness of the tongue, fever, and a slight head-ach. He took the stramonium about two weeks, rapidly grew better in his reason, and was discharged from the hospital as cured. Dr. Rush considers the occurrence of head-ach in maniacal cases as a favourable symptom, and I have seen it frequently occur in his patients at the hospital as soon as their derangement had much diminished or ceased. Might not this salutary symptom in mania be accelerated, or induced in many cases of this disease by the use of stramonium?

M. R. wild and melancholic, having been deranged for some months, took one grain of the powdered leaves of stramonium morning and evening. Previously to taking the medicine he was bled and purged several times. The medicine seemed to make little or no impression on him, but upon increasing it gradually to four grains, he became affected with fever and pains in his limbs. He now became rational; but successive suppurations occurred in the parts where the pains existed. Openings were occasionally made into the collections of pus; and at this time he appears to be on the recovery, and continues to be rational.

To M. F. melancholic, a female patient, one grain of an extract of the leaves was given night and morning, which quantity was gradually increased to three grains. She was delighted with the medicine, seemed to get better, and would anxiously call for the pills when they were out. By some means she at length discovered that they were made of what she called Jameson weed, and refused to take any more of them. Upon taking the medicine in increased doses, she informed me that she frequently saw small dark spots in the air; and that sparkles of light sometimes seemed to proceed from her eyes. Dr. Archer within these few days informed Dr. Rush, that in a certain case in which he gave stramonium double vision was produced.

For J. B. labouring under melancholy madness, Dr. Wistar last summer prescribed stramonium in doses of half a grain, to be taken twice in the day; but the medicine affected her stomach with so much sickness that it was discontinued.

It was given to several persons in the cells of the hospital who had been deranged for many years; some of these it seemed to relieve by exciting violent diarrhœa; others again it seemed to injure. In these latter instances some advantages might perhaps have been derived from it, if its use had been premised by bleeding and purging.

IN TETANIC FEVER.

Dr. Archer's letter informs me that he once moderated the symptoms of tetanus by giving stramonium; and that Dr. Simmes, formerly his student, now living in Georgia, had succeeded in the cure of a tetanus by using this medicine. Dr. Archer adds, that he attributes his own late ill success in some cases of tetanus, wherein he administered stramonium, to his not having preceded the remedy by evacuants.

IN HYDROPIC AND PARALYTIC FEVERS.

In one hydropic, and two paralytic cases, in which our remedy was tried, it seemed to prove injurious.

IN FEVER WITH SWEATS.

To a person having nocturnal fever, attended with sweats, two grains of the powder were given several hours before going to bed, for about a week; but it was necessary to discontinue the remedy, as the fever and sweats were increased. He was afterwards cured by purges and elixir of vitriol.

IN INTERMITTING FEVER.

In a case of intermitting fever, in which two grains of the powdered leaves were given about two hours before its expected attack, the paroxysm was prevented.

IN AFFECTIONS OF THE EYES.

In a patient labouring under gutta serena, where the pupil had not lost the whole of its irritability, the seeds taken twice in a day, first half a grain of them, and afterwards three grains, seemed to produce some increase of vision; but on account of the medicine disagreeing with his stomach he relinquished its use. In this case the local application of an infusion of stramonium, might perhaps have answered a good purpose. Two old women with weak eyes seemed to derive some benefit from applying to the eye the juice of the leaves diluted with water. The strength of the preparation was a drop of the juice to an ounce of water. A weak infusion of the dry leaves would perhaps be equally serviceable as a preparation of the diluted juice.

IN RHEUMATIC FEVER.

Dr. Rush prescribed last winter, a spirituous tincture of the leaves for a pain in the knee, in the hospital. It seemed to relieve the affection by causing eruptions on the skin. An ointment made by boiling the green leaves of the plant in hog's lard, I once saw prove very serviceable in violent rheumatic pains. Dr. Bache informed me that an old lady in the country, near the city, applies this ointment with great success, in cases of club-feet. I lately saw it of great service in one of these cases. It is said that this ointment is very useful in burns and in piles.

I must now conclude these pages. Time will not permit me to dwell any longer on this interesting subject. The preceding experiments and observations have been accomplished at the expense of some ease, and some health. But if they should in any way extend our knowledge of a powerful vegetable substance; if they should collect into a point some of its different effects on the animal system; and, if they should have the least tendency to obviate any portion of human misery, my ends will be answered, and I shall conceive that my labours have been rewarded with an ample recompence.

AN INAUGURAL DISSERTATION

ON

DIGITALIS PURPUREA,

OR

FOX-GLOVE;

AND ITS USE IN SOME DISEASES.

SUBMITTED TO THE EXAMINATION OF THE

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THE TRUSTEES, AND MEDICAL FACULTY OF THE UNIVERSITY OF PENNSYLVANIA,

ON THE THIRTY-FIRST OF MAY, ONE THOUSAND EIGHT HUNDRED.

FOR THE DEGREE OF DOCTOR OF MEDICINE.

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INTRODUCTION.

AS Digitalis has excited a great deal of attention within a few years, and as it has been used in two very different diseases, viz. dropsy and pulmonary consumption, with remarkable success, I have made it the subject of my Inaugural Dissertation.

I am, however, fully sensible of the truth of the following observation made by Dr. Withering: "I wish it was easy to write on Digitalis. I despair of pleasing myself or instructing others, on a subject so difficult. It is much easier to write upon a disease than upon a remedy. The former is in the hands of nature, and a faithful observer, with an eye of tolerable judgment, cannot fail to delineate a likeness. The latter will ever be subject to the whims, the inaccuracies, and the blunders of mankind."

As there is very little pretension to originality in any part of the following essay; as the parts which are extracted from authors, can be easily distinguished by every medical reader, and as I here make particular acknowledgments to those authors, I hope to be excused for omitting minute references to them.

Most of the experiments which are related, were repeated several times, but as their results were similar, or nearly so, to those mentioned, and as I wished to be as concise as possible, I have not detailed them.

A DISSERTATION ON DIGITALIS.

CHAPTER I.

A SHORT DESCRIPTION OF THE PLANT.

THIS plant is the Digitalis Purpurea* of Linnæus. It is very common in England, and is found also on different parts of the continent of Europe. It may be cultivated to advantage in the United States. It belongs to the second order of the fourteenth class, or the Didynamia Angiospermia of Linnæus. The essential characters of the genus are, Cup with five divisions. Blossom bell-shaped, bulging. Capsule egg-shaped, two celled. Chives. Threads crooked, white. Tips yellow. Pointal. Seed-bud greenish. Honey-cup at its base more yellow. Summit cloven.

S. Vess. Capsule not quite so long as the cup.

Root. Knotty and fibrous.

Leaves. Slightly but irregularly serrated, wrinkled; dark green above, paler underneath. They resemble in some degree, the leaves of mullein,† and it has been gathered for digitalis. Lower leaves egg-shaped; upper leaves spear-shaped. Leaf-stalks fleshy, bordered.

Flowers. Numerous, mostly growing from one side of the stem, and hanging down one over another.

Floral leaves. Sitting taper pointed. The numerous purple blossoms hanging down, mottled within; as wide and nearly half as long as the finger of a common sized glove, are sufficient marks whereby the most ignorant may distinguish this from every other plant.

It grows in dry, gravelly or sandy soils, particularly on sloping ground. It is a biennial, and flowers from the middle of June to the end of July.

No cattle are observed to eat it. The root, the stem, the leaves, and the flowers have a bitter herbaceous taste.

* The trivial name, *purpurea,* is not a very happy one, for the blossoms though generally purple are sometimes of a pure white; I shall therefore, hereafter call it simply digitalis.

† Verbascum of Linnæus.

This plant ranks among the Luridæ, one of the Linnæan orders in a natural system. It has for congenera, nicotiana, hyoscyamus, datura, solanum, &c.

Fuchsius, who wrote in the year 1542, is the first author who notices it. From him it received its name of Digitalis, in allusion to the German name of Finger-hut, which signifies a finger-stall, from the blossoms resembling the finger of a glove.

Sensible qualities. Leaves bitter and nauseous.

Sensible effects. In large doses it produces sickness, nausea, vomiting, vertigo, confused vision, increased secretion of urine, with a desire to discharge it, and a tendency to salivation.

Every part of the digitalis possesses similar properties, but the leaves, according to Dr. Withering, are much the most efficacious part; and I believe that inattention to his directions, is a principal source of failure among physicians.

As Dr. Withering's treatise may not be in the hands of every one who may read this dissertation I shall give his method of preparing it, in his own words.

"The leaves should be gathered when the plant is flowering. The leaf-stalk and mid-rib of the leaves should be rejected, and the remaining part should be dried, either in the sun-shine, or on a tin pan or pewter dish before the fire.

"If well dried, they readily rub down to a beautiful green powder, which weighs something less than one-fifth of the original weight of the leaves. Care must be taken that the leaves be not scorched in drying, and they should not be dried more than what is requisite to allow of their being readily reduced to powder."

As he mentions a heat barely sufficient to make them suitable for powdering, and as it is certain that its active quality may be greatly dissipated by being long dried before the fire, we have great reason to believe that the method of curing it, may be very often a cause of its failure, since we see how much care is necessary.

CHAPTER II.

OF THE PRIMARY EFFECTS OF DIGITALIS ON THE HUMAN BODY.

Many of the authors who have written on this subject, have taken notice of its remarkable effect on the arterial system; but their observations have been principally confined to its power of diminishing the frequency of the pulse. In order to ascertain if possible the principles by which its effects were to be explained, I instituted the following experiments with the dried leaves, picked in the manner recommended by Dr. Withering.

As its effects can be more clearly ascertained in a state of health, than when complicated with the symptoms of disease; and as its operation in morbid affections will be more properly considered after its mode of action has undergone examination, I shall here confine myself to an enumeration of its effects on the body in a state of health, and particularly of its primary effects on the arterial system.

Experiment I. Having for two days previously had some pain in my head, at two o'clock, P. M. half an hour after eating a light dinner, I took one grain and a half of digitalis made into pills with the mucilage of gum. Arab. my pulse beat seventy-two strokes in a minute, and the following changes were observed in it; it was reckoned immediately before, at, and after the periods mentioned in the first line of the following table:

In	5	10	15	20	25	30	35	40	45	50	55	60	70	75	80	85	90	105	120 minutes,
Pulse beat	72	76	76	78	78	76	78	74	72	72	70	68	66	64	60	60	58	60	60 strokes.

Twenty minutes after taking the medicine, I felt an increase of the pain of my head, accompanied with pain in my eyes; in forty-five minutes, some increase of fulness in my pulse was very perceptible. Sixty minutes having elapsed, my pulse lost the increase of fulness, and my stomach was slightly affected with nausea. In one hundred minutes I felt but very little pain in my head. During the remainder of the afternoon, I had several times a little nausea, which however soon went off, and in the evening the pain of my head returned as before.

Experiment II. At ten minutes before twelve o'clock, A. M. I took three grains of digitalis;* my pulse beat sixty strokes in the minute, its natural standard. In twenty minutes it was increased six strokes; in thirty-five minutes it was smaller, and it continued between sixty-six and sixty, for seventy-five minutes, when it remained at sixty-two. I had during the experiment several times a slight nausea.

Experiment III. At ten o'clock, P. M. three hours after drinking a little coffee, and eating some bread and butter, I took three grains of digitalis; my pulse beat sixty strokes in a minute.

In	5	10	15	20	25	30	35	40	45	50	55	60	65	80 minutes,
Pulse beat	60	60	60	56	55	54	58	58	56	56	58	60	60	50 strokes.

In twenty-five minutes had considerable nausea, and my pulse was diminished in force; in sixty minutes nausea continued, and my pulse was a little fuller; in two hours the nausea was so great that it was with

* I may remark here, that in all my experiments, I took and gave the medicine in the form of pills, made with the mucilage of gum. Arab.

difficulty I could avoid vomiting, and my pulse did not appear (for it was not counted) to be more than forty strokes in a minute.

EXPERIMENT IV. At twenty minutes before seven o'clock, A. M. my worthy friend and colleague, Dr. Enoch Wilson, took (fasting) three grains of digitalis; he had some pain in his head, and his pulse beat sixty-four strokes in a minute.

In	5	10	15	20	30	35	40	45	50	55	64	75	85	90	95	100	105	110	115	120	125	135	m.
p.b.	66	66	68	70	72	72	72	70	70	68	68	68	66	62	62	62	62	62	60	60	58	58	st.

In fifteen minutes he had a sense of heat at his stomach; in twenty-five minutes his pulse was evidently fuller, and the pain of his head somewhat increased; in sixty minutes had some uneasiness at his stomach, with an increased flow of saliva, which continued as long as the disorder of the stomach; in sixty-five minutes his pulse was diminished in force; in eighty-four minutes had some nausea which continued for one hundred and thirty minutes, when his pulse was much smaller. He had some nausea several times during the remainder of the day, and three copious evacuations from his bowels, but had some doubt about ascribing this effect to the medicine, because he had caught a cold.

EXPERIMENT V. To George Tavener, aged forty, a robust healthy man, who had been accustomed to drink freely of ardent spirits, I gave four grains of digitalis, ten minutes before seven o'clock, A. M. and before he had eaten any breakfast or drank any liquor: his pulse beat sixty-eight strokes in a minute.

In	5	10	15	20	25	30	35	40	45	50	60	70	80	90	100	minutes,
Pulse beat	68	68	72	72	72	72	70	70	72	72	72	70	68	68	68	strokes.

In twenty minutes he complained of vertigo; in forty minutes had some nausea; in sixty minutes his pulse was weaker than before the experiment; in eighty minutes he was very sleepy, which might in part have been owing to his sitting still, as he is generally very active. During the remainder of the day he had three copious discharges from his bowels. He observed to me, that his sensations during the day were very similar to those he experienced after drinking freely of ardent spirits. I am induced to believe that these sensations were produced by the digitalis, from his making the observation without being interrogated; nor was any idea of such effect hinted to him previously: they were not occasioned by spirits, because I cautioned him against drinking any during that day, and I could depend upon his veracity.

EXPERIMENT VI. Twenty minutes after six o'clock, A. M. I gave to Thomas Maborough, a black man aged forty-eight, six grains of digitalis; his pulse beat ninety strokes in a minute, its natural standard.

In	5	10	12	20	25	30	35	50	55	60	70	75	80	85	95	105	110	120	130	140	minutes,
P.b.	90	90	90	84	86	84	84	82	84	80	80	79	79	78	78	80	79	70	70	70	strokes.

In fifty-five minutes after taking the medicine, he was so sleepy that it was with difficulty he could keep awake; in sixty minutes he had some nausea; in eighty minutes the nausea was increased, accompanied with pain in his stomach; in one hundred and twenty minutes the sickness abated and the pain descended from his stomach to the intestines; in two hours and a half he was perfectly well, and the digitalis did not increase any of the excretions.

I gave digitalis to the same man several times, in smaller doses, with exactly the same effects, only in a more moderate degree; it did not in any instance raise his pulse above the natural standard.

Experiment VII. Fifteen minutes before six o'clock, A. M. Dr. Enoch Wilson took three grains of digitalis, before he breakfasted. His pulse beat sixty strokes in a minute, its natural standard.

In	5	10	15	20	25	30	35	40	45	50	55	60	66	76	90	100	112	120	130	minutes,
P. beat	60	60	62	64	66	68	70	70	71	72	68	68	68	68	68	64	62	60	60	strokes.

In twenty minutes he felt some uneasiness at his stomach; in twenty-eight minutes his pulse was increased in force, and he had a sense of heat at his stomach, and vertigo, with a confusion of mind; in thirty-five minutes he had a sense of fulness in his head, and the vertigo continued; in seventy minutes his pulse was considerably weaker. Between sixty and one hundred minutes, he had an unusual degree of languor, with vertigo; also, great thirst, which was very uncommon for him at that time in the morning. During all the experiments, and particularly those made on Dr. Wilson, great care was taken to avoid every circumstance that could have any influence on the circulation, and therefore motion was especially avoided.

Experiment VIII. I took three grains and a half of digitalis, early in the morning, which raised my pulse eight strokes in fifteen minutes; and in fifty-eight minutes it was reduced ten strokes: its effects on my stomach were similar to the other experiments; my pulse in fifty minutes was fuller and stronger, and I had some pain in my head, which, however, soon went off.

I took several doses of digitalis and gave a considerable quantity to other persons, who were in good health, with results so similar to those above related, that I do not think it necessary to repeat them.

I might here insert four experiments, which were communicated to me by my friend, Mr. Joseph Trent,* of Virginia, and which were con-

* Mr. Trent had chosen digitalis for the subject of his Inaugural Dissertation, but when he heard that I had also chosen it, he very politely favoured me with his experiments, for which I wish him to accept of my acknowledgments.

ducted with great accuracy; but as their events are very analogous to those I have made, it will not, I imagine, be considered as essential to detail them minutely; I shall therefore, refer to them presently, as if related.

The opinions of physicians, relative to the operation of digitalis, were unanimous in favour of its sedative power, without regard to its primary effect; and it was considered as a sedative medicine, from the time of its first introduction into the materia medica, until the winter 1798-9, when Dr. Barton taught, in his lectures, that notwithstanding all that had been said in proof of its lowering or diminishing the frequency of the pulse, he was induced to believe that it was a stimulant. My experiments, or at least, the first, second, fourth, fifth, seventh and eighth, lead to a very similar conclusion, and seem sufficient to establish the opinion above stated. If we mean by a stimulant, a medicine which will always increase the force and frequency of the pulse, digitalis may not be entitled to that appellation; but, if I mistake not, there are medicines denominated stimulants, or incitants, which will not, on every constitution and habit produce the same effect of accelerating the pulse. I am inclined to believe, that opium is one of these medicines, whose primary effect in particular constitutions, will be to depress instead of raising the pulse; and this opinion is very much strengthened by recollecting the notion which was so long maintained of its sedative property.

From attending particularly to the experiments of Dr. Crumpe, I do not find the stimulating property of opium (as to the heart and arteries) so great, as some authors assert it to be; we find, for example, from his sixth experiment, that his pulse was only increased six strokes; in the seventh, the young man's pulse was increased six strokes; in the eighth, his own pulse was increased eight strokes, and in the seventeenth, ten strokes; which was the greatest height it arrived at.

If we attend to the fourth and seventh experiments which I made with digitalis, we may observe, that in the former, my colleague's pulse was raised eight strokes in half an hour, and in the latter, twelve strokes in fifty minutes, from the time of taking the medicine. In the first and second experiments, my pulse was increased six strokes, and in the eighth, eight strokes. In the fifth experiment, George Tavener's pulse was raised four strokes.

The experiments of Mr. Trent, tend likewise to corroborate the idea of its stimulating power, for in the three first, his pulse was increased in frequency, and in the fourth, it was diminished in frequency, but increased in force.

In the other experiments, it is true, that it did not raise the pulse, but in most of them an evident fulness was perceptible, during its primary operation on the sanguiferous system; and a very eminent author does not ascribe to opium any other effect on the arterial system, than that of lessening the frequency, and increasing the fulness of the pulse; and yet this same author classes it among the stimulating medicines.

There is little doubt, that a very considerable similitude exists between the operation of opium and digitalis; perhaps, as much as there is between conium maculatum, nicotiana, hyoscyamus, datura, solanum and opium. Let us take a short view of their similar effects on the living system. Opium increases the frequency and fulness of the pulse; digitalis very frequently does the same. Opium produces sometimes an increased flow of saliva; so will digitalis. Opium produces profuse perspiration; one of the patients in the alms-house, who took digitalis, had for the first three or four days, more copious perspiration than before. Opium is sometimes a diuretic; this is one of the most prominent effects of digitalis. Who has not seen opium in some instances prove laxative? Digitalis purged in several of the experiments, when I gave it to the healthy subject Opium in a large dose, produces vertigo, confused vision,* thirst and stertorous respiration; the same effects follow large doses of digitalis.

Analogical reasoning is, perhaps, on no subject less tenable than on medicine; when the suppossed certainty or doubt of any one point, may prove destructive to many patients. If however, we were permitted to make use of it, we should derive great support from comparing our medicine with those which are allied to it by botanical affinity, and which are acknowledged to be stimulants: they were mentioned in the description of the plant.

From saying that in many respects the operations of opium and digitalis are similar, let no person suppose, that I mean to attempt to explain their operations on the nervous system. The former of these has employed the ingenuity of the most learned physicians for generations long since past; and notwithstanding all that has been done on this very interesting subject, much yet remains to do. Perhaps we may look forward for a satisfactory explanation of all the peculiarities in the operation of opium, to generations or probably ages yet to come.

With regard to the operation of digitalis, I am alike unqualified to offer any explanation. Some of the preceding experiments, and subsequent cases, show very clearly, the wonderful power it possesses in certain circumstances, of lessening the frequency of the pulse: the manner in which it does this must remain a secret, until physiologists acquire more certain ideas of the laws, functions, connexions and dependencies of the nervous system, and its influence over the arterial.

* Jesse Scott, in the alms-house, complained very much of his sight being impeded during part of the time that he took digitalis.

CHAPTER III.

ON THE USE OF DIGITALIS IN DISEASES.

If we attend to the history of digitalis, since it has been considered as a medicine, we shall find that from the sixteenth century to nearly the present period, it furnished a subject for applause to one practitioner, and for unqualified condemnation to another. It is somewhat extraordinary, that its diuretic property should have been so long overlooked; for no writer has noticed it, I believe, previous to the year 1770. Anterior to that time, they confined themselves principally to its use in epilepsy and scrophula; and in the latter of these it has been particularly famous.

Haller mentions several hereditary instances of this disease, said to have been cured by it: six or seven spoonfuls of the decoction produce nausea, vomiting, and purging; not without some marks of a deleterious quality. The same author likewise mentions its use in gout and rickets, in the form of ointment and in powder.*

Ray and Boerhaave, mention its operation as generally deleterious. Dr. Withering, I believe, was the first physician who treated dropsies successfully with digitalis; and his practice is sufficiently confirmed by every practitioner who has used the medicine in the manner he directs; particularly in that species of dropsy called anasarca.

Among other diseases, digitalis has been recommended in phthisis pulmonalis, or consumption of the lungs. Dr. Salmon, who wrote at the commencement of the present century, speaks of it as a specific in phthisis; and Dr. Withering has given us a manuscript note of a Mr. Saunders, found in Parkinson's Herbal, which mentions consumptions as infallibly cured by a decoction of fox-glove leaves in water, or wine and water, taken for constant drink. The doctor's opinion of it, notwithstanding his expressed wish that it may be farther tried in this disease, was not, it is true, very encouraging; but it should be recollected, that even with him it succeeded in one case, (No. cxx.) that it relieved another, (No. xl.) very far advanced, and that the remaining cases in which it was given by him were lost before recourse was had to digitalis.

Dr. Darwin and Sir George Baker directed their attention particularly to its use in pulmonary consumption, but they were somewhat disappointed in their expectations.

Within a few years our medicine has excited the notice of Dr. Beddoes, who tried it without much benefit; but two of his correspondents, Drs. Fowler and Drake have been much more successful with it. The latter

* Haller's Hist. Stirp. Indigen. Helvet.

of these gentlemen, (Dr. Drake) has communicated two cases to Dr. Beddoes, which the doctor has published in a work entitled, "Contributions to physical and medical knowledge." As these cases are accurately described, and appear to have been genuine phthisis, and as the books which contain them are very scarce, I shall take the liberty of giving as brief a detail of them as possible.

The first case which Dr. Drake relates was of a Mr. James Marris, aged sixteen, who complained of considerable difficulty of breathing upon motion, and of pain in the right side: he had a frequent short cough, attended with a copious expectoration of what appeared, and upon trial, with the vitriolic acid and caustic alkali, proved to be in great proportion pus; it was dense, fœtid, and occasionally mixed with blood: pulse one hundred and twenty, and the morning and evening exacerbations, especially the latter, strongly marked; great emaciation and prostration of strength; tongue clean; thirst not considerable; appetite not impaired; belly regular; sleep interrupted, and he could not lie with ease on his left side: slight perspirations towards morning; skin hot and dry; shivering fits every two or three days; urine high coloured. Had a hereditary predisposition to phthisis from his relations. He was evidently likewise of the form and habit which physiologists consider as predisposing to tubercular consumption.

June 26th. Pulse still one hundred and twenty, with great debility, and the symptoms increased. He took the following prescription.

℞ Foliorum digitalis purpureæ in pulverem crassum trit: unc: 1. spiritus vini rectificati et aquæ puræ a a unc. ii. Digere leni calore, sæpe agitans per horas xxiv, et cola.

℞ Kali p. p. scrup: i.
Succi limonis unc: ss.
Aquæ puræ drachm: vi.
Tinct: digitalis purpureæ gtt. xv.
Fiat haustus primo mane et horis duabus ante prandium sumendus.

Ordered a little wine and animal food. He continued this medicine gradually increasing the tincture of digitalis until the 5th of July, when his pulse was reduced to seventy-six; and all his symptoms mitigated.

℞ Infusi cinchonæ unc: 1. ss.
Acidi vitriolici diluti gtt. x.
Tinct: digitalis purpur: gtt. xxxv.
Fiat haustus mane et meridie sumendus.

July 17th. He took fifty drops of the tincture twice a day, with more wine and animal food: his pulse was reduced to forty-four; expectoration was very much diminished, and he was in every respect much better.

July 22d. Pulse forty, intermitted after every third stroke; doses of the tincture diminished to forty drops twice a day. His pulse continued at forty and from that to fifty until the 15th of August, when it had not been beyond fifty for better than four weeks. From that time he took the infusion of Peruvian bark with quassia, elix. vitriol. and the tinct. of digitalis: his pulse gradually returned to its natural standard, every symptom disappeared, and he was on the 29th of August in all respects perfectly well.

The second case. September 10th, 1797. Mr. George Grimes, aged nineteen, complained of very acute pain in his right side, which was increased by expectoration; he had incessant cough, and great difficulty of breathing, accompanied with frequent expectoration, which was evidently purulent, and very fœtid. Pulse one hundred and twenty, and hard; complexion very florid; tongue foul; thirst great; appetite much impaired; body regular; urine high coloured and depositing a copious sediment; little or no rest; frequent shivering fits, and his health was rapidly declining. He had been liable for many months to slight pulmonary complaints, as cough, hectic flushings, and occasional expectoration. His father, mother and sister all died of phthisis pulmonalis.

℞ Kali p. p. scrup: i.
Succi limonis q. s.
Lactis amygdal: unc: i.
Tinct: digitalis purpur: gtt. xx.
Fiat haustus mane et meridie sumendus.

℞ Mucilaginis sem. cydanii mali et aquæ cinnam: unc: iii m. et sumat cochleare largum urgente tussi.

On the 18th of September, his pulse was reduced to fifty, and he was in every respect much better. Ordered a decoction of Peruvian bark and diluted vitriolic acid, with the tincture of digitalis. On the 19th, pulse forty-eight; 29th, pulse forty-four. He relapsed on the 9th of October, and his pulse rose to one hundred and eight; but by the 19th, he was again restored to health, and his pulse fifty: he was allowed animal food, wine and porter, and continued in good health.

In the first of the above cases, not any sickness was induced during the whole course of the cure. In the second case, a sickness and vomiting came on when the tincture was increased to forty-eight drops twice a day; but these symptoms readily disappeared when the medicine was omitted for a short time. In Mr. Marris's case, one day when his pulse was below fifty, from a wish to ascertain the result, the two doses of the tincture were omitted, and his pulse next morning beat one hundred and twelve; whereas in Mr. Grimes's case, though the digitalis was entirely omitted on the 24th of September, in consequence of the nausea, and on the 2 of October, the pulse had not risen beyond forty-eight.

in the minute, and never appearing to beat at the same rate during one minute."

In the Philadelphia alms-house, where a number of patients are admitted annually, with phthisis pulmonalis, and consequently a considerable number fall victims to that unconquerable disease, a medicine which has been used successfully even in a few instances, demands particular attention; accordingly, the digitalis was exhibited in several cases of phthisis, since last February. I shall relate a few of these cases, but do not think it necessary to detail all the symptoms of each case: 1st, because they would occupy more space than the limits of this dissertation would admit of; and 2dly, because the physicians of the house saw them, and had no hesitation in pronouncing them confirmed phthisis.

CASE I.

ELEANOR WELLS, aged forty, was admitted into the alms-house, as a patient, on the 15th of February, 1800. The history which she gave of her case was the following: at the commencement of the winter season she was attacked with pleurisy, for which she was bled twice, and took some medicine which relieved her considerably, but not entirely; owing as she supposed, to her being obliged to spend a great part of her time in a damp cellar. She, however, so far recovered as to be able to attend to the duties of a small family, but was harrassed constantly by a cough and pain in her breast. In this state she remained, till about eight weeks previous to her admission, when all the symptoms were greatly increased. When Dr. Wilson saw her, she complained of a fixed pain immediately under the sternum; had a very troublesome cough, accompanied with a copious expectoration, of a substance which appeared to the eye to consist of pus; great heat of the palms of the hands and soles of the feet, together with profuse night sweats; flushed cheeks in the afternoon; chills and fever, which however, did not observe the stated periods, that a true hectic commonly does. When Dr. Church visited her, which was soon after her admission, from the above and other circumstances relative to her case, he believed that she laboured under genuine phthisis, and ordered her to take a grain of the powdered leaves of digitalis three times a day, and a blister to be applied to the breast. After the use of these remedies for a few days, she was much relieved; the medicine was therefore persisted in, and the dose gradually augmented to two grains three times a day, beyond which it was never carried, for the sickness induced by it was so distressing as to forbid more. The medicine was continued till the latter end of March, when she no longer complained of the pain of the breast; her cough and night sweats were gone, and in short, every unfavourable symptom had disappeared, and she only remained debilitated, for which she took a decoction of Peruvian bark;

from a quart of fluid, evidently purulent, down to half a pint in twenty-four hours.

April 16th. Pulse seventy-eight, soft and regular; he rested well last night; less perspiration; cough better. Ordered an infusion of Peruvian bark with the elixir of vitriol, and one grain of digitalis, three times a day. He continued the medicine with an abatement of all his pulmonary complaints, till the 21st, when his pulse was but fifty-four, full and irregular; continues the infusion of bark, &c.

April 22d. Pulse forty-eight, irregular with an intermission after every third stroke; continues the medicine. Ordered a glass of sherry wine three times a day; in the evening of the same day, his pulse rose to one hundred;* has taken two doses of the infusion, says he is better.

April 24th. Pulse fifty-two; had a violent pain in his face and jaw, which was induced by taking cold.

April 25th. Pulse forty-eight, irregular; continues the wine and digitalis; in every respect much better; appetite good; no cough; expectoration diminished to a gill in twenty-four hours.

April 26th. Pulse seventy-two, small; the pain in his face and jaw has returned. Ordered a blister, which very much relieved him; continues the digitalis.

May 3d. Pulse sixty-six, soft and regular; has not had any pain in his breast for more than three weeks; no burning in the palms of his hands; cough entirely gone; expectoration amounts to three table-spoonfuls in twenty-four hours. He was this day discharged from the alms-house to go into the country: he took a quantity of digitalis in order that he might omit it gradually. I have not heard from him since he was discharged, but think it very probable, if he can use moderate exercise, his pulmonary disease will be entirely cured.

CASE III.

Jesse Scott, aged forty-three, was admitted into the alms-house on the 7th of April, 1800. About two months before, he was attacked with pneumonia, for which he had no medical assistance, and took nothing but two small phials of medicine; he did not know what they contained. Since that time he has had a very distressing cough, which is now accompanied with a very copious expectoration, amounting to three pints of fluid, mixed with pus, in twenty-four hours; occasional flushings and chills; burning in his hands and feet; night sweats; pain under the right scapula; very much emaciated; his belly regular; tolerable appetite.

April 9th. His pulse ninety-two; ordered the following medicine:

* I examined the pulse morning and evening, of the cases wherein I mention its effects on the pulse, but cannot be so minute as to detail them all.

℞ Sal sod: scrup: i.
Tinct: digitalis saturat: gtt. xv.
Aquæ cinnam: unc: ss.
Fiat haustus mane et meridie sumendus.

April 13th. Pulse eighty, hard and quick; very profuse perspirations at night; expectoration still very copious; body regular; he is able to walk across the ward, but with great difficulty. Ordered to continue the draught, increasing the tincture of digitalis five drops every dose.

April 16th. Took one hundred drops of the tincture yesterday, at two doses, pulse eighty; passed a restless night; complains of griping; expectoration and cough better. Ordered to take fifty drops of tincture twice a day, without any of the soda.

April 17th. Pulse eighty; griping more severe accompanied with some diarrhœa; cough better; expectoration greatly lessened. He took a dose of the oleum ricini, which relieved the griping, and he took likewise a dose of the tincture at noon.

April 18th. Pulse eighty; complains of great pain in his belly, with nausea and diarrhœa. Ordered to take one grain of opium immediately, which relieved him.

April 19th. Ordered to take forty drops of the tincture twice a day, and increase it as before.

April 20th. Pulse eighty; has taken fifty-five drops of the tincture at a dose; pain under the scapula better; very profuse perspirations at night; cough still better; appetite good.

April 21st. Pulse fifty-one, full and irregular with a pause after every sixth stroke. Other symptoms as yesterday.

April 22d. Morning, pulse eighty; rested well last night; perspirations very much diminished; takes sixty drops twice a day; complains of vertigo and drowsiness after taking the medicine. Allowed a little animal food.

April 23d. Pulse fifty-four; has considerable nausea; complains of his vision being obstructed, and says he has had more or less of it for three days past; it comes on at ten o'clock in the morning, and goes off between three and four in the afternoon.

April 30th. Pulse sixty; takes fifty drops of the tincture twice a day.

May 11th. Pulse varies from forty-six to seventy-eight; has no obstruction in vision; expectoration diminished to about a gill in twenty-four hours; no night sweats; very little cough; in short, he is so much better, and his strength has so much increased, that there is good reason to expect he will be perfectly restored to health.

The above cases shew very decidedly the wonderful dominion which digitalis possesses over the heart and arteries; and also that phthisis pulmonalis, even when confirmed, is not as it has been generally considered, necessarily an incurable disease.

Digitalis was exhibited in several other cases of phthisis with manifest advantage; its good effects were especially remarkable in relieving the cough, and diminishing the expectoration; these cases, however, have all relapsed, and will probably fall victims to the disease, except one, who was lately discharged very much relieved; but as he used other remedies, we could not ascribe the whole of the benefit he experienced to digitalis alone.

I am very far from being of opinion that digitalis ought to be considered as infallible, in cases of pulmonary consumption; but that it may be regarded as a very proper remedy in some cases, and is worthy of attention in every one, few I think will deny.

AN INAUGURAL DISSERTATION

ON THE

UNITY OF DISEASE,

AS OPPOSED TO

NOSOLOGY.

SUBMITTED TO THE EXAMINATION OF THE

REVEREND JOHN EWING, S. T. P. PROVOST;

THE TRUSTEES, AND MEDICAL FACULTY OF THE UNIVERSITY OF PENNSYLVANIA.

ON THE THIRTY-FIRST OF MAY, ONE THOUSAND EIGHT HUNDRED.

FOR THE DEGREE OF DOCTOR OF MEDICINE.

BY ALEXANDER MAY, OF PENNSYLVANIA.

"E PLURIBUS UNUM."

INAUGURAL DISSERTATION.

THE unity and simplicity of nature, in all her operations, is obvious to every judicious observer.

The nearer we approach to truth in exploring the phænomena of the universe, the greater identity and uniformity we discover; and the more complex our notions, the more the mind becomes enveloped in error. The frugality of nature is conspicuous in all her operations; where one agent is sufficient to affect her purpose, she never uses more.

The motions of the heavenly bodies, the change of seasons, the succession of day and night, the tides, winds, and the different phænomena of motion, were most absurdly explained, till one single power was discovered to be the sole governing principle of them all.

The science of chemistry affords many proofs of this law of unity and simplicity existing throughout the works of nature. A few elementary substances are found to be the basis of all the various forms of matter which compose the universe.

All life, animal and vegetable, however variously modified, is the effect of one agent, viz. *stimulus*. Health and disease, are effects of the same agent, differing in degree. The simplicity of nature's operations often prevents their being seen. Men in their investigations extend their views beyond the simplicity of truth, and consequently become lost in error. The circulation of the blood was long a mystery; but its simplicity appears to have been the only cause of its obscurity: and the physiology of the nervous system, the *arcanum* of the present day, probably lies concealed under the same simplicity.

Some have become infidels in religion, from no other cause, than that truth was too simple to be believed. The same cause makes men infidels in the science of medicine, by leading them beyond the simplicity of nature for the cause of disease.

I. Disease is simply, *morbid excitement*, or *wrong action;* or, in other words, it consists in a morbid state of the system, in which *some* of its functions are carried on in an uneasy and irregular manner.

This morbid excitement, whether it exist in the form of *convulsive action*, *suffocated action*, *spasm*, *itching*, or *heat*, is the same thing, and all these are effects of different degrees of force in the remote, or exciting causes, or of the difference of organization in the affected parts; they all, moreover, occur at different times, under different circumstances of treatment, in the same form of disease; as in *gout* and *yellow fever*.

From an ignorance of the *unity* of disease, physicians have fabricated a nosology, dividing disease into as many different diseases as the various forms in which it appears; and arranging them into different *classes*, *orders*, *genera* and *species*, according to their various causes, seats, or symptoms.

Nosology, or a history of diseases, has long engaged the labours of physicians. The ancient Greek and Roman physicians began the attempt. Since them, Platerus, Sagar, Sauvages, Sydenham, Baglivi, Linnæus, Vogel, Cullen, and others, have laboured to bring it to perfection; but all to very little purpose.

"All of the attempts," say the learned and ingenious editors of the Medical Repository, "to arrange morbid affections, have been exceedingly imperfect, and it is likely they will always remain so. We suspect there is a radical difficulty in all these nosological attempts, which it is impossible to remedy; and that is, that nature has not distinguished symptom from symptom, in diseases, with the same exactness, by which plant differs from plant; or one animal, or mineral, varies from another; but, on the contrary, has interwoven the tissue of diseases by threads which are inextricable, but by a more correct and scientific acquaintance with their causes."

It is impossible to divide diseases, or give them names which can delineate with any accuracy their different natures, from the different forms of morbid action.

Cullen's *pyrexia*, would lead us to believe, from the definition of the word, that this *class* of diseases* is always attended with a preternatural degree of heat, which is not the case, as all the different orders of this class frequently appear with a temperature of the body, that is natural, or preternaturally cool.

Yellow fever is a name given to specify a particular form of disease; but the symptom from which it derives its name does not occur oftener, perhaps, than once in twenty cases: here we are liable to be deceived nineteen times in twenty by the name.

Fevers have been called *intermittent*, *remittent*, &c. by way of distinction; but these names are not characteristic of any particular form of fever; as the symptoms by which these are designated, occur more or less in all forms. Thus the impropriety and uncertainty of naming

* The plural number is used in conformity to custom.

disease, from any form whatever, might be shewn, did it not seem like an attempt to illustrate what must already appear self-evident.

We might as well expect to comprehend the nature of a storm, by attending to its various forms, and distinguishing them by hard names, as to divide fevers by their remissions or intermissions.

II. Diseases have been divided from their remote causes; but this is improper, as they all act more or less in one way, viz. by *stimulus*.

Dr. Brown says: " All stimulant hurtful powers, are participant, but of one effect." Poisons, intemperance, opium, external violence, miasmata, contagions, vicissitude of heat and cold, and passions of the mind, all produce diseases so similar, that the most discriminating nosologist could not distinguish them. The poison of the viper produced a disease so much like a pleurisy, that Dr. Tennant was induced to administer the same remedies in the cure of both.

Intoxication, in the fall of 1799, produced a fever with violent convulsions, delirium, and inability to stand or speak. The cure was the same as if marsh miasmata had been the remote cause. The loss of a pint of blood, restored the patient to the use of his feet, speech and reason.

A case of fever from opium, occurred in 1798; the remote cause not being known, it was suspected to be yellow fever: the face was flushed, the eyes inflamed, the pulse high; by copious bleeding, purging and blistering, the patient recovered, and acknowledged the remote cause to be a draught of laudanum.

Heat and cold, in the extreme, produce similar effects; inflammation, vesication, and pain, are the common effects of both.*

Bilious fever and pleurisy, have different remote causes; but they have symptoms exactly similar, and are both cured by depletion, which proves that they are the same disease, for even Dr. Cullen admits that " disorders, which are cured altogether by the same remedies, are of the same nature."

The fever succeeding a broken bone, surgical operations, as lithotomy, amputation and trepanning, has the same symptoms as pleurisy and bilious fever, and is cured by the same remedies

The remote cause of small-pox, though different from any other, produces the same symptoms that occur from other remote causes.

* The similar sensation produced by the *frigorific mixture*, to that of heat, was experienced by a number of the chemical class, as well as myself, last winter, when the brilliant experiment of freezing mercury, was performed by the indefatigable and accurate chemist, Dr. Woodhouse. The cold produced, caused the mercury in the thermometer to fall sixty degrees below o; and the mixture felt like coals of fire. This experiment was never performed before in America.

" The small-pox and measles, are cured by the same means as peripneumony, or any other sthenic disease," says Brown. And " disorders which are cured altogether by the same remedies, are of the same nature," says Cullen.

In the present month, a middle aged man, of a robust habit, was taken ill, with every symptom of a highly malignant fever, at Chester in this state; he had drunken spirits freely through the day; was bled a quart in the evening; purged; bled next morning, purged again. The remote cause was unknown, till the small-pox made their appearance. Three days afterwards, he walked out, and complained only of pain from the eruption. In the same way, physicians often prevent death, from the small-pox, by mistake, without a knowledge of the remote cause, or the name of the disease.

That the cure of small-pox, is the same as of other diseases, is proved by the success of the present mode of inoculation. By the abstraction of stimulus, we as certainly lessen the malignity of small-pox, as if marsh miasmata had been the remote cause; and by the addition of any stimulus, we as certainly aggravate its symptoms. Even the eruption, which is its specific characteristic, may be prevented by copious depletion. The pustules are the mere effects of the disease. Erysipelas, miliaria, shingles, nettle-rash, phlegmon, abscesses, cancers, buboes, scrophulous and scorbutic ulcers, are all local affections, induced by fever. They are not diseases, but *disorders*, the effects of disease, and mere accidental circumstances.

" Eruptions, are fevers translated to the skin; the prickly-heat, the rash, and the essere of authors, are all states of misplaced fever."* Sydenham calls dysentery, *febris introversa;* we may with the same propriety, call eruptive fever, *febris extraversa.* This variety in the effects of disease, is nothing more than pervades all nature; but cannot affect the unity of disease, which depends on unity of cause, and unity of cure.

As many different remote causes, produce similar effects at one time, so at another time, one produces the different effects of all.

The same stimulant power supports life and health; and, in a different degree, causes disease. The same remote cause produces effects directly dissimilar: what is more so, than the *cold* and *hot* fits of ague?

The various symptoms of bilious fever, caused by marsh miasmata, afford a proof of the same thing; these are apoplexy, coma, convulsions, rigor, sore throat, hoarseness, giddiness, faintness, delirium, pain in the head, eye-balls, back, hips, limbs, neck and ears, nausea, vomiting, burning in the hands and feet, hæmorrhage from the nose, mouth and

* Dr. Rush's Inquiries and Observations, vol. iv.

bowels.* These are all symptoms of but one disease, and Cullen says, "that some similarity of the cause, argues a similarity of disorder produced by it."

A physician of a neighbouring village informed me, that the cold of last January, produced, in the course of his practice, pleurisy, rheumatism, gout, apoplexy, palsy, nephritis, hæmoptysis, quinsy, pneumony, ophthalmia, hemiplegia, catarrh, stricture in the urethra, and cynanche trachealis, and that depletion cured them all. These could not be different diseases, for they arose from one cause, and were cured by one remedy. A nosologist would have attempted to find names for all these different symptoms; which would have been as useless in leading to a proper mode of treatment, as a knowledge of the names of the different persons affected.

The venereal virus seldom affects different persons in the same way. One is affected with gonorrhœa, another with chancres, some with buboes, others with phymosis, and chordee; whilst many receive no injury, who have all been equally exposed to the same infection. All these different effects also occur in the same person, at different times, from the same cause.

Inflammation appears in different forms, as phlegmon, gangrene, and scirrhus. The two latter are only effects of the former; and the difference in form is caused by different modes of treatment, the susceptibility of the parts, and the different degrees of the cause producing them.

Pleasure and pain are both the effects of one cause, differing in degree. Thus, *friction* when gentle gives pleasure; when violent, pain. So *heat*, in cold weather, produces both, according to the quantity applied; and *cold* in hot weather has the same effect.

So uniform is the power that produces disease in its operations, that could we ascertain the force of the acting power, and the strength or susceptibility of the system acted on, we might almost with certainty predict the final issue of disease.

When we see effects so similar, from causes so different, and when we see these effects removed by the same mode of treatment, we conclude with Dr. Cullen, "that disorders which are cured altogether by the same remedies are of the same nature."

When we see effects so different, produced by the same cause, we also conclude with Cullen, "that some similarity of the cause argues a similarity of the disorder produced by it." And our final conclusion must be, that all the remote causes, however various, unite in their operations, and produce but one disease, viz. *morbid action.*

III. Diseases cannot be divided from the predisposing cause; it is a *unit*, viz. *debility;* or a derangement of the equilibrium of excitement

* See Dr. Rush's account of bilious fever, vol. i.

and excitability, which is the standard of health. This derangement is induced by the power of the remote cause. When it operates feebly on the system it only induces debility, and the power of the exciting cause is necessary to produce disease; hence debility is not disease, but its predisposing cause.

IV. Diseases have been divided from the *exciting cause;* but these have only one mode of action, and are reinforcements to the remote causes acting by the same stimulant power on the accumulated excitability, and producing the *proximate cause* of disease.

V. Diseases have been divided from their proximate cause; but this is improper, for the proximate cause is a unit, viz. morbid excitement or the disease itself; and as disease which is caused and cured in the same manner, is a unit, so must the proximate cause be a unit; but out of this one disease, Cullen has fabricated one thousand three hundred and eighty-seven different diseases, for many of which he gives a different proximate cause; his success in this attempt may readily be imagined, from his unfortunate choice of SPASM for the *proximate cause* of fever.

Says Cullen again: "But as medicines are only applied to diseases for the purpose of removing the proximate cause, it must necessarily be, that disorders which are cured altogether by the same remedies are of the same nature." Here the proximate cause and *nature* of the disease depend on the success of the medicine. If bark and wine do not cure the remittent and intermittent fevers they are necessarily diseases of different *natures*, with different proximate causes; but when depletion cures them both, they become one, and when bleeding cures yellow fever, bilious fever, gout, and small-pox, of necessity they become diseases of one *nature*, with but one proximate cause.

Since the mode of cure has been found to be a unit, the number of drugs is reduced in our shops, prescriptions are less complex, and the whole science is rendered more simple and intelligible. A student now may acquire more useful knowledge in a few years, where simplicity and unity are taught, than in an age in the schools of the nosologists. Here we have theory founded on the firm basis of reason and experience, and facts to prove the truth of our theory.

VI. Nosologists have even given different names to diseases from their different seats. Pain in the head is called cephalalgia; in the ears, otalgia; in the teeth, odontalgia; in the limbs, rheumatism; in the feet, gout, and in the side, pleurisy. We might with the same propriety give different specific names to clouds, from the different parts of the hemisphere they occupy.

Spasm in the glottis is named croup; in the bowels, colic; in the lower jaw, tetanus; in the limbs, cramp; in the extreme vessels, Cullen's proximate cause of fever.

Eruption on the face is erysipelas; on every part but the face, miliaria; on part of the body, shingles; on all the body, rash, prickly-heat, and hives. These are all symptoms or effects of disease, determined to a weak part. To divide these different forms into different genera and species, or to call them by different names every time they change their seat, is as absurd, as to say a man changes his species whenever he changes his situation, or to call him by a different name every time we meet him in a different place.

Inflammation in the brain is named phrenitis; in the liver, hepatitis; in the kidneys, nephritis; in the stomach, gastritis; in the intestines, enteritis; in the lungs, pneumony; in the eyes, ophthalmia; in the schneiderian membrane, coryza; in the trachea, cynanche trachealis, and in the tonsils, cynanche tonsillaris. All these would be treated differently by a nosologist, according to their names and situation; as if fire which breaks out in the kitchen were specifically different from that in the parlour, and required different applications to extinguish it; but fire is still the same thing, and the simple article water, extinguishes it with equal success in every part.

The form of nervous fever, which is called typhus gravior, is represented as being a specific disease; but it is found to be peculiar in nothing but in degree; which instead of being of the lowest, as has been supposed, is of the highest inflammatory type. It occurs in confluent small-pox, which is the highest grade of this form of fever. We often find the pulse raised by blood-letting in this fever, from a low *typhus* to a violent *synocha*. When the system is stimulated to the extent of its power, it succumbs under the load of greater stimulus and is prostrated; the pulse is depressed; but depletion relieves it, and permits it to act again with violence. A patient in this form of fever could not sit up for weakness; his physician desired his pupil to bleed him; he lost thirty ounces of blood, and large bleedings frequently repeated restored him to perfect health.

Dr. Brown mentions a desperate case of typhus gravior cured by bleeding, which he says puzzled him; and similar cases often occur, which are cured by bleeding, to the utter astonishment of all Brunonians and nosologists who have never experienced the happy effects of it in this form of fever.

Those who distinguish diseases from their effects or symptoms, are under the necessity of changing their names as often as the symptoms vary; but, says Cullen, "characters in nosology ought not to be usurped by any means, till after a long continuance of the disorder; perhaps not till it is finished! To *name* a disease after it is finished! Are these the words of the illustrious Dr. Cullen? How great the absurdity of nosological arrangement, to produce such a declaration from so great a man! Delenda est nosologia!

To specify the genera and species of disease from the effects or symptoms, is impossible; because many which are said to be of different genera and species, have symptoms exactly similar. Hysterical and hypochondriacal symptoms frequently occur in gout and malignant states of fever. Small-pox and stone are both attended with symptoms of nephritis; and the same disease often affects all parts of the body, as the gout; yet none call it by different names; it is still gout, whether seated in the head, stomach, or extremities. All the different symptoms of disease are but varieties of the same thing. The same cause seldom produces similar effects in different constitutions, nor in the same constitution at different times; and the same disease appears with different symptoms in different countries, among different nations, and in different climates and seasons, affecting all variously. As when a hurricane invades the oaks of the forest, all feel the shock, and each one stands, or falls, or breaks, or bends, according to its strength: would a nosologist divide the cause of this storm into genera and species, from its different effects?

To prescribe for the symptoms of disease leads to a most absurd mode of practice. If a patient, after amputation, complained of an itching or a burning sensation in the foot, would a nosologist call it gout, and recommend warm flannels to be applied to the part affected?

If it is improper to specify the nature, genera, and species of disease, from the effects or symptoms, nosology is entirely hypothetical; because, on these is founded the whole nomenclature of diseases.

It is contrary to the nature of things, that effects, essentially different, can arise, *cæteris paribus*, from one cause. Effects from the same cause may vary in form, but can never change the nature of the cause. Animals and vegetables may change their appearance; but their nature, genera, and species, remain unchangeable. Animals have never imparted their specific characters to others of a different class; but one disease runs through the different classes, orders, genera and species of all, and all again unite in one. Hysteria, the *Proteus* of disease, appears in almost every possible form; and all the different forms of disease, appear in gout. Consumption is often transformed into head-ach, rheumatism, diarrhœa, and mania; and phrenitis, nephritis, gastritis, and enteritis, are frequent symptoms of yellow fever. Were we thus to examine all the diseases of Cullen's nosology, we should not find a single symptom, in any one disease, which had not occurred in diseases of a different name. The changeable forms of disease render them incapable of division into genera and species, or of being designated by any unchangeable character. All the different forms must be taken for a whole; as soon as a division is attempted, the whole is thrown into confusion. "These forms," says my preceptor in medicine, "should no more be multiplied into different diseases, than the numerous and different effects of heat and light upon our globe, should be multiplied into a plurality of suns."*

If the classification of animals and vegetables; substances possessing uniformly the same properties, is still imperfect and uncertain; if the line of distinction between animate and inanimate matter, is yet undetermined, how absurd is it in nosologists to attempt a classification of diseases, which are ever varying their seats and forms!

Nosological arrangements of diseases have rendered the science of medicine incomprehensible, by unmeaning names, which are never understood, nor exemplified in practice; they have seduced the attention of the physician from an exclusive attention to the state of the system; they have crowded the science of medicine with mysterious diseases, such as *opprobria medicorum*, diseases *sui generis*, and a long class of incurables, all of which have originated from nosology, and are only to be removed by adopting the unity of disease.

"To pronounce a disease incurable, is often to render it so. The intermittent fever, if left to itself, would probably prove more frequently, and perhaps more speedily fatal, than cancers."†

"The want of success in the treatment of those diseases which are thought to be incurable, is occcasioned in most cases, by an attachment to such theories as are imperfect, or erroneous."‡

Consumption, dropsy, gout, rheumatism, and cancers, were long considered as incurable; but since these have been found to be only different effects of one primary disease, they have all yielded to the same mode of treatment.

The unity of disease, abolishes the whole class of incurables, and gives the greatest encouragement to believe, that what is practicable in one form of disease, may be accomplished in every other. A conviction of the truth of this principle, encourages the physician to persevere in the use of remedies; and renders him capable of administering to his patient, hope, at once animating and salutary.

Many patients have been abandoned by their physicians, from the notion of the disease being incurable; some of whom were afterwards cured by nature, accident, or quacks: and others suffered to fall victims to a disease, which probably might have been cured in a few days, had it never been distinguished by a place in nosology.

* Dr. Rush's Medical Inquiries and Observations, vol. iv.

† Ibid. vol. i. ‡ Ibid. vol. ii. preface.

The names of diseases have often been the cause of their proving fatal, by leading the nosologist to treatment contrary to the state of the system. A case of what is called yellow fever, with depressed pulse, by a *nosologist*, is named typhus gravior; bark and wine are administered, which generally hasten the termination of the disease in death.

Another case of the same form of fever, appears with the symptoms of what has been called worm fever: pink-root tea is prescribed for it, which is as effectual in checking the progress of the disease, as it would be in calming the ocean.

If the yellow fever were to receive different names from the different seats and forms in which it appears, it would nearly monopolize all the names in Cullen's nosology. To treat all these forms differently, according to their names, would cause such diversity, perplexity and uncertainty in practice, as to render it also an incurable disease.

Nosology is the nurse of empiricism. Were physicians obliged to prescribe for the state of the system, without naming the disease, or if the Chinese custom of prescribing, from feeling the pulse only, without seeing or conversing with the patient, were imposed on physicians, exclusive empiricism could no longer exist. Persons inattentive to the state of the system, would not attempt to cure what they were ignorant of, and men of science only would be consulted by all ranks of people.

The notion of a specific difference in the nature of disease, probably gave rise to the ridiculous practice of specific remedies. Black cat's blood for the cure of shingles; mare's milk for the hooping-cough; sheep's saffron for the small-pox; flower-water for the dropsy, have all been prescribed by the most learned physicians of the last century; they were the offspring of nosology; they have all perished; may nosology speedily perish with them, never to revive again!

Nosology is the ignis fatuus of medicine; it is only seen in darkness; and whilst we pursue the fleeting phantom, it flies with equal speed, or finally leaves us plunged deeper in obscurity. "To describe diseases by any fixed or specific characters, is as impracticable as to measure the dimensions of a cloud in a windy day. Much mischief has been done by nosological arrangements of diseases. They erect imaginary boundaries between things which are of a homogeneous nature. They degrade the human understanding, by substituting simple perceptions, to its more dignified operations of judgment and reasoning. They gratify indolence in a physician, by fixing his attention upon the name of a disease, and thereby leading him to neglect the varying state of the system." "The whole materia medica is infected with the baneful consequences of the nomenclature of diseases; for every article in it is pointed only against their names, and hence the origin of the numerous contradictions among authors who describe the virtues and doses of the same medicines."*

A belief in the unity of disease, will always lead a physician to prescribe for its varying forms and stages. It will lead him likewise to attend to the effects of the medicines prescribed, and to continue or withhold them as circumstances may require. This mode of practice, it is true, will not be relished by the idle practitioner, for it requires frequent visits, and a close examination of symptoms in every form of disease; but medicine can never be perfected in any other way.

The benefits of adopting the unity of disease will appear farther, in its prostrating what is called the diagnosis of disease. A physician, instead of drawing on his memory for a hoard of definitions, attends only to the state of the system. A knowledge of this is soon acquired, and just prescriptions as soon follow. By knowing the cause of gout, pleurisy and malignant fevers to be the same, it would lead to depletion for each, under equal circumstances; it would terminate disputes about disease and medicine among physicians, by directing their attention to a single object, and thus remove those controversies, in the medical science, which nosology is calculated to create.

By admitting but one disease, we likewise prostrate the too frequent use of the term, " complication of diseases." This idea has often led to a belief, that patients have had as many diseases, as they have had pains. Thus yellow fever has been called phrenitis in the morning; gastritis at noon; colic at night; next nephritis; then rheumatism; and last of all convulsions. This supposed complication of diseases, vanishes on the fifth day in a black vomit; it is then known for the first time to be a yellow fever.

By admitting the unity of disease, we render it less necessary to investigate their remote causes. The business of a physician is to remove their effects only, except when the causes continue to act, and are subject to his controul. Thus the mariner lets go the haliards in a squall, without regarding the quarter from whence the wind comes. He knows full well that the wind is a unit, and that its mode of destruction is the same, whether it blows from the east, the west, the north, or the south.

Many have seen and lamented the uncertainty, complexity, and obscurity of medicine; but few have seen its simplicity and unity. Dr. Balfour reduced four diseases, viz. cholera morbus, diarrhœa, dysentery, and colic, to the intestinal state of fever. These constitute the febris introversa of the discerning Sydenham, who likewise had a glimpse of the unity of disease, when he saw that all its different forms, in any season, assumed the type of the reigning epidemic. This distant view of

* Dr. Rush's Medical Inquiries and Observations, vol. iv.

unity affords a more useful hint to physicians, than all the names in nosology; it gives us warning when a malignant epidemic is prevalent, never to consider any disease as trifling, and it leads us by the sameness of its cause, to the same mode of cure.

It is said of Dr. Brown, that between the fifteenth and twentieth years of his medical studies, "A very obscure gleam of light, like that of the first break of day, dawned upon him." Had this gleam been so bright as to have discovered to him, that his two forms of disease, sthenic and asthenic, were but one, it would have prevented many of the errors of his system. Mr. John Hunter's "incompatibility of action," was a near approach to the unity of disease, and did honour to his extraordinary genius; but truth on the simplicity and unity of disease, never appeared in its full lustre till it was unfolded in the lectures and publications of my respected preceptor,* from whom the principles contained in this dissertation have been imbibed; for whose friendly instruction, both public and private, I shall ever feel the warmest gratitude; to whom the medical world will always be indebted; and whose name and memory will be dear to thousands, long.....long after he has bidden adieu to all sublunary things.

* Dr. Rush.

INTRODUCTION.

DURING the reign of the Humoral Pathology, the opinion, that substances were conveyed unchanged into the circulation, was necessarily adopted by the supporters of that doctrine. It was supposed that disease was seated in the fluids of the human body, and that medicines were valuable in proportion to their power of correcting or altering the vitiated fluids. Succeeding and more accurate observations having induced the belief that the doctrine of the Humoral Pathology was not founded upon sufficient grounds; the opinion respecting the operation of medicines was likewise called in question: from remarking that certain powerful substances exerted their effects so speedily, that it could not possibly be supposed they were carried into the circulation, many philosophers were led to search for some other mode in which medicines operated. They founded their rejection of the old opinion principally upon two circumstances, viz. that they were not able to discover, in any part of the course of the circulation, active substances which had been taken into the stomach; and that any fluid, even milk, which is the most assimilated to the blood, when injected into the veins of a living animal, produced sudden death. They asserted that the mutative power of the chylopoetic viscera was such, that every thing noxious was rejected, and only the nutritious parts of substances were permitted to pass into the sanguiferous system. Finally, they referred all the phænomena, which were inexplicable to them, to a certain vague term called sympathy; which perhaps involves as many or more difficulties than the former opinion. It is not pretended to be denied, that in the operation of certain medicines a sympathy does appear to exist between certain parts of the body; but it also seems probable, that this sympathy has had too great a latitude; and that certain circumstances are referred to it, which are more easily explained on other principles. Many respectable inquirers continue to entertain the idea that some substances are found in their active state after having entered into the circulation; and of consequence that they must have passed unchanged, or if changed, have regained their original properties, by some process unknown to us.

They grounded their opinion upon experiments and observations made with accuracy and fidelity; the accounts of these are to be found in various writings from ancient down to modern date; but, so far as is known to the author, there has been no compilation or collection of them into one mass. As the only way to arrive at the knowledge of the truth, is to possess a clear view of the evidence; and as this question is deemed of some importance, it is contemplated in the following essay to exhibit to the reader a brief sketch of the arguments in favour of the opinion, that substances are found in their active state after having entered the circulation.

The subject is divided into three sections.

In the first I shall endeavour to shew that the question is not of a trivial nature, or interesting only as a matter of curiosity; but that it is of much importance and practical utility. In the second section will be given in support of the opinion adopted, the proofs as they appear in the fluid parts of the body. In the third, the proofs as they appear in the solid parts.

INAUGURAL DISSERTATION.

SECTION I.

OF THE IMPORTANCE OF THE QUESTION.

THE present question is not merely of a speculative nature, interesting only to the curious observer, and not to the practical physician. It embraces a wider scope, and the determination of it will be of essential importance in many cases that fall under our notice. A superficial view of the subject may induce us to suppose, that it cannot be of any real utility; but by the reflecting inquirer, a different opinion will be formed. In endeavouring to investigate a question of this nature, we ought to discard the ingenious subtilties of metaphysical reasoning, which often confuse, while they do not convict. Our data should be facts well authenticated, from which we are to draw fair and just conclusions. Taking truth for our guide, and not suffering ourselves to be warped by a prejudice for any particular opinion or theory, however plausible, we shall most probably accomplish the object of our pursuit. It is to be observed, that the ancients were strongly impressed with the idea, that certain substances were conveyed, with little or no change of their properties, into the circulation. Hence they prescribed in certain diseases, the milk of animals, which had fed upon peculiar vegetables proper for those diseases. We know that milk constitutes a considerable portion of the aliment of man, in almost every part of the globe. Some nations, as the Laplanders, have scarcely any other subsistence, during one season of the year. If certain active substances are conveyed into it unchanged, it must surely be of importance to ascertain these; as their introduction into the body, at some periods of life, and in certain states of the system, may be productive of pernicious consequences. In like manner, much advantage may result from introducing into the diseased system, in this way, medicines which could not be administered in any other. Subsequent facts will tend to prove, that very destructive habits may be thus acquired. An inattention to diet in a nurse, is often the unsuspected cause of distressing complaints in the sucking child. Active medicines, taken into the circulating fluids of a nurse, will affect the child in an alarming manner. Instances of this kind are not rare. If by the collection of facts on this subject, any hints may be given, which may lead to the discovery of a solvent of urinary or biliary calculi, it would be of essential service to mankind. That this idea is not visionary or impracticable, will be allowed by those who have investigated this subject with attention. Although disappointment may be frequently the reward of our exertions, yet by persevering industry, we often accomplish our undertakings.

It has generally been supposed, that the preparations of lead, externally applied, are innocent, and under some circumstances salutary. But if cases occur, in which it is absorbed into the system, producing mischievous effects, it is surely of importance to keep these in view in using lead. We may thus account for anomalous symptoms, which cannot be satisfactorily explained in any other manner. By knowing the cause of an evil, we may often remove the effect. On the contrary, the physician who rejects entirely this opinion, will be perplexed about the situation of his patient; and by not removing the source, will aggravate the disease.

If it is discovered that certain medicines, by being externally applied, will be conveyed through the medium of the circulation, to different parts of the system, and produce the same effects as when taken internally, will not this discovery be of considerable importance in peculiar habits, and in diseases which will not admit of the internal use of medicines? Facts, to be related hereafter, will at least render this supposition probable.

If it be ascertained, that while most substances are animalized by the chyliform process, and rendered subservient to the nutrition of man, others are conveyed into the circulation unchanged; it is surely of importance to inquire what are these substances, and in what circumstances they are peculiarly injurious.

These remarks, it is hoped, are sufficient to excite the attention of the medical philosopher, and to convince him, that the question proposed for consideration, is not one of mere curiosity, but of real practical utility.

SECTION II.

OF THE PROOFS, THAT SUBSTANCES ARE FOUND IN THEIR ACTIVE STATE AFTER HAVING ENTERED THE CIRCULATION, AS THEY APPEAR IN THE FLUIDS.

Notwithstanding what has been asserted respecting the wonderful mutative power of the chyliform process, there are numerous instances of substances, after having entered the circulation through the lacteals, being found in their original state in the different fluids of the body. We

into it, produces death; granting for a moment that this is the fact, we may however observe that the case is different where substances are immediately introduced into the blood vessels, and when they pass through the common routine of the circulation: we may observe further, "that what passes by the lacteals or lymphatics is carried into the thoracic duct, and there mixed with a large portion of the chyle and lymph, by which its acrimony is sheathed and diluted, or its chemical properties changed before it enters the mass of blood."* We are informed of cases, where foreign matters were actually seen floating in the blood. An instance is recorded of a milky discharge from the groin of a boy, which recurred several times and continued several days each time.† The chyle does not immediately become assimilated with the mass of blood, but floats in it for some time in its original state: the following instance is in point. "A maid, after eating a good breakfast about seven in the morning, was let blood about eleven the same day in her foot. The first blood was received in a porringer, and within a little while it turned very white. The last blood was received in a saucer, which turned white immediately, like the white of a custard. Within five or six hours after, I chanced to see both; and that in the porringer was half blood and half chyle, swimming upon it like a serum as white as milk; and that in the saucer all chyle, without the least appearance of a drop of blood. And when we heated them distinctly over a gentle fire, they both hardened as the white of an egg when it is heated, or just as the serum of the blood doth with heating, but far more white. This maid was then in good health, and only let blood because she never had her courses, yet of a very florid clear complexion."‡

Experiments prove moreover, that the injection of medicines into the veins, is not necessarily fatal; but that administered in this way, they often produce their effects as certainly as when given in the ordinary manner. Thus we are informed by Haller,|| that a poison or medicine injected into a vein, will produce certain determinate effects, as vomiting in the stomach, purging in the intestines, and drunkenness in the brain. Wahrendorf, in a village of Lusatia, injected wine into the veins of dogs, and remarked that it made them drunk. A solution of opium, injected into the veins, exerts its narcotic power even for two days. Vinous medicines, similar to opium in their intoxicating power, produced similar effects on living animals. Poisons, infused into the veins, exert a specific effect upon the different viscera. Certain emetics administered in this way, excite vomiting in the same manner as if

* Percival. Essay on the Operation of Medicines.

† Edinburgh Medical Essays. vol. v.

‡ Lower. Tractatus de Corde, &c.

|| Elem. Physiolog. vol. i.

If credit be due to these facts, we cannot hesitate in believing that active substances may mix with the blood in their original state. We must also grant, that so far from being productive of any ill consequences, they may be subservient to beneficial views. It is not probable, however, that this mode of exhibiting medicines will ever be brought into general practice, as there is some inconvenience attending it, and as it is seldom or ever necessary. The examples are cited only to confirm the justness of the general principle, that active substances may enter the circulation with impunity.

Several additional arguments may be adduced in support of what has been already advanced. Thus, it is asserted by Mr. Bell and others, that mercury cures lues venerea by mixing with, in the blood, and neutralizing it. "Mercury will pass into the system in various forms from the surface of the body. Lues venerea has been cured by frequent immersions of the feet and legs in a solution of corrosive sublimate. The application of a mercurial plaster to the surface of the body, if of any considerable extent, will also cure the disease."*

If the poison of contagious diseases can circulate in the sanguiferous system without injury, why may not medicines, which are not more active, also pass into the system with impunity? It is well known that the lues venerea is communicated from the mother to the fœtus in utero. Some cases of the small-pox being communicated in like manner have occurred. Mr. Turnbull relates the case of a lady, who was inoculated in the seventh month of her pregnancy. Nine days after the eruption she received a fall, and in a few days after that was delivered of a dead child, which was covered with variolous pustules in a state of suppuration. The matter was proved to be variolous from its communicating the disease to several persons who were inoculated with it.†

It has been denied by some that variolous matter could enter the blood in its active state; but this and other instances convince us that it can. We should therefore be cautious in inoculating pregnant women, as we may sometimes unintentionally destroy their embryo offspring.

Some cases are mentioned where the small-pox was communicated by variolous matter introduced into the stomach.‡ In these there can be no doubt that the matter was taken into the circulation in its active state in order to produce the eruption.

In a pregnant woman, who had used a considerable quantity of saffron, the liquor amnii is said to have been tinged of a saffron colour.||

* Bell on Lues Venerea, chap. 4. sect. 4.

† Memoirs of London Medical Society, vol. iv. p. 364.

‡ Medical Repository, vol. i. p. 258.

|| Haller. Elements of Physiology, vol. viii. de Fetu.

The fact being ascertained, that active substances can enter the circulation and produce their specific operation on different parts of the body, according to the different qualities which they possess, we have a clue afforded to extricate us from the labyrinth, in which we have been wandering. Certain phænomena, which have been hitherto shrouded under the dark veil of sympathy, are more easily explicable, it is conceived, by the opinion we advocate.

Tobacco beaten up into a poultice with vinegar or brandy, and applied to the stomach, produces violent vomiting, and is very effectual in removing hard tumours of the hypochondria. Groundsel beaten down to a pulp, and applied to the stomach, produces vomiting and cures agues.*

Oil of tobacco, dropped upon the tongue of a cat, impregnated the whole of the animal with its odour. The decoction of this plant, when the head was washed with it, caused vomiting, fainting, and convulsions; applied to the belly, it produced vomiting and intoxication; the oil in the form of ointment produced purging; the roots of white hellebore applied to the stomach, occasion vomiting. Bitter medicines applied externally to the abdomen, destroy worms. Crocus metallorum, applied externally to cure a herpetic eruption, produced vomiting.†

The experiments of Mr. Sherwen would lead us to conclude that tart. emet. externally applied, is taken up by the absorbents and conveyed through the medium of the circulation to the stomach. Five grains of tart. emet. rubbed into the palms of the hands, in six hours produced a slight nausea, burning of the skin and increased perspiration. A larger quantity, rubbed into the hands and wrists, produced, in a few hours after, a sickness, brisk evacuation by stool, and an increased flow of urine for several days. In one case, the medicine, after producing slight nausea and gentle catharsis, was succeeded, in two or three days, by a rash with a considerable itching all over the skin, which continued some days. In a case, where disagreeable symptoms had succeeded the drying up of an old ulcer in the leg, tart. emet. administered in this way produced nausea, profuse perspiration, and a discharge from the ulcer, by which all the symptoms were relieved. Experiments with arsenic, made in the same manner as with tart. emet. prove, that it occasions slight nausea and increased flow of urine.‡ The dangerous properties of arsenic have hitherto intimidated practitioners from an extensive use of it internally. If therefore experiments of this kind should lead us to discover that arsenic, externally applied, is conveyed into the system, and is a safe and efficacious diuretic, we shall add a new weapon to our store for combating the dreadful disease of dropsy.

* Edinburgh Medical Essays, vol. ii.

† Haller. Elements of Physiology, vol. v.

‡ Medical Memoirs, vol. ii.

Certain Italian physicians have instituted experiments on this subject, the result of which would appear to favour our doctrine. A woman having violent pains, and refusing to take opium by the mouth, was a fit subject for experiment. Dr. Chiarenti dissolved three grains of pure opium in two scruples of the gastric juice of a crow; and after suffering it to remain at rest some hours, he mixed it with simple ointment and rubbed it on the backs of her feet. In an hour the pains were wholly removed, and did not again return. Squills and fox-glove were used in the same manner with equal success. It appears, from experiment, that the application of these substances was most successful, when they were dissolved in gastric juice or saliva.

"Dr. Ballerini, of Mantua, cured one dropsy with frictions of six grains of squills, dissolved in gastric juice, made every second evening; and another with one scruple of squills, dissolved in one drachm of saliva, rubbed in at three times during the course of the day. The assistants, who made the frictions, had likewise an increased flow of urine."

Professor Brera used opium, in this manner, with success in chlorosis; also squills, digitalis purpurea, corros. sublim. aconite, and tart. emet. in other diseases. From his observations he concludes, "that every animalized fluid is fitted by nature to render remedies capable of being absorbed."*

We have numerous instances of the unhappy consequences attending the introduction of lead into the system, which are perhaps more rationally explained on this principle than on any other.

Litharge carried under the arm-pits caused dyspnœa, fainting, nausea, vomiting, &c. Ceruse applied to a part that had been chafed produced similar effects.† The immoderate use of the saturnine lotion for six days to a leg and foot, from which the cuticle had been stripped, produced colic, trembling of the limbs, continual nausea, and frequent vomitings.‡

The application of Goulard's poultice to the knee, continued for some time, produced a violent pain in the bowels, which did not cease until the removal of the poultice. Instances have occurred of convulsions being produced in children by ceruse sprinkled on excoriated parts. It is not improbable that litharge, the common basis of plasters, when employed in dressing issues, produces some of the common effects of the preparations of lead taken internally.||

Dr. Baker observes "that he met with a most violent and obstinate colic, which seemed to have been occasioned by some litharge mixed in a cataplasm, and applied to the vagina with a view to allay a troublesome itching."

* Annals of Medicine, vol. iii. † Haller. Elem. Physiolog. vol. v.
‡ Percival's Essay on the Poison of Lead. || Medical Transact. vol. i.

The vapour of lead, which exhales when it is melted, will excite colica pictonum, as in the case of plumbers and potters, and those who make shot. This vapour, when the metal is heated by mere friction, will produce this colic, with all its terrible consequences.*

" We want no authorities to testify, that the too fashionable application of ceruse to the skin, has been followed by obstinate colics, pains, tremors and resolution of the limbs, slow wasting fevers and pulmonary consumption. For such frequently has been the fate of those who have thus endeavoured to supply the defects of their persons, by a vain and temporary imitation of beauty.†

" The vinegar of lead, diluted and rubbed upon the skin, cures breakings out, redness, inflammations, and the erysipelas; it gives a whiteness and beauty to the skin, but proves pernicious to the body; at length occasioning a consumption, as appears by many melancholy examples."‡

The ung. saturnin. applied for ten days to parts, from which the cuticle had been removed, occasioned a severe colic, resembling in many of its symptoms, the colica pictonum. A gentleman, having strained the tendo achillis of each leg, was advised to use a bath of vegeto-mineral water. The bath was so constructed, that he could immerse his legs to the height of the calves. He used it for five or six minutes every morning and evening for a week, when he was obliged to desist, from the violent spasmodic and paralytic affections it occasioned; nor did he recover from them for some time.‖

There is an instance upon record, of a palsy of both legs and arms, being induced by the application of sacch. saturni. to venereal warts, for the purpose of destroying them.§

" A gentleman, who had for many years had a fontanel, finding that the pea was not sufficiently depressed for two or three years past, applied occasionally, a piece of the thinnest lead over the oil-skin which covered the pea. This apparently answering the purpose, the oil-skin was by degrees omitted; so that the lead was generally in immediate contact with the pea and the orifice of the fontanel.

" This was the case about the end of June, 1771, when an uneasiness and oppression were felt at the præcordia and diaphragm, with anxiety and difficulty on making a deep inspiration. The disorder daily increasing, became, towards the end of July, so grievous as to require the serious attention of the person afflicted. On recollection, he began to suspect that his complaints might be owing to the noxious quality of the lead, which covered the fontanel. He therefore immediately threw it off, and from that time, without the use of any medicines, the disorder very

* Med. Transact. vol. i. † Baker.

‡ Boerhaave. Elem. Chem. vol. ii. process 172.

‖ Med. Transact. vol. iii. § Ibid. vol. ii.

soon abated, and in about one month was entirely removed; nor has it in any degree returned."*

The inferences to be drawn from the above mentioned facts, must be sufficiently obvious to the candid observer. If we take into consideration the various circumstances attending the external application of lead in these cases, some light may be thrown upon the subject. It was necessary for a certain portion of time to elapse before the poison manifested its effects. If it operated by sympathy, the effect should be instantaneous, as we know it to be in other cases where the phænomena are referred to an unknown sympathy. But as we find that a space of time, sufficient for the introduction and diffusion of the poison in the circulation, must intervene before its effects are perceived, we have a right to suppose that it does enter the circulation, and thus produces its mischievous consequences. The symptoms attending, coincide so exactly with those which succeed the internal use of lead, that they afford strong reason to believe the poison acts immediately upon the parts affected. This can only be effected by its entering into the circulation in its active state.

The dangerous consequences which sometimes result from the external use of the preparations of lead, under certain circumstances, should teach us caution in the management of this metal. This appears to be more especially necessary in cases where the parts have been excoriated, as in those situations the absorbents seem to be peculiarly active. The pernicious practice of applying to the skin pigments, of which lead is a constituent part, cannot be too strongly reprobated. By diminishing the sense of the danger, we encourage the growth of the evil. It is only by inculcating the opinion, that the poison of lead may be absorbed into the circulation, and thus be productive of the most dreadful consequences, that we can hope to arrest this destructive fashion. Mercury is often adulterated with lead, and its use is sometimes attended with the peculiar effects which succeed the use of the preparations of lead. It is therefore of some consequence to keep this circumstance in our remembrance, when we are exhibiting mercury.†

Observation would lead us to suppose, that lead internally used, passes into the circulation in its active state. Is not this supposition rendered probable from the palsies of the limbs, which succeed colica pictonum? Here is an affection of parts distant from those to which the poison is directly applied. Colics, which arise from other causes, are not attended with similar consequences. Is not the supposition further strengthened by the fact, ascertained from Mr. Hunter's experiments, that the application of sacch. saturni. to the muscles of dogs, produced in them the same appearance which is exhibited by the muscles of painters labouring under

* Baker on the Poison of Lead. † Ibid.

paralysis? Sour wines sweetened with lead have produced many unhappy consequences of this kind. It is known that sacch. saturni. will render gin, which has a yellow tinge, of a beautiful transparent whiteness. This iniquitous process has been too frequently the unsuspected cause of many distressing symptoms. The honey extracted by bees from the kalmia latifolia, (bastard laurel) and some other poisonous plants, has the remarkable property of proving errhine, after it has been taken into the stomach some time. It produces sneezing occasionally for two or three days afterwards.* The powder of the different parts of the kalmia latifolia is considerably errhine.† How can we account for this strange effect, but by supposing that the honey is taken into the mass of blood and that it continues to circulate in its active state for some days?

From what has been said concerning the proofs of the opinion, which is maintained in this essay, as they appear in the blood, we learn in the first place, that active substances, directly mixed with the blood, are not necessarily fatal, but sometimes salutary. Secondly, knowing this fact, we can more easily explain, on this principle than on any other, certain phænomena resulting from the external application of medicines. Thirdly, circumstances attending the internal exhibition of some medicines, are most easily accounted for by this opinion.

III. Milk. Certain substances manifest their presence in the milk by their colour, taste, smell or peculiar effects.

Milk is faintly tinged of a red colour from eating the Indian fig, and also from madder: it assumes a blue colour from the use of indigo.‡

Saffron imparts its colour to the milk of women using it.||

The sugar of milk depends upon the quantity of sugar contained in the aliment which is used. Its acescency is supposed to proceed from the use of vegetable aliment. Cows, fed upon certain vegetables, give milk of a colour and taste similar to those vegetables, as the horse-chestnut, madder, &c. All the siliquosa communicate a peculiar taste and odour to the milk of animals feeding on them.§

It is a fact very generally known, that where animals feed upon garlic, pepper-grass, salt-marsh, &c. their milk, and the butter obtained from it, partake of the taste and smell of the substance used.

Animals, feeding on a certain species of gentian, have their milk, and the cheese made from it, of a bitter taste. We may discover in milk both the smell and taste of the strong treacle-mustard, and also of saffron. The bitterness of wormwood and the smell of thyme are often perceptible in milk.**

* Barton's Lectures on Materia Medica.

† Essay towards a Materia Medica of the United States.

‡ Haller. Elem. Physiolog. vol. vii. de lacte.

|| Ferris on Milk.

§ Barton's Lectures on Materia Medica.

** Haller. Elem. Phys. vol. vii.

The peculiar effects produced by the milk of animals, which have fed upon particular plants, evince that the active matter is carried into the circulation, and retains its properties even in the secreted fluids.

A case is related by Dr. Cooper, of a woman who was salivated, producing the same affection in a child sucking her. Gmelin asserts, that the milk of salivated animals will salivate a child. Dr. Hamilton detected globules of mercury in the milk of a salivated woman, by slow evaporation.*

The properties of spurge have manifested themselves even in cheese; the eating of such cheese having occasioned dangerous vomiting and purging. The milk of cows, which feed on the hedge hyssop, is purgative. A nurse having taken a purgative medicine and afterwards suckling a child, a hypercatharsis was induced on the child; but the nurse felt no ill effects from the medicine. A boy, who was sucking a nurse that had drunk spirituous liquor, was thrown into violent convulsions.† Infants, from being suckled by drunken nurses, have contracted a propensity for strong drink.‡ A child was intoxicated by the milk of a nurse who had taken a considerable quantity of opium.||

The ancients were so strongly persuaded that substances were taken into the circulation in their active state, that in certain diseases they prescribed the milk of animals which had fed on vegetables proper for the cure of those diseases.§

The milk of goats, which have fed on astringent and balsamic herbs, is recommended as an excellent remedy in certain species of diarrhœa. Thus also the properties of pellitory, madder, the lesser nettle, lettuce, purslain, on which cows have fed, are carried into the milk so unchanged, that they produce their usual effect in the sick who use this milk.**

Some modern writers also entertain the opinion, that the active properties of substances are found in the milk. The celebrity of these writers cannot fail to give additional support to the opinion. A nurse, by eating of cabbage, or of other flatulent vegetables, always gave her sucking child the windy gripes.†† To prevent the belly-ach, which is so frequent among sucking children, their nurses should be careful to avoid eating vegetable food.‡‡ In cholera infantum, when the child is reduced so low in the latter stage of the disease that it cannot swallow cordial medicines; if it be sucking, the medicines are to be administered to the nurse, and they will in this way enter the system of the child.|||| These observations teach us a new mode of exhibiting medicines, from the proper management

* Barton's Lectures on Materia Medica.

† Haller. Elem. Physiol. vol. vii. ‡ Ferris on Milk.

|| Barton's Lectures on Materia Medica. § Ferris on Milk.

** Haller. Elem. Physiol. vol. vii. †† Percival's Essays, vol. i. p. 168.

‡‡ Rush's Lectures. |||| Ibid.

of which much advantage may be derived. In constitutions, where peculiar circumstances render the direct application of remedies improper, we may perhaps administer medicated milk with essential benefit. In cases where it is necessary that medicines should be exhibited for a long time, gradually, and in small quantity, no more agreeable mode could be discovered. The facts that have been related clearly prove, that the active properties of some substances are found in milk; and it is at least worth the trial, to endeavour to apply this principle to some useful purpose.

IV. SALIVA. Some proofs may be adduced, which appear in the saliva. The property of exciting an increased flow from the salivary glands, which is manifested by certain substances, some time after they have been taken internally or applied to the external surface, is only to be explained by supposing that these substances are conveyed in their active state, through the medium of the circulation to the parts affected.

A partial and temporary salivation may be produced by topical stimulants applied to the glands. This ceases shortly after the irritant, which excited it, is removed from the mouth, and may be referred wholly to the direct external application of stimulus. But the salivation, occasioned by mercury, and some other articles, remains long after their use has been relinquished, and can only be accounted for on the principle we have adopted.

Mercury is taken into the circulation in many cases, especially where it excites salivation. It reaches and acts upon the organs of perspiration. The long continued use of it, produces an inflammatory crust upon the blood. Certain preparations of antimony, also salivate; and the same effect is produced by oxygen gas, nitric acid, citric acid and polygala seneka, &c.*

Lead, used internally, has been known to salivate. In the case of professor Thumberg, related by himself, it is observed, that in eight days after the salivation commenced, lead was perceived in the saliva.† The oil of the melaleuca leucadendron, rubbed upon the soles of the feet, manifested its taste in the mouth. Petroleum, dropped upon the head, discovered its taste in the mouth.‡

V. URINE. The facts, which are related respecting the presence of substances in their original state, in the urine, are so numerous and well authenticated, that none will hesitate in giving his assent to them. Certain articles impart to the urine, their colour, taste, and smell, or imbue it with their peculiar properties.

" Extract of logwood, taken internally, sometimes gives a bloody hue to the urine." Its astringent property, often accompanies its colouring

* Barton's Lectures on Materia Medica.

† Voyage to the Cape of Good-Hope, vol. i.

‡ Haller. Elem. Physiolog. vol. v.

matter.* Persons eating the Indian fig, have their urine of a blood colour; the same takes place from eating red beets, or madder. It becomes blue from indigo. Asparagus and olives, communicate a strong smell to it. Turpentine gives it a violet smell. It is also scented by nutmegs, mace, cloves, juniper, parsley-roots, fennel, carrot, parsneps. It is made bitter by balsam copaibæ. Vinous spirits are sometimes found in the urine; also oil shortly after it has been taken in. A certain species of mushroom, possessing an intoxicating property, is found to retain this property, after passing into the urine.†

Rhubarb, taken into the stomach, colours the urine for many hours after. It is said to possess a diuretic property. The oil of savin sometimes exerts diuretic effects upon the kidneys, and in these cases the urine is impregnated with its smell.‡ Nitre, taken internally, is found in the urine in its native state. When applied to the external surface of the body, it is taken up by the absorbents and conveyed through the medium of the circulation into the bladder, unchanged. A solution of nitre, applied in a pediluvium, is said to have been absorbed into the circulation; for a piece of paper, dipped in the urine and dried, burnt in the same manner as touch-paper.||

Turpentine acts as a diuretic; when taken in large doses it produces strangury, diabetes, &c. and hence we infer that it has a peculiar action on the kidneys. Whether applied externally or internally it discovers its smell in the urine. We have a proof that it is taken into the circulation and carried to the bladder, from its correcting the peculiar smell which is observed in the urine of persons who have eaten of asparagus.§

Kaauw Boerhaave relates, that a man, holding turpentine in his hands for some time, perceived in his urine the same violet smell, which is observed when turpentine has been taken into the stomach.**

This fact is familiar to anatomists, who are in the habit of injecting dead bodies and washing their hands with spirits of turpentine.

The different species of garlic appear to pass into the circulation, particularly affecting the urinary system, and sometimes discovering their smell in the urine and perspiration. From their supposed property of passing into the urine unchanged, they have been recommended as lithontriptics. Some of the siliquosa are used in dropsies, with advantage. Their active matter is probably carried into the circulation and thus to the kidneys, occasioning heat of urine, &c.††

* Percival's Essays. † Haller. Elem. Physiolog. vol. vii. de Urinæ.
‡ Barton's Lectures on Materia Medica. || Zoonomia, vol. iii. p. 361.
§ Barton's Lectures on Materia Medica. ** De Perspiratione. No. 430.
†† Barton's Lectures on Materia Medica.

der; yet it appears from experiment that from a certain quantity of fresh made urine, one fifth of its bulk of pure fixed air was obtained. Drinking water containing this air may impregnate the urine with it, and make it more efficacious in dissolving calcareous matters than it would otherwise be.*

The drinking of waters impregnated with fixed air is recommended and extolled by Hoffman and others, as very efficacious in preventing and dissolving calculi. Human calculi, by being macerated in these waters, were considerably diminished. They are also diminished by immersion in the urine of those persons, who had drunk water impregnated with fixed air; while the urine of a person in health, not using such water, had no effect in lessening their bulk.†

From this fact the inference must certainly be drawn, that fixed air is conveyed unchanged into the urine.

Doctor Sydenham entertained the idea that malt liquors alleviated the pain and irritation arising from gravel. In his own case, he observes, that whenever he was obliged to ride over stones, it was his custom to take one or two large draughts of small beer, which prevented bloody urine.

Doctor Dobson observes, "that upon the whole, the sedative and solvent powers of fixed air, in cases of the stone, are so far ascertained, as to give it a claim to the particular attention of the faculty. Further experience can alone determine whether, by the steady and long continued use of this medicine, a cure may not in some instances be happily effected."

It appears that the urine of persons, using alkaline remedies becomes alkaline, and that it exerts some degree of a solvent power upon urinary calculi immersed in it.‡

Doctor Falconer speaks very highly of the use of the aqua mephitica alkalina, or solution of fixed alkaline salt, saturated with fixible air in calculous complaints. In the case of Mr. Colbourne, who had occasionally passed small stones and was much troubled with nephritic symptoms, he observes, "that during the use of the mephitic alkaline solution, he parted with no gravel, his urine deposited no sediment whatever, nor discoloured the vessel; though, if it was omitted even for a few days, these appearances took place and small bits of gravel were perceivable in his water." The use of this solution is observed to correct the fœtor and disposition in the urine to putrefy, which manifested themselves previously to its use.|| It is well known that fixed air will retard the

* Priestley on Air, vol. ii. p. 216, 17. † Dobson on Fixed Air.

‡ Home's Clinical Experiments and Histories.

|| Falconer on Aqua Mephit. Alkal.

putrefactive fermentation out of the body, and hence the foregoing fact furnishes us with an analogical argument that this air is conveyed into the bladder.

Human calculi, immersed in this solution, were found to lose a considerable part of their weight. A daily portion of the urine of a person, who used the solution, was poured upon a fragment of a calculus for six months, at the end of which time it had lost two-thirds of its original weight. Upon another fragment of the same calculus, a daily portion of the urine of a person in health, not using the solution, was poured for two months, but did not exert any solvent effect.* Does not this prove that the solution is conveyed unchanged into the bladder? The preparations of some of the metals are found in the urine in their original state, as is discovered by the usual tests. "Iron appears to possess the property of passing into the circulation under the form of æthiops. The valuable experiments of Menghini, published in the memoirs of the institutes of Bologna, have proved that the blood of persons, who take martial remedies, is thicker and contains more iron. Mr. Lorry observed that the urine of a sick person, to whom he administered iron in a state of extreme division, was manifestly coloured with the nut-gall."†

From what has been related, respecting the presence of substances in the urine, it must be granted by all, that certain matters are found in this fluid in their original state.

It is, however, denied, by a celebrated and ingenious writer,‡ that active substances are conveyed through the course of the circulation into the bladder. He asserts, that the lymphatic vessels of the bladder, communicating with the absorbents of the intestines, take on a retrograde action, and thus an easy and direct passage is made into the bladder. One of the principal arguments for this opinion rests upon the phænomena observed in diabetes. It is supposed that the immense quantity of urine, which is sometimes discharged in this disease, cannot be secreted by the kidneys, but must pass by some direct communication between the alimentary canal and the bladder. This opinion is said to be supported by the circumstances attending the drinking of mineral waters. It has been supposed that the short time, in which the flow of urine takes place after receiving these waters into the stomach, demonstrates the existence of some more direct route than through the ureters. "But in this case the stimulus of cold water received into the stomach, like external cold applied to the skin, causes a concussion of the bladder and urinary parts, by which they are solicited to repeated discharges of the old urine which was before in the body, and not immediately of that

* Falconer. † Chaptal's Elements of Chemistry, part iii. chap. x.
‡ Darwin.

which was last drunk. Again, one thousand ounces of blood are conveyed through the kidneys in an hour, and this is surely sufficient to furnish twenty or even fifty ounces of urine. Finally, it is certain that both men and brute animals perish if the ureters are obstructed by a ligature or otherwise. We then observe also that no urine can be found in the bladder."*

The limits of this essay will not allow a fuller investigation of the doctrine of the retrograde motion of the absorbent vessels; and the more especially as it is not strictly connected with the present subject of inquiry.

VI. Perspiration. Marks, that substances have entered the circulation in their active state, are sometimes found in the fluid excreted from the surface of the body.

Opium is absorbed into the blood-vessels. Haller asserts, that the smell of opium is sometimes observed in the perspiration, particularly in his own case.†

The active property of camphor appears to reside in a volatile vapour; it is sometimes absorbed into the mass of blood, is known to salivate, and discovers its smell in the perspiration and sweat.‡

The common garlic and the red onion, when taken into the stomach, pass off by perspiration, occasioning considerable thirst, &c. The smell of garlic is said to be perceived in issues, fistulæ, &c.||

It is probable that elixir vitriol is conveyed into the vascular system and excreted by the pores of the skin. It is said to perform a cure in nurses, who are affected with the itch, and also in children sucking them.§

Dr. Russel remarks, that the people of Aleppo, who take large quantities of oil internally, are found to have oil transuding through the pores of their skin. Oil obtained from the livers of cod-fish, administered internally, in the Manchester hospital, manifested its nauseous taste and smell in the perspiration of those who used it. "An oil of the same kind forms no inconsiderable part of the food of many northern nations; and it is said to penetrate and imbue the deepest recesses of the body."**

We have thus endeavoured to exhibit a comprehensive view of the proofs of the opinion, maintained in this essay, as they appear in the fluids. Considered individually they may not perhaps amount to a demonstration; but taken collectively, it is presumed, they will make some impression upon the mind of the candid inquirer.

* System of Anatomy, vol. ii. p. 411.
† Barton's Lectures on Materia Medica.
‡ Ibid. || Ibid.
§ Percival's Essays. ** Ibid.

That medicines should be conveyed into the circulation unchanged, or, if decomposed, that they should be recomposed, is not more strange or unaccountable than that urine, semen, &c. should be secreted from the blood. Chemistry presents us with an analogy. We see, that by the union of two different substances, a third is produced differing in quality from either. By the addition of another substance we procure one of the first constituents in its original form and properties. May not a process similar to this be carried on in the circulation? Much yet remains to be explored.

SECTION III.

OF THE PROOFS, AS THEY APPEAR IN THE SOLIDS.

The proofs that have been already adduced in support of the opinion, that certain substances enter the circulation in their active state, afford at least a high degree of probable evidence. We now come to consider the proofs, with which we meet in the solids. These, although few in number, are decisive, and must carry conviction to the lover of truth. They are arranged as they appear in the skin, flesh, and bones.

I. Skin. Sulphur, after passing through the circulation and being conveyed to the skin, evidently recovers its original properties. It communicates its particular odour to the perspiration, and blackens silver, &c. A gentleman, who was in the habit of taking sulphur daily, perceived, after some time, that his silver knee-buckles were made black and also his watch.

It is observed by bishop Watson, that persons, using cosmetic lotions containing lead, and at the same time drinking sulphurated waters, will have the parts, to which the lotions are applied, changed black.

"Dr. Swediaur relates the case of a protestant minister, near Hamburgh, who took, by the direction of an empiric, some nitrate of silver for an obstruction of the liver. After continuing this medicine for several months, his skin began to change gradually, till at last it became almost perfectly black. This colour lasted during several years, but is now wearing off."*

The active part of the diet of some animals, seems to enter into the course of the circulation, and to manifest itself perceptibly in their integuments. The turkey-buzzard (vultur aura of Linnæus) is a carnivorous bird, and feeds upon the putrid carcases of animals. The quills of

* La medicine éclairée par les sciences physiques, &c. Fourcroy.

these birds have generally such a stinking odour that we are obliged to keep them a long time, before they become fit for use.*

II. FLESH. The active properties of certain substances, which are eaten by animals, are conveyed into the circulation in such an unchanged state, that the flesh of these animals will produce the same effect, as is produced by those substances. It is a fact, well known, that the flesh of wild pigeons, which have eaten the berries of the phytolacca, or poke root, will purge, if a considerable quantity of it is taken into the stomach. The flesh of pheasants, which have fed upon kalmia latifolia, or wild laurel, one of the most fatal poisons, has been known in several well authenticated instances, to exert deleterious effects, in a few hours after it has been taken into the stomach.†

Dr. Barton informs me, that he has been considerably purged by eating the flesh of deer, which had fed upon the leaves of the kalmia latifolia. Dogs, who had eaten the flesh, were affected with convulsions and paralysis of the hinder legs.

Kæmpfer, in his history of Japan, mentions a fish,‡ which being fed with a certain poisonous plant, is infected with its peculiar deleterious properties, and destroys the persons who eat of it.

The red sea-bream when found in the South sea, salivates. This fish taken in the Pacific and Atlantic oceans, does not produce this effect. The peculiar property of salivating, is occasioned by its eating the medusa or sea-blubber, which is known to possess this property.||

The balsam of the populus balsamifera, called balsam or tacamahaca tree, is so very penetrating, that it communicates its peculiar smell and taste to the flesh of certain birds, which feed upon the buds.§ The onion has such a durable strong taste and smell, that it is perceived in the flesh of peacocks who are fond of eating it.**

It is generally known, that the flesh of some animals, killed at a particular season of the year, is strongly tainted with garlic. This is particularly the case with sucking calves, who receive it through the medium of the milk. "The London mutton is known to taste strong of turnips, with which the sheep prepared for market are chiefly fed."

The following fact is transcribed from an ancient book, in its original style and language.

* Barton's Lectures on Natural History.

† Medical Repository, vol. i. p. 161.

‡ Tetraodon ocellatus.

|| Cook's Voyages.

§ Barton's Essay towards a Materia Medica of the United States.

** Haller. Histor. Stirp. Indigen. Helvet. vol. ii.

"Here a multitude of the inhabitantes, as well women as men, "resorted to hym (Columbus) with cheerefull countenaunce and without "feare: bringynge with them popinjayes, breade, water and cunnyes. "But especially stoke-doves muche bygger then owres: which, he "affirmeth, in savoure and taste to be muche more pleasaunt then oure "partryches. Wherefore, as in eatinge of them he perceaved a certyne "savoure of spyce to proceade from them, he commaunded the croppe to "bee opened of such as were newely kylled, and fownde the same full of "sweete spyces, whiche he argued to bee the cause of theyr strange "taste. For it standeth with good reason, that the fleshe of beastes "shulde drawe the nature and qualitie of theyr accumstomed nuryshe-"mente."*

Wormwood affects not only the milk, but also the flesh of animals, with its intense bitterness.†

"Our table was always plentifully and even luxuriously furnished with truffles, red-legged partridges, and a great variety of small birds; the latter were not indeed very palatable to us at first, on account of the high flavour of the juniper berries on which they fed."‡

Birds, which live wholly on fish, have their flesh to taste of fish. Mr. Hunter observes, "this fact was so well known, that it was hardly necessary to put it to the test of an experiment. Yet he took two ducks, and fed one with barley, the other with sprats for about a month, and killed both at the same time; when they were dressed, the one fed wholly with sprats was hardly eatable, it tasted so strongly of fish."||

Let the candid reader weigh, with mature consideration, what has been said under this head, and then decide whether any doubt remains with him on this subject.

III. Bones. We meet with but few proofs, in the bones, of substances being conveyed into the circulation in their active state. Perhaps this is owing to want of attention. One solitary fact, however, would be more in point, than a thousand which are negative.

Mercury is received into the blood unchanged; for it has been found fluid and in its native state, in the cells of the bones.§

It would appear that some substances, which lose their properties on entering the circulation, again acquire them after having passed through it. Thus madder does not tinge the skin, muscles, ligaments or fat, but when carried to the bones, it colours them.**

* Translation of the Decades of Peter Martyr, decad. i. p. 16, 17. London edition, 1555.

† Barton's Lectures on Materia Medica.

‡ Smith's Tour, vol. i. p. 145.

|| Animal Economy, p. 177.

§ Haller. Elemen. Physiolog. vol. vi.

** Percival's Essays.

"The bones of the Canada porcupine, during winter, are of a greenish yellow, owing, as is supposed, to the bark of the pine on which the animal feeds in that season of the year."*

In the disease of rickets, which is supposed to be occasioned by a deficiency of bony matter, the exhibition of lime-water, both internally and in the form of bath, has been found exceedingly advantageous. May we not reasonably suppose that the lime is carried into the circulation, and there, meeting with the phosphoric acid, supplies the bony matter where it is wanting?†

Circumstances, unavoidable, do not permit the further investigation of the present interesting inquiry. I regret that the same circumstances prevented me from bestowing a longer and more minute attention upon this subject than I have done. The field is truly extensive, and patient, persevering labour may glean much valuable fruit from it. No other merit is claimed in this dissertation, than the having collected together into one body the scattered remarks and opinions of various writers. If this essay should direct the attention of some future enterprising genius to this too much neglected subject, and if it should meet with the approbation of those, whose esteem the writer is most anxious to deserve, he will be amply repaid for his exertions in the field of science.

To depart from this university, without expressing a strong sense of the advantages it offers, in the prosecution of the study of medicine, would be highly unjust and ungrateful. To the numerous testimonies of the zeal, diligence, and abilities of the different medical professors, mine is now cheerfully added. To some of them I am indebted, not only for public instruction, but for numerous private acts of friendship and politeness. To these I now offer publicly my sincere thanks.

* Pennant's Arctic Zoology, vol. i. p. 126.

† Medical Repository, vol. i. p. 427.

EXPERIMENTS ON THE BILE.

IN the investigation of a subject like this, to which much time, and great talents, have often been devoted, little else can be effected by the hand of inexperience, than to retrace the road, which towering and successful genius has pointed out.

Little advantage, however, has resulted from the inquiries of our predecessors, who, unaided by the rapid improvements which have recently been made in the science of chemistry, deduced inferences from those results alone, which they were capable of obtaining by the assistance of heat; the fallacy of which must be evident in a minute analysis, and from which many prejudices, at present existing, have originated.

To the mere speculation of Boerhaave, are we indebted for an opinion, which modern chemistry has combated in vain: the saponaceous nature of the bile, founded on analogy alone, has been received by successive generations as a well established point in physiology; and the reputation of that great man has sanctioned the error. The support which the opinion of Boerhaave has received, from the experiments of Cadet, are insufficient, however, to establish it on the firm basis of truth. Resting the result on the action of heat, he obtained by his experiment a quantity of oil, soda, &c. he, therefore, immediately concluded the bile to be a true animal soap. This was contested in a publication at Stratsburg, by a Mr. Roederer, who advanced the coagulation of milk by bile, as a sufficient refutation of its alkaline nature. Without entering into the consideration of this refutation, we may easily convince ourselves of the fallacy of Cadet's inductions, by reflecting on the tendency of heat to produce different results, according to the mode of its application; but a sole reliance on heat was improper: the salts existing in the bile, must inevitably be decomposed, their earthy or alkaline bases will remain, and as there certainly does exist an oil in the bile, are we on such slight foundation to infer the saponaceous nature of it? The experiments which I have instituted, clearly evince the existence of a considerable proportion of phosphoric acid, and that combined with an earth and alkali; the union therefore of the oil and alkali will be prevented; the presence of an acid, and the formation of soap being incompatible.

Of the various fluids of the human body, none has engaged the attention of the physiologist more than the subject of the present essay. Its presence, in almost all the classes of animals, must have indicated some salutary effect; and the defect of its introduction, in the chylopoetic viscera, by disease, was sufficient to establish the efficacy of its operation. But, although all were willing to allow the advantages of an uninterrupted excretion, yet many regarded the bile as a necessary agent in the formation of chyle; while others adopted the opinion of its being merely excrementitious, and separated from the blood as a mass, unfit for the purposes of the animal economy.

Numerous are the arguments which might be advanced in favour of either opinion. On the one hand, a deficiency of chyle has been attributed to the obstruction of the biliary ducts; for if the bile is a requisite ingredient in the composition of chyle, the want of it will present a substance improper to repair the constant waste incident to the human system; and its action on the alimentary canal, in assisting the protrusion of its contents, has long been considered as indispensable. On the other hand, may not its agency, in the formation of chyle, be justly called in question, when we reflect on the support which the system receives, independently of the bile, during the disease of jaundice; if chyle is not formed, the disease must necessarily be fatal; if it is formed, (which for the patient to survive it certainly must be) it is effected without the assistance of the bile, and as this takes place in one instance, why not at all times? But does not the retention of the fæces strongly exhibit the necessity of its operation? Having at one time exerted its influence on the viscera, the cessation of that stimulus must certainly be prejudicial; does this, however, evince any more than that parts accustomed to the operation of any stimulus will be incapable of performing their accustomed functions when deprived of it? Does it amount to a positive demonstration of an original incapacity in the intestines, to be excited to a regular action, but by means of the bile? Had this fluid some other outlet from the body, would not the contents of the stomach prove sufficiently stimulant? For as the irritability of the parts would be less impaired, an inconsiderable stimulus must be capable of exciting a sufficient degree of action. The stomach, unaided by the bile, drives onward its contents, for its occasional presence there must be considered an error loci, the constant effect of disease; and the stimulus of distention appears sufficient to excite the muscular fibres of the œsophagus to their accustomed duty. The important operation attributed to the bile, in the process of chylification, as the medium of combination, between the oily and aqueous portions of the chyme, thereby forming that most important fluid, the chyle, and the precipitation of the fœcal part, for the purpose of its elimination, wears the garb of deceptive hypothesis, rather than of conclusive experiment. The saponaceous nature of the bile, we have already attempted to disprove; but, admitting this property, and that nature had kindly afforded us this medium for the proper union of the repulsive parts of the unassimilated chyme, to what must this union be attributed, when by disease the usual supply has been prevented? To an improper stimulus in the arterial system, must the uneasy sensations of the patient be ascribed, and not to a defect of nourishment. Are not the fæces separated in the disease of jaundice? The torpor, indeed, of the intestines, owing to the privation of an accustomed stimulus, prevents their speedy expulsion, and the activity of the absorbents deprives them of their usual moisture; but

"Non nostrum, inter hos, tantas componere lites."

I must therefore wave the further consideration of these opinions; much learning and ingenuity has, without doubt, been displayed on both sides. To experiment, the sole and never failing source of truth, must the decision be referred; reasoning, without its aid, is the mere mantle of fallacy, and plausibility alone has enslaved whole ages in the bonds of error!

Although a considerable diversity of sentiment existed, respecting the nature of the operation of the bile, more unanimity prevailed as to the source of its supply. The peculiarity of the structure of the liver, in the unusual number and size of its vessels, when compared with any other viscus, and the necessity of the returning blood of the intestinal canal taking that route, previous to its being reconducted to the general circulation, were too striking to escape observation, and too important to be neglected by our speculative predecessors. They supposed that this blood possessed some principle, which the parts where it had been distributed were in a particular manner calculated to afford; and which was not to be detected in any other portion of the vascular system: to this the ambiguous term phlogiston was appended, a term of singular service, in expressing whatever is little understood; and that this phlogiston ultimately separated from the blood in the liver, appeared under the form of bile. This idea is, I believe, or ought to be, entirely discarded. When we consider the great length of the intestinal tube, how numerous and minute must be the ramifications of the mesenteric arteries for the purpose of their supply, we may readily conceive, that in this tedious route, the oxygenous principle which it had received in the lungs must be dissipated; but, in no respect, does it differ from the refluent blood of the extremities.

But is the presence of oxygen unfavourable to the secretion of bile? By no means: accidental dissections have satisfactorily established this point; and the case of Mr. Abernethy alone, wherein the vena portarum communicated immediately with the ascending cava, and the hepatic artery was found adequate to the nourishment of the liver and the purposes of the bilious secretion, proves to a demonstration, that the process is carried on, though the parts be supplied solely with arterial blood.

The opinion which Maclurg maintained, in his celebrated treatise on the bile, namely, that its secretion was calculated to separate that portion from the blood, which in the tedious course of the circulation, had acquired the putrefactive taint, has been sufficiently refuted; not alone by opposing argument to argument, but by an appeal to the most decisive tribunal, that of experiment.

In the frequent analyses of the bile, which have been given to the world, the results have been very different. Thus, for instance, the bile has been considered a true soap, formed by the combination of an animal oil and soda, to which a small proportion of mucilage and resin has been added. At another, we find the soda united with an acid, either the muriatic or phosphoric, and sometimes both. Some more fortunate than the rest, have detected the presence of iron; while others, with the same views have experimented in vain. Nor must lime and ammoniac be omitted in this brief review; to which, if the saccharine substance, similar to the sugar of milk, mentioned by Cadet, be added, we shall have nearly ascertained the sum total of opinions on this subject.

But are we to conclude from this, that the bile of the same species of animal, uninfluenced by disease, can, at different periods, afford such varying results? May we not with greater propriety, attribute them to ignorance, prejudice, or mistake? A preconceived opinion too often influences the inductions from the best experiment; by ignorance, phantasms are taken for realities; and mistake has been a frequent clog to the wheel of science.

Although much remains to be done on this subject, little satisfaction can the reader derive from the perusal of the following pages. Owing to the limited period in which I was to make an election, and conclude the consideration of a subject, (the period of a few weeks) I was obliged to confine myself to one view of the subject, viz, to the difference, if any, which exists between the bile of different animals. The number has unavoidably been small.

My experiments were first made with the bile of the ox. To obtain the results more satisfactorily, I took advantage of the assistance of heat, and the requisite chemical agents,

Experiment. I exposed ten ounces of recent ox bile in a retort, to which was appended a tubulated receiver, over Argand's lamp, to a low degree of heat; at the end of some hours, I obtained eight ounces of transparent colourless water, which was strongly impregnated with the odor submoschatus, or aroma of the bile, but entirely free of bitterness: immediately after this, the retort was filled with a white dense vapour, of such gravity, that it fell to the bottom of the receiver, and was there condensed.

A small quantity of water which fell from the neck of the retort with this vapour was immediately mixed with it, and presented a colour as white as milk: half an ounce of it was obtained; it was slightly empyreumatic, and being placed aside for an hour, a thick white substance was precipitated; the supernatant liquor, however, remained still somewhat coloured by it. This I consider an oil; the colour must be owing to the low degree of heat used. After which I obtained half an ounce of a brown oil strongly empyreumatic: a hard bitter mass remained in the retort.

ANALYSIS OF THE BILE OF THE OX, BY CHEMICAL TESTS.

NINE ounces of ox bile being taken, a quantity of alcohol was added; it was then laid by for forty-eight hours, at the end of which time by means of filtering paper, a gluten was separated, which after desication weighed twelve grains.

The brown, transparent saccharine and intensely bitter liquor which passed the paper, was evaporated to the consistence of a dense extract; on this extract distilled water was poured, for the purpose of separating that portion which has been called resinous; this being again filtered, left nothing behind that could be denominated a resin. Evaporation was a second time performed, and carried so far that the residuum underwent an imperfect crystallization, when taken from the lamp; this was divided into several portions and treated with the following tests:

To one, the oxalic acid was added; the precipitate which ensued demonstrated the presence of lime.

To a second, the muriated barytes; the phosphate of barytes was precipitated.

To a third, a few drops of the alcohol of galls; no change of colour ensued.

To a fourth, the prussiate of pot-ash; the presence of iron, however was not detected by it.

To a fifth, the nitrate of silver; a copious precipitate ensued; but are we from this experiment to conclude that the muriatic acid exists in combination in the bile?

Many, I am aware, have inferred the existence of it, by this experiment alone; without considering, that, as the phosphoric acid exists with the soda or lime, the addition of nitrated silver must cause a precipitation; owing to the stronger affinity which exists betweeen the nitric acid of the silver, and the soda of the phosphoric salt, whereby the phosphoric acid seizes on the silver, and forms an insoluble compound.

Does there exist any difference between the component ingredients of the ox's bile and that of the calf; are any of them wanting in the latter which age may be supposed to supply? or may they not all exist, and the only difference be in the proportions? To satisfy myself on this point, the bile of a calf, two weeks old, was subjected to examination.

Two ounces of this bile, with the addition of a sufficient quantity of alcohol, after remaining the usual time, for the proper separation of the glutinous part, were subjected to the filter; the gluten which remained on the filter was in very small quantity, adhering to the paper, extremely yellow, and very slightly bitter.* The filtered liquor was brown, diaphanous, and possessed of an aromatic bitterness not to be met with in the more pungent bile of the ox; distilled water was added to this, to separate the resin; no precipitate however was obtained; the liquor was evaporated: a portion of the extract dissolved in distilled water, with the addition of the oxalic acid, produced a cloud barely to be distinguished.

To a second portion, the addition of the muriated barytes, caused a phosphate of barytes to be precipitated.

Neither the prussiate of pot-ash, nor alcohol of galls, produced a change in point of colour.

A drop of the nitric acid by chance fell in the glass where a small portion of the extract was dissolved, and a slight purple tint was produced; this was greatly increased by the addition of two or three drops more.

By this we perceive that what is found in the bile of the ox, is also to be detected in that of the calf, the proportions however being much smaller in that of the latter.

ANALYSIS OF THE BILE OF THE DOG.

Having obtained some dog's bile, to an ounce and a half of it were added two ounces of alcohol; it was laid by for two days, then committed to filtering paper; a dark green liquor was separated, and left a light green substance on the paper, which had been precipitated by the alcohol. This substance carefully taken from the paper, was weighed when moist, and amounted to twenty-two grains; the external surface assumed a dark green appearance, by the action of the air: the surface which rested on the paper, was of a light grass-green colour: applied to the tongue it was perfectly insipid: when perfectly dry, it weighed but three grains: although the specific gravity of this substance was so small yet the bulk was considerable; so much so, indeed, that previous to my weighing it the second time, I judged it could not be less than fifteen or twenty grains: when broken, the edges were shining; the central parts of a dusky green, and very compact: one fourth of it was put in a

* This gluten could not have exceeded a grain in weight.

small quantity of water; the water was rendered turbid; and on resting a few minutes the whole fell to the bottom in the form of a floccous precipitate.

Another portion was tried with the nitric acid, it was entirely dissolved; the colour of the solution was of a reddish yellow.

A third in the muriatic acid; it was also dissolved completely, and was of a beautiful bottle-green colour; when water was added to this, the whole assumed a beautiful blue colour; and a floccous substance fell to the bottom: some of the phosphate of lime, was added to a portion of that solution; the colour was still preserved.

To a fourth portion some concentrated alcohol was added: it had no action on it.

The liquor which had passed the filter was next tried. It was exposed in an open vessel to a gentle heat, and the alcohol driven off.

To the residue, about an ounce of distilled water was added, for the purpose of obtaining the resinous part which the alcohol had dissolved: the liquor after filtration was clear, somewhat sweet, intensely bitter, and of a dark green colour; the resinous part which remained on the paper was very small in quantity, brown and slightly bitter. The liquor was again evaporated, the extract was of a sweet taste, still extremely bitter, although the resinous part, in which the bitterness is said to reside, was separated, and very tenacious or viscid. After further solutions and evaporations, the extract was analysed to ascertain the nature of the salts.

To five grains of this extract, dissolved in distilled water, two or three drops of the oxalic acid were added: a precipitate was obtained, which evinced the presence of lime in no inconsiderable quantity. The proportion was much greater in this bile than in that of the ox; it was impossible for me to ascertain the exact quantity, as it readily passed the filter, and by rest was converted into a pretty strong pellicle, adhering to the sides of the glass.

To a solution of the above quantity of the extract in distilled water, a few drops of the nitrate of silver were added; a copious precipitate was the consequence, a dusky red substance adhered to the sides of the glass: this precipitate was principally, if not entirely, a phosphate of silver.

The phosphoric acid was clearly indicated to be present by two other experiments, wherein precipitates were obtained by the addition of muriated barytes and the acetate of lead, or saccharum saturni.

To a solution of the extract, a small quantity of the alcohol of galls was added; no black colour was produced; neither was a change of colour observed when the prussiate of pot-ash was made use of.

ANALYSIS OF THE BILE OF THE ACEPENSER STURIO, OR STURGEON.

THE bile of fishes being known to be much more acrid, in general, that of terrestrial animals, it was a matter of curiosity to ascertain whether this analysis, would present me with the same results as those I had obtained in the preceding investigations.

The colour of this bile when taken from the gall bladder of the fish, was of a strong and as elegant a green as I had ever observed, and to the taste, it was intensely bitter.

To an ounce measure of it, was added the same quantity of highly concentrated alcohol; it remained unmolested for two days, at the end of which time a considerable precipitate, occupied the bottom of the glass: the whole was thrown on filtering paper.

The glutinous part which remained on the paper, did not amount to more than half a grain. The liquor which had passed the filter was evaporated: to the extract some distilled water was added; the whole was taken up by the water; on filtering it no resinous substance remained on the paper; the liquor was again evaporated, and the extract was treated in the following manner, after being re-dissolved in distilled water.

To a portion of it, a drop or two of muriated barytes were added; a precipitate was obtained; this was without doubt the phosphate of barytes.

To a second, the addition of the oxalic acid, evinced the presence of lime in a very small proportion.

To a third, I added the alcohol of galls, no change of colour was produced; nor did the prussiate of pot-ash evince the presence of iron.

EXAMINATION OF THE HUMAN BILE.

HAVING obtained the bile of a person, who, afflicted with chronic mania for a number of years, ultimately fell a victim to pulmonary consumption, I wished to ascertain whether any particular change is effected in this fluid by disease; consequently kept it separate from the bile of another person who had been differently affected. The quantity I obtained rather exceeded an ounce; the colour was of a dark brown, edged with a shining yellow; the taste moderately bitter: a couple of ounces of alcohol being added, it remained undisturbed for several hours, at the end of which a very copious precipitate fell to the bottom. It was now committed to the filter; the gluten which was separated, was of a yellowish green colour, in the course of a few hours it became dry; I found it weighed ten grains. Externally it was of a deep green, frequently interrupted by a yellow spot or streak and interspersed with a number of thin scales of a micacious brilliancy; the internal surface when exposed by fracture, evinced a greater portion of the yellow than the external; the scales were distributed, but not very thickly, through the entire substance. This substance was soluble in the saliva, but of no perceptible bitterness when applied to the tongue. The paper through which the mixture of the bile with alcohol had passed, and on which the glutinous part remained, presented an interesting appearance, which was, that on the evaporation of the alcohol, white brilliant plates, similar to those in the glutinous portion, but infinitely more abundant, were spread upon it. Was this a portion of the resin held in solution by the alcohol, which the paper had absorbed, and which, after an exposure to the atmosphere and the consequent dissipation of the spirituous part, remained on the paper in a crystallized form? Or was it the size, with which the paper was impregnated? No such appearance, it must be confessed, was observed in any experiments with the bile of other animals: if it proceeded from the paper, it should have taken place in every experiment. Some of these plates when taken up by the point of a knife, and applied to the tongue, were perfectly insipid or without taste. The proportion of this substance was very small, probably not more than one and a half or two grains.

After repeated additions of distilled water, and evaporations of the liquor which passed the filter, the salts which remained were coloured with a small portion of a brown extractive matter, very similar in consistence to a dense syrup, which made them adhere to every thing that came in contact with them, which extractive matter I could not separate from them, as it was equally soluble in alcohol or water: the salts, together with this brown substance, which was in very small quantity, weighed only two grains.

The whole was dissolved in distilled water, and divided into several portions.

As I wished to ascertain the presence of the muriatic acid, which has been mentioned by some writers as existing in the bile, and knowing that the ordinary method was inaccurate, I ventured to try the following:

To a portion of the saline substance, dissolved in distilled water, a quantity of the sulphate of iron was added, sufficient to decompose all the salts; by it I expected to have formed a phosphate of iron; and, if the muriatic acid existed in combination in any of the salts, a muriate of iron. The phosphate of iron, I knew, was insoluble; it would consequently be detained in the filter. The muriate of iron is soluble, that necessarily would exist in the filtering liquor; and the sulphate of lime or gypsum could be separated; by this means I expected, with the nitrate of silver, to detect the muriatic acid, if it existed.

After the addition of the green vitriol, the liquor was turbid; on resting, a brown substance was precipitated, which was the phosphate of iron: the whole was thrown on filtering paper; the brown, extractive substance, mentioned above, adhered to the precipitate, and the filtered liquor was not in the least discoloured: a thin pellicle on the surface of the liquor, gave me reason to suppose that the sulphate of lime had passed through the paper; this I was more inclined to believe was the case, as in some former experiments I found the oxalate readily passed through: I therefore doubled my paper, and refiltered it: the nitrate of silver was now added, and a turbidness in the liquor was the consequence: from this, therefore, I think the conclusion ought to be, that the muriatic acid exists in combination in the bile.

A second portion of the saline matter being dissolved, as above, a drop or two of the muriated barytes were added: the phosphate of barytes was precipitated.

To a third, the addition of the oxalic acid produced an oxalate of lime. No change of colour was produced on the addition of the alcohol of galls, or prussiate of pot-ash.

From these experiments it is necessary to draw some inference. I am sorry that it is not in my power to add an analysis of the bile of an animal purely carnivorous; it might have given strength to the opinion which those I have performed enable me to advance, on the nature of bile in general. An opinion deduced from experiments, has, I hope, more than plausibility, to recommend it.

I shall here attempt a separate consideration of some of the most important parts of the bile. The colour of the bile as immediately attracting observation, I shall first speak of.

This in different animals varies considerably; it is observed in animals of the same class. The bile of the ox differs from that of the sheep: the one is of a deep brown, the other of a greener hue. Human bile approaches nearer to that of the ox; and that of the dog is a deep bottle-green; how different also is the black bile of the cuttle-fish, from the lively green of the sturgeon, and many others. To what is this owing? Some there are who attribute the colour to the presence of iron; and Cadet thought it probable, that a ferruginous calcareous earth, together with a peculiar animal oil, were the causes of bitterness and colour.*

To the first (the opinion of M. Durade of Geneva) I must answer, that as I have been constantly unsuccessful in my attempts to detect the presence of iron, and as the evasive manner in which it has been mentioned by this very author, induced Maclurg to suspect that some prepossession was necessary to detect it; from these considerations, I am

* Medical Commentaries, vol. i. p. 69.

unwilling to ascribe an effect so material, to a cause, the existence of which is far from being undisputed. If the presence of iron should be proved, could analogy favour us with one argument in support of the opinion? Experiment I have reason to suppose could not. The hypothesis of Cadet has something more to recommend it; the ferruginous earth, to which he attributes so important an operation in the economy of this secretion, taken in this view, does not deserve the least attention. Calcareous earth certainly does exist in the bile, and admitting it to be ferruginous, no advantage can be derived from it when an acid is present. Neutral salts, when they exist, are incapable of effecting a change; they are not necessary to the formation of bile; their presence must be considered fortuitous, but constant. Is there a person who considers carbonic acid a constituent part of atmospheric air? Yet who has ever failed in detecting it? Neutral salts have their definite characters, and as long as these characters are preserved, no change can be produced; this must only be effected by decomposition, which argues a non-existence; and when causes cease to exist, are effects to be looked for? This reasoning is not applicable to the experiments of Cadet; he does not suspect the existence of a neutral salt. As far as he went his analysis is just; to be satisfactory, it should have been more minute. His inductions were erroneous, because his view of the subject was incomplete.

Although my acquaintance with the subject is but partial, it yet affords me sufficient grounds for these arguments; the presence of neutral salts cannot be denied: the experiments are easy, and any person may satisfy himself on this score; this being the case, the opinion of Cadet must fall! He has mentioned an oil; this, I have reason to believe, must materially influence the colour of the bile; but not as he supposed, when united with an earth.

I have hinted above, that the presence of salts is not required in the formation of bile; they exist in every animal fluid, and characterize none. What are the more prominent features of this secretion? I answer colour, taste, consistence, and smell; the principles which, when united, produce these, are its component ingredients, and these consist in a gluten, a volatile and fixed oil, and an acid. I shall endeavour to account for the colour, by a union of some of these; although supported by the experiment, I may be mistaken. After the gluten had been separated from the bile of a dog, it was divided into several portions, and an acid added to two of these, the nitric to one, the muriatic to the other; in both cases it was dissolved; the first was of a reddish yellow colour, the last a beautiful bottle-green; when water was added to the last, the colour was changed to a blue. From this, I conceive that an acid, acting on an animal substance, produces a colour which is considerably modified by the presence of an oil, and that the difference of colour is owing to the proportion of each; this is rendered probable by the following experiment: the gluten of sheep's bile is very yellow; I took a few grains of it, and added a tea spoonful of spermaceti oil; they were intimately mixed; it was of a deep chocolate colour. This was increased by the addition of the muriatic acid; but on the addition of water, the whole put on a light green appearance.

I had an idea that this acid might be supplied by the decomposition of sea salt, and that those animals which make the greatest use of it, are possessed of a brighter coloured bile; but there may be many unanswerable objections to this; and as the phosphoric exists in greater quantity than the muriatic, and the same phænomena occur when it is used, it is immaterial which of the two be present; but probability seems more in favour of the former.

The aroma, I am apt to believe, depends on a volatile oil, which comes over with the water in distillation; but on the addition of alcohol, acids, &c. is destroyed; to the presence of this oil, and even one of a more fixed nature, I ascribe the formation of the resin mentioned by all writers; I know no proof of its formal existence. Do we detect it when fire alone is used? Some have called the residuum after the distillation of bile has been carried to a certain length, " a brittle resinous or pitchy mass;" atmospheric air aided by heat, may have inspissated the remaining oil; but by increasing the heat, it comes over in a highly empyreumatic state. How do chemists explain the formation of vegetable resins; is it not by the oxygenation of the volatile oils? " By exposure to the air," says Fourcroy, " they become thick, and in process of time assume the character of resin."

On the addition, therefore, of alcohol or an acid, with the intention of separating the gluten, a partial decomposition ensues, and a sufficient quantity of oxygen is afforded to thicken the volatile oil; this change we have reason to suppose, takes place as the aroma is destroyed. Now, as this aroma forms an essential part of such an oil, and as we know that this principle can only be lost (when it is so by long keeping) by a change being effected in the nature of the oil, is it irrational to conclude, that when a greater quantity of oxygen is afforded, the same thing should take place in a short space of time? In recent bile, therefore, I have no idea of the existence of a resin; but in what manner are we to account for the strongest characteristic of the bile, I mean its bitterness? Authors have laid great stress on its residing in the resin; but when the resinous part is separated, by the addition of water to the alcohol which holds it in solution, and the liquor which passes the filter is evaporated, the extract when formed, is very bitter. How are we to account for this? Ought bitterness to remain when the principle in which it consists is taken away? I have observed in all the extracts I made, a dark coloured viscid substance, tenaciously adhering to the saline portion; this was equally soluble in water or alcohol, and while it remained the residuum was intensely bitter. Was this a resin? a resin is insoluble in water: a mucilage? to have answered this character it could not be dissolved in alcohol: was it a saccharine matter? a pungent bitter, and strongly saline taste alone was perceptible. I conceived it to be an oil, somewhat changed however, by the treatment it had undergone; and principally to this fixed oil, I think the bitterness of the bile is to be ascribed.

The consistence of the bile is much increased by remaining in the gall-bladder; this has properly enough been ascribed to an absorption of the aqueous parts. This viscidity may be referred to the gluten, in a great measure, but the quantity of oil present must have a considerable influence. I have deviated from the general opinion of those who have written on this subject, in considering the substance separated from the bile by alcohol, a gluten. They have denominated it a mucilage; the characters of these substances are sufficiently marked to prevent one being taken for the other; and as chemists have written on the subject, want of consideration may probably be imputed to me in deviating from received opinions. Although I may be mistaken, as to the nature of the substance, still I must confess, that I never have detected any thing which answers my idea of a mucilage. I formed my opinion of the nature of this substance, by some experiments on the precipitate, afforded by the mixture of dog's bile and alcohol: they are related in the analysis of the bile of that animal. By them I found that acids dissolved it completely after some time; that water rendered it soft, but did not dissolve it, and that it was insoluble in alcohol. I was dissatisfied with the name of mucilage, when applied to a substance insoluble in water. The appearance of that of the dog, when perfectly dry, was dense and uniform; that of the sheep was granulated. I could not detect the saccharine matter, mentioned by Cadet, similar to the sugar of milk. The taste of recent bile, is however considerably saccharine; that of the ox more particularly. To the presence of ammoniac, I have paid little attention; it is very frequently a creature of our own formation, and a variation in some circumstances, is adequate to its production.

Thus, to conclude the investigation which was the object of these experiments, I am of opinion, that the bile of all animals is alike, in those parts which are most material or requisite for its formation; the principles which I have mentioned above are the same in all; the difference I conceive, is to be found only in the proportions; this I grant may be very great in point of colour, taste, &c. In the elaborate compilation of Haller on this subject, can there be found any two persons who exactly correspond in opinion.

In fine, this performance has been executed in haste: to do justice to the subject, time and indefatigable perseverance are required.

While I was engaged in the foregoing experiments, a most inveterate case of jaundice occurring under the care of my preceptor, presented me with an opportunity of instituting sundry experiments, in order to detect the presence of the bile in the blood, as characterizing the disease of jaundice.

Many there are, who do not consider the bile as capable of existing in the blood-vessels without occasioning death; and others, on the contrary, are too apt to ascribe a yellowness of skin, &c. so frequently in many diseases, to the absorption of it.

On this disease it is not my intention to say any thing; all I shall endeavour to prove will be, whether the bile can exist in the blood-vessels with impunity; for this purpose I obtained a quantity of jaundiced blood. After it had separated, the serum was remarkably yellow, of a saline taste, perfectly void of bitterness.

Experiment I. A table spoonful of the serum was exposed to the heat of a candle; the albuminous part after the evaporation of the serous portion was very yellow; this on being tasted betrayed not the least sensible bitterness.

Experiment II. Some of the crassamentum was next taken; it was freed from the coagulable lymph by washing; the lymph was somewhat yellow; no taste of the bile, however, was perceptible in it. That part of the coagulum which remained after the separation of the lymph, was exposed to heat, the aqueous part being dissipated, the residuum was somewhat salt, not bitter.

Experiment III. A portion of the crassamentum, freed from the coagulable lymph, was put in a wine-glass; to this some alcohol was added; a partial coagulation was effected, which was soon after dissolved; a little water was next added, which occasioned the precipitation of a coagulated substance; a part of the supernatant liquor, holding the red globules in solution, was carefully poured off and exposed to a gentle heat; after the evaporation, a scum of a brown colour remained, of a saline taste, but not in the least bitter. Finding that it would in this way be impossible to detect its presence, I resolved to avail myself of the aid of chemical tests.

Experiment IV. A table spoonful of the jaundiced serum was poured in a glass; three or four drops of the muriatic acid were added; an immediate green colour was produced.

To ascertain whether the marine acid is a good test for bile, twelve drops of that of the ox, were mixed in a little water; to this a few drops of the acid were added; the green colour was produced.

I found that the nitric acid, also produced a green with the jaundiced serum; and when added to bile mixed with water, the same thing took place.

When the sulphuric acid was used, a black colour was the consequence, attended with the disengagement of caloric.

The following experiments were required, to ascertain the action of the acids on healthy urine.

Experiment VII. To half an ounce of healthy urine, ten drops of the nitric acid were added; a considerable effervescence followed; the liquor was of its pristine colour.

Experiment VIII. To the above quantity of the same urine, ten drops of muriatic acid were added; a slight purplish tint was observable; ten more of the acid being added, the whole was changed to a slightly purple hue; no effervescence took place.

The effect of a little ox bile mixt with the healthy urine, exposed to the action of the acids, was next tried to ascertain the affinity of the result betwen this and the jaundiced urine.

Experiment IX. To half a wine-glass of healthy urine, twelve drops of the ox bile, with the same proportion of nitric acid were added; the green colour was produced; an effervescence then took place, which immediately destroyed the above colour.

Experiment X. The same proportions of urine and bile being mixt, twelve drops of the muriatic acid were added; the green colour was produced, and was permanent; no effervescence took place.

From these experiments, I think we may safely conclude, that a considerable quantity of bile must be present in the urine of the person the subject of them.

By the experiments also, we find that the muriatic is the best test. The reason why the nitric is not so good, must proceed from the stronger affinity which that acid has for the bases of the urinary salts, whereby, although the green colour is at first produced, yet the affinity it has for these bases being greater than that with the bile, it soon quits the bile for them; effervescence is produced, and the green colour lost. With the muriatic this does not take place; no effervescence ensues, owing to this acid being a component part of some of the urinary salts, or other acids in combination with an alkaline, or earthy base, having a stronger affinity for them than the muriatic has.

The event of the above experiments, being sufficiently satisfactory to establish the presence of something more than the colouring matter of the bile in the blood, I yet determined on performing an experiment which has been mentioned by several authors, namely the tying up of the hepatic and cystic ducts, in order to produce the disease of jaundice, and to ascertain the presence of the bile not by the mere colour as they have done, but in a more unequivocal manner, by the aid of chemical tests Two experiments were performed on dogs, but as my intention was to preserve them several days after the operation, in order to effect more completely what I had in view; some accident or other generally occurred which entirely defeated my designs. The following experiment, however, may not be entirely unworthy of notice. The great hardiness of hogs was an inducement for me to try the above mentioned experiment on one of this kind. For this purpose, a pig being obtained, ligatures were made on the cystic and common ducts, on the 5th of April; the gall bladder was well distended with bile; the flow of it into the ductus choledochus was prevented by the ligature nearly at the mouth of the cyst. He continued very well until the 22d of April, when, by an unfortunate accident, he was killed. On examination, the wound in his abdomen had entirely healed. On opening, the intestines were firmly agglutinated to the peritoneum; the parts being carefully separated, the liver appeared of a colour nearly natural, and the gall-bladder, which, when tied up, contained a considerable quantity of bile, appeared now to be filled with a transparent liquor; and that portion of the duct included between the two ligatures, where the hepatic joined the cystic, to form the common duct, was distended almost to the size of the gall-bladder, and contained a quantity of genuine bile. Here then was the formation of a new cyst; and by some means or other, the bile opened a passage into the duodenum. Both the ligatures were found on the ducts, covered with coagulable lymph: no jaundice was produced in this case; the urine when tried by the nitric and muriatic acids, evinced no change of colour. Although I was disappointed in the object of the experiment, yet the appearance of this white transparent fluid in the old cyst, and the formation of a new one, were phænomena in some degree calculated to lessen my regret: I shall however forbear any comments. When the liquor of the old cyst was received in a glass, a quantity of a mucous substance immediately fell to the bottom, which left the supernatant liquor clear; the liquor was very viscid, for on the application of the finger it was drawn out into small threads, and when tasted, perfectly void of bitterness, and inconsiderably saline.

A part of this liquor was taken and mixed with distilled water, and on the addition of the muriated barytes, a precipitate was the consequence, which indicated the presence of the phosphoric acid. Another portion was tried with the oxalic acid, and lime was detected; indeed the salts, in this fluid, appeared to be exactly the same as those which exist in the bile.

To the mucous substance, which we mentioned above to be precipitated from the transparent liquor, was added a little muriatic acid; it was turned green by the action of it: when water was added, the colour was lost.

Although a want of success attended my experiments on the dogs and pig, there still remained one, which, if successful, would incon-

AN EXPERIMENTAL INQUIRY

INTO THE

PRINCIPLES OF NUTRITION,

AND THE

DIGESTIVE PROCESS.

SUBMITTED TO THE EXAMINATION OF THE

REVEREND JOHN ANDREWS, D. D. PROVOST PRO TEM.

THE TRUSTEES, AND MEDICAL PROFESSORS OF THE UNIVERSITY OF PENNSYLVANIA,

FOR THE DEGREE OF DOCTOR OF MEDICINE.

ON THE EIGHTH OF JUNE, ONE THOUSAND EIGHT HUNDRED AND THREE.

BY JOHN R. YOUNG, OF MARYLAND,

MEMBER OF THE AMERICAN LINNÆAN AND PHILADELPHIA MEDICAL SOCIETIES.

"We ought in every instance to submit our reasoning to the test of Experiment, and never to search for truth, but by the natural road of Experiment and Observation."——LAVOSIER.

AN EXPERIMENTAL INQUIRY.

MAN is endowed with motion, sensation, and thought. These are dependent on some internal or inherent principle, and also on various external agents; when they are all regularly performed, they are said to constitute perfect animal life. When we contemplate this life, we are struck with motion, as its principal characteristic; and when we take a farther view, we must perceive that this motion and its laws must necessarily tend to waste the machine in which they reside. It becomes essential therefore to the existence of the living body of man, that he be provided with means to counteract his tendency to decay. To effect this, he is furnished with an apparatus which prepares new materials, to supply the waste of the old; and these the beneficent hand of nature has plentifully diffused, over every part of the globe. Lest he should neglect them, he is furnished with faithful centinels, which seldom fail to admonish him of the exigencies of the system; and as life would be endangered were those admonitions but feeble and temporary, Hunger and Thirst are among the strongest, and most impatient of all sensations, and the gratification of them is accompanied by the most exquisite pleasure.

In the present Essay it is proposed to consider the substances which supply this waste and growth of the system, and the changes they undergo previous to their entering the circulating mass; or the Nutrientia and their Digestion.

NUTRIENTIA.

BY the Nutrientia are meant, such substances, as taken into the system, are suited to supply its growth, and the waste of its solid and fluid parts. The articles taken as aliment by man, comprehend an immense variety of substances, in the animal and vegetable kingdoms; and, as we presently shall attempt to shew, some from the mineral. It has been, however, a disputed question, whether his natural food be confined to animals or vegetables, or whether he be carnivorous or phytivorous; we shall take it for granted he is both, or an animal "ad omnia," which seems clearly evinced by his instinctive appetites, which urge him to use them promiscuously, in whatever clime he may be situated.

Oil. This enters into almost all our aliments, and composes part of the daily food of all nations. Its universal use clearly evinces its nutritive property. Dr. Stark lived fourteen days on a diet of olive oil and flour, and found in that length of time he gained in weight, four pounds eleven ounces and six drachms. On a diet of suet and flour for the same length of time, he gained four pounds eleven ounces and two drachms.

Sugar. Many proofs might be adduced to prove this one of the nutrientia; even our instinctive appetites when children, urge us to relish this, in preference to every other article of food. It is well known the negroes in the West-Indies, when preparing this article from the cane, live almost entirely on it, and become quite fat at this season. Many are the instances of the crews of ships subsisting on their cargoes of sugar, when their provisions have become expended. Dr. Cullen supposes sugar is not alimentary in its pure saline state, but only when combined with an oleaginous matter, but the above shews to the contrary.

As these two last articles, oil, and sugar, compose the principle of nourishment, in most of the articles we use as food, plethora and serious consequences would result from taking them in any considerable quantity, when in their pure state: but these are obviated, by the stomach kindly becoming satiated when they are taken in but a small quantity, and almost uniformly rejecting them when taken in excess. A remark made by the late Dr. Stark, was appropriate to the present point: "Does not an excess in sweets give a greater shock to the system than an excess in fats? Is there any article of food so hurtful as either, when taken immoderately?"*

Gum. Dr. Cullen supposes this alimentary, and that it derives this property from the sugar, oil and acid, which he imagined entered into its composition. Modern chemistry, however, rejects this analysis of gum, and teaches us it contains neither of these principles; it has however an acidifiable base, but requires another agent, oxygen, to form an acid. Independent of these principles, however, it is a nutritious substance. Haselquist relates in his travels, of a caravan having expended their provisions, in passing the deserts of Africa, who had recourse to their cargoes of gum Arabic, and subsisted on them alone for fourteen weeks. Professor Barton† informs us, that having a child labouring under great debility

* This was the last remark made by this indefatigable physician, when experimenting on himself, on the effects of different kinds of diet. He was using honey and flour as his constant food, which considerably affected his alimentary canal, and system generally. His diseased state advanced, and as usual he marked down every effect. His symptoms at length became alarming, and the last remarks he was able to note down, were the above judicious questions. Alas! his own fate soon answered them; in a few days he was cut off in the midst of his experiments, and in the blossom of life.

† MSS. Lectures.

and emaciation, he prescribed large quantities of sugar, but to little purpose; a solution of gum Arabic was then had recourse to, and with the desired effect; the child thrived well, and subsisted on this alone for six weeks.

In order to ascertain whether other animals could be supported by gum alone, the following experiment was made. A rabbit was confined in a cage, and supplied with gum Arabic, but it refused to eat it for twelve hours. By the following artifice, however, it was brought to relish it very well: a roasted apple was put in some boiling water, in which it remained some time, until the water was impregnated with the flavour of the fruit. Upon dipping pieces of gum in this, it was eaten greedily; it was indulged in this manner for two days, after which becoming accustomed to its new diet, it eat it alone. It was kept upon this for fourteen days, which was thought a sufficient time to prove its nutritious property. The animal, however, lost flesh on this diet, but appeared well, and was as active at the expiration of this period, as when it began to eat the gum.

Calcareous earth. This has hitherto been found not to be decomposed, and of course looked upon as a simple, or elementary body. Analysis teaches us the composition of the bones to be calcareous earth, united to the phosphoric acid. We are forced then to suppose this earth is one of the nutrientia, as it in a great measure supplies the growth of the bones. But lime is not only present in the bones, it is found in small quantity in all the solids and fluids of the body. We are led to believe it is taken up as regularly as any of the nutrientia, since the bones, like other parts, are liable to a constant waste. That they are liable to waste, appears: First, From their vessels transmitting an osseous matter, which no doubt must supply them with an increase: if additions were regularly made, and no waste occurred, they would be liable to increase in size, even in the adult. Secondly, It is well known that madder colours the bones of animals, when they are fed with it; here a supply must take place, and consequently a loss. That bony matter can be absorbed we know well, from what occurs to bones pressed upon by enlarged arteries; and to the alveolar processes when the teeth are extracted.

In the vegetable kingdom we have abundant proof of its nutritious property. It is found in every part of vegetables even in their seed. Its use in agriculture is well known.

Water. This in a great measure supplies the waste of the fluids, which constantly takes place in the system; but it does not alone supply the waste of the fluids; experiments prove it also goes to form the solids. Seeds, it is well known, vegetate, and arrive to their full growth in pure water. Dr. Fordyce informs us he put a gold-fish in a glass vessel, and supplied it with spring water; the fish lived in this manner for fifteen months, grew to more than double the size it was when first confined, and threw out much feculent matter. Lest it should be supposed the fish lived on substances held in the water by solution, he used distilled water and impregnated it with the air of the atmosphere, and put other gold-fish in the water thus treated, and kept them six months, during which time they threw out feculent matter, and thrived as before mentioned.

The common spring frog (rana pipiens) I have kept for two months on pure water alone, during all which time they were as plump, and active as when first confined. Indeed water appears to be their principal nourishment; for though they be brought from springs in good health, and their stomachs distended with food, and be kept out of water, they generally die in less than thirty-six hours, as I have observed from very frequent trials. When I exposed them in a dry jar but one night, they frequently became quite unable to leap, and by keeping them thus confined for twenty, or twenty-four hours, their extremities became quite dry, and withered; these consequences were always however prevented, or obviated when they did occur, by putting them in water.

It may not be improper to enter more minutely into this subject, which will be of use to us when we come to speak particularly of digestion.

To shew how dependent frogs are upon water, the following experiment will serve, as the result of many performed on this subject. A frog, six hours after being brought from a spring, in good health, weighed four drachms and two grains. It was put in a jar containing water, where it remained for twelve hours, in order that it might regain any vigour it had lost; at this time it weighed four drachms fifty grains: it was then put in a dry jar, and in twenty-eight hours was found dead, and weighed only three drachms and twenty-five grains. Many thus exposed died sooner, and others again survived longer, but one or two, however, lived to the third day.

That water was taken into the systems of these animals, was evident from their great increase of weight, when put into it after being previously exhausted: but upon opening several of them after this increase, I never could find any water either in the stomach or bowels; it was conjectured therefore, that the weight they gained arose from an external absorption; to ascertain this the following experiment was performed. A frog weighing five drachms and forty-six grains, was confined in a dry jar for twelve hours. At this time it had lost a great deal of its plump appearance, and was scarce able to leap, and weighed, but four drachms and thirty-six grains. It was now extended upon a thin piece of board, and laced firmly; in this situation it was immersed in water as high up as its fore legs, and let remain so for five hours: not a particle of water could be taken in by the frog's mouth, as its head was above water, and firmly fixed to the board. At the expiration of this time it was dryed, and found to have gained fifty-two grains, so that it had externally absorbed this weight of water. Many experiments of a similar kind were performed and all with the same result.

Their external absorption was ascertained in another manner; by exposing them in damp grass during the night, after being previously exhausted by keeping them from water, they always regained their original weight, or the loss they sustained by being kept dry.

Having thus ascertained that frogs are nourished by an external absorption of water, it occurred that on this principle we might explain how snakes are supported many months, without eating the least particle of food. Dr. Barton has kept a rattle-snake for eighteen months during all which time it never eat any thing: and I myself kept a copper-snake during the last summer, for more than three months, all which time it refused to eat, though its natural food was frequently presented to it. This occurrence appeared quite inexplicable to me; how action and excretion could go on such a length of time when no food was taken: I was led therefore to suppose it could not have abstained from food all this time, but must occasionally have caught insects, that might have crawled or fallen into its cage; but I had no reason to have recourse to these conjectures, when I heard of the above fact occurring under so careful an observer as Dr. Barton. With a view of ascertaining whether snakes derive nourishment from an external absorption, the following experiment was performed.

A snake was kept confined in a dry jar for five days; at this time it was lean and quite inactive, and weighed one ounce four drachms and fifty-eight grains. It was now extended upon a narrow board, on which it was laced firmly; in this situation its whole body, except about three inches of its upper part, was immersed in water, where it remained for three hours. Upon being taken out and dried, it was found to weigh, one ounce seven drachms and twelve grains, so that it absorbed two drachms and fourteen grains, in the short space of three hours. Hence it would appear, that frogs and snakes, like plants, derive a considerable portion of their nourishment from water alone; and that snakes, when kept confined, are nourished by absorption from a moist atmosphere, as from moisture condensed on their surfaces.

How simple water can go to form an animal substance, we shall not pretend to offer a conjecture; we only state the fact, and shall leave the hydrogen and oxygen of the water, together with the different gasses, taken in by the lungs, to the chemist, who by variously combining them, may explain the phænomenon.

Oxygen. As this enters into the system and becomes fixed, thereby supplying a waste that constantly takes place, it properly comes under our definition of nutrientia. The intimate connexion of life, with a constant supply of oxygen gas, is now fully established: it is the least to be dispensed with, of any of the nutrientia. The experiment of Dr. Priestley, who exposed a bladder containing venous blood, to an atmosphere of oxygen gas, which passed through the coats of the bladder, and turned the external surface of the blood to the arterial red, shews in a beautiful manner, how the vital air of the atmosphere penetrates the tender membranes of the lungs, and oxides the blood.

LIGHT. Animal as well as vegetable life, exists but in an inferior degree, when deprived of light; that life may continue however without it, the unhappy subjects of tyrants have too often witnessed; but as animals do not arrive to their full growth and vigour when deprived of light, it deserves a place here. Professor Barton mentions, his having found a hoard of young mice, in a cellar, where they had been deprived of light; they were all white and sickly. The experiments of Newton prove light to be material and divisible; but whether as a matter it enters into the system, we cannot say; we are therefore doubtful whether to consider it strictly as one of the nutrientia, or whether it might not with more propriety be looked upon in the light of a condiment.

Having thus considered the nutrientia, we now proceed to examine the processes these undergo, previous to their entering the circulating mass.

The food is first masticated, and during this process is well mixed with saliva. This fluid is poured into the mouth from the parotid, submaxillary, and sublingual glands. These are excited into action by pressure, or the action of the lower jaw, by stimulating substances applied to their ducts, and by the action of the mind*, all these causes operate

* Is not the secretion of the saliva and gastric fluid synchronous? It is highly probable from long habit, the actions of these two sets of vessels become associated; hence, when the stomach and its vessels are irritated, as in nausea, there is always a flow of saliva, though nothing stimulating has been applied to the mouth. The excitement of the vessels of the one, seems to keep pace with that of the other; when the nausea is so great that vomiting is just at hand, the flow of the saliva is proportionally increased; and when we make an unsuccessful effort to vomit, we generally throw out a mouthful of saliva. If this association of vessels be admitted, will it not explain the modus operandi of salivating medicines? All the preparations of mercury affect the stomach and its vessels, and as we suppose, by association, the salivary glands. When one dose of turpeth mineral is taken, it produces a temporary flow of saliva, as soon as it acts on the stomach: when, therefore, this or other preparations of mercury, operate permanently on this viscus, a permanent flow of saliva takes place. But how does it operate when externally applied? We answer by its action being always determined to the stomach; being in this respect on a footing with many other substances. Thus, ipecacuanha affects the stomach, even when injected into the blood-vessels; and

when we are eating, and the fluid thus flows in greatest abundance, when most required. By deglutition it is then conveyed into the stomach, where it meets with a second fluid, the *gastric*, which constantly flows from the coats of this viscus. It bears a great resemblance to saliva, but differs from it and all other animal fluids, in being a powerful menstruum for animal and vegetable matter. The food after being retained in this reservoir from three to six hours, is expelled into the duodenum, in a dissolved or pultaceous mass; here it meets with two other fluids, bile and pancreatic liquor, and being mixed with these, a mass is formed, capable of affording chyle.

We now proceed to examine this process, in a more particular manner.

Digestion is performed in a similar manner, in all animals with membranous stomachs; they all have a general receptacle into which the food is received, and exposed to the action of a dissolving menstruum; and all have bile and pancreatic liquor with which it is afterwards mixed. As food is then exposed in all, under the same circumstances, similar effects must be produced upon them, or the process, generally speaking, is the same. We shall avail ourselves, therefore, of this similarity, and throughout, we shall not confine our observations to man alone, but also attend to that of other animals, by which we may draw plausible, if not conclusive, inferences concerning our own digestion. We go on to speak generally of

SOLUTION.

It would be unnecessary to recite particular experiments, to prove the solvent property of the gastric fluid, this being admitted on all hands. Under this head we shall, therefore, only make general observations concerning the solution of various substances by different animals.

The effects of solution are most remarkable in such animals as swallow their food without mastication; we will therefore relate a few experiments made on some of these.

Our common large bull-frog (rana ocellata) was chosen in order to observe the effects of the gastric fluid, as they swallow all their prey whole. They have a large membranous stomach, which when distended, occupies the whole anterior part of the abdomen: the œsophagus is very wide, so that their food can be examined at pleasure. Two of a very

tobacco nauseates when externally applied, and in the form of enema. Many other substances salivate, as digitalis, seneka, tart. emet. squills, &c. and all in small quantities will puke. This theory will also explain, why children, under a certain age cannot be salivated, because these two sets of vessels have not acted long enough together for their motions to become associated.

large size were procured, and their stomachs were found to be greatly distended with food: being desirous of seeing what was their natural aliment, and the effects of their digestive power upon it, by means of a pair of forceps, one of their stomachs was easily emptied of its contents; and to my surprise, and that of others who witnessed the fact, it was found to contain a common sized spring frog, and afforded a fine opportunity to see the effects of their gastric liquor. The whole external surface of the frog was acted upon, the muscles having, superficially, quite lost their texture; some parts of the back bone were bare, the spinous processes of which were quite soft. Upon introducing a forceps, a second time, the hinder parts of a second frog were found, which shewed the effects of their fluids in a still greater degree: the muscles of the thigh were reduced to a complete jelly, though still retaining their form; some parts of the bones that were covered with flesh, were quite soft and flexible. Upon extracting the contents of the stomach of the second frog, it was found to contain a field mouse, about a third larger than our common mouse: its whole surface was quite soft, having entirely lost its texture; the fore legs were nearly disconnected from its body, the bones of which were soft; the bones of other parts of the body were also examined; they were all soft. But what was most surprising, the teeth of this animal did not escape; the incisores were, as Dr. Jacobs witnessed, soft and flexible, having the appearance of a piece of half dried tendon. Neither the frog nor the mouse had any acid or putrid smell.

It appeared very evident from the preceding experiment, that the fluids of these animals acted upon bones; but in order to ascertain whether they could dissolve them completely down, the following experiment was performed. The head and all the bones of the mouse were cleared of their flesh, and forced into the empty stomach of one of the frogs; he was then put into a jar of water. In two days, the bones were all discharged in the form of a mortar; by rubbing it between the fingers, small pieces of bone were distinguishable. This will serve to shew us, the powerful action of an apparently inert fluid on an animal matter, sparing not bones, nor even the teeth of animals.

Being desirous of knowing the length of time they would require to dissolve down a small frog, the following experiment was performed. A packthread was tied to the hind legs of a living spring frog; its head was then put into the mouth of one of the large frogs; as soon as he felt it move it was swallowed greedily. In five hours it was drawn up by means of the thread; the skin and external surface of the muscles were tender. It was again introduced; in the space of seven hours, it was drawn up a second time; the abdominal muscles were now dissolved, and the intestines had protruded; the bones of the feet were soft, and separable from the leg by the least force; in a word, the whole was a complete dissolved mass. It was swallowed a third time, and attempted to be drawn up in six hours afterwards; but it had so far lost its texture that the two legs, to which the thread was tied, could only be brought up; the bones of these were soft and flexible, as before mentioned. Many experiments of this kind were made to see the effects of their gastric menstruum: in many cases, after giving them small frogs, the trunk and head of these animals were drawn out of their stomachs complete skeletons, but the bones were always soft, and felt like tender cartilage. In all the half digested substances which were at different times taken from their stomachs, as frogs, veal, beef, &c. an acid was constantly found present: they were seldom examined before two hours after being swallowed; at this short interval when their surfaces were touched with litmus paper, it was turned red.

Snakes, like the large frogs, also swallow their food without mastication: many experiments were therefore also made on them, by forcing frogs, lizards, &c. into their stomachs, to see the effects of solution: they agreed in every respect with what has been said of frogs, like them perfectly dissolving down entire animals. The only difference between them was, that the solution of snakes went on only about half as fast as that of the large frogs.

The gastric fluid of man, and that of frogs and snakes agree perfectly in their action on flesh, as the experiments of Spallanzani prove that the first of these powerfully dissolves meat out of the body. As the menstruum of the two latter animals acted so uniformly on bones, it appeared highly probable, the fluid of our own stomach would also. To ascertain this, the condyles of the thigh bone of a chicken, weighing eleven grains, were swallowed; the bone remained a considerable time in the stomach, as was supposed from some uneasy sensations that were occasionally experienced for between two and three days; the fourth day it was discharged, reduced to a shell, weighing only three grains. Thus far the digestion of man and these animals perfectly agree in solution, being the first step towards the conversion of food into chyle; but they differ in some particulars, and probably by attending to these, they may be of use to us.

First. They are cold blooded animals: heat is a powerful agent in all solutions, and the experiments of Spallanzani prove it greatly assists the action of the gastric liquor out of the stomach.

Secondly. They do not masticate their food.

These two inconveniencies are obviated, by these animals never drinking when their digestion is going on, so that their fluid acts in its undiluted state; whereas in man, it is always diluted, as he seldom eats without drinking. That this was the case with these animals I had clear proof; for although I examined the contents of their stomachs so often, in no one case could I find any fluid more than a jelly like substance, appearing to be made up of gastric juice and dissolved flesh. Supposing, however, that the pressure used in bringing up the food of the frogs might have forced the more fluid parts into the duodenum, I resolved to ascertain the fact in another way; this was easily done. A tea-spoon could readily be passed into their stomachs, and with this the dissolved food could all be brought up; it was always however of the consistence above mentioned.* During the time these experiments were made, they were constantly kept in large jars of water. The attention to this circumstance by these animals, which swallow their prey entire, is a necessary part in their digestion, as they require a very powerful menstruum, so as to dissolve not only entire muscles, but also bones. The inference we would draw from it, would be, to attend occasionally, to what necessity urges them to observe constantly. Thus when our stomachs are weak, or we are troubled with dyspeptic symptoms, like them we ought to avoid much diluting our gastric juice; so that although it were secreted not perfectly healthy, yet having the advantage of acting in its uncombined state, solution and digestion may go on, when it otherwise would not, with the common quantity of drink. Indeed our stomachs in this respect act a kind part to us; for when we make our first dish on broth it seldom relishes much solid aliment after it; hence soups are the first dish at the table of the temperate, and the last at that of the epicure.

While speaking of the solvent property of the gastric fluid, it may not be improper to observe, it has lately been ingeniously proposed as a solvent for the stone in the bladder. In this disorder we have hitherto only had recourse to the knife; but such a formidable and dangerous operation makes other means desirable. Dr. J. S. Dorsey has proved the gastric fluid may be introduced into the bladder with safety; no endeavours therefore ought to be lost in ascertaining what fluid may act on calculi, with most effect. From the facility with which bones and teeth were dissolved by frogs, it appeared highly probable, their fluid would also operate on calculi. On this subject the following experiment was made.

A calculus was obtained from Dr. Jacobs, of a very firm texture weighing exactly fifty grains. It was introduced into the stomach of one of the large frogs. In two days it was taken out for examination: at first sight it was evident solution had taken place, for the gastric juice which

* The eagle appears to observe the same rule respecting drink. Mr. Peale informs me his eagle never drinks during the cold season; and that he only gives it water in the hot summer weather, when it is fond of washing itself in it, and will then occasionally drink, but very sparingly. It is highly probably most carnivorous animals that swallow their prey whole, or piece-meal, observe the same rule.

adhered to it was coloured with some of the dissolved stone: it was found to weigh forty-five grains. It was forced into the stomach a second time, where it remained for two days; it now weighed thirty-eight grains: from this it appears, it is well worthy of more attention. When introduced into the bladder, with the heat of the human body, we have little doubt it would act upon calculi with much effect. Their fluid is easily procured, and without the necessity, as in other animals, of sacrificing a life every time we wish to obtain it: by means of a tea-spoon it is readily brought up from their stomachs.

Spallanzani and most modern naturalists take it for granted, that some carnivorous animals, particularly birds, cannot digest vegetable matter. This opinion if well founded is of importance, not only as it concerns the natural history of those animals, but also as it relates generally to the theory of digestion. There is a great analogy subsisting between man and such birds, in their digestive processes; they both have membranous stomachs, into which food is received, and exposed to heat and moisture; circumstances equally favourable in each to a fermentation; but if it shall appear such birds cannot digest vegetable food, which under equal circumstances ferment sooner than flesh, we are justified in concluding a fermentation has nothing to do with their digestion; and from the great analogy just mentioned, we raise a strong argument in opposition to fermentation being the efficient cause of our own digestion.

Spallanzani and Reaumer have both attempted to support this opinion by experiments: I know well the distinguished rank both these authors hold in the philosophic world, and the weight of their authority in any opinions they may have advanced. Perhaps no man in the age in which he lived, considered subjects in a more philosophic point of view, or threw more light on such as he undertook to examine, than the former of those naturalists: his works will remain as long as experimental philosophy holds its present footing among the learned. With due deference then to this high authority, I would beg leave to differ, or at least to raise a doubt to the opinion above stated: "Aliquando bonus dormitat Homerus." Experiments are the sure and unerring guides to truth; when they light the path we ought with confidence to pursue it, though authority should oppose.

Before attempting to point out the fallacy of their opinion, we shall premise the following experiments and observations.

The large frogs so often mentioned are purely carnivorous, as I never could find any vegetable matter in their stomachs. Into the stomachs of different ones, were introduced beans, peas, wheat, and bread, enclosed in linen bags. In thirty hours they were all taken out for examination: the peas, wheat, and beans were entire, and not the least acted upon; but the bag that contained the bread was quite empty. This experiment confirmed to me a conjecture formed by looking over the experiments of those two naturalists mentioned above. Does not the *living principle* in seeds resist the digestive powers? But this will be answered better presently.

Bags containing beans, peas, and wheat were again introduced into the stomachs of frogs, in which they remained for three days: upon being examined at this interval they were all found swelled, but quite entire. Bags containing beans and peas well bruised, and bread, were then forced into the stomachs of these frogs, where they remained two days: upon examination they were all found empty. Life or a susceptibility of life certainly exists in seeds, and is in some inexplicable manner connected with organization. Seeds as above stated when their texture was not destroyed, uniformly resisted digestion; but on the contrary, when their organization was destroyed, they were as uniformly digested: hence we think ourselves justifiable in concluding the gastric fluid cannot act upon seeds as long as they remain entire, or that their living principle resists digestion.

It occurred, if entire seeds resist digestion, would they not vegetate if retained a sufficient length of time in the stomach? This was easily put to trial. Two beans were enclosed in a bag, and into a second, was put wheat: these two bags were then forced into the stomachs of different frogs. In six days they were examined: the wheat and beans were swelled and soft. They were again forced down, enclosed as before. In six days more, they were drawn up for examination: the tender germs had now protruded in both the wheat and beans, as was quite evident, and witnessed by Dr. Barton and many others.

From the above experiments we will be able to give credit to a case related by the great Italian anatomist Morgani. He informs us that a young lady living entirely on vegetables, (it being lent) was seized with a violent affection of her stomach, and great emaciation ensued. Different medicines were used, but without the least alleviation of her symptoms. At length a violent vomiting commenced, and to the astonishment of all present, she threw up a small plant, with perfect leaves and roots! This at first sight might be looked upon as approaching the marvellous; yet why should we doubt it? the authority of our author is as respectable as any other of our profession; and we have just seen that seeds will vegetate when retained a sufficient length of time in the stomach. The probability here was, that the young lady had swallowed the seed of some small plant, without destroying its texture by mastication; which being retained in the stomach, and exposed to heat and moisture, vegetation progressed.

Having thus, we hope, established one point, that seeds resist the digestive powers unless their organization or vital principle be previously destroyed, we are now prepared to examine the experiments of those authors, before mentioned, which seem to prove some animals cannot digest vegetables.

The following are the experiments M. de Reaumer founded his opinion upon, that some carnivorous birds could not digest vegetables. To a kite he gave beans enclosed in tubes, which were retained in the stomach of the bird for a considerable length of time; upon the tubes being thrown up, the beans contained in them, were not the least affected by the gastric fluid, though it had free access to them. Peas and wheat were tried in the same manner, and with a similar result. These experiments appeared to our author, satisfactory; but his erroneous conclusions from them will be apparent, from what has already been said: had he bruised his seeds, we have little doubt he would constantly have found his tubes empty, when thrown up by these birds.

Spallanzani gave great weight to Reaumer's opinion, from the number of the experiments and variety of animals on which he performed them: we shall candidly examine all he advances in support of his principle. After making some remarks on the subject, he observes, "I had proof of this opinion being true, from what occurred to my owls: they would swallow a whole sparrow, and thus receive into their stomachs, the feathers and undigested food those little birds contained in their crops. After the flesh had been digested, the feathers were vomited in the form of a hard ball, and along with the feathers the grain, which though softened by maceration, yet continued whole: and when the small birds have eaten bread, if the matted feathers be disentangled, we may generally perceive traces of bread." The seeds here thrown up are no proof of his doctrine; neither is the bread, if we attend particularly to what he himself tells us. "Traces of bread were" only "generally to be perceived." We have little doubt our naturalist, by attention, would just as generally perceived traces of flesh; for to me it is impossible to conceive how feathers contained in the stomach, could be matted into a ball, without some of its contents being entangled and matted with them, and of course traces of bread and flesh to be perceived when the ball was opened.

His second experiment is in all respects similar, and equally as exceptionable as Reaumer's, consisting in forcing tubes containing entire beans and peas into the stomachs of different owls; the result our reader will anticipate; none were dissolved.

His third experiment was on the falcon: he enclosed peas, portions of apples and pears in tubes, which were forced into the stomach of his bird; though retained for a length of time they were all discharged, and none of their contents dissolved: the peas we pass over; but the apples and pears were not acted upon. Does not this prove the falcon could not digest, at least, some vegetables? We believe not, and would explain this on the same ground we have all the rest. We do not understand the nature of life, but we may lay it down that every substance of the vegetable and animal kingdoms, which, under certain circumstances, resists a spontaneous dissolution or fermentation, has life. Apples and pears are regularly organized, have vessels, juices, &c. and for a time resist dissolution, and therefore have life; and consequently cannot be dissolved by the gastric fluid until their texture be completely destroyed.

Spallanzani anxious to extend this principle, next had recourse to the eagle; but unfortunately for his doctrine, he first tried it with bread: when he gave it to the amount of six ounces of this at a time, it was not thrown up as indigestible substances were, nor did it appear in the excrements; he was therefore "obliged to conclude this species of vegetable was digested." He next gave this bird seeds of cerealea and wheat; but these were not altered! At this he expresses his astonishment (at the same time tacitly informing us our doctrine is true) by observing, "it is somewhat surprising that this should be the case with wheat, when wheaten bread is so perfectly digested."

We have only one more objection to obviate, which is, in one or two experiments, boiled seeds were tried with some of these carnivorous birds, yet were not digested. In answer to this, we state the following quotation from Dr. Barton's Elements of Botany. When speaking of how extremely tenacious seeds are of the vital principle, our author observes: "Thus the late illustrious Spallanzani discovered there are certain kinds of seeds which do not refuse to vegetate, even after having undergone the operation of boiling in water, and Duhamel mentions an instance of seeds germinating, after they had experienced in a stove a heat of 235 degrees of Farenheit. Spallanzani even found that the seed of mould, which is a true vegetable, survive a heat infinitely greater than this."

We have thus examined this subject at large, which, from its importance, it seemed to demand. As yet we think there is no proof but that all animals can digest vegetables, and by habit may be brought to live upon them: dint of hunger will learn, and habit will soon confirm these wide transitions; so, that animals naturally carnivorous, will subsist on vegetables; and graminivorous, on flesh. In proof of these assertions we state the following: Dr. Barton informs me, he has received information from a respectable gentleman, Mr. Watkins, stating that he has seen two polar bears (ursus arctos) that had subsisted on vegetable food alone, from the time they were taken from their mothers' breasts; and that they were more than half grown, and very fat. These animals in their natural state, are as purely carnivorous as the lion. Spallanzani acknowledges his eagle could digest bread; and Buffon informs us, "when they are unable to procure flesh they feed on bread." The large bull-frogs, purely carnivorous, digested and were supported by bread; on the contrary graminivorous animals will subsist on flesh. The Italian naturalist, so often mentioned, by dint of hunger learnt a pigeon to eat meat, of which it became so excessively fond, that it preferred it to every other kind of food, even to wheat, which in their natural state, they eat before any thing else.

With this we dismiss our observations under the head of solution, and shall now proceed to examine how aliment is converted into chyle.

We have already seen the powerful action of the gastric fluid, by which the food is completely dissolved. But will simple solution explain the conversion of aliment into chyle; a fluid differing from it in so many particulars, and yet always the same, whatever be the nature of the food? The most natural explanation of this phænomenon, would be by a fermentation, by which we know bodies are entirely altered in their nature. This opinion bears the face of probability, inasmuch as the food is detained in a warm reservoir, and exposed to moisture, circumstances highly favouring this process. On this account, and from the respectability of those who have stood in its defence, as Pringle, Macbride, Cullen, Dr. Rush, &c. it deserves our particular attention. We therefore proceed to examine the doctrine of

FERMENTATION.

Chemists divide fermentation into three kinds, the vinous, acetous, and putrefactive; the product of the first is vinous spirit, or alcohol; of the second acetous acid, or vinegar; of the third ammoniac or volatile alkali.

In order to ascertain whether a vinous fermentation could take place in the human stomach, the following experiment was performed. My friend, Mr. Mitchell, avoided his usual breakfast, in the place of which he took, between the hours of eight and ten, twelve ounces of sugar. Nothing more was taken until one o'clock. Having the power to ruminate, it was at this hour thrown up; the mass was sweet: upon being put to rest no intestine motion or disengagement of air was to be perceived. It was then submitted to distillation: a limpid fluid passed over into the receiver, which was sweetish, but had none of the properties of a vinous spirit. Carbonic acid gas is constantly evolved during the vinous fermentation; Mr. Mitchell therefore paid particular attention to this, as long as the sugar was on his stomach; but there was not the least eructation of air during the whole period the experiment was going on. If ever a vinous fermentation took place in the stomach, we expected to have found it in this experiment; as this viscus was plentifully supplied with saccharine matter, which passes so readily to this state: but as nothing of the kind occurred, we conclude the vinous fermentation has nothing to do with the digestive process.

We next speak of the acetous fermentation.

The arguments advanced in support of the opinion are, 1st. Heat and moisture of the stomach. 2dly. The disengagement of air from this viscus. 3dly. The fluids with which the food is mixed quickly running to the acetous fermentation. 4thly. The presence of an acid in the stomach. These we will examine in order.

To the first argument we give its full weight, being founded in truth; but to the second, the disengagement of air from the stomach, we object, as giving any support to this doctrine. This is constantly spoken of as a uniform occurrence, whereas every one who has attended to the state of their own stomachs, must confess they have frequently digested meals without the eructation of a particle of air: with much propriety therefore, we might reverse this argument, and bring it as a strong objection to this doctrine.

3dly. The fluids with which food is mixed quickly running into the acetous fermentation. The saliva is said quickly to pass into this process as the experiments of Macbride and Dr. Rush clearly prove. Those gentlemen mixed some vegetable and animal matter with saliva, and others with water. Upon placing these under equal circumstances, in a moderate heat, the vegetable and animal matter mixed with saliva fermented much the soonest. From this they draw a strong argument, apparently favouring the same taking place in the human stomach, the food there being plentifully supplied with this fermenting juice. Aliment is not however, in these experiments, exposed under all the circumstances it is when in the stomach; a fluid of much more importance than the one just mentioned is left out, and of course the inferences drawn from them cannot be valid. The gastric juice, Spallanzani found powerfully to resist fermentation; and even restored putrid substances to their original sweetness, as I have also witnessed with my frogs. In the following experiment however, food is exposed under all the circumstances it is when in the stomach, but with a different result from those just mentioned. On an empty stomach I made a light dinner, on chicken pye, and drank simple water: in half an hour, by irritating my fauces, it was thrown up; at this time it was plentifully supplied with gastric fluid, as well as saliva, as the quantity of food was but small. It was then exposed in a tumbler, to a heat equal to the human temperature. For the space of nine hours, there was not the least intestine motion, nor any disengagement of air. As digestion is performed sooner than this period, it was not attended to any longer. From this experiment we are forced to draw conclusions directly in opposition to those above mentioned.

4thly. The basis of this doctrine is the presence of an acid in the stomach: that this is the case we are fully convinced; but from what has already been said it appeared to us an acetous fermentation did not take place in the human stomach. We were therefore led at first to suppose, the acid was only present when this viscus was in a morbid state; but experiments proved to us the contrary: in all the different animals we examined, an acid was almost constantly found present. Hunter observes, "that in all the animals, whether carnivorous or not, which he examined, he always found an acid present in their stomachs, though not a strong one." It was before observed, that when small frogs were digested in the stomachs of larger ones, the dissolved mass was always acid; here "nature was interrupted in her regular operations," nothing morbid could therefore be said to be present.

The following experiments satisfied us to what this acid was to be referred. A piece of fresh veal was introduced into the empty stomach of one of the large frogs: in two hours it was examined; the surface was a little tender; upon being touched with litmus paper it was turned red. Here digestion was progressing quite regular, yet an acid was present. It appeared impossible at the same time to conceive, the meat could become sour in so very short a time, and in so very low a temperature; it was therefore conjectured, the acid was to be referred not to the meat, but to the gastric juice, which the following experiments confirmed us in. A frog was kept starving for two days; a piece of litmus paper was then forced into its empty stomach, by means of a pair of forceps; upon being drawn out, it was covered with gastric juice, and the litmus turned red. The naked gastric juice was afterwards often examined, by bringing it out of their stomachs with a tea-spoon, and constantly found to be slightly acid. Being thus fully persuaded the acid, in the digested food of frogs, did not arise from a fermentation, but was to be referred to their gastric juice, we were led by analogy to suppose, the acid of our own stomachs was to be attributed to the same origin: but this analogical reasoning might be called mere probability; the following experiment was therefore performed. Early in the morning my stomach being empty, I irritated my fauces with a view of throwing up some gastric juice: though many efforts were made, none could be vomited. The following day, I took some meat on an empty stomach: in half an hour afterwards, by irritating my fauces, the meat was thrown up, and with it some gastric fluid: upon being tested, an acid was very evidently present. Here no one can suppose the acid was to be referred to the meat. We have little hesitation, therefore, in saying, that the acid so constantly found in the stomach of man, and almost, probably, all animals, is to be referred to their gastric fluid.

Having thus, we hope, traced the acid of the stomach to its proper origin, we next attempted to ascertain its nature by chemical tests. Mr. Mitchell being in good health, and having the power to ruminate, frequently threw up the contents of his stomach for me; which being filtered, a transparent and acid fluid was obtained: on this fluid the following experiments were performed.

I. To a portion of this fluid, acetate of lead was added, a white precipitation immediately took place: this being washed, muriatic acid was added, which decomposed it, a very white powder remaining at the bottom, and a fluid above.

Comparative precipitations of urine and this fluid, by the above agents, were in every respect the same. The explanation* of urine treated in this manner is, that the phosphoric acid of this fluid, decomposes the acetate of lead, forming an insoluble phosphate of lead; this being washed, by the addition of muriatic acid it is decomposed, plumbum corneum or a muriate of lead formed, while the phosphoric acid remains in a liquid state above, which by disoxygenation affords phosphorus.

Though great accuracy, and many varied experiments are required to ascertain certainly the presence of an unknown acid, yet we are disposed to believe any person who had witnessed the great similarity in the comparative precipitations just mentioned, would have pronounced the same explanation was to be applied to both, or that the acid in the filtered fluid was the phosphoric.

II. To a solution of silver in the nitric acid, some of the acid fluid was added; a precipitation immediately ensued.

III. Mercury was precipitated by it from its nitric solution.

IV. Lime was precipitated from lime water.

Authors inform us, the phosphoric acid precipitates all these compounds as above mentioned, forming phosphates. Experiment the first proves, the acid of our fluid is not the acetous, as it precipitated lead from the acetous acid.

From the acid in the stomach being the phosphoric, we explain why some metallic substances, are so uniformly acted upon, when taken in the stomach. The Italian physicians recommend iron in its pure metallic state as a tonic; and experience has confirmed† its efficacy: here the iron is acted upon by the phosphoric acid.‡ Copper is also dissolved in the stomach and by the same agent. In the acidity of dyspeptics and pregnant women, where the quantity of acid in the gastric fluid, is morbid in quantity, similar to what the lithic is sometimes in affections of the kidneys; we learn the superiority of lime water as a corrector, from its great affinity to phosphoric acid.

We shall conclude on this subject with one more observation. We have already seen lime is one of the nutrientia: no one supposes this

* See Lavoisier.

† Does not the uniform effects of iron in its metallic state, prove that an acid is always present in the stomach?

‡ Dr. Barton's MS. Lectures.

obstructed, and also when the pancreas is schirrous. That the absorbents have a secreting or digestive power, we learn from the following. Dr. Wistar informs us of a remarkable case, which occurred under his own observation, of a person who was supported for many weeks, by nourishing enemata, alone. Here it cannot be said there was bile, gastric and pancreatic liquors to assimilate the injected fluid into chyle; yet chyle was formed and the system nourished. If the lacteals acted the part of simple absorbing, or capillary tubes, their contained fluids ought to partake of the sensible properties of the mass from which they are absorbed. But the reverse of this is the case: chyle has always the same taste, however different the sensible properties of the contents of the intestines may be, whether they are acid, bitter, &c. We draw a strong argument in truth of this opinion, by turning to the vegetable kingdom, throughout the whole of which, the digestive process is seated in the absorbents. Water is to them, what the fluids of the primæ viæ are to the digestion of man: it dissolves their food, which being exposed to their vessels is taken up; but the fluid thus taken up, cannot be imitated by any mixture of earth and water, any more than we can imitate chyle, by combining aliments with the fluids of the alimentary canal. As we thus have proofs the one is a secretory process, why not admit that of the other to be so also, since the circumstances of each so perfectly agree.

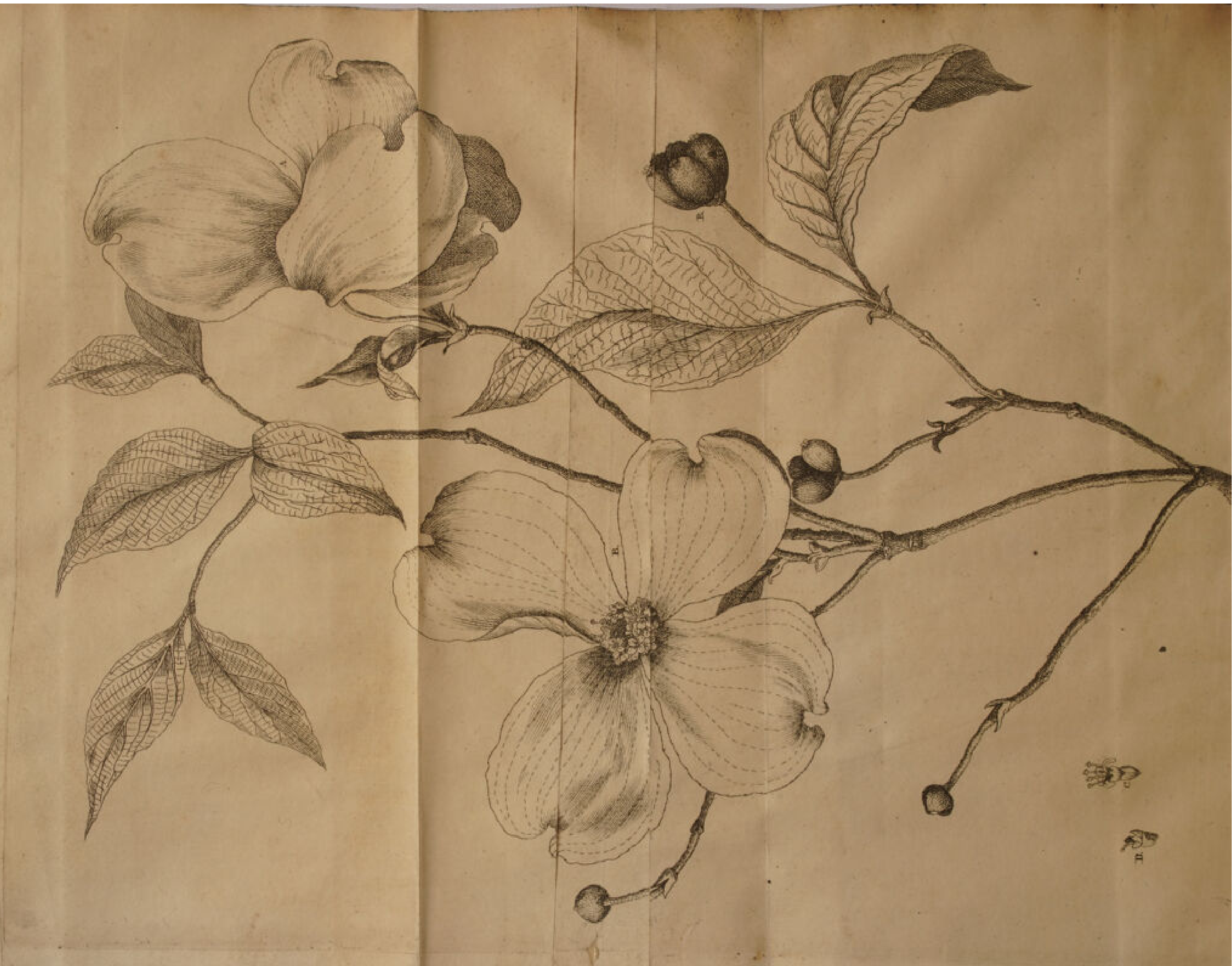
There is as yet one point remaining, which it is necessary for us to notice. The powerful action of the gastric fluid has been frequently mentioned in the preceding pages, as dissolving animal and vegetable matter: if animal substances are so readily dissolved by it, why does not this fluid also dissolve the stomach?

Hunter in the course of his dissections, frequently observed the large curvature of the stomach to be dissolved, particularly in such subjects as were carried off by sudden death: this dissolution he ascribes to the agency of the gastric fluid; but in the living body he supposed the vital principle of the stomach effectually resisted its action. This ingenious explanation has been objected to; the solution mentioned is said to arise from a putrefaction; that the same takes place in the intestines, where no such solvent fluid is to be found. Spallanzani though he appears to accede in a great measure to Hunter's idea, yet he observes, "too much is attributed to this principle." He supposes whatever possesses life is capable of resisting the action of the gastric fluid: his arguments by no means prove this. The following considerations will render the general proposition doubtful. Fish dissolve and digest living crabs, lobsters, &c. 'The leech is concocted by the human stomach, though it has no pores, and can sustain a temperature equal to that of man."

As the large frogs swallow animals alive, it was supposed they would answer very well to investigate this principle. As it was observed of the fish above, these large frogs without doubt swallowed living animals, and very soon digested them. But does the gastric fluid act upon them before death? The following experiment proved to us clearly it did not.

A packthread was tied to the leg of a living frog; it was then given to one of the large frogs, who immediately swallowed it. In five minutes it was drawn up, and as soon as it was extricated from the mouth of the large frog, it leaped away. It was swallowed a second time, and remained in the stomach for eight minutes, when it was drawn out; it was still alive, though quite unable to leap. It being swallowed a third time, it was examined in fifteen minutes afterwards, when it was dead. Upon opening the mouth of the large frog when it contained the lesser one in its stomach, the manner in which their prey was destroyed was easily explained; the passage of the œsophagus, though it can be greatly enlarged, yet it is completely closed by doublings of its substance, and thus effectually prevents any accession of air into their stomachs: their prey is therefore soon destroyed for want of air. The following experiments, we however hope, decisively prove that the living principle does effectually resist the action of the gastric fluid.

Two threads were tied around the fore legs of a living common sized spring frog; its whole body, except the head and fore legs was introduced into the stomach of one of the large frogs: the fore legs of the lesser were made to clasp around the lower jaw of the larger frog, and firmly tied in this situation; each of the threads were then tied to the fore legs of the larger frog also: thus situated it was impossible the small frog could be entirely swallowed down, or thrown out of the stomach of the larger one. They were then put in a basin containing a little water, where they remained undisturbed for one day and a half. The small frog upon being drawn out was perfectly alive, and its whole body covered with gastric juice, and not the least dissolved in any part. The same frog was then killed, and again introduced into the tomach of the large one in the very same situation as just described. In five hours it was drawn out, when its whole surface was completely diss,lved. Having this clear proof of the living principle, resisting the action of the gastric fluid, it was next proposed to try whether the action of this fluid would be resisted by a part in which the living principle was weakened. The great sciatic nerve of a living frog was therefore divided, and introduced into the stomach of a large one as above; it was drawn out in twelve hours, when it was still alive, and the paralysed extremity was quite sound.



AN EXPERIMENTAL INQUIRY

INTO THE

SIMILARITY IN VIRTUE

BETWEEN THE

CORNUS FLORIDA AND SERICEA,

AND THE

CINCHONA OFFICINALIS OF LINNÆUS:

TOGETHER WITH

AN INQUIRY INTO THE MODUS OPERANDI

OF

ASTRINGENT VEGETABLES

IN PREVENTING THE PUTREFACTIVE FERMENTATION.

SUBMITTED TO THE EXAMINATION OF THE

REVEREND JOHN ANDREWS, D. D. PROVOST PRO TEM.

THE TRUSTEES, AND MEDICAL PROFESSORS OF THE UNIVERSITY OF PENNSYLVANIA,

FOR THE DEGREE OF DOCTOR OF MEDICINE.

ON THE EIGHTH OF JUNE, ONE THOUSAND EIGHT HUNDRED AND THREE.

BY JOHN M. WALKER, OF VIRGINIA,

MEMBER OF THE AMERICAN LINNÆAN SOCIETY AT PHILADELPHIA, AND MEMBER OF THE PHILADELPHIA MEDICAL SOCIETY.

Every theory founded on experiment, and not assumed, is always good for as much as it will explain.——BURKE.

INTRODUCTION.

IT was an opinion early held in Medicine, that every country possessed an antidote to its diseases. The history of the Peruvian Bark tended greatly to confirm this, and if there be any justness in it this opinion may be again revived in America. For if Nature has given us, in our swamps and marshes, a Pandora's Box, she has in our forests given us a Cornu Copia: has she in our vallies sowed the seeds of disease, she has on our hills planted the Cornus Florida; or has she, more grievously to us, given wing to the volatile miasmata, that, under protean forms, attack us in a thousand ways, she has most providently scattered in our swamps the Cornus Sericea.

But I would not be understood to overvalue these provident gifts of nature, in implying specifics. Conscious of the short-lived reputation which exaggerated virtues beget for their medicines, I shall not deal in hyperbole; much less shall I detract from the Peruvian Bark, its justly accumulated praises, to heighten the blaze of contrast; nor shall I even introduce from obscurity into notice, inert undeserving vegetables:—Already have their virtues been perceived by the penetrating eye of professor Barton.*

I have only then to shew, upon analytical principles, what foundation there is for a similarity in virtue between the Cornus Florida and Sericea, and the red Peruvian Bark.

The most satisfactory mode of doing which, appeared to be that of subjecting the Corni to every experiment, to which the Peruvian Bark has been subjected. The desire of utility, and not the love of novelty, nor the merit which is generally annexed to it, could have prompted to such an analysis; for so many valuable experiments and observations have already been made on the Peruvian Bark, that little now remains to be added. My claim to indulgence must, therefore, rest upon the intention alone, and not the performance.

* Vide his Collections for an Essay towards a Materia Medica of the United States, p. 12.

Too long has America paid tribute to foreign countries; too often have her physicians been baffled in their practice from the adulteration of the bark. Every attempt, therefore, to liberate us from importation and obviate imposition is praiseworthy in itself; but how much more so must it be, when it is directed to our own too long neglected treasures. Flattering myself that your zeal to encourage such an attempt, will draw the mantle of forgetfulness over its imperfections, I shall proceed to the investigation of the subject, in prosecuting which I cannot follow a better method than the canon of Linnæus, "Systemate, Qualitate," Experimenta, " et experientia, eruitur omnis usus plantarum."

EXPERIMENTAL INQUIRY.

SYSTEMATE.

THE genus Cornus, to which the English have given the name of Cornel, or Dogwood, is arranged by Linnæus in the class and order Tetrandria Monogynia of his sexual system. In this artificial system it is associated with a number of genera, the species of which are often endued with properties very essentially different from those of the two species of Cornus, which form the principal objects of this dissertation.

In his work on the natural orders of vegetables the great Swedish naturalist has arranged the genus Cornus, in his forty-seventh order, to which he has given the name Stellatæ, or Stellate plants, a name originally applied by the illustrious Mr. Ray. In this order, the Cornus is associated with a number of genera, some of which are endowed with highly useful medical, or other properties. I shall here mention the names of some of them: they are galium, rubia, spigelia, coffea, and psychotria.

In the system of Mr. De Jussieu, the Cornus is arranged in the third order of his eleventh class, to which he has given the name of Caprifolia. Some of the principal genera with which it is here associated, are lonicera, triosteum, caprifolium, viscum, viburnum, and sambucus; and the medical properties of these, as far as we are yet acquainted with them, are not very similar to those of the species of Cornus which are about to claim our attention. The viscum, or misletoe, has, indeed, been recommended by some practitioners, as a remedy in intermittent fevers, and as a tonic in other diseases.

The following is Linnæus's abridged definition of the genus Cornus: Involucrum 4—phyllum sæpius. Petala supera, 4—Drupa Nucleo 2—loculari.

Of this genus, many species are now known to the botanists, and of these, several, especially of the North-American species, were unknown to Linnæus; with the Cornus Florida, and Cornus Sericea, however, he was acquainted.

The Cornus Florida, which is well known in many parts of the United States by the name of Dogwood, and less generally by that of Boxwood, is a very common vegetable in many parts of North-America. The following account of the Dogwood, is taken from an interesting manuscript copy of the Geographical History of the Plants of North-America, read to the American Linnæan Society, by Dr. Barton, president of said society. "The Cornus Florida grows as far north as latitude 45° and it extends as far south as latitude 28°. It is a tree of moderate stature. It does not in general attain the height of more than from eighteen to twenty-five feet. Sometimes, however, it makes a nearer approach to the more lofty trees of the forests, attaining to the height of at least forty feet, and near one foot in diameter." The trunk is strong and covered with a rough bark, which is much disposed to separate into longitudinal and transverse fissures or cracks. The wood is extremely hard and durable; hence it has received one of its names, that of New-England Boxwood. The branches are numerous and spreading, and sometimes placed opposite, but frequently by fours, arising from opposite points, and pretty regularly disposed. The leaves are oval, pointed, entire, and very veined. The flowers are produced at the extremity of the small branches in clusters, the individuals of which are more or less numerous. They consist of an involucre, which is composed of four very large obcordate folioles, of a fleshy texture, and of a fine white colour. The extremity of each foliole, is marked by a notch, which sometimes exhibits the appearance of having been bitten. These parts of the involucre are generally of a dusky rose colour. The flowers, which are situated in the centre of the large involucre (involucrum maximum of Linnæus) are very small and of a yellowish colour. The calyx or empalement is monophylous, or one leafed, very small and four toothed above. It is deciduous or falls off before the ripening of the fruit. The corolla consists of four petals. The stamens are four, and of the same length. There is one pistil consisting of a roundish germ, which is inferius or beneath. The style is filiform and nearly the length of the corolla. The stigma obtuse.

The flowers generally make their appearance, in Pennsylvania and other middle parts of the United States, about the beginning of May. They exhibit a most beautiful appearance; the large and white involucre forming a fine contrast with the green of the forest. They are succeeded by oblong drupes, commonly called berries, which are of a rich crimson colour. The berries ripen in September, and are the food of various species of birds, such as the robin, (turdus migratorius,) thrush, (turdus rufus,) &c. They have a very bitter taste, and a spirituous impregnation of them is much used as a morning bitter, and sometimes as a remedy in intermittents, in many parts of the United States.

EXPLANATION OF THE PLATE.

(A) A view of the four folioles of the involucre.

(B) An expanded flower, exhibiting the involucre, in the centre of which are placed the flowers.

(C) A single blossom exhibiting the calyx, petals, and four stamens.

(D) The four-toothed calyx, and the pistil.

(E) The ripe drupes, or berries.

The Cornus Sericea, or American Red-rod Cornel, agrees in its generic character, with the Cornus Florida. It grows in a moist soil, by the sides of creeks and rivers and seldom attains the height of more than ten or twelve feet. In general, a considerable number of stems arise from the same root and are very straight. The bark or rather epidermidal covering of the young shoots is very smooth, shining, and of a rich dark red colour. This circumstance added to the manner of growth of the stems, has procured to this species, the improper name of the Red Willow, by which name it is known in many parts of the United States. In Virginia it is called the Swamp Dogwood, and Rose Willow. The branches are placed opposite as are also the leaves, which a good deal resemble those of the Cornus Florida. Their under surface has a somewhat silken appearance, on which account Linnæus gave to this species the name of Cornus Sericea. The flowers are produced in clusters or cymes, of a whitish colour, and commonly make their appearance in June or July; they are succeeded by succulent drupes or berries, which are of a blue colour, inclining to green when ripe. They are eaten by different species of birds.

I am informed by professor Barton, that the Indians of North-Carolina used to scrape the inner bark of this tree and smoke it in their pipes, when their tobacco was scarce; and they would frequently mix the bark of it along with their tobacco to smoke. This habit would seem to imply something narcotic.

For an excellent figure of the Cornus Sericea, I may refer the reader to the late Mr. L'Heritier's valuable Monographia on the genus Cornus.

On the uses of the Corni in the arts, much might be said; but I shall merely enumerate a few of the most important purposes to which they may be applied.

The fine texture, hardness, whiteness, and durability of the wood of the Cornus Florida, renders it an important article to the cabinet-makers and joiners, in inlaying and ornamenting their various works. To the mechanics in general, in making the most durable handles to their instruments, their guages, squares, and plane stocks, &c. for all of which purposes, it is but little inferior to the English Boxwood. To the dentist it is no little acquisition, being the only kind of wood which will answer the purpose of plugging in transplanted teeth.

The straight slender red sprouts of the Cornus Sericea, are often employed for making baskets.

The bark of these two species of Cornus are very similar in those properties which interest the mechanic. The more abundant bark of the Cornus Florida will, therefore, recommend itself in making ink, tanning, dyeing, galling, &c. For the quantity of gallic acid in the bark of the Dogwood, intitles the latter to rival the oak and the galls in the above processes.

For the purpose of encouraging the use of our own indigenous vegetables, as well as lessening the necessity of importing the galls, I have here given the result of several experiments on the subject of making ink. By mixing,

½ oz. Pulv. Cort. Cor. Flor.
2 dr. Sulph. Iron.
2 sc. Gum. Arab.
16 oz. Aquæ Font.

an excellent black thin ink was made, fit for immediate use; and with which the whole MS. of this dissertation has been written. I must here observe that it is not necessary to be particular in choosing the particular parts or size of the part of the tree, in procuring the bark for this or medical purposes. I attended to this in the early part of my experiments, but I found the difference in virtue between the bark of the root, body, or branches of a moderate size, not to be worthy of notice.

QUALITATE.

The majority of practitioners of the present time, pay so little attention to this mode of investigating the medical qualities of plants, that I shall not be very minute on this part of my subject, which, according to the design and order above given, first presents together the Cornus Florida and Sericea, and the Cinchona* Officinalis for similar investigation.

Plants, like animals, have a considerable analogy in their structure; hence what is said on the bark of the one will apply, with but little exception, to the other two. A horizontal section of a branch of the Cornus Florida, exhibits an external thick covering or bark, which is perceptibly divided into three distinct rings or layers, the outermost of which is called epidermis,† the middlemost parenchyma, and the inner-

* For I deemed it unnecessary to say any thing on the botanical history of the Cinchona. It has been too long a favourite article in the materia medica, to require, now, an historical account.

† Grew and Malpighi, were the first who minutely examined the structure of vegetables, and gave names to the different parts. The latter was a celebrated anatomist and physiologist. Hence the above anatomical names.

most is called cortical; this latter is the part which is to occupy our attention in the present and subsequent divisions of this dissertation. The colour of this cortical part is, in the Florida, yellowish, in the Sericea, grayish, and in the Cinchona, red. Its taste is in all three of these vegetables nearly similar, though somewhat more bitter and astringent in the Corni than the Bark: the former when retained in the mouth sometime, only impart to the tongue these two tastes, along with a pleasant warmth; whereas when the latter is retained the same length of time, along with this bitterness and astringency, it imparts an indescribable taste, which will be easily recognized by every one who has taken the bark.

EXPERIMENTA,

INCLUDING THEIR CHEMICAL ANALYSIS.

So important is analysis to chemistry, that many, learned in that science, have honoured it by calling chemistry the science of analysis: and certainly no researches, in which man has been engaged, have unfolded the powers of his mind, in a more ample manner, than this part of chemistry. In its history we perceive the gradual developement of his intellect, in shaking off the trammels of Egyptian priests, and emerging from the veil of alchymical hieroglyphics. But in no view do we behold him more nobly engaged, than in tracing, link after link, that elective attraction, which binds together the particles of matter, and which, it would seem, were intended by nature to conceal them under eternal darkness.

But the destructive flame, while it conceals its own simplicity, developes the composition of those bodies on which it operates. By our acquaintance with this promethean torch, we approximate our works to those of nature, since we do not the less imitate her, by commencing where she terminates; and though we cannot equal her in composition, we certainly rival her in resolving bodies into their primordial simplicity. When has a natural analysis taken place, without destroying itself in obedience to synthetic laws?

But the chemist, when he has broken the vinculum of union in bodies, collects the separated particles, and reserves them for future investigation. This is the business of analysis. In the progress of this operation, two stages may be marked; first, that which resolves compounds into their proximate or secondary principles,* such as gums, resins, &c. and secondly, that which decomposes these again into their ultimate particles, as carbon, hydrogen, &c.

Since my object is to treat the Corni as articles of the materia medica, I shall only prosecute their analysis to the first stage; this being the only

* Fourcroy

one which enlightens the subject in such a view: for though plants by their diversity in size, colour, texture, and form, display to our observation characters apparently the most dissimilar, yet they afford, by their ultimate analysis, results so simple and similar to each other, as not to be the objects of the materia medica.

In prosecuting this analysis, I have laboured to be concise. To have detailed separately the various experiments, to which each individual article was subjected, would have extended this dissertation far beyond the ordinary limits of an inaugural thesis. I have, therefore, arranged such experiments, as admitted, into a condensed tabulated form.

I have likewise laboured to be accurate. Many experiments were performed in such manner, that the succeeding either confirmed or disapproved the accuracy of the preceding: thus, in ascertaining the solvent power of different menstrua, by weighing the menstruum before and after maceration on the article, the difference in weight shewed its solvent power; this was again confirmed by the residue, after evaporation, agreeing with the above difference.

The first experiment which I made, was distillation. Equal quantities, that is one ounce of the pulverised bark of the root of the Cornus Florida and Sericea, and of the red Peruvian Bark, were macerated in six ounces of water, in retorts, during six hours. Receivers were then luted to the retorts, and heat gradually applied, and continued until near half of the fluids had come over; the receivers were then changed, and empty ones adapted as before; the heat was then increased to the intensity which Argand's lamp would afford, and continued until the powders appeared, in the retorts, like dried cakes.

The products of the first distillation, of the Corni were transparent whitish fluids, possessing a slight aromatic odour, resembling new whisky, without any perceptible taste. They did not produce any change with the following re-agents, employed with the products of the second distillation. Knowing the etherial oil, which is the base of vegetable aroma, appears under different forms, I did not conclude against its presence in the Corni, because it did not swim on the surface of the distilled liquor. The whiteness of the fluid indicated the presence of something which I concluded to be essential oil, having its specific gravity, equal to that of the fluid, and might, therefore, remain in any part of the fluid, without rising or sinking.

The fluid, distilled from the Peruvian Bark, differed from the above in no respect, but in possessing a flavour not aromatic, but peculiar to the Bark. The fluid was clear and transparent.

The fluids, furnished by the last distillation, were more disagreeable in smell, with a taste somewhat acerb. Those from the Corni acquired a lemon colour; that from the Peruvian Bark was tinged with red.

The following is a synopsis of the changes which took place upon mixing the fluid, distilled from

	*With litmus paper.**	*Oxy-sulphate.*	*Acc. Lead.*	*Carb. Alumen.*
the Corn. Flor.	Red.	Black.	Precipitate.	Effervescence.
the Corn. Seri.	Red.	Black.	Precipitate.	Effervescence.
the Cort. Peru.	Red.	Brown.	Precipitate.	Slight Effervescence.

This experiment sufficiently proves that the virtues of the Corni and Peruvian Bark are for the most part fixed, and that each contains the gallic acid, though it is in greater quantity in the Corni than in the Bark, as is manifest from the difference in the colour, produced by their mixture with the oxy-sulphate. The gallic acid likewise comes over in distillation in an uncombined state, as appeared evident upon applying a piece of litmus paper, which had been dipped into the distilled fluid, with a similar piece taken out of a saturated solution of the carbonate of potash, a perceptible effervescence took place.

This experiment likewise adds some weight to those of Dr. Skeete, who with a boldness paramount to his ingenuity, has denied to the Peruvian Bark an aromatic quality, which has been conferred upon it for near a century, and which had been the foundation for many of its praises. Attentive to this circumstance, I conducted the distillation with a gradually increasing heat, furnished by Argand's lamp, which enabled me to regulate it in the manner I judged most favourable for the production of essential oil, but, upon examination, I could not detect the least quantity.

Discouraged at the result of this experiment, and recollecting the success of the ancient chemists, many of whom laboured thirty years in the distillation of dried vegetables, without obtaining a different result, I did not choose to prosecute the distillation farther, but preferred following the moderns in the employment of menstrua. For the improvement of this kind of analysis, the materia medica is greatly indebted to the labours of Bouldoc and Fourcroy, since it is this kind which has enriched it with some of its most important articles, as gums, resins, extracts, &c. The following table exhibits, in a synoptical view, the solvent power of water, under different modes of treatment. There is but little requisite to be said in explanation of it, since the slightest attention will render it intelligible. But it may here be observed, that the chalybeate solution, mentioned in the last column, was the oxy-sulphate of iron, dissolved in water, in the proportion of one drachm of the former to four ounces of the latter, which is the proportion to be recollected, whenever this solution is mentioned, and the proportions of the preparations tested, and the test is half an ounce of the former to half a drachm of the latter.

* Of the shops.

TABLE I.

SHEWING THE SOLVENT POWER OF THE SAME MENSTRUUM UNDER DIFFERENT MODES OF PREPARATION.

	ARTICLES EXPERIMENTED WITH.	Proportion of Solvend and Solvent in dr.	oz.	Time of preparation in Hours.	SENSIBLE QUALITIES. Colour.	Taste.	Weight of Menstruum per ounce in dr.	sc.	grs.	Quantity peroz. taken up in grains.	Colour with Chalybeate.
	Water employed in all the Experiments.						7	2	11		
EXP. II. Decoction of the	Pulv. Cort. Rad. Corn. Flor.	4	6	1	light red	bitter acerb	8	0	7	16	dark black
	Pulv. Cort. Arb. Corn. Flor.	4	6	1	light red	bitter acerb	8	0	$6\frac{1}{2}$	$15\frac{1}{2}$	black
	Pulv. Cort. Rad. Corn. Seri.	4	6	1	dark red	bitter	8	0	5	14	dark black
	Pulv. Cort. Arb. Corn. Seri.	4	6	1	dark red	and acerb	8	0	5	14	black
	Pulv. Cort. Peru. Rub.	4	6	1	red	bitter do.	8	0	$0\frac{1}{2}$	$9\frac{2}{3}$	dark
EXP. III. Infusion of the	Pulv. Cort. Rad. Corn. Flor.	3	5	24	red	bitter	8	0	$2\frac{1}{2}$	$11\frac{1}{2}$	dark black
	Pulv. Cort. Arb. Corn. Flor.	3	5	24	red	and acerb	8	0	1	10	ditto
	Pulv. Cort. Rad. Corn. Seri.	3	5	24	dark red	bitter	8	0	2	11	ditto
	Pulv. Cort. Arbo. Corn. Seri.	3	5	24	dark red	and acerb.	8	0	2	11	ditto
	Pulv. Cort. Peru. Rub.	3	5	24	red	ditto	7	2	16	5	black
EXP. IV. Hot triturated Inf. of the	Pulv. Cort. Rad. Corn. Flor.	4	6	$\frac{1}{2}$	light red	bitter	8	0	3	12	ditto
	Pulv. Cort. Arb. Corn. Flor.	4	6	$\frac{1}{2}$	light red	and acerb	8	0	$2\frac{1}{2}$	$11\frac{1}{2}$	ditto
	Pulv. Cort. Rad. Corn. Seri.	4	6	$\frac{1}{2}$	dark red	bitter	8	0	1	10	ditto
	Pulv. Cort. Arb. Corn. Seri.	4	6	$\frac{1}{2}$	dark red	and acerb	8	0	$1\frac{1}{2}$	$10\frac{1}{2}$	ditto
	Pulv. Cort. Peru. Rub.	4	6	$\frac{1}{2}$	light red	ditto	7	2	19	8	dark
EXP. V. Cold triturated Inf. of the	Pulv. Cort. Rad. Corn. Flor.	3	5	$\frac{1}{2}$	light red	bitter	8	0	6	15	dark black
	Pulv. Cort. Arbo. Corn. Flor.	3	5	$\frac{1}{2}$	light red	and acerb	8	0	5	14	ditto
	Pulv. Cort. Rad. Corn. Seri.	3	5	$\frac{1}{2}$	dark red	bitter	8	0	3	12	dark black
	Pulv. Cort. Arb. Corn. Seri.	3	5	$\frac{1}{2}$	dark red	and acerb	8	0	3	12	ditto
	Pulv. Cort. Peru. Rub.	3	5	$\frac{1}{2}$	light red	ditto	7	2	$18\frac{1}{2}$	$7\frac{1}{2}$	black

Though few comments upon this table are required, it will not be improper to point out some of the peculiarities which prevailed in these experiments, and which could not be noticed in the table. The difference in all these preparations is not so great as to require describing them in separate classes. I may therefore, add, that though the taste of the Corni is a more simple and agreeable bitter than the Peruvian Bark, it has nevertheless considerable austerity combined with it; the decoctions possess most of the latter, and the hot triturated infusions the next. I may likewise add that the decoctions and hot infusions were less elegant preparations; the hot menstruum held in suspension some of the fine powder, which was not entirely deposited by cooling, nor in passing through the filter.

This circumstance accounts for the generally received opinion, that heat increases the solvent power of water on vegetables in general, and in so considerable a degree, upon the Peruvian Bark in particular, that Dr. Skeete estimated the specific gravity of the decoction to be, to that of the infusion as five to two. In this part of the doctor's valuable observations on the Bark, his usual correctness must have deserted him. The result of the experiment in table 1st. article the last, in experiments the second and third, shew their specific gravity to be as, 1.900 to 1.000. Nor is this superior gravity of the decoction over the infusion owing, as the doctor supposed, to the superior solvent power of the hot over the cold menstruum, but to the mere suspension of the fine powder of the Bark in the menstruum, which becomes so incorporated with the gum and mucilage, by the heat increasing their liquidity, and by the agitation of boiling, that it is difficult to separate it afterwards. This is rendered probable by the after analysis of the gum. What could the superior darkness of the solution of the gum obtained from the decoction, be owing to, unless to something which was insolsuble in water and alcohol? Nor did the decoction manifest stronger virtues by its sensible qualities, or from the change it produced with a solution of the oxy-sulphate; nor did the effects produced on the pulse, hereafter to be mentioned, manifest its superior power. I shall not dwell longer on this part of the table, in pointing out the peculiar advantages which each mode of preparation has, but shall only add, that experiment the fifth shews the cold triturated infusion to be equal to any, and it has the advantage of expedition.

As the ascertaining the exact quantity of gum, resin, or extract, in any given quantity of the articles in substance, would be more curious than useful, and more difficult than either, from the tenacity of these substances to the vegetable fibre, I have* omitted such inquiry, while I

* For which omission, I may expect pardon, when I add, that Dr. Percival, whose patience was equal to his zeal for science, acknowledged that twenty-five coctions, and thirty macerations were insufficient to exhaust the virtues of the Cinchona.

have directed my experiments to the more useful investigation of what part of the vegetable their virtue resides, and the proportion which these parts bear to each other, in the more common preparations of infusions, tinctures, &c. The utility of such an inquiry must be palpable to every one who is acquainted with the powers which chemistry furnishes us, by which we may imitate, or modify these proportions when they shall be known.

Already much has been done by the artificial combination of bitterness and astringency; and though I have not the vanity to suppose I shall add any thing new, to the long sought for constitution of the Bark, yet my object will be answered in tracing its analysis with that of the Corni.

Experiment VI. The object of this experiment was, to ascertain the constituent parts of the gum-like mass, furnished by the evaporation of the decoction of the bark of the root of the Cornus Florida. Two drachms of this gum, which were furnished by seven and a half ounces of the decoction, were macerated in successive quantities of the best alcohol, until the last portion ceased to be changed in colour and taste; this, like the former portions, was separated from the gum by the filter; after the gum had dried upon the filter, it was collected, and weighed only half a drachm. The dried gum was then dissolved in a small quantity of water. Its solution was imperfect, not transparent, nor bright coloured; it possessed no particular taste, which might not be ascribed to its viscid consistence; and it produced no change of colour with a solution of the oxy-sulphate of iron. The want of transparency led me to suppose there might be some mucilage in the solution; to determine which I added, in small portions, diluted sulphuric acid to the solution: a precipitate slowly fell to the bottom in a coagulated form. When the precipitation had ceased, it was separated from the solution by the filter, and evaporated to dryness, at the same time with the solution. By weighing each residuum, I found the mucilage to be in the proportion of three to five, that is eighteen grains of gum, and twelve of mucilage.

I should here observe, that upon the addition of the acid, the solution turned dark, and that I do not ascribe the want of transparency in the gummy solution to the presence of mucilage entirely; but to the fine powder of the medicine, which the viscidity of the fluid suspended and concealed; and probably the change of colour, just mentioned was owing to the carbonation of these particles by the acid.

The alcohol which had been employed in the early part of this experiment was next examined, and found to possess an intense bitter taste along with considerable astringency: it produced an intense black colour with the oxy-sulphate of iron. Its colour was a beautiful dark red, not inferior to the tincture of kermes. By evaporating the alcohol I obtained a drachm and a half of what ancient chemists called saponaceous matter, but what I shall, after the example of Mr. Hermstadt,* to whom I am indebted for the suggestion of many of the re-agents employed in these experiments, call extractive matter; which he says may be distinguished from all other vegetable matter, by being soluble in water and alcohol, and not in sulphuric ether. But I must here observe, that this character is more extensive than Mr. Hermstadt appears to be apprized of, for it equally applies to the Tannin principle. May not Tannin be the extract of vegetables altered by the gallic acid? The above character of extractive matter suggested the maceration of it in ether, to ascertain whether the water, by boiling the medicines in substance, had taken any of their resin. Accordingly two ounces of sulphuric ether were poured on the ninety grains of extract, which, at the end of thirty-six hours, was separated by the filter; and this was repeated as often as the last portion of ether was changed in taste or colour; which changes indicated the presence of resin. After the last filtration the extract was suffered to dry upon the filter; when collected and weighed, it had lost only six grains of its former weight.

The ether had acquired a bitter taste, without astringency. It did not produce a black colour with a solution of the oxy-sulphate of iron. The evaporation of the ether left a brown resinous mass in the vessel, which weighed nearly six grains. From the difficulty, if not the impossibility, of accurately separating the tannin and the gallic acid from the extractive matter, at this stage of the analysis, I omitted the attempt, and preferred considering the extractive matter as a ternary compound, possessing a very bitter taste, along with considerable astringency; it produced a black colour with the oxy-sulphate.

The gum from the decoction of the bark of the root of the Cornus Sericea, and of the Peruvian Bark, were treated in a similar manner. Their ingredients, along with their proportions, may be seen in the following synopsis.

The sensible and chemical qualities of the decoction of the bark of the trees Cornus Florida and Sericea, differed so little from those of the roots of their respective trees, that I thought it unnecessary to analyse their gums in the above manner.

	Gum.	*Muc.*	*Res.*	*Ext.*		
In $7\frac{1}{2}$ oz. of decoc. of Cor. Flor. there is in grs.	18	12	6	84	making	120 grains of gum-like mass.
In $8\frac{1}{2}$ oz. of decoc. of Cor. Ser. there is in grs.	18	10	9	83	making	120 grains of gum-like mass.
In 13 oz. of decoction of Peruvian Bark,	20	6	14	80	making	120 grains of gum-like mass.

* Vide Med. and Phys. Journal of London.

TABLE II.

SHEWING THE SOLVENT POWER OF DIFFERENT MENSTRUA, PER OUNCE.

ARTICLES EXPERIMENTED ON		Proportion of Solved & Solvent. dr.	oz.	Time in Days.	Sensible quality. Colour.	Weight of Menstruum per ounce in dr.	sc.	grs.	Quantity taken up per oz. in grains
	SPIRIT VINI.				colourless	6	2	4	
EXP. VII. Tinc. with Spirit Vini.	Pulv. Cort. Rad. Corn. Flor.	3	6	9	light red	6	2	19	15
	Pulv. Cort. Arb. Corn. Flor.	3	6	9	light red	6	2	18½	14½
	Pulv. Cort. Rad. Corn. Seri.	3	6	9	deep red	6	2	16½	12½
	Pulv. Cort. Arb. Corn. Seri.	3	6	9	deep red	6	2	16	12
	Pulv. Cort. Peru. Rub.	3	6	9	dark red	7	0	2	18
	PROOF SPIRIT.				Light yellow	7	0	4	
EXP. VIII. Tinc. with proof Spirit of	Pulv. Cort. Rad. Corn. Flor.	4	6	8	lemon colour	7	1	4	20
	Pulv. Cort. Arb. Corn. Flor.	4	6	8	ditto	7	1	3½	19½
	Pulv. Cort. Rad. Corn. Seri.	4	6	8	deep red	7	1	0	16
	Pulv. Cort. Arb. Corn. Seri.	4	6	8	ditto	7	1	0	16
	Pulv. Cort. Peru. Rub.	4	6	8	dark red	7	1	3½	19½
	PORT WINE.				deep red	7	1	9	
EXP. IX. Vinous tincture of	Pulv. Cort. Rad. Corn. Flor.	4	6	8	increased	7	2	0	11
	Pulv. Cort. Arb. Corn. Flor.	4	6	8	ditto	7	1	18	9
	Pulv. Cort. Rad. Corn. Seri.	4	6	8	dark red	7	1	19	10
	Pulv. Cort. Arb. Corn. Seri.	4	6	8	ditto	7	1	18½	9½
	Pulv. Cort. Rub. Peru.	4	6	8	deep red	7	1	17	8

This table, as well as the former, proves the superior solubility of the Corni in aqueous and diluted spirituous liquors, while the Peruvian Bark is equal to it in alcohol. The inferior solubility of the Bark in aqueous fluids can only be owing to its possessing resin in the greatest quantity, which is sparingly soluble in such fluids; while the Corni possessing more gum and extract, are nearly soluble alike in water and alcohol. They are likewise more miscible in water than the Bark, as might be inferred from the latter possessing the greatest quantity of resin. And I may observe that the Cornus Sericea approaches nearest to the Bark, in the proportion of its constituent parts, as may be seen by an attentive examination of their analyses. To add more on their sensible qualities, would only be a repetition of the sensible qualities of the Bark, which, it is presumed, are sufficiently known; for, as far as I can perceive, they are exceedingly alike, their colours being different shades of red, and their tastes bitter and astringent.

The tenth experiment had for its object the separating the extractive matter taken up by the alcohol, from the resin. With this intention I macerated one drachm of the resinous mass of the Cornus Florida with repeated small quantities of sulphuric ether: the solution was very imperfect. The first and second portions of ether acquired a dark colour, the third was so little altered in colour, that I judged it had taken up all the resin. This portion of ether, like the other two, was separated from the insoluble mass by the filter, and mixed with them. The ether was now of a bitter taste, without much astringency; it did not strike a black colour with the oxy-sulphate: upon evaporation it afforded forty-five grains of resin, of a yellowish colour. The extract now collected from the filter was of a dark colour, with considerable bitterness and astringency; produced an intense black colour with the oxy-sulphate, and weighed fifteen grains. It is to be recollected that the small quantity of the medicine, employed in this experiment, to be tested, was equal in all these articles, and for which allowance has been made. The following synopsis exhibits that

In	Resin.	Extract.	making grs.
4 oz. of alcoholic tinc. of Cor. Flor. there is in grains	45	15	60
$4\frac{1}{2}$ oz. of alcoholic tinc. of Cor. Seri. there is in grains	$47\frac{1}{2}$	$12\frac{1}{2}$	60
4 oz. of alcoholic tinc. of red Peruvian Bark, there is in grains	51	9	60

I am here to observe that the extract is to be considered as a compound of extract, tannin, gum, perhaps a little mucilage, and the gallic acid; for alcohol has the property of taking up all these, in a small quantity, from vegetable matter.

A summary recapitulation of these experiments shew that the Cornus Florida and Sericea, and the Peruvian Bark possess the same ingredients, that is gum, mucilage, and extract, which last contains the tannin and gallic acid, though in different proportions. The Florida possesses most of the gum, mucilage, and extract; the Sericea the next, which appears to be an intermediate between the Florida and Peruvian Bark; while the latter possesses most of the resin. Their virtues appear equally similar in their residence. The extract and resin possess all their active virtues. The extract appears to possess all their tonic power. The resin when perfectly separated from the extract, appears to be purely stimulant; and probably the tonic power of the extract is increased, when combined with a portion of the resin, as in the spiritous tincture.

THEIR RELATIVE POWER OF RESISTING THE PUTREFACTIVE FERMENTATION.

By this term, which was first introduced into chemistry in the sixteenth century, by Van Helmont, is understood "that spontaneous decomposition which takes place in vegetable and animal substances, after death." Although the surprising phænomena which attend this natural analysis of bodies, were not unnoticed by the ancients, yet they remained for many years in the greatest obscurity. Nor was it until the close of the eighteenth century, that this cloud began to be dispelled, by the illustrious but unfortunate chemist, whose discoveries have immortalized his name by giving a new æra to chemistry. Lavoisier, the ornament of philosophy, and the boast of chemistry, taught us the nature of those gasses which have a principal share in this process. It was Lavoisier, for whose untimely fate philosophy mourned and chemistry acknowledged her loss, that gave origin to the present theory of chemistry, which now adorns the nineteenth century. But unfortunately for this subject, the rapid improvements which distinguish this age have not been equally extended to it. For notwithstanding the labours of a Macbride, a Percival and a Priestley, the complicated changes which take place in putrefaction are still enveloped in darkness and uncertainty.

Equally uncertain is the modus operandi of certain medicines in preventing or checking these changes, when they have commenced. I feel the less diffidence in declaring their manner of action to be the object of my present inquiry, since I shall not wander in the maze of theory, nor go a step beyond the broad basis of experiment. And as "every theory founded on experiment, and not assumed, is always good for as much as it will explain," I shall confine my observations to astringent vegetable substances.

Since the time of Anaximenes, with whom nature's great law* was uniformity, philosophers and chemists have united in acknowledging the homogeneousness of nature's works: from this universal principle sprung the co-extensive law of chemistry, that every body has either an efficient or predisposing affinity for every other. These affinities are changeable into each other, and upon their mutual conversion, which, however, is modified or prevented by every possible variety of circumstances, depends

* Enfield's History of Philosophy.

those secret and wonderful operations of nature. Thus the sugar in the matured grapes, possesses, within their integuments, a predisposing affinity for oxygen, and no sooner do the circumstances of its developement into a fluid, the access of air, and increase of heat, take place, than this predisposition is changed into efficiency, which constitutes the vinous fermentation, whose degree and continuance is modified by the paucity of sugar, or deficiency of heat. Hence the vast variety of alcoholic products. Or it is changed by the too long continuance of these circumstances, or their existing in too favourable a degree, which facilitates the too speedy union of the oxygen with the carbon of the sugar, and thereby reduces it to mucilage, by the decarbonation of the sugar in the form of carbonic acid. Hence the acetous fermentation. Or this conversion of affinities may be again changed by the too great heat, too free access of air, or too great fluidity, which, while it evolves the gluten of the vegetable, conducts to the putrefactive fermentation, which ends in the volatilization of the ingredients. Or, lastly, this change may be prevented by destroying the efficient affinity of the sugar, mucilage, or gluten, for oxygen. Thus has nature, in stamping similarity on her productions, planted within them the germ of their destruction; thus does she make affinity the principle of synthesis and the cause too of analysis; and thus does she accomplish the perpetual circle of compositions and decompositions, which demonstrates her fecundity, while it anounces equal grandeur and simplicity in her operations. Hence the difference between nature and art. Nature is rich in poverty; art is poor in riches. Nature has few materials,* her works are innumerable; art has many, her works are few.

Such are the principles of pneumatic chemistry, which teach us, we have only to substitute gelatin for gluten, to make the above illustration apply to animal putrefaction.

The illustration already given shews us too, the foundation for two kinds of antiseptics, which may be called mechanical and chemical. The enveloping the gelatin in a body impermeable to air, as resin, or the condensing the particles of the gelatin within the sphere of a too strong attraction, by boiling and drying, as in making portable soup, belong to the first, while those bodies, which present to the gelatin an affinity superior to that for oxygen, belong to the latter; such are astringent vegetable substances.

The first experiment which was made on this class of vegetables, was with a view to ascertain the comparative antiseptic power of the Corni and Peruvian Bark. Accordingly four drachms by weight of fresh veal, were immersed in equal quantities, that is two ounces of the filtered infusion of the Cornus Florida and Sericea, and the Peruvian Bark, in separate

* Or elements.

tumbler glasses. The immersion of the veal soon occasioned a precipitate, of a grayish colour, in the infusion from the Corni, and reddish in that from the Peruvian Bark, and also of unequal quantities, being greater in the Florida and next in the Sericea. The result of this experiment (see table 3. exp. 11.) shewing considerable proportion between the antiseptic power of the articles, and the precipitates, induced me to ascribe the antiseptic power, to the precipitate; which, upon examination, was found to be the tannin principle; a substance to which our attention was first called by Sequin, and afterwards investigated by Proust.

The changes which the muscular fibre underwent, greatly confirmed this opinion; its juices were soon decomposed, the red blood lost its colour, and the fibres appeared corrugated and condensed, though these changes were less in the infusion from the Peruvian Bark. These changes rendered it very probable that the gelatin of the fibre, and of the blood united to the tannin, and formed a compound capable of resisting putrefaction in proportion to the quantity of the tannin present: for on the sixth day, at which time the veal in the infusion from the Peruvian Bark was offensive, a portion of veal was taken out of the infusions from the Cornus Florida and Sericea, which was perfectly sweet; when dry I found it pulverable between the fingers, but could not trace the fibrous texture: so intimate was the union with the tannin. For though there was a difference in this respect, between the muscle and tanned leather, I nevertheless ascribed the change to the tanning process, judging the difference in appearance and qualities, to be owing to the difference in texture.

To prove this more decisively, I precipitated the tannin from fresh infusions of the medicines, by a saturated solution of glue. The liquor was then separated from the precipitate, formed in this manner, by the filter, and four drachms of fresh veal were added, as in the former experiment, but with a very different result. Its antiseptic power was destroyed. See table 3. exp. 12.

It is unnecessary to dwell longer on this part of the experiment, since the following table shews the experiments which were made, and the changes which took place.

For the better understanding the table, it is to be recollected that all the infusions of the different medicines were made under similar circumstances, and in similar proportions. Thus the simple infusion employed in experiments 11, 12, 13, 14, were made with two drachms of the powder, and four ounces of water, macerated twenty-four hours, and then filtered. The compound infusions were made by triturating two drachms of the powder, and one drachm of the calcareous earth, with six ounces of water; which, after standing the same length of time, were filtered.

TABLE III.

SHEWING THE ANTISEPTIC POWER OF THE INFUSIONS OF DIFFERENT MEDICINES, IN THE TEMPERATURE OF ABOUT 72° OF FARENHEIT'S THERMOMETER.

Changes which took place (the figures stand for days.)

	MEDICINES.	Proportion of Infus. oz.	Proportion of Veal. dr.		The relative quantity of Tannin. A	Change with Gallic acid. B	Time it remained sweet. C.
EXPERIMENT 11, Simple infusion of the	Simp. aqua.	2	4	1 Pale, 2 texture diminished and greenish, 4 putrid.			
	Cort. Rad. Corn. Flor.	2	4	1 Copious precipitate, 6 sweet, fibre condensed, 15 inf. acid, 16 mouldy on the surface, taken out and dried, sweet.	10	pale brown.	26
	Cort. Arb. Corn. Flor.	2					
	Cort. Rad. Corn. Ser.	2	4	1 Copious precipitate, 6 sweet, fibre condensed, gray colour, 14 acid and mould infusion, and nearly evaporated, taken out, on 15.	9	ditto	26
	Cort. Arb. Corn. Ser.	2	4				
	Cort. Peru. Rub.	2	4	1 Small precipitate, 4 fibre lax and pale, 6 greenish, 8 penetrating smell.	4	ditto	14
	Cort. Querci. Rub.	2	4	1 Copious precipitate, 10 sweet, 15 solid and firm, 18 infusion mouldy.	15	ditto	30
	Rad. Columb.	2	4	1 No precipitate, 2 pale and soft, 4 offensive.	0	ditto	7
	Pulv. Gallae.	2	4	1 Copious precipitate, 4 condensed, 8 sweet, 10 mould infusion, 18 acid.	12	brown.	30

Changes which took place when the tannin was precipitated.

EXP. 12. Simple inf. of	Cort. Rad. Corn. Flor.	2	4	1 No precipitate, nor corrugation of fibre, pale, 8 green, 5 putrid.
	Cort. Rad. Corn. Ser.	2	4	1 Ditto pale and relaxed, 3 greenish, 4 unpleasant smell, 5 putrid.
	Cort. Peru. Rub.	2	4	1 Ditto, 3 greenish colour, soft, 4 unpleasant, 5 putrid.

TABLE III. &c.—CONTINUED.

Changes which took place upon mixing the Inf. with Putrid Veal.

	MEDICINES.	Inf.	Put.		A Quantity taken up.	B Change with acid. Oxalic.	C Change with acid. Gallic.	D Sensible quality, taste.
EXP. 13. Simp. infusion of	Cort. Rad. Corn. Flor.	2	4	1 Fermentation suddenly checked, 4 sweet, 6 hardened, 10 acid.				
	Cort. Rad. Corn. Ser.	2	4	1 Ceased disengaging air, 2 hardened, 6 sweet, 10 acid smell.				
	Cort. Rub. Peru.	2	4	1 Suspended, 2 colour pale, 4 discharges air, 6 offensive.				
				Changes upon mixture with fermenting mass.				
		Inf. oz.	Mass. oz.					
EXP. 14. Simp. infusion of	Cort. Rad. Corn. Flor.	2	2	1 Fermentation checked, 4 dark coloured, 6 acetous smell.				
	Cort. Rad. Corn. Ser.	2	2	1 Checked the disengagement of air, 6 acetous smell.				
	Cort. Peru. Rub.	2	2	1 Moderated the fermentation, 4 acid to taste and litmus.				
		Infus. oz.	Veal. dr.					
EXP. 15. Comp. infus. with cal. lime, and	Cort. Rad. Corn. Flor.	2	4	1 No precipitate, 3 fibre lax, 4 bluish, 5 putrid.	19	precipitate.	black	Nauseous without astringency.
	Cort. Rad. Corn. Ser.	2	4	1 Ditto slight change in colour, 5 putrid.	18 1-2		black	
	Cort. Rub. Peru.	2	4	1 Ditto muscle reddish, 4 soft and offensive.	10		black	
EXP. 16 Com. inf. with cal. mag. and	Cort. Rad. Corn. Flor.	2	4	Nearly the same changes took place in this and the 17th exp. as in the 15th, all putrid in 5 1-2.	20	ditto	ditto	ditto
	Cort. Rad. Corn. Ser.	2	4		18 1-2			
	Cort. Rub. Peru.	2	4		11			
EXP. 17. Comp. inf. with carb. mag. and	Cort. Rad. Corn. Flor.	2	4	ditto	19	ditto	ditto	ditto
	Cort. Rad. Corn. Ser.	2	4		18			
	Cort. Rub. Peru.	2	4		10			

COMMENTS

ON THE CLASSES OF EXPERIMENTS CONTAINED IN THE PRECEDING TABLE.

STRONGLY impressed with the justness of the principle, which the great Newton laid down on experimental evidence, which is, that "though the arguing from experiments and observations, by induction, be no demonstration of general conclusions, yet it is the best mode of arguing which the nature of things admit of,and may be looked upon as so much the stronger, by how much the induction is the more general;" I have, to profit by the authority of so great a philosopher, extended my experiments to several articles which were not the objects of my dissertation; the plain induction from all of which, is, that the tannin principle of astringent vegetables, which is thrown down by the gelatin of the flesh, is the antiseptic principle; and that it acts by chemically uniting with the gelatin, and thereby destroying the efficient affinity of the latter for oxygen, which is the septic principle. The changes which the muscle underwent in the infusions, as far as they could be expressed in the table, agree with the quantity of tannin present in those infusions, as expressed under A. experiment 11. The relative quantity of tannin, expressed in this column, was ascertained by adding equal quantities of a saturated solution of gelatin to equal quantities of the different infusions concentrated by partial evaporation, the quantity of precipitate showed the relative quantity of tannin: letter B. of the same experiment, shews that the gallic acid is precipitated along with the tannin, when gelatin is the precipitant; for upon the addition of the oxy-sulphate of iron, to the infusion out of the tumblers, a very slight change took place.

C. This shews the length of time the simple infusions will remain free from acidity, in the common stopped phials; the figures stand for days. Why those infusions which have the greatest quantity of tannin, should remain unchanged the longest, is difficult to account for; unless it be that the tannin principle has a stronger affinity for oxygen than the mucilage of the infusion, upon the union of which with oxygen, acidity depends. This is somewhat probable from the circumstance that tannin will reduce the oxy-sulphate of iron to the common sulphate. May not this be the cause of the change of colour of the blood and muscle, above mentioned? The tannin uniting to the oxygen of the blood deprives it of that principle, to which the present chemico-physiologists ascribe its red colour.

What farther can be required to prove the above manner of accounting for the antiseptic power of astringent vegetables, is given in the 12th experiment, which proved, that the previous precipitation of the tannin destroys this power.

Experiment 13th shews, that these medicines check putrefaction, after it has commenced, in the same manner and proportion. The 14th, shews they prevent or check the acetous fermentation, probably by absorbing oxygen from the mucilage of the fermenting mass.

Experiments 15, 16, and 17. B. The precipitate which takes place upon adding the oxalate of ammoniac to these compound infusions, shews that the calcareous earth is dissolved in them, which, by forming a new compound with the tannin, destroys* its astringency and antiseptic property. C. D. The results in these two columns, agree with the experiments of professor Woodhouse.† For though the astringency of the compound infusion was destroyed by the mixture with the calcareous earth, it nevertheless struck a black colour with the oxy-sulphate, but did not possess the taste of astringency, nor corrugate the flesh immersed in it.

A. Soon after the introduction of the Peruvian Bark into practice, physicians not content with its solubility in aqueous menstrua, combined many different articles with it, to increase its solubility, and virtue in other respects; among which were magnesia, lime, &c. These experiments prove, that whatever the combination with lime or magnesia, may do in other respects, its solubility cannot be said to be increased; for B proves the superior weight of the infusion to be owing to the calcareous earth; D, that its astringency is destroyed; and the 15th, 16th, and 17th experiments at large, prove that its antiseptic power is also destroyed.

Experiment XVIII. On the styptic power of the Corni, and Peruvian Bark. With a view to ascertain this, I cut out three portions from the glutæi and vasti externi of a dog, with a dull scalpel to imitate common incised wounds, on which I sprinkled the three powders; the Florida and Sericea soon stopped the hæmorrhage; the Peruvian Bark was slow in stopping it; the blood and powder appeared to be chemically united, and formed a defence to the open tubes; the Cornus appeared not only to act upon the surface of the wound, in corrugating it, and thereby diminishing the orifice of the bleeding vessels, but the tannin likewise precipitated the gelatin of the blood, with which it formed an adhesive mass, that remained on the surface of the wound.

EXPERIMENTS ON THE TANNIN PRINCIPLE.

It is not my intention to give, in this place, a treatise on the art of tanning. But as the Corni promise to be profitably subservient to this art, and as there has lately appeared on this subject some ingenious speculations, which, as well from the author, as their own merit, deserve attention, I hope the following digression will not be unacceptable.

* Vide sixth Law of the Affinity of Composition.

† See his Observations on the Combination of Acids, Bitters, and Astringents.

Though the art of tanning is of ancient date, yet the tannin principle is of modern discovery, with the particular nature of which, we are not well acquainted; chemists, however, suppose it to be a distinct principle in vegetables. The ancients were content with ascribing this property to such vegetables as contained a gum-resinous matter, along with astringency; and their choice of tanning substances was chiefly confined to such vegetables. Though an established opinion and long practice in any art, only give sanction, without proving the correctness of such opinions; yet I am convinced the choice of the ancients was very judicious.

The ingenious Mr. Biggins, supposes the tannin principle is all that is necessary for this process of tanning; and "as the gallic acid* corrugates the surface, and does not seem to combine with the matter of skin," he thinks "it not only useless but detrimental."† Induced by the novelty and ingenuity of this opinion, I instituted the following experiment, to see how far it would obtain in practice. I obtained some pure tannin, by partially evaporating a strong decoction of the Cornus Florida, and adding to it a saturated solution of the carbonate of potash; a copious precipitate fell down, which was collected upon the filter, afterwards washed in a small quantity of cold water, then dissolved in the like quantity of boiling water. This was tested with litmus paper to detect the excess of alkali; the paper being slightly changed green. Diluted sulphuric acid was now added until the litmus paper indicated it to be neutralized. A grayish precipitate began again to appear, which increased by standing. When it had ceased subsiding, it was again separated by the filter, and then dissolved in a small quantity of cold water, to which was added a small piece of fresh calf-skin, previously deprived of the hair and small pieces of flesh, by the ordinary means for such pursoses. It was examined on the sixth day, but there was no appearance of the action of the tannin; and the skin appeared as unaltered as if it had lain the same length of time in pure water; it was soft, white, and slippery between the fingers, and had undergone what tanners call swelling. Convinced that the tannin thus separated, differed from the tannin in the fresh decoction in no other respect than the deprivation of the gallic acid, I determined to see what effect another vegetable acid would have. Strong acetous acid was accordingly added to it, under the above circumstances. The changes which took place in the skin in ten days afterwards, were so similar to such as appeared in a similar piece of skin, which had been immersed in some of the decoction‡ from which the tan-

* It is here to be understood that Mr. Biggins means by the gallic acid, the astringent principle. † Philosophical Transactions.

‡ I shall here give the reason, why the decoction was preferred as a standard. From the experiments of Messrs. Davy, Proust, and Sequin, it appears that heat evolves the tannin principle: their experiments were made chiefly upon coffee; they

nin had not been precipitated, and which had been employed as a standard by which to judge of the facility of the two processes, that I could not forbear concluding, that astringency was essential, in the process of tanning. I should here observe that the gallate of tannin (for they unite together by a strong affinity) is very astringent. Now as the gallic acid, though somewhat acerb in taste, cannot, from its weakness in this respect, be strictly called astringent, it is probable that the tannin contains some alumen, the union of the gallic acid with which, Dr. Woodhouse has satisfactorily proved to be astringent: this is somewhat confirmed by the increased astringency of the acetite of tannin; for the potash above employed, by saturating the gallic acid, might have precipitated the alumine along with the tannin, and which, by uniting with a portion of the acetous acid, might have increased its astringency.

In what manner astringents act in tanning is difficult to be explained; but it appears probable that they serve the same purpose in tanning, which mordents do in dyeing; they fix the tannin and gelatin to the cuticular fibre.

The success of the acetate of tannin above mentioned, gives considerable countenance to the conjecture already made, that the tannin principle is the mere extract of vegetables altered by the gallic acid.

Under this impression I endeavoured to imitate the tannin principle by uniting the extract of common flour* with weak alum-water, to which mixture a thin piece of skin, properly prepared, was added: the changes which took place, being perfectly similar to the tanning process, convinced me that this artificial combination of extract and acid possessed the true properties of the natural tannin.

Hence I conclude, astringency is essential in tanning.

The present subject involves the consideration of astringency in so palpable a manner, that I cannot, though I willingly would, have passed it over in silence; for astringency is a problem that has never been satisfactorily solved; nor do I pretend to such a solution, but only claim the privilege of opinion.

found that a strong infusion of this article did not exhibit any marks of the tannin principle, nor of the gallic acid; but if it was first toasted, or a decoction made from it, the liquor thus prepared contained the tannin principle, and struck a black colour with the oxy-sulphate of iron. Is it not probable that the heat produced this change, by increasing the affinity of the base of the gallic acid, for oxygen, which was furnished by the air in toasting, or the water in boiling, and thus produced the gallic acid, which united to the extractive matter, and formed the tannin principle. However this may be, it is worthy the attention of tanners, for I am convinced from the experiments I have made, that thin skins may be perfectly tanned, in the decoction of the Corni or oak bark, in ten days.

* In many parts of our country, the country people tan thin leather, by first immersing it in flour and water several days, and then placing the skin in alum-water.

From the experiments of authors, as well as those which I have made upon this subject,* I am convinced we cannot limit astringency to the combination of a single acid, the gallic, with a calcareous base. Astringency, like bitterness, is the result of many binary combinations. Its production depends upon those secret changes which take place upon the union of bodies, in conformity to the sixth law of the affinity of composition.† Limited would be the expression, that the sulphate of magnesia was alone the bitter principle; equally limited must it be, if it affirms the gallate of alumine to be alone the astringent principle. Are not the sulphate, the muriate, the nitrate, and acetate of alumine, astringent? And what experiments have detected the presence of the gallic acid in these salts? or, how many detections would convince us of its existence, when we know that the sulphuric, muriatic, and nitric acids, convert the gallic into citric, malic, or oxalic acids; as little do we know of the tests for the astringent principle. Doctor Woodhouse, whose ingenious labours have greatly enriched chemistry, proved in a pamphlet, which has already been quoted, that the property of producing a black colour with the chalybeate solution was fallacious. Taste has been supposed the most certain criterion of its presence. But so little do we know of the modus operandi of astringents on the organ of taste, or on the solida viva, that even with this we are subject to fallacy. Thus, alcohol, by attracting the saliva from the surface of the mouth, produces a taste similar to that of astringency. The dry air, by favouring the evaporation of the saliva, produces a similar taste, and were it not for our senses correcting the deception of taste, we should taste astringency whenever we walked in dry air.

Many other articles, by stimulating the absorbent system, the excretories of which being more simple and facile in motion than the secreting, evacuate their tubes as fast as they are filled, and consequently produce corrugation of the part, by the sides of the emptied tubes approaching each other, which disposition of parts constitutes the astringent taste. This opinion, that astringents act upon the solida viva, in consequence of their stimulus, is not a novel one. Percival and Darwin, long since, entertained it. What are we then to conclude respecting astringency;

* I thought it unnecessary to introduce the experiments which I made on the astringent principle, because their results only agreed with the experiments of Dr. Woodhouse, that the gallate of alumine was astringent. But they, in no manner, tended to shew that astringency was an undivided principle, confined to a single neutral salt. But, on the contrary, every salt, of which alumine was the base, was astringent to the taste, and many other articles are confessedly astringent.

† According to which, compounds possess properties different from their component parts.

shall we, with Dr. Moore,* deny their existence; or, shall we, with more probability, acknowledge it the property of many combinations.

EXPERIMENTS ON THE HEALTHY SYSTEM.

The following is a synopsis of the effects of the different medicines, and their different preparations, on the healthy human body. And I must here observe, that the greatest attention was paid to obviate those circumstances which affect the pulse, by myself, as well as by my friendly fellow graduates, who assisted me in these experiments; and whose names I with pleasure insert to be Messrs. Massie, Downey, Wilson and Young, and my friend Mr. Gregg.

To avoid unnecessary prolixity, I have only expressed the quantity and kind of medicine taken, in each experiment, without inserting the name, time of day, &c. since it is to be presumed that every circumstance was attended to, which could be favourable to the success of these experiments. And to render them as satisfactory as possible, I have noted down the state of the pulse, and the affection of the system in general, opposite to the time when they took place.

A SYNOPSIS OF THE EXPERIMENTS ON THE PULSE.

EXPERIMENT 20.			EXPERIMENT 21.			EXPERIMENT 22.		
30 gr. Pulv. Cort. Rad. Cor. Flor.			30 gr. Pulv. Cort. Rad. Cor. Seri.			1 gr. Pulv. Cort. Per. Rub.		
Min.	Pulse.		Min.	Pulse.		Min.	Pulse.	
0	62	soft, natural	0	70	naturally full	0	66	natural
5	62	slight change	5	72	quick, soft	5	66	no change
10	63	full, heat at	10	73	do.	10	67	quicker, ful-
15	63	stomach	15	74	slight nausea	15	67	ler do.
20	65	quick and full	20	76	full	20	68	full and
25	66	full and strong	25	76	nausea ceased	25	69	tense
30	68	do.	30	78	full and tense	30	69	do.
35	69	quick tense	35	78	do.	35	70	do.
40	70	flushed face	40	79	do. red face	40	70	strong and
45	70	tense, heat in-	45	79	regu. hard	45	71	regular
50	70	creased	50	78	full	50	71	do.
55	70	do.	55	78	do.	55	72	do.
60	70	do.	60	78	do. head-ach	60	72	do.
75	68	full, regular	75	77	do. quick	75	71	slight head-
85	68	do.	85	75	do.	85	70	full [ach
95	65	reduced in	95	73	slight	95	68	do.
105	63	fulness.	105	70	decrease.	105	67	nearly natural

* Vide his Materia Medica.

EXPERIMENT 23.		EXPERIMENT 24.		EXPERIMENT 25.	
* 12 gr. of the Resin of Cor. Flor.		* 12 gr. of the Resin of Cor. Seri.		* 12 gr. of Resin of P. Bark.	
Min.	Pulse.	Min.	Pulse.	Min.	Pulse.
0	62 soft, natural	0	72 natural	0	64 natural
5	62 slight disgust	5	72 small	5	64 full
10	61 nausea	10	72 do. and	10	65 and
15	61 in a slight deg.	15	73 quick, consi-	15	66 regular
20	62 quick and	20	75 derable naus.	20	68 do.
25	63 fuller	25	75 fuller	25	68 do.
30	63 quick and	30	76 full	30	68 full and
35	66 frequent	35	77 do.	35	69 tense
40	68 do.	40	77 do.	40	69 do.
45	70 do.	45	79 do. tense	45	70 do.
50	72 do.	50	80 do.	50	71 regular
55	73 do.	55	82 tense, regular	55	72 do.
60	71 do. slight	60	82 & flushed face	60	72 do.
65	69 head-ach	65	80 heat of the skin	65	73 full, flushed
70	67 full	70	79 tense	70	73 face
75	68 irregular	75	79 do.	75	70 do.
80	65	80	77 full and softer	80	66 do.
85	63	85	75	85	65 diminished in
95	60 small	95	72	95	65 fulness.

EXPERIMENT 26.		EXPERIMENT 27.		EXPERIMENT 28.	
† 12 grs. of Extract of Cor. Flor.		12 grs. of Extract of Cor. Ser.		12 grs. Extract of P. Bark.	
0	68 natural and	0	76 natural	0	66 natural
5	68 soft, full	5	76 soft and full	5	60 full and
10	69 quick and	10	77 quicker	10	67 strong
15	70 full	15	78 anxiety	15	68 quick
20	70 do.	20	78 full and	20	69 do.
25	72 do. agreeable	25	79 regular	25	69 do.
30	73 heat, tense	30	80 tense, flushed	30	69 do.
35	73 and regular	35	81 face	35	70 full and
40	74 do.	40	81 do.	40	70 tense
45	76 do.	45	82 do.	45	72 do
50	77 fuller, tense	50	82 do.	50	72 do.
55	77 do. flushed	55	80 do.	55	72 do.

* Obtained by the simple evaporation of the alcoholic tincture : for its constituent parts, vide p. 315.

† See Chemical Analysis, p. 315.

Min.	Pulse.		Min.	Pulse.		Min.	Pulse.	
60	77	flushed face	60	79	diminished in	60	71	full and tense
65	77	do.	65	79	hardness	65	70	do.
75	76	hard and tense	75	78		75	68	diminished in
95	69	stro. than nat.	95	77	soft and full	95	67	strength

EXPERIMENT 29.			EXPERIMENT 30.			EXPERIMENT 31.		
† 12 grs. of Gum of Cor. Flor.			12 grs. of Gum of Cor. Ser.			12 grs. of Gum of P. Bark.		
0	62	natural	0	64	natural	0	72	natural
5	62	slight change	5	65	full and	5	72	no change
10	63	in fulness	10	65	regular	10	73	fuller
15	65	full and quick	15	67	quicker	15	75	do. and
20	66	do.	20	67	and fuller	20	75	quicker
25	67	do.	25	68	do.	25	77	tense
30	70	fuller and	30	71	tense and	30	78	do.
35	70	regular	35	71	strong	35	80	do.
40	70	do.	40	72	do.	40	81	fuller and
45	71	moderately	45	73	do.	45	82	stronger
50	70	tense	50	71	slight change	50	82	do.
55	70	do.	55	70	full	55	82	do.
60	69	do.	60	69	do.	60	80	quick and full
65	69	do.	65	68	do.	65	79	do.
70	68	soft but full	70	68	quicker than	70	76	do.
75	64		75	67	natural.	75	75	above natural.

EXPERIMENT 32.			EXPERIMENT 33.			EXPERIMENT 34.		
2 oz. of Decoction of Cor. Flor. R.			2 oz. of Decoction of Cor. Ser. R.			2 oz. of Decoction of P. Bark.		
0	66	natural	0	68	natural and	0	62	natural
5	67	increase in	5	68	quick	5	62	soft
10	68	strength and	10	69	fuller	10	63	do.
15	68	fulness	15	70	do.	15	65	quicker
20	70	do.	20	71	do. quicker	20	65	with fulness
25	72	do.	25	71	do.	25	67	do.
30	72	do.	30	73	do.	30	68	do.
40	77	tense and	40	74	do.	40	69	considerable
50	80	strong	50	74	flushing of	50	71	tension
60	80	bounding	60	76	the face	60	72	do.
70	78	slight pain in	70	80	full and	70	73	do.
75	77	the head	75	79	strong	75	71	tense with

† By evaporating the aqueous solution: for its component parts, see p. 312.

Min.	Pulse.	
80	74	and flushing of
90	73	the face
100	67	quick and soft

Min.	Pulse.	
80	76	slight affection
90	73	of the head
100	70	quick pulse.

Min.	Pulse.	
80	77	flushing of the
90	69	face
100	66	full and quick.

EXPERIMENT 35.

2 oz. of Infusion of Cor. Flor. R.

0	72	natural
5	73	quicker
10	74	do.
15	74	do.
20	75	fuller
25	76	do.
30	78	full and
40	79	tense
50	81	do.
60	84	do.
70	84	strong
75	83	do.
80	81	do.
85	79	flushing of
90	78	the face
100	75	above natural.

EXPERIMENT 36.

2 oz. of Infusion of Cor. Seri.

0	76	natural
5	76	slight change
10	79	in quickness
15	80	fuller
20	81	quick and full
25	82	do.
30	84	do.
40	84	tense
50	85	do.
60	87	do.
70	85	do.
75	84	do.
80	82	heat of the skin
85	79	
90	79	quicker.
100	77	

EXPERIMENT 37.

2 oz. of Infusion of P. Bark.

0	64	natural
5	64	no change
10	66	quicker
15	68	do.
20	69	fuller
25	70	do.
30	72	tension
40	73	increased
50	74	do.
60	76	full and
70	76	tense
75	76	do.
80	74	do.
85	74	full
90	73	do.
100	66	above natural.

I have but little to add on these experiments. From the difficulty of performing them, from the long and constant attention they require, and the difficulty of avoiding every circumstance, which though insignificant in itself, often affects the pulse in a considerable degree, it is not pretended but some slight inaccuracy may have been noted down: but it is hoped they will shew the resemblance between the three medicines. They likewise will shew the greater solubility and quicker action of the Cornus Florida. The Sericea appears to be next. This agrees with their chemical analysis. Their regularity, and durability of action is likewise apparent, for in no one of them did the pulse return exactly to its natural state, but was often fuller and stronger, and always quicker.

EXPERIENTIA.

The little opportunity of applying a new medicine to practice, by the student of medicine, must be known to every one. It will not, on that account, be expected that many experiments on the diseased subject, will be related. However, from the kindness of Dr. Church, to whose friendship and attention I am greatly indebted, I shall relate the success of an experiment with the Cornus Sericea in the case of an intermittent fever which came under the doctor's direction.

April 13th, 1803. W. F. aged thirty-four, was taken with a chill about ten o'clock, A. M. which continued four hours, and was succeeded by a fever which lasted ten hours; it went off in the ordinary manner by a copious perspiration.

14th. Free from fever, but debilitated.

15th. A similar paroxysm as on the 13th.

16th. As on the 14th. He now commenced with the arsenical solution of Fowler, in the dose of ten drops three times a day.

17th. Had another severe paroxysm. The drops were now omitted and blisters applied to his wrists.

18th. He had given him six papers of the Cornus Sericea, containing a half drachm each, to be taken three times in the day.

19th. Free from fever. His intermittent has not returned, May 10.

The following is a case afforded me by my friend and fellow graduate Mr. Hutchison, in his own words.

On the 25th of April, 1803, I was desired to visit Samuel Anderson, aged thirty, with an intermittent fever. I found his pulse active, tongue furred, and his skin warm, he complained of pain in his head and back, he informed me he had two paroxysms previous to my visit.

Four grains of tartar emetic were given him, which produced a copious vomiting. On the morning of the 26th I found him free from fever, and ordered thirty grains of Cort. Peru. to be taken every two hours. This was continued until eleven o'clock A. M. of the 27th, when the paroxysm returned.

28th. Finding him free from fever, I gave him twenty grains of the Cornus Sericea in powder, every three hours, which was continued for several days; he has had no return of his fever, May 6, 1803.

It may here be added, that this species of the Cornus was used by the physicians of the French army in America, during the revolution, as a substitute for the Peruvian Bark.

On the subject of the Cornus Florida, I have received a communication through the hands of my friend and fellow graduate, Mr. Warmsley, from Dr. Amos Gregg, of Bristol, Pennsylvania, which will be found doubly interesting; first, because it comes from a practitioner, whose success and zeal for the promotion of medical science, endears him to his medical brethren; and, secondly, because his opinion of the Cornus Florida is founded on an experience of twenty-three years practice with it. The following is an extract of the communication:

" About the year 1778, during the American revolution, the great scarcity and high price of the Peruvian Bark, and the embarrassment from the want of it, induced me to search for a substitute. With this intention I tried the yellow poplar, in which I was disappointed. The common Dogwood (Cornus Florida) was the next which I selected; and having at that time the intermittent fever, I took several ounces of the decoction of it, which effectually cured me, though it produced some pain in my bowels, which was relieved by a few drops of laudanum. This property of affecting the bowels with pain, I found it to possess only in its recent state; and never after it was twelve months old, did I find it disagree, in exciting pain, cathartic or emetic effects. I have, therefore, at different times, had considerable quantities well dried and pounded, so as not to be without it in my shop for twenty-three years. During which practice, I have found its virtues such as to convince me that it was not inferior to the Peruvian Bark in curing intermittents, nor inferior as a corroborant in all cases of debility. I must observe, however, that I have generally given the Dogwood in doses of thirty-five grains, which I have always found equal to thirty of the Peruvian Bark. I have used the Dogwood in several other cases, the most interesting of which are: first, in a dropsical patient, who, after a few days of violent pain in his legs, had them swelled to a very large size, and considerably inflamed: soon after, small blisters appeared upon them, which in eight and forty hours turned of a dark purple colour; at this time I gave him thirty grains of the Dogwood in powder, with six grains of Virginia snake-root every half hour for two days, and once an hour for the succeeding twenty-four hours. The man recovered.

"The other was a patient, who, by accident, had a great portion of the muscular part of his leg torn off; the weather being excessive warm, the purulent discharge soon became very great and offensive. I gave him the Dogwood joined with the snake-root as above, the man soon recovered, and is now living. I have often used the Dogwood, joined with gentian, columbo, camomile, and with aromatics in bitters, and have found it equal to the Peruvian Bark, and therefore concluded it is a valuable medicine."

APPLICATION.

When we look back upon the similarity between the Corni and the Cinchona, in their sensible qualities, in their chemical analysis, and their similarity of action on the incised, and dead fibre, and particularly when we view their similar operation on the healthy and diseased subject, we cannot but receive the most flattering inducements to estimate these provident gifts of Nature. And when we reflect upon the causes of the various forms of disease, which are the endemics of our country, we cannot but receive additional inducements to regard the Corni as the most valuable vegetable, which Nature, in the prolificness of her bounty, has scattered through the wide forests of North-America. For so long as the mouldering ruins of our swamps, and the uncultivated condition of our marshes shall afford materials for the peccant operation of an autumnal sun, we shall view, with peculiar delight, the virtues of these two vegetables, which inherit the two essential characters of the most valuable division of the materia medica; I mean, bitterness and astringency; to the "happy union" of which the Corni have a claim, as respectable as that which has procured for the Peruvian Bark a celebrity as extensive as the bounds of rational medicine. Indeed, so striking is the similitude, so exact the result, from comparative trials, that in this attempt to recommend the Cornus Florida and Sericea, to the attention of practising physicians, I cannot even review the forms of disease, in the particular states of which the Corni are indicated, without encroaching upon the reputation of the Cinchona: for, in truth, it may be said, that in whatever form of disease the Cinchona has been decidedly serviceable, the Corni will be found equally so. And if we make allowances for the chances and inducements to adulteration in the former, for our relationship to the latter, for its wide extent through the very soil in which are engendered the seeds of those maladies, their virtues are fitted to remove, we must acknowledge their superiority. Experiments, of a diversified nature, warrant this conclusion. They are, like the Bark, bitter and astringent in the mouth, tonic and febrifuge in the stomach, and their chemical analysis afford results perfectly analogous. But shall I, on this account, recommend, in intermittents, remittents, choleras, diarrhœas, and dysenteries, the corroborant virtue of the Corni, because the same virtue, in the Cinchona, has been said to be their specific remedy? This would, indeed, be following the usual mode of introducing new medicines into notice. But would it not be adding a specific to nosological titles, and one more powerful nostrum to the long catalogue of empyricism?

Had the improvements in pathology and therapeutics kept pace with the rapid enlargement of the materia medica, an attempt at this day to have added one more article to the latter, would have been regarded as fruitless and unnecessary.

But while the medical mind was busily employed in drawing nosological distinctions, and wasting the efforts of genius in searching for specifics, the embryon of rational science was rising in the western horizon. In its growth it viewed, with grief and horror, the species of diseases multiplying* themselves in numbers and augmenting their malignity. And no sooner had it arrived at its zenith, than its first act

* In proof of which the following is given as an example.

Sauvage has, in his Nosology, 10 classes, 44 orders, 300 genera, with species and varieties innumerable.

Linnæus has, 11 classes, 37 orders, species proportionally numerous.

was to liberate medicine from the trammels of nosological complexity, and restore to disease that unity and simplicity, which allied it with its proximate cause.

Fully impressed with the inestimable value of this improvement in medicine, which does distinguished honour to its author, I shall neglect the nosological order; nor shall I say that the Corni are indicated in remittents or intermittents, merely because they consist of paroxysms, remissions or intermissions, but shall pay particular attention to that state of the system, which affords the only rational indication for tonic medicine. I assume it a truth, sufficiently established by the illustrious professor of clinical medicine, in this university, "that disease is the same, however variously it may be modified by age, constitution, climate, season, remote or exciting causes, or by its duration." But if none of these circumstances, are sufficient to establish a specific difference in the nature of disease, what criterion are we to adopt, by which we may regulate the administration of the Corni, or any other article of the materia medica, all of which, though differing greatly in their respective qualities have nevertheless an appropriate point in the scale of morbid excitement? The very circumstances carry along with them the answer; namely, the existing state of the system, which is to be judged of by the season, age, and duration, &c. but more particularly by the state of the arterial excitement, as pointed out by the pulse. So important is the nosometrical power of the pulse in shewing the force of arterial action in the system, that it may emphatically be called the *alter oculus* of physicians.

In taking, therefore, this nosometer of the system for my guide, my labour, in the application of the curative powers of the Corni will be greatly abridged. Instead of tracing their application through the catalogue of nosologists, I am guided to that particular state of the system, which may occur in every form of disease, and which whenever it does occur, calls for the Corni, or such medicines as possess similar virtues.

In tracing the pulse, in disease, we find it in two very opposite states. In the one we find it full, strong, hard and frequent, accompanied with heat of body, thirst, restlessness, &c. to which state has been attached the term of inflammatory action. In the other it is weak, small and

Vogelius has, 11 classes, containing 560 genera, with their species and varieties proportionally numerous.

Cullen, 4 classes, 59 orders, 350 genera, with their species and varieties.

Sagarus, 13 classes, 54 orders, 350 genera, containing 2,500 species with their varieties

"Hei mihi! Tot mortes homini quot membrana, malisque
"Tot sumus infecti, mors ut medicina putetur."

quick, accompanied with debility and prostration of strength, heaviness and dejection of spirits, and various other circumstances, which has attached to this state, the various terms of nervous, jail, hospital, ship, petechial, putrid or typhus fever. Every form of disease in its various modifications, approaches more or less near to one of these two opposite states, between which there are several gradations of morbid excitement, indicated by corresponding degrees of arterial action.

I have only, then, to say that the Corni or other tonics, whose virtue consists in increasing and supporting the strength of arterial action, and thereby removing that debility which is the principal symptom of this latter state, are indicated here. And that their exhibition may be extended, with probable success, to every other form of disease, in proportion as it approaches to this latter described state.

But I cannot here omit the golden rule, in the exhibition of this class of medicines, for the authority of which we have the exalted names of a Darwin, a Rush, and a Barton, that is, whenever in the exhibition of tonics or stimulants, the pulse becomes slower, fuller, and stronger, their administration is judicious, and should be continued; but when, on the contrary, it becomes quicker, more frequent with an increase of heat on the body, anxiety, and dryness in the mouth, they are improper, and we may say with Ovid, "parce stimulis, et utere loris."

In attending to disease, as it occurs in our own country, we perceive that some forms of disease approach more uniformly to that particular state, just described as requiring tonics, than others, such are those which occur in the summer and autumnal months. I shall therefore briefly enumerate them, in attempting which, I feel sentiments of gratitude in adopting the division and doctrines of the illustrious professor of the practice of physic, who enumerates six original forms of autumnal disease; in all of which the Cornus Florida and Sericea, may be employed with the happiest effects, according to circumstances; they are, "1. The malignant yellow fever. 2. The inflammatory bilious fever. 3. The remittent bilious fever. 4. The intermittent bilious fever. 5. Chronic fever, and 6. Febricula." These embrace all the affections of the alimentary canal, in the shapes of cholera, diarrhœa, and dysentery; of the skin, in various eruptions; of the pulmonary system, in the form of peripneumony notha; of the spleen and pancreas, in inflammations and obstructions; of the brain, in the shape of hydrocephalus and coma; and of the arterial system, in all the grades of arterial excitement, from the malignant to the typhus action.

My limited time will not admit my dwelling on these forms of disease; all that I shall say therefore is, that the disease which generally occurs at this season of the year, generally shapes itself in such a manner, as to enable us to derive the greatest advantage from the tonic power of these

the inestimable principles which I have received from each and of all you, in the science of medicine. For it is from the most impressive evidence I add, that it is in this university, that new avenues to medical knowledge have been unfolded, and the just importance of former ones established: in which university, the genuine principles of medicine have received a developement, and a kind of demonstration hitherto unknown. And lastly, in which its students enjoy that profound liberality, and indulgence in sentiment which banishes superstition, and its concomitants blind veneration and credulity.

thou shalt bring forth children," and which was announced to the mother of the human race, as a punishment for her disobedience; and secondly, upon the erect position of the human body.* It having been supposed necessary that nature should have formed such a structure of parts as would counteract the effects of gravity, and prevent premature labour; and though these ends are obtained, yet the means she has employed create those obstacles which impede delivery, and are unavoidably the cause of the excruciating pains which the unhappy sufferer is compelled to endure.

That labour is frequently a tedious and painful operation will not be denied; but that pain should be a necessary consequence of the form and structure of the human body, or that the Supreme Being should have enjoined it as a curse upon the female sex, appears so derogatory to the idea we have of his wisdom and goodness, that we cannot admit it.

That a woman may bring forth in sorrow, and yet not be subjected to much bodily pain, may very readily be conceived. When she reflects upon the dangers attending the puerperal state, the slow and difficult recovery of some, the death of others; when she considers that half of the human race do not attain the age of seven years; that hardly a half of the remainder arrive to years of maturity, and that even should the fruit of her womb be so fortunate as to escape death in its youth, yet that it will be exposed to dangers and temptations, in the world, to which it may fall a prey, and which may render life a burden to itself, and a reproach to its parents: under circumstances like these, it may truly be said, that in sorrow does she bring forth.

But that it is not the intention of nature that labour should be a painful operation is sufficiently evident from the ease and facility with which many women are delivered. "We know that many women are safely delivered of full sized children, so suddenly that they have scarce time to call for assistance: that, sometimes waking from their sleep with a slight pain, labour is hurried through in a few minutes, while the woman is almost unconscious of what has happened. Some women, again, have been taken in labour while walking, and have not even had time to be conveyed to any house, or convenient place, but have dropped their burdens where they happened to be first seized. Others, while on a visit, at dinner, or in the midst of some amusement, have been surprised by labour, and have parted with their children with little pain or trouble."†

If these facts be admitted, they will prove that pain does not occur in every case of parturition; and hence it will follow, that if all women are not necessarily and inevitably subjected to pain, none are; but, that pain,

* Osborn's Essays, p. 10. Denman's Midwifery, vol. 2. p. 18.

† Bland on Parturition, p. 24.

or absence of pain, must depend upon adventitious causes. We cannot suppose that a Being who has established general laws for the government of the universe would be partial in this particular.

In proof of this we find, that women who live in a certain state of society, in a certain climate, or who follow a particular mode of life, are entirely free from pain. Bruce,* speaking of the women of Abyssinia, says, they do not confine themselves even a day after labour, but wash and return to their work immediately. "A Morlack woman neither changes her food nor interrupts her daily fatigue on account of her pregnancy, and is frequently in the fields, or on the road by herself, and takes the infant, washes it in the first water she finds, carries it home, and returns the day after to her usual labour, or to feed her flock."† It is related by Hennepius,‡ that "the Spaniards in Brazil, who perform the office of midwives to their teeming consorts, receive the infant, tear the navel string, and wash and paint it. The lying-in woman does not meet with more indulgence than the infant: as soon as she is disburdened, she goes and washes herself, and immediately sets about her work, without suffering the least inconvenience from it." He also says, "The wives of the Livonian peasants use the same custom. The women retire to some private place when the time of delivery is at hand, and return immediately after to their work." "The Moorish women," says Sagnier and Brisson,|| "have no midwives, but are usually alone at the moment of delivery, laid on the ground, under an indifferent tent. They have seen these women depart even on the day of their delivery, to encamp at the distance of fifteen or twenty leagues."

The labours of the Sicilian women are accompanied with so little pain and danger, that they appear perfectly well the day after delivery. "For in this happy climate," says Brydone,§ "child-bearing is divested of all its terrors, and is only considered as a party of pleasure." The Indian women of our own country also possess this happy privilege to a great degree: "when taken in labour while marching with their husbands, they will retire behind a bush, deliver themselves, and in an hour's time rejoin their companions."

This testimony in favour of the ease and facility of parturition, sufficiently proves, that it is not the intention of the Supreme Being, that it should be a painful operation, and that it would at all times be performed with ease and safety, had not mankind, as they became more civilized,

* Travels to discover the source of the Nile, vol. 2. p. 21.

† Dobson's Encyclopædia, article Morlachia.

‡ Ceremonies and Religious Customs of various Nations, vol. 3. p. 20.

|| Voyage to the Coast of Africa, p. 494.

§ Tour through Sicily and Malta, vol. ii. letter 22.

introduced customs and modes of living so very different from those of their ancestors. To this alteration, therefore, in their diet and habits of life, we may attribute the pain and difficulty that so frequently attends labour.

All the soft parts concerned in parturition in a natural and healthy state, are prepared and disposed to dilate; they therefore make little resistance to the expulsion of the child, which is effected with very little exertion, and almost without pain. But from "irregular and improper customs and habits of living" they acquire such a firm and rigid texture, that it requires considerable force and time to dilate them. Although all the soft parts concerned in labour, are susceptible of this rigidity, yet in general it is more particularly confined to the os uteri, and the strongest and most violent contractions of the uterus, are sometimes unable to overcome it.

This firm and rigid texture of the os uteri, has been noticed by most writers on the obstetric art, as a cause of difficult labour; and we have frequently known parturition delayed for hours, and even days, in some instances, from this cause, when every other circumstance was in favour of a speedy and happy termination of the patient's sufferings.

For the purpose of dilating the soft parts and expelling its contents, the uterus is possessed of a strong power of contraction, by which means the liquor amnii and the fœtus is pressed against the os uteri, and thus gradually dilate it. This action of the uterus we shall consider as threefold.

First. A permanent contraction, by which, like other hollow viscera, it adapts itself to its contents, as the blood-vessels to the blood, and the bladder to the urine.

Secondly. The occasional contraction. This it owes to muscular structure. It is a temporary contraction, and cannot be continued long at a time.

Thirdly. An irregular, spasmodic, or convulsive action.

By the first of these actions, when there is no resistance from the soft parts, the contents of the uterus is expelled. By the second, the os internum and externum is dilated. These two actions we consider as the natural, and healthy action of the uterus. The last is a disordered action, arising from great irritability of the uterus; from inflammation of the os uteri and parts adjacent, occasioned by violent pressure of the child's head, or from irritation of the os uteri by frequent examination or attempts to dilate the parts with the hand.

This irregular action is never of any service in dilating the parts, or in expelling the fœtus, and is generally attended with the most excruciating pain. It frequently occurs after the delivery of the child, and by closing the os tincæ or by the contraction taking place in the middle of uterus, it prevents the accoucheur from extracting the placenta.* It is always overcome with difficulty. In some cases it is impossible to introduce the hand, and even dangerous to attempt it.

The resistance to the expulsion of the fœtus, from the rigidity of the soft parts, and the irregular action of the uterus, we conceive to be the principal causes of pain. Hence the indications of lessening pain, are, to give to the soft parts a disposition to dilate, and to restore the uterus to its natural and healthy action.

We do not find that any thing has been offered for the express purpose of lessening the pains of parturition until very lately.† Accoucheurs believing it impossible to give to the parts their disposition to relax,‡ contented themselves with confining their attempts to dilate the os tincæ, to very lingering or preternatural cases only.

Opium|| and the warm bath have been recommended for the purpose of relaxing the uterine parts. But to produce any such effect by opium, it would be necessary to give it in large doses; this might be attended with danger, as it has, when thus exhibited, "been known to produce convulsions."§ Some advantage might be derived from the relaxing power of the warm bath, but the application of it is attended with many inconveniences, and its good effects are not certain: we therefore do not consider either of them as proper remedies.

The means usually employed were fomentations and mechanical dilatation, by moving the hand in a rotatory manner. From the inability of producing a sufficient degree of relaxation by these means, recourse was too frequently had to instruments, by which the life of the child was often destroyed, and considerable injury done to the mother.

In consequence of the many inconveniences and dangers arising from this mode of practice, accoucheurs at length thought it most prudent not to interfere, but in all cases (preternatural presentations, or where there was great deformity of the pelvis, excepted) to trust entirely to the efforts of nature.

Dr. Hamilton** relates a case in which, though the contractions of the uterus were "strong and frequent," yet it required two days before the os tincæ was sufficiently dilated; and in his remarks on that case observes, that it "shews the advantages of waiting patiently for the efforts of nature." What I would ask, are those advantages? The case

* See the writings of Burton, Giffard, Mauriceau, La Motte, Exton, Johnson, Perfect, Dease, Denman, and Osborn.

† See Dr. Rush's letter to Dr. Miller. Medical Repository, vol. 6. p. 26.

‡ Denman's Midwifery, vol. 2. p. 69. || Mead's works, vol. 3. p. 137.

§ Denman's Midwifery, vol. 2. p. 418.

** Cases in Midwifery, by J. Hamilton, jun. M. D. order 1st. case 4th.

proves that nature, left to herself, had power sufficient to expel the child; but surely subjecting the patient to the most exquisite pain for forty-eight hours, cannot be considered an advantage. It is the more surprising that Dr. Hamilton should advocate this doctrine, as he appears to be well acquainted with the effects of blood-letting, in relaxing the os uteri, and recommends it in cases where we might apprehend a rupture of the uterus.*

That the powers of nature, in a simple state of society, and in those in whom a general relaxation of fibre is produced by climate, or particular habits of living, are sufficient for the expulsion of the fœtus, has already been noticed. We also grant, that in general she is adequate to the task, in those who, from different modes of life, have produced the rigidity of the soft parts, which we have considered as the principal cause of difficulty; but here she requires a considerable time to effect her purpose, and her patient is subjected to much unnecessary pain, and great anxiety of mind.

To view the distress of a fellow creature, from whatever cause it may arise, is to the mind of sensibility extremely painful; but there is no situation which excites greater solicitude, or in which our feelings are more interested, than that which we are now considering. Certainly then we ought not to be inactive spectators, when we have it in our power to give almost instantaneous relief, but should make use of those means by which a mitigation of suffering may be obtained. And more especially when the means to be employed would not only produce present relief, but also prevent future danger.

But there are cases in which nature requires such a considerable time to accomplish the delivery, that by waiting for her efforts irreparable injury is frequently the consequence. From great rigidity of the os uteri, and strong and violent contractions of the uterus, there has occurred:

1. Rupture of the uterus.†

2. A laceration of the os uteri.‡

3. A protrusion of the os uteri before the head of the child, which, in some cases has been divided before delivery could be effected.||

4. From long continued pressure of the head upon the soft parts, has arisen inflammation and mortification, which has sometimes proved fatal; and in other cases, though the patient has survived, she has been subject to an involuntary discharge of fæces or urine through the vagina.§

* Hamilton's Cases, p. 152.

† Noticed by Foster, Dease, Denman, and J. Hamilton, jun.

‡ Perfect's Cases, case 142. Foster's Midwifery, p. 246.

|| Annals of Medicine, for 1798, p. 331.

§ Dease's Midwifery, p. 38 and 75. Clarke's Practical Essays, p. 63.

5. From frequently repeated and violent contractions, the uterus becomes exhausted, an atony succeeds, and even though the soft parts should then become relaxed, the powers of the uterus are insufficient to expel the fœtus. In some such cases we must have recourse to instruments, nor does the injury end here; from an inability of the uterus to contract, an hæmorrhage supervenes, which is generally fatal to the patient.*

And when we add to these, the risk of convulsions,† rupture of blood-vessels,‡ or laceration of the perinæum,|| all of which have happened, and may again happen under similar circumstances, we cannot suppose that any great advantage can be derived from "waiting patiently for the efforts of nature."

Having pointed out the ill consequences arising from a rigid state of the soft parts, and an irregular action of the uterus, we proceed to the consideration of the means by which these evils may be remedied. We shall notice, first, Such as are necessary during gestation, and which will prevent a morbid irritability of the uterus, and enable the soft parts to retain their disposition to dilate. And, secondly, The means to be employed at the time of parturition, by which we may effect a dilatation of the soft parts and restore the uterus to its natural and healthy action.

From the stimulus of distension, the indulgence of the appetite, and a deficiency of exercise, during the period of uterine gestation, there is a constant tendency to a plethoric disposition; this, if not actual disease, is the cause of most of the irregularities of the system attendant on pregnancy, and lays the foundation, for the causes of pain and difficulty in parturition, already mentioned. Hence it becomes necessary to pay particular attention to the state of the system, and when symptoms of disease occur, to be early in administering remedies for their removal. As most of the complaints of pregnancy arise from this plethoric disposition of the system, it will be proper in almost every instance to meet them with the lancet. The safety of blood-letting in pregnancy is already sufficiently established, and although it is not necessary to bleed when there is no symptom of disease present, yet if we wish to conduct our patient in such a manner as to ensure her an easy labour, it would be highly improper to neglect blood-letting when these symptoms do occur.

The remedies to obviate this plethoric disposition, are, first, A low diet. The appetite during pregnancy is frequently considerably increased, and much mischief has accrued from an inordinate indulgence of it. It

* Hamilton's Cases, order 2. case 1st. † Denman, vol. 2. p. 403.

‡ Ibid. vol. 2. p. 50.

|| Foster, p. 246. Dease, p. 35. Denman, vol. 1. p. 67, and 383.

will not only be necessary to lessen the quantity of food, but also to attend to its quality. Animal food, strong drinks, spices, and all substances of a stimulating nature, should, in a great measure, be avoided, and a diet of fruit, vegetables and milk, should be enjoined. The easy labours of the Indian women of America, have very properly been ascribed by Dr. Rush* to their scanty diet. In the following case its good effects are apparent.

CASE I.

D. W. aged twenty-five years, was admitted pregnant into the alms-house, on the 14th of December, 1802. In consequence of an ulcer on her neck, she was ordered into the surgical ward, and was confined to a low diet, consisting entirely of vegetable substances, until the 12th of January, 1803, when she was taken in labour: it came on so suddenly, that there was scarcely sufficient time to remove her to the lying-in room before she was delivered, and with so much ease, that it was observed by a medical student present, that it might be truly called a case of parturition without pain. Immediately after her delivery she arose, made up her bed and undressed herself, with as much unconcern as if nothing had happened to her. This patient was not confined an hour to her bed, either from debility or disease.

Secondly, Exercise. It is observed by Aristotle, "that those women who take most exercise endure uterine gestation and labour with greater ease and safety."† This observation has been confirmed by most of the succeeding writers on midwifery. Hence we account for the more easy labours of the women residing in the country, than of those in cities. And to the combination of exercise and scanty diet, may be attributed the little pain and difficulty attending parturition among the female slaves of the southern states.

Thirdly, Laxative medicines. These should be administered two or three times a week, for a month or six weeks previous to parturition. Their efficacy in disposing the parts to dilate is evident, from the advantages following the use of sweet oil among the women of Turkey, which is supposed by Dr. Rush‡ to act merely as a purgative. For the purpose of ascertaining its effects the ol. ricini was prescribed in the following case.

CASE II.

December 1st, 1802, I was requested by Mrs. M. C. to attend her at the time of parturition, which she supposed would take place in about six

* Medical Repository, vol. 6. p. 26.

† Introduction to Smellie's Midwifery, p. 16.

‡ Medical Repository, vol. 6. p. 26.

weeks. She was thus early, she said, in her application, because in all her previous labours she had suffered extremely from the duration of labour, and the intensity of pain with which it was attended; and if any thing could be done whereby she might obtain an alleviation of pain, she would willingly comply with any directions that I might give for that purpose. I desired her to take half an ounce of the ol. ricini, every other day, and continue the use of it until the time of parturition. During the six weeks she made use of twelve ounces. On the 10th January, 1803, I was sent for, but before I arrived at the house, she had parted with her child. The information which I received, was, that she had sent for me on the approach of the first pain; that before that pain had left her the membranes were ruptured; that it was quickly succeeded by another which expelled the child, and that the pain she experienced was so trifling, that she could not say she had suffered any.

But as practitioners are seldom consulted respecting the regimen to be observed during gestation, and frequently are not called until the moment when their assistance becomes necessary, they are precluded from advising the measures recommended for the prevention of the causes of pain and difficulty. We therefore meet with these causes, and must attempt their removal at the commencement of labour.

We now proceed to the consideration of the means mentioned under our second general head, viz. Such remedies as may be employed at the time of parturition, for the purpose of effecting a relaxation of the soft parts and restoring the uterus to its natural and healthy action. They are, first, Blood-letting. This of itself is fully adequate to the purpose, and is the best remedy we can employ; it not only produces present effects in lessening the pains of labour, but extends its beneficial influence to the prevention of subsequent dangers. In cases where blood-letting is objected to, perhaps some advantage might be derived from, secondly, Nauseating doses of emetics. Of this remedy we can say nothing from experience, but from observing the relaxation that has frequently followed nausea and vomiting in difficult cases, and from the common observation of the old women, "that a sick labour is an easy one," we have been led to suppose, that much benefit might be obtained from adopting a mode of relief pointed out by nature. So great a degree of relaxation is produced by nauseating medicines, that they have been recommended by Dr. Physick,* when blood-letting is objected to, in cases of luxation, where the reduction of the bone is prevented by muscular contraction.

Blood-letting for the express purpose of lessening the pains of parturition, was first recommended by Dr. Rush.† It was adopted by several practitioners of medicine with the happiest effects: of its efficacy we can

* MS. Lect. on Surgery. † Medical Repository, vol. 6. p. 26.

speak from experience. The following cases which we have selected from a number of others, will shew in the most striking manner the great utility derived from the use of it.

CASE III.

In November, 1801, I was desired by Dr. Dewees to visit a woman who had been in labour nearly two days; on arriving at my patient's, I received the following account from the midwife in attendance. The woman, she said, was large and fat, in labour with her first child, and twenty-eight years of age; that she had been in labour thirty-six hours, at the commencement of which every thing appeared to do well; that the labour, though slow, gradually progressed until the head of the child had passed the os tincæ, after which, notwithstanding the pains were strong and frequent, no further progress was made. In this situation she had passed the twenty-four hours previously to my arrival. On making an examination, I found the head firmly wedged in the vagina, the external parts very firm and rigid, and it was with difficulty that I could introduce two fingers into the os externum.

It was here evident, that the rigidity of the external parts was the sole cause that retarded the birth of the child. I therefore immediately determined to bleed my patient, in order to effect a relaxation of the parts. My intention was to bleed ad deliquium animi; but after drawing off forty-eight ounces of blood and finding there was no tendency to syncope, I stopped the bleeding to examine the state of the parts. I now found that they were very much relaxed, and that the head was slowly advancing; yet so perfectly easy was the patient, that it could not be ascertained from external appearances that the uterus was contracting. I then requested the midwife to take the seat that I might tie up the arm, and before this was effected my patient was delivered of a large healthy child, and declared that she did not experience the least degree of pain during its expulsion.

This was at that time to me a very interesting case, and from it may be deduced the following inferences.

That a large quantity of blood may be drawn, without injury to the patient.

That it is not necessary to bleed until fainting is induced, in order to effect a relaxation of the parts, or to restore the uterus to its natural or healthy action.

That after the resistance from the rigidity of the soft parts is removed, the permanent contraction of the uterus is sufficient to expel the child.

That when a relaxation of the soft parts is effected, and the uterus restored to its healthy action, parturition will take place with very little pain.

CASE IV.

In December, 1802, I was requested by A. B. to visit his wife, who was then, he said, in labour: on my arrival I was informed, that she had had several severe pains, but that they had left her, and she was now easy. On making an examination per vaginam, I found the os uteri very little dilated, and its edges extremely hard. I proposed bleeding, which was readily assented to. She had heard, she said, of its good effects, from several of her neighbours whom I had attended, and wished to try the experiment: I accordingly took from her twenty-four ounces of blood.

Having a case at the same time to attend to in the alms-house, I was under the necessity of leaving her, but desired they would send for me immediately on the return of her pains. I heard no more of this patient until the next day, when on meeting her husband, and inquiring after his wife, he informed me, that about one hour after I had left her, she had a return of her pains, that she requested him to go for me, but that before he could leave the room the child was born. On visiting this patient afterwards, I learnt that the pain she experienced during the expulsion of the child, was considerably less than that she had suffered previously to her being bled.

CASE V.

E. R. aged twenty-eight years, was admitted into the alms-house on the 3d of February, 1803. A few hours after her admission she complained of slight labour pains, which gradually became weaker, and at length entirely subsided. An injection was given, and at ten o'clock, P. M. I took from her twenty ounces of blood, and left her, with directions to have me called as soon as her pains should return. About two o'clock, A. M. of the 4th, I was called by the nurse, but before I could reach the room, although not more than ten minutes had elapsed, I was informed that the child was born. This patient declared, that she sent for me the moment she experienced the least degree of pain; that she had felt but two or three slight pains before the child was born; and also, that she had suffered much more in a former labour from one pain, than she had in this, during its whole continuance.

CASE VI.

M. C. aged thirty years, became in labour on the morning of the 27th of January, 1803. During the whole of this day her pains were strong and frequent, yet had so little effect in dilating the os uteri, that on the morning of the 28th, it was only sufficiently dilated to ascertain that the vertex presented. From this time to twelve o'clock, she suffered considerably, from the violent contractions of the uterus; but notwithstanding this strong action, the os uteri remained inflexibly hard and tense, and was not dilated to more than the size of half a crown. She now lost twelve ounces of blood; this however produced no effect; at one o'clock the os uteri retained its pristine rigidity. I now drew off twenty ounces more of blood. So immediate was the dilatation, that in ten minutes she was safely delivered.

From the foregoing cases we may infer, that blood-letting at the time of parturition is a safe, easy, and effectual means of lessening the pains of labour. This inference is confirmed by a number of other cases, in which we have employed that remedy for this purpose; in no instance did any ill consequence arise from the use of it, and in every case the patient had a speedy and happy recovery. Cases fourth and fifth, will shew the impropriety of leaving our patients after a large bleeding.

Blood-letting to be effectual must be copious. We have not observed, in any instance, that a less quantity than twenty ounces, has produced any great effect upon the os tincæ; but in those cases where the patient lost from twenty to forty ounces, it frequently acted as a charm in producing an immediate dilatation of the soft parts. We should, therefore, repeat the operation, when we find that no effect is produced by the first bleeding.

There are, however, exceptions to this as a general rule: from those "who have been debilitated by previous disease, or by accidental evacuations of any kind," it would not be necessary to take so large a quantity; the parts being disposed to dilate, easily yield to the slightest impulse. Hence, we find that labours in the last stage of chronic diseases, are generally attended with but little pain.

It may be objected to this practice, that copious blood-letting would occasion syncope, whereby the uterus might lose its power of contraction, and consequently there would be an inability to expel the fœtus. Such an accident we believe would not occur, unless considerably more blood than necessary was taken. We have used blood-letting for the purpose of lessening pain, in a number of cases, and in no instance was a faintness induced. The system, during pregnancy, and at the time of parturition, will bear the loss of blood in larger quantities without its producing faintness, than at any other time. From a mere disposition to faint we would apprehend no danger. "Blood-letting lessens sensibility but not irritability."* And the uterus "acts, or makes its efforts to act, in sleep, during faintness, and sometimes even after death."† In this state of the system, such a complete relaxation takes place, that the permanent contraction of the uterus would be sufficient to expel the child.

In those cases in which blood-letting was employed at the commencement of labour, it generally suspended the action of the uterus for some

* Rush's MS. Lectures. † Denman's Midwifery, vol. 2, p. 332.

time: during this suspension of action, the parts assumed their disposition to dilate, and on the return of the uterine contractions, the delivery was accomplished in a short time, and attended with very little pain. But in cases where it was not employed until twenty-four or thirty hours after the commencement of labour, its effects were immediate.

Nor is the lessening the pains of parturition the only advantage we derive from blood-letting. By the use of this remedy we prevent all those accidents which have been enumerated as arising from the rigidity of the soft parts, and the irregular action of the uterus. By blood-letting we prevent the premature rupture of the membranes, and thereby have the assistance of the waters in dilating the parts. It facilitates the delivery of the placenta by preventing spasmodic contractions of the uterus. It prevents after pains, puerperal fever and mania.

In preternatural presentations, blood-letting, by removing the spasmodic action of the uterus, and relaxing the parts, very much facilitates the introduction of the hand, and delivery of the child. And in cases where there is a deformity of the pelvis, though we cannot by bleeding hasten parturition, yet by its relaxing effects, it will enable us to make use of the necessary means, with more ease to our patients as well as ourselves.

To the medical professors of this university, for the instruction I have received from their truly valuable lectures, I return my most grateful thanks. And that each of you, gentlemen, may long enjoy that honour and happiness, which you so deservedly merit, from your indefatigable exertions in the promotion of useful knowledge, is my most fervent wish.

AN EXPERIMENTAL INQUIRY,

RESPECTING THE

VITALITY OF THE BLOOD.

BY CHARLES CALDWELL, M. D.

PREFATORY NOTE.

IT is nearly three years since most of the experiments and observations detailed in the following sheets, were completed. If there be any thing new or valuable in them, the author ought, in justice to himself, to have published them sooner. If there be not, it is his misfortune, that he obtrudes them on the public, even at this late period. Be the matter, however, as it may, he is willing to own them, and to take his chance of meeting whatever public sentiment they may be calculated to awaken. Some of them have been already spoken of, and attributed to, if not claimed by, other persons, who have certainly no knowledge of them, except what they have derived from verbal information. For, though never printed, they have been repeatedly mentioned in private conversation, and also on occasions of a more public nature. They were included in a course of medical instruction, delivered last winter, in the infirmary of the Philadelphia alms-house. It was at that time that they were moulded into Lectures, the form under which they now appear. They are submitted, with due deference, to the consideration of the medical class of the university of Pennsylvania, and of the physicians of the United States in general. Should they either serve to heighten zeal in physiological pursuits, or furnish faint glimmerings that may lead other more fortunate inquirers to luminous and important truths, the ambition of the author will be fully gratified.

Philadelphia, January 1st, 1805.

AN EXPERIMENTAL INQUIRY.

LECTURE I.

GENTLEMEN,

FROM the colour, warmth, and other striking properties of the blood, from the force with which it issues from wounds and lacerations in living animals, and from syncope and death proving oftentimes the consequence of an excessive loss of it, this fluid must, even in the earliest times, have become at least an object of attention, if not a subject of philosophical investigation. Nor has it ceased to attract the notice, awaken the curiosity, and exercise the intellects of men, from that period to the present day. There is scarcely an age or a nation, however barbarous and uncultivated, from which we have not received some record, either written or traditional, bearing testimony to this universal regard bestowed on the blood. Before entering, therefore, on the experimental part of this inquiry, it is but justice to those who have gone before me, and may not prove either uninteresting or useless to you, to take a brief retrospect of the opinions entertained respecting the nature of this fluid, by certain enlightened and distinguished characters, both of ancient and modern times.

We learn from sacred history, that as early as the death of Abel, an immediate descendant of the progenitor of mankind, the blood was regarded as a fluid of no common qualities. For evidence of the truth of this, we need only turn to the inspired page of the great law-giver and leader of Israel. When the Deity there addresses Cain in the language of reprimand and malediction for the murder of his brother, he says, "What hast thou done? the voice of thy brother's *blood* crieth unto me from the ground."

What, I beg leave to ask, can be the foundation of this bold apostrophe? and whence arises such a striking *personification* of the blood? It is not said that the *flesh*, the *bones*, the *brain*, the *heart*, or any other solid part of the body of the deceased, cry aloud for vengeance on his guilty brother; to the *blood* alone is declared to belong the prerogative of making this high and solemn appeal for justice. But it is by no means probable that this would have been the case, had that fluid been considered as a mass of inanimate matter. It would seem that nothing short of a belief in *its vitality* could have procured for it, on so solemn an occasion, a rank and an office so distinguished and honourable.

I know it will be said in reply, that the scriptural passage here quoted, is purely figurative, and will not, therefore, either admit of a literal construction, or warrant a philosophical inference. That the passage is figurative will not be denied; for figure is so interwoven with most of the oriental languages, that, if divested of it, they would be scarcely sufficient for the common purposes of life. But the use of figures is as much under the guidance of principles and rules, as that of any other mode of speech. In *personification*, particularly where, as in the present case, the figure is so bold and animated as to call forth language from any thing not really possessed of utterance, the skilful rhetorician never ventures to personify an object of *secondary* consideration. Such an error would expose him to censure, if not to ridicule. It would be like transplanting an illiterate clown, from his humble dwelling, to the august floor of a senate chamber. In this lofty flight of personification, the object that speaks must correspond in its dignity to the office it performs. Hence, the very circumstance of representing the blood of Abel as addressing itself to heaven for justice and vengeance, if it did not bestow on it a decided preeminence, affords at least sufficient evidence, that it was considered as no way inferior, in rank and importance, to any of the solid parts of his body. Being made, as it were, the advocate of the whole man, it could not have been viewed in any other light, than as one of his most respectable and dignified parts. But, without possessing the attribute of *life*, it could not have been entitled to such a standing. These considerations justify us in concluding, that, even among the first family of mankind, the blood was held to be a living fluid.

In confirmation of the justice of my remarks, on the proper use of *personification*, I would refer you to the Lectures of Dr. Blair, whose authority in rhetoric is universally admitted. That enlightened critic takes just exception at a passage in Pope's "Eloisa to Abelard," in consequence of the personification of two subordinate objects. After representing her as suffering all the agony that could arise from a tumultuous conflict between love and devotion, attachment to her Abelard and duty to her God, the poet makes his fair enthusiast give utterance to her feelings, in the following tender and impassioned apostrophe.

"Dear fatal *name!* rest ever unreveal'd,
"Nor pass these lips, in holy silence seal'd.
"Hide it, my *heart*, within that close disguise,
"Where, mix'd with God's, his lov'd idea lies:
"O! write it not, my *hand!*—his name appears
"Already written—Blot it out, my tears!"

"Here," observes our author, Dr. Blair, "are several different objects and parts of the body personified; and each of them is addressed or spoken to; let us consider with what propriety. The first is the name of Abelard; "Dear fatal name! rest ever," &c. To this no reasonable objection can be made. For, as the name of a person often stands for the person himself, and suggests the same ideas, it can bear this personification with sufficient dignity. Next Eloisa speaks to herself, and personifies her heart for this purpose: "Hide it, my heart, within that close," &c. As the heart is a dignified part of the human frame, and is often put for the mind, or affections, this also may pass without blame. But, when from her heart she passes to her hand, and tells her hand not to write his name, this is forced and unnatural; a personified hand is low, and not in the style of true passion; and the figure becomes still worse, when, in the last place, she exhorts her tears to blot out what her hand had written: "O! write it not," &c. There is, in these two lines, an air of epigrammatic conceit, which native passion never suggests, and which is altogether unsuitable to the tenderness which breathes through the rest of that excellent poem." But to return from this digression.

Passing from the time of Adam to that of Abraham, we still discover the prevalence of a similar sentiment, respecting the nature and qualities of the blood. We find that at the latter period, this fluid was used in sacrifices and oblations to the Most High, as well to appease his supposed indignation, as in grateful commemoration of favours he had bestowed. It appears to have been selected for this high destination, in consideration of its possessing, or being supposed to possess, something precious, sacred, and altogether different from the properties of common matter. For, certainly, nothing inferior to the most choice and precious of productions, would have been deemed worthy to be offered on the altar of the Majesty of Heaven. If Abraham and his posterity were in the habit of offering, in sacrifice, the first-born of their flocks, and the first fruits of their fields, it is not probable that they would, on any occasion, have mocked and insulted their God, by pouring out in his presence, what they considered as nothing but an inanimate fluid. The inference, therefore, is plain, that they must have believed the blood to be possessed of life.

At a subsequent and more enlightened period, when the descendants of Abraham were returning from Egypt, the nature and properties of the blood appear to have been further developed, and better understood. Moses, who, from having been bred up in the court of Pharaoh, was master of all the literature and science of the Egyptians, not only continued the use of this fluid in sacrifices, from a belief in its peculiar and perhaps sacred qualities, but forbade its being eaten by his companions, assigning as his reason for such prohibition, that it was the immediate seat of animal life. "I will even," says this inspired penman, addressing his followers in the person of the Deity, "set my face against that soul that eateth blood, and will cut him off from among his people: *For the life of the flesh is in the blood.*"

It has been frequently made a question, whether or not, on this occasion, the leader and prophet of Israel uttered his own private sentiment, or officiated as the immediate oracle of heaven. In either case, however, his words are entitled to our attention and respect. If he spoke from the impulse of divine inspiration, no one will call in question the evidence of his word. For whoever believes in inspiration from above, must admit it to be a source of immaculate truth. But, supposing Moses to have addressed his followers in the character of a mere man, it is to be remembered that his opportunities for acquiring a knowledge of the subject under consideration, had been favourable and extensive. Egypt, from its first settlement, must have been no less famous for the diseases of its climate, than for the fertility of its soil. The intemperate heat of its atmosphere, and the annual and extensive inundations of the Nile, taken in conjunction with the plagues which afflicted the subjects of Pharaoh, and still continue the endemic scourge of that country, afford sufficient evidence of the truth of this remark. It is reasonable and natural, therefore, to suppose, that the healing art, in its various branches, was among the earliest and favourite pursuits of the learned Egyptians. For what will sooner excite the human intellect to action, or urge it to more expedients or greater exertions for the acquisition of knowledge, than a desire to relieve ourselves and others from the pains and dangers of accident and disease? But we have reason to believe, that, as early as the days of Moses, the priests and other enlightened characters of Egypt, had made considerable progress in certain branches of liberal science, as well as in many of the arts of life. And we are further assured, as already remarked, that that enterprising and extraordinary character, contained within himself an epitome of the knowledge of his native country. These considerations, together with the many enlightened precepts which he delivered to his countrymen on the subjects of cleanliness, regimen, health, and disease, give us an assurance not easily shaken, that Moses possessed an acquaintance with medical science. Being, therefore, one of the most enlightened characters then living, his mere opinion, respecting the nature and properties of the blood, is entitled to notice and regard, even at the present day.

From Persia, which has been considered as the birth-place of mankind, and Egypt, the acknowledged cradle of the arts and sciences, let us pass on with the progress of learning to the countries of Europe. Here, our attention is first attracted to the states of Greece, and some of the adjacent islands. In these places, famed as the nursery of human greatness, but subject, like the United States, to summer and autumnal diseases, many distinguished characters, led by motives of interest or humanity, devoted their time to the cultivation of medicine. Of these, though Hippocrates attained the greatest celebrity as a physician, Aristotle seems to have bestowed most attention on the science of physiology in general, particularly on the nature and qualities of the blood. This was a necessary consequence of his attachment to the study of the natural history of animals, which led him, no doubt, to numerous dissections. In his physiological writings, this great philosopher assigns to the blood a very distinguished place among the component parts of the human body, and speaks of it as a fluid characterized by striking and peculiar properties. On some occasions he very explicitly declares it to be possessed of *life*. In the 19th chapter of the 3d book of his "History of Animals," we find the following sentence: "*Et semper quamdiu vita servatur, sanguis unus animatur et fervet.*" In the 2d chapter of the 1st book of the same work, our author says; "*Sanguis nempe, instar laris familiaris, est anima ipsa in corpore.*" And, in another place, he observes; "*In sanguine reperitur divinum quid, respondens elemento stellarum,*"

From these, and many other passages which I forbear to quote, it is evident, that Aristotle considered the blood as one of the *vital* parts of the body. Nor ought his authority on this subject to be treated with disrespect. Those who will take the trouble to examine his writings with the attention they merit, will find, that the knowledge he possessed of the blood, both in a healthy and a morbid state, would not disgrace even an enlightened physician of the nineteenth century. Though there is no reason to believe that he ever suspected it to be a circulating fluid, he was perfectly acquainted with the difference between venous and arterial blood. He had also a correct knowledge of the fibrina, or coagulating lymph, and declares that if this be removed, "*Sanguis neque concrescit neque spissatur.*" I shall take leave of Aristotle and his writings, by adding, that the existence of cold and hot blooded animals, and their relation to each other in the scale of nature, appear to have been as familiar to him as they are to the naturalists of the present day. To the catalogue of ancient philosophers, who believed in the vitality of the blood, we may add the respectable names of Empedocles, Critias, and Lactantius.

If, in matters of science, it were admissible to adduce authorities from works of imagination, I might further refer you to the Iliad of Homer, and the Æneid of Virgil, for the opinions entertained by the ancient Greeks and Romans, on the subject of the vitality of the blood. In the last and fatal interview between Hector and Achilles, Homer, as translated by Pope, puts into the mouth of the Trojan hero the following words:

> "I shall not fall a fugitive, at least,
> "My *soul* [or *life*] shall bravely issue from my breast."

This is evidently a figurative expression, where a part is made to stand for the whole. By *life* issuing from the breast, we are unquestionably to understand the poet to have meant the *blood*, which he considered as a *living* fluid, or the repository of life.

In describing the fall of Pallas by the sword of Turnus, Virgil says,

> " *Ille* [meaning Pallas] *rapit calidum frustra de vulnere telum:*
> " *Unâ eademque viâ, sanguisq; animusq; sequuntur.*"

which may be thus rendered in English:

> In vain the youth withdrew the reeking dart,
> For *life* and *blood* gush'd mingled from his heart.

In this last line the bard and philosopher evidently meant to express his belief, that in the living animal body, life and the blood are inseparably connected; or, in other words, that healthy blood is at least itself a *living fluid*, if not the common fountain of life to the solid parts of the body.

Passing over the long list of medical characters who flourished in Rome and other parts of the continent of Europe, as being little else than humble followers of their predecessors in Greece, I come down to the time of Dr. William Harvey, of England. That celebrated physician and anatomist, instead of tamely treading in the footsteps of those who had gone before him, exhibited, from his youth an originality and independence of mind, which prevented him from doing homage to any authority but that of nature. He lived and wrote about the beginning of the seventeenth century, and was decidedly the first medical philosopher of his age. Indeed when we consider the state in which he found his profession, and contrast it with that in which he left it, we are forced to acknowledge, that with regard to the importance of his contributions to the promotion of the healing art, he has not been surpassed by any of his successors. His discovery of the *circulation* of the blood, is an event familiar to every student in anatomy. But his opinion respecting the *nature and properties* of this fluid, can be learned only by such an examination of his writings, as is but rarely made by the physicians of the United States.

In pursuing his inquiries, relative to the generation of animals, Dr. Harvey was led to pay particular attention to the first appearance and progressive formation of the blood, as well as to its properties and uses in the living system. In the course of these pursuits he soon found reason to believe, not only that this fluid is itself possessed of life, but that it is the immediate fountain of both being and life to the other parts of the body. Hence he denominates it at one time "*particula genitalis prima*," and at another "*pars primigenia corporis;*" and, in support of the justness of these appellations, assigns the following reason, viz. "*Sanguis enim est, qui primus in generatione conspicitur.*" He further declares it to be the true "*primum vivens et ultimo moriens*" of animals. This opinion he endeavours to substantiate by arguments, which I shall make no apology for detailing in his own words.

"*Nec sanguis,*" says he, "*solum pars primigenia et principalis dicendus est quod in eo et ab eo motus pulsusque principium oriatur; sed etiam, quia in eo primum calor animalis innascitur, spiritus vitalis ingeneratur, et anima ipsa consistat.*"

"*Vita igitur,*" continues our author, "*in sanguine consistit, quia in ipso vita atque anima primum elucet, ultimoque deficit. Crebra enim, ut dixi, vivorum dissectione expertus sum, moriente jam animali nec amplius spirante, cor tamen aliquamdiu pulsare vitamque in se retinere. Quiescente autem corde, motum videas in auriculis superstitem, ac postremo in auricula dextra; ibique tandem cessante omni pulsatione, in ipso sanguine undulationem quandam et obscuram trepidationem sive palpitationem (extremum vitæ indicium) reperias.*"

Again: "*Quoniam itaque sanguis supra vires elementorum agit dictisque istis virtutibus pollet, atque summi opificis instrumentum est; nemo facultates ejus admirabiles et divinas satis unquam depraedicaverit. Habet profecto in se animam primo et principaliter, non vegetativam modo, sed sensitivam etiam et motivam.*"

I shall close my quotations from Dr. Harvey's works, by the following sentence, which exhibits, in a few words, the general outlines and substance of his opinion respecting the nature, properties, and importance of the blood.

"*Clare constat,*" says this able writer, "*sanguinem esse partem genitalem, fontem vitæ, primum vivens et ultimo moriens, sedemque animæ primariam; in quo, tanquam in fonte, calor primo et principue abundat, vigetque; et a quo reliquæ omnes totius corporis partes, calore influente foventur et vitam obtinent.*"

Considering the distinguished rank which Dr. Harvey holds among the medical characters of modern times, the extent to which he carried his inquiries respecting the blood, and the decided manner in which he expresses his belief in its vitality, it is somewhat singular, that his works should have been passed over, without notice, by subsequent writers, particularly by the late Mr. John Hunter, of London, whose writings on this subject now claim our attention.

With the foregoing opinions of Aristotle, Dr. Harvey, and several other writers, fresh in my recollection, I am at a loss to determine in what light to view the following passage in Mr. Hunter's "*Treatise on the Blood, Inflammation, and Gun-shot Wounds.*" "That the blood has life," says our author, "is an opinion *I have started for above thirty years,* and have taught it for near twenty years of that time in my lectures; it does not, therefore, come out at present as *a new doctrine*," &c. &c. If Mr. Hunter means by this to set up a claim to the *mere opinion*, that the blood is a living fluid, it is evident that his pretension is unfounded. So far from being a "*new doctrine*" in medical science, at the time he wrote, it had been believed and inculcated even for centuries. A moderate attention to the writings of his predecessors on this subject, would have taught him, that it was not till the disciples of Hoffman and Cullen, had too far discarded the humoral pathology, that any one ventured pointedly to deny the vitality of the blood.

With the writings of his countryman, Dr. Harvey, in particular, I am willing to believe that Mr. Hunter was unacquainted. For a want of information, even where it might have been easily obtained, is more excusable than a want of candour. Had Mr. Hunter, while actually possessing a knowledge of the sentiments of Dr. Harvey on the subject, endeavoured to establish a belief that he was himself the author of the doctrine of the vitality of the blood, he would be deservedly ranked with the worst of plagiarists. My regard for the memory of so great a man forbids me to admit that this was the case.

But while we deny the validity of Mr. Hunter's claim to the original doctrine of the vitality of the blood, it is but justice to acknowledge, that he was the first who brought this doctrine to the test of experiment. He possessed, unquestionably, much more enlightened, definite, and practical ideas on the subject, than any medical character who had gone before him. He had the invention and address to subject to the evidence of sense, what others had barely believed and reasoned on, as a point of speculative doctrine. While we withhold from him, therefore, the title of a *discoverer*, justice compels us to bestow on him that of an original and an able *experimenter*.

In pursuing his inquiries, relative to the nature and properties of the blood, Mr. Hunter's first object appears to have been, to fix on one or more phænomena or facts, that might serve as ultimate and incontrovertible tests of the existence of life in animal matter. By making, in this respect, an enlightened and judicious choice, he laid the best possible foundation for conducting his experiments to a satisfactory issue. The phænomena, or rather properties, which he selected for this purpose, were, 1st. *An inherent power of self-preservation.* And 2dly. *A susceptibility of the impressions of stimuli.* This last property is denominated by most physiologists, *irritability*, *excitability*, or stimulability, and by Dr. Darwin, *the sensorial power of irritation.*

As there appears to be throughout all nature a constant warfare between life and death, or between organized being and the powers destined to destroy it, the first and most simple act or operation of the vital principle is, to countervail the works and encroachments of dissolution. Whatever body or substance manifests a power of self-preservation, beyond that of common matter, placed under circumstances precisely similar, may be regarded as a being possessed of life. Thus a fresh egg will resist the influence of both heat and cold much longer than one that is stale or putrid. The reason of this is obvious. The former is in possession of the principle of vitality, whereas the latter has lost that preservative power. For a similar reason, a turnip or potato just taken out of the earth, is more difficult to freeze, and will resist the encroachment of putrefaction longer than one that has undergone the process of boiling.

But a mere resistance of the work of dissolution must be regarded as a passive property of life, or in other words, as life in a latent state. The first or lowest manifestation of active life, or life in a sensible state, is, motion resulting from the impression of stimuli. If we pass the electric spark or the galvanic influence through a piece of metal or charcoal, neither of these substances suffers any visible motion whatever. But if we pass them through a bit of muscle, just taken from the system of an animal not yet dead, the part is immediately thrown into lively contractions. Hence we are led to say, that the muscle is possessed of stimulability, or one of the lowest properties of life, while we regard the metal and charcoal as nothing but masses of dead matter.

Influenced by these views of the subject, Mr. Hunter very justly inferred, that if he could discover in the blood, after its elimination from the vascular system, a power of self-preservation, accompanied by a capability of being excited into motion by the application of stimuli, he would thereby prove it to be possessed of life. How far he succeeded in his attempt at this, appears from the result of his experiments detailed in his Treatise on the Blood, most of which I have myself repeated, and some of which I shall here recite.

In relation to a self-preserving power in the blood, we have to regret that our author has left his inquiry in a very imperfect state. He appears to have made but two experiments on this head, and even these have only an indirect reference to the point to be determined. The first of them was intended to ascertain, whether the blood of a young or of an old person will putrefy soonest. I shall relate the experiment in the writer's own words.

"June 24th. Some blood," says he, "was taken from a woman of twenty years of age, and its surface, after coagulation, was covered with an inflammatory crust.

"On the same day, some blood was taken from a woman, aged sixty, when the crassamentum was also covered with an inflammatory crust.

"These quantities of blood were set by.

"The blood from the old woman putrefied in two days. That from the young woman kept quite sweet till the fifth day, when it began to smell disagreeably; in this state, it continued two days more, and then emitted the common odour of putrid blood.

"Several experiments," he continues, "were made in the course of the summer, of a similar nature with the last, in all which it appeared that the blood from young people kept longer sweet than that which was taken from old."

The following was most probably the train of reasoning which induced Mr. Hunter to institute this experiment. Old age is an approach towards death. The principle of vitality, therefore, whatever it may be, is in a more feeble condition in the systems of old people than in those of young. But this principle will preserve from putrefaction, the parts of the system in which it resides, for a length of time directly proportioned to the degree of its strength. Now there exists, on chemical principles, no reason why the blood of old persons should putrefy sooner than that of young. If, therefore, it be found that it actually does putrefy sooner, the phænomenon can be explained in no other way, than by admitting, that the blood of both old and young possesses life, but that this principle exists in a much more feeble state in the blood of the former, than it does in that of the latter.

But, it has been already remarked, that this experiment is by no means decisive. The inference to be drawn from it is necessarily indirect, because the ground on which it was instituted is only constructive. Nor can I speak much more favourably of our author's second experiment on this head, the immediate object of which, to use his own words, is "To see if recent blood or coagulated blood lost their heat soonest.

"Four ounces of blood," says he, "after coagulating, was heated till it raised the mercury of a thermometer, placed in the middle of the coagulum to the 98th degree. The thermometer was placed in a similar quantity of blood, immediately after it was taken from the vein, and the mercury stood at 90°. These were placed by each other, and the thermometer put alternately in each, to observe how they parted with their heat.

"Coagulated blood	. . . .	98°.	Recent blood	. . . .	90°.
Do.	after 2 minutes	97°.	Do.	after 2 minutes	89°.
Do.	after 4 do. more	93°.	Do.	after 4 do. more	88°.
Do.	after 2 do. more	92°.	Do.	after 2 do. more coagulated	87°.
Do.	after 2 do. more	91°.	Do.	after 2 do. more	86°."

To understand this experiment it must be remembered, that Mr. Hunter had, by previous experiments, ascertained that *living bodies*, whether animal or vegetable, when exposed to a medium colder than themselves, part from their heat more slowly than such as *are dead*. In the instance under consideration, he appears to have regarded the coagulated blood as little else than *dead matter*, and to have held the comparative facility with which the two portions of this fluid might let go their heat, to be a proper test to determine, what difference existed between them in point of vitality. As the blood recently drawn, proved most retentive of its heat, this circumstance can be accounted for only, by ascribing its superior power of retention, to the operation of the vital principle.

Mr. Hunter was more fortunate in his attempt to detect in the blood the second property, viz, *stimulability*, or a power of active life. He conceived that from this property arose the coagulation of the blood, a process which he considered as perfectly analogous to muscular contraction.

"To see," says this enlightened physiologist, "whether a stimulus can be applied to the blood, so as to make it coagulate faster than it does naturally, I desired the following experiment to be made.

"Three ounces of blood were taken from a boy about ten years of age, and immediately after the cup was put into water heated to 150°. A similar quantity was taken in another cup from the same boy, at the same time, which was put into water heated only to 48°. The first coagulated completely in five minutes, but the latter remained perfectly fluid for twenty minutes, and then began to coagulate, but was not completely coagulated for five minutes more.

"This experiment shows," continues he, "that heat above the natural standard acts as a stimulus upon the blood, and makes it coagulate considerably sooner than cold does."

An experiment which Mr. Hunter made on the muscle of a sheep, compared with the foregoing, exhibits such a striking analogy between the coagulation of blood and muscular contraction, as cannot fail to produce at least a strong presumption, that these two phænomena depend on the same causes.

"As soon," says our author, "as the skin could be removed from a sheep that was newly killed, a square piece of muscle was cut off, which was afterwards divided into three pieces, in the direction of the fibres. Each piece was put into a basin of water; the water in each being of different temperatures, viz. one 125°, about twenty-seven degrees warmer than the blood; another 98°, the heat of the animal; and the third 55°, about forty-three degrees colder than the animal. The muscle in the water heated to 125°, contracted directly, so as to be half an inch shorter than the other two, and was hard and stiff. The muscle in the water heated to 98°, after six minutes, began to contract and grow stiff; at the end of twenty minutes it was nearly, though not quite, as short and hard as the above. The muscle in the water heated to 55°, after fifteen minutes began to shorten and grow hard; after twenty minutes it was nearly as short and hard as that in the water heated to 98°. At the end of twenty-four hours they were all found to be of the same length and stiffness.

"Here," continues the writer, "is also a similarity in the excitements of coagulation in the blood, and of contraction in muscles, both apparently depending on the same principle, namely, *life*."

The following is another interesting experiment of our author, in his inquiry respecting the *excitability* of the blood.

"Some blood," says he, "was taken from the arm into a basin, stirred, and then mixed with different infusions as follows:

"Two ounces with the same quantity of the infusion of gentian; two more with two ounces of the watering solution of opium; and two ounces were kept in a vessel by themselves.

"The blood which had been mixed with the bitter infusions, and the simple blood all coagulated at the same time, viz. in six minutes; but the blood which had been mixed with the solution of opium did not coagulate for twelve minutes, and then the coagulum was very loose.

"This experiment with the opium was repeated, and the result was exactly the same."

In this instance the solution of opium appears to have acted so suddenly and powerfully on the excitability of the blood, with which it was mixed, as to produce in it a state of exhaustion bordering on death. Hence, that portion of the blood coagulated more slowly and feebly than either of the others.

But Mr. Hunter appears to have drawn his favourite argument in support of the vitality of the blood, rather from the uses and functions of this fluid in disease, particularly in inflammation, than from any experiments performed on it, after its emission from the vascular system. In cases of wounds he found coagulated blood to constitute the *bond of union by the first intention*. For, says he, this "union is not that of two divided parts to each other, but the union of these parts to the intermediate extravasated blood; so that it is the blood and parts uniting, which constitutes union by the first intention."

It may certainly be considered as a self-evident truth, (if any physical truth can be called self-evident) that no healthy and permanent union can take place between living and dead animal matter. Such an union, being necessarily the result of reciprocal affinity or appetency, calls for active life in both of the uniting parts. If, then, as our author asserts, and as I believe to be true, extravasated blood, and divided flesh, form a real and living union, the former must possess the principle of life, in common with the latter.

But Mr. Hunter carries his ideas on this subject still further. He maintains, that blood extravasated and coagulated in any part of the living system, is not only to all intents and purposes a living substance, but even possesses within itself a kind of creative power. That it is capable, not only of uniting to the surrounding and contiguous parts, but of forming within itself, and by its own operation, both vessels, nerves, and bone.

"Blood thus extravasated," says he, "forms either vessels in itself, or vessels shoot out from the original surface of contact into it, forming an elongation of themselves, as we have reason to suppose they do in granulations. I have reason, however, to believe that the coagulum has the power, under necessary circumstances, to form *vessels* in and of itself." In the next page he observes, "As the coagulum, whether wholly blood, or coagulating lymph alone, has the materia vitæ in its composition, which is the cause of all its actions, it soon opens a communication with the mind, forming within itself *nerves*. Nerves have not the power of forming themselves into longer cords, as we conceive vessels to have; for we know, that in the union of a cut nerve, where a piece has been taken out, it is by means of the blood forming a union of coagulum; and that the coagulum becomes gradually more and more of the texture, and has of course more and more the use of a nerve, somewhat similar to the gradual change of blood into a bone in fractures."

As it would be difficult to deliver the whole of Mr. Hunter's arguments in favour of the vitality of the blood, without reciting too great a portion of his work on that subject, I must earnestly recommend the work itself to your close and serious attention. I cannot, however, close my account of that performance, without giving the following quotation from it, which shows, perhaps, in a more striking manner than any other passage it contains, the elevated rank which our author assigned to the blood, as a component part of the animal system.

"No part of the body," says he, "is to be considered as a complete living substance, producing and continuing life, without the blood: so that blood makes one part of the compound, without which life would neither begin nor be continued. Any living part, or whole, shall die in a little time, by simply preventing the blood from moving through the vessels: under this idea it is not clear to me, *whether the blood dies sooner without the body, or the body without the blood.*"

LECTURE II.

GENTLEMEN,

Having presented you with a summary view of the sentiments of others, respecting the vitality of the blood, I proceed to detail to you my own experiments and opinions on that subject. In doing this I disclaim all pretension to original discovery. Actual discovery in physiological science, is an honour of which few persons are privileged to boast. My only merit (if indeed I have a just claim to any) in the present instance, consists in having taken up the matter where Mr. Hunter left it, performed sundry experiments which he had omitted, and delivered, in a form somewhat compact, a chain of reasoning on the subject, which he had either totally neglected, or scattered through such a mass of irrelevant matter, as to impair its perspicuity and weaken its force.

As the success of this inquiry does not depend on a determination of the question, What life is? whether it be material or immaterial, a substance or a quality? I shall decline entering on the discussion of that intricate point. It is proper, however, to remark, that whatever may be the nature or essence of life, we have facts sufficient to convince us, that it neither results from, nor has any necessary dependence on, the mere organization of matter. Organization is indeed essential to certain given modes, functions, or operations of life; but not to the simple existence of life. It modifies and gives effect to vital action, but cannot become its real source; in the same manner as the barrel of a musket determines the direction of the ball, but can never set it in motion, without the explosion of the powder within.

In entering on my course of experiments, I took, as standards or tests of vitality, the same properties which had been received as such by Mr. Hunter, namely, *stimulability*, and *an inherent power of self-preservation*. I considered the coagulation of blood, after its emission from the vessel that contained it, as arising from the former of these properties, and as being in all respects analogous to muscular contraction. My first object was, therefore, to try the effects of various stimuli, not before employed for that purpose, in quickening the commencement and progress of this phænomenon. The first stimulus applied with this intention was electricity.

EXPERIMENT I.

Four ounces of blood were drawn from the arm of a person in health into a small tin cup, and set aside as a standard of natural coagulation. From the same orifice were immediately drawn four ounces more, into a similar cup, which was connected to the prime conductor of an electrical machine. Through this latter portion of blood repeated shocks were passed, alternated with sparks drawn from various parts of it. It coagulated sooner by several minutes, than the blood to which no electricity was applied. The coagulum, when formed, was alike firm in each portion. This experiment was repeated more than twenty times, under various modifications, and on the blood of different persons, both sick and well, old and young, without any material difference in the result.

In cases of disease, where the blood taken from the patient and suffered to coagulate *naturally* was sizy, that subjected to the influence of electricity was free from size. The reason of this is obvious. That portion of blood to which electricity was applied, coagulated, in consequence of this artificial stimulus, too suddenly, to allow the red globules to sink to the bottom, and the fibrina or coagulating lymph to ascend, and form a stratum above them. For the size of blood is nothing but a stratum of the coagulating portion, formed in that way. And it will be found on examination, that blood which coagulates suddenly, let the cause of this coagulation be what it may, never exhibits a sizy covering. I am induced, therefore, to believe, in opposition to the opinion of Mr. Hunter, that this covering arises from a degree of exhaustion, or in other words, an enfeebled state of life in the blood. Perhaps I might properly term it a partial paralysis of the blood. Yet on some occasions blood of this description, when stimulated to action, contracts with a preternatural degree of force. This is to be regarded, probably, as a tonic spasm of the fibrina of the blood.

In inflammatory fever, where sizy blood constitutes, for the most part, one of the characteristic symptoms, this fluid may be considered, like other parts of the system, in a state of exhaustion, in consequence of the excess of stimulus to which it has been subjected. When drawn from the vein, therefore, it requires a greater length of time, than if it were in a natural and healthy state, to coagulate, or contract itself, analogous to what happens to muscular fibres, when partially exhausted of their excitability, by an excess of stimuli. This tardiness of coagulation allows the red globules, being the heaviest portion of the blood, time to descend, before they become entangled by the coagulating lymph, which they leave, in the form of a white or yellow membrane, on the top of the crassamentum. I have found by experiment, as will hereafter appear, that by drawing healthy blood into a tin cup reduced to the temperature of about fifty degrees, and allowing it to stand in a medium of the same temperature, it will generally, if not uniformly, in consequence of its slow coagulation, exhibit a sizy covering.

EXPERIMENT II.

To ascertain whether or not the effect of electricity on muscular substance, be analogous to that which it produces on the blood, the following experiment was performed.

A bit of muscle, about four inches in length, was taken from the neck of a calf, immediately after the animal had been knocked down, and while it still manifested strong symptoms of life. This muscle being divided longitudinally into two equal parts, one half of it was placed on a table where it lay undisturbed, while the other had frequent shocks of electricity passed through it. The electrified portion of muscle was perceptibly convulsed, and soon became nearly half an inch shorter than the other. The experiment was repeated several times, without any sensible difference in the result.

Here we see that the coagulation of blood, and the contraction of muscular fibres, are alike accelerated by the influence of electricity. Is it not reasonable, then, to infer, that these phænomena, so precisely analogous, result from the same cause, namely, the action of stimulus on excitability?

EXPERIMENT III.

My next object was to ascertain the effects of different gases on the coagulation of the blood. For this purpose I took five small glass jars, one of which I filled with oxygenous gas, another with carbonic acid gas, a third with nitrous gas, a fourth with hydrogenous gas, and the fifth with common atmospheric air. Into each of these I drew from the same orifice in the arm of a robust, healthy, young man, about three ounces of blood, agitating the jars as soon as the quantity of blood was received into them, in order that the gases might come in contact with every part of this fluid. Those portions of blood subjected to the action of the oxygenous and carbonic acid gases, coagulated at about the same space of time after their emission from the arm; those recieved into the jars containing hydrogenous gas, and common atmospheric air, were some minutes longer in coagulating; but that recieved into the jar of nitrous air could not be properly said to coagulate at all, as the gas acted chemically on it, changing it in some measure into a new compound. This portion, like the others, became solid, but it had not the solidity of true coagulation, in the light in which we apply this term to the blood. The result of this experiment, under several repetitions, was in substance the same. Those portions of blood exposed to the action of the oxygenous and carbonic acid gases, always coagulated first, because, as I conceive, these airs are more active stimulants than the others. All the portions (that in contact with nitrous air excepted) after having stood twelve hours, were precisely alike, with regard to their separation into serum and crassamentum.

EXPERIMENT IV.

THIS experiment was made with a view to determine the effect of temperature on the coagulation of the blood. Four small tin cups were taken, into each of which three ounces of blood were drawn, from the arm of a person in health. One of these was exposed to a temperature of 125°, another to a temperature of 98°, a third to a temperature of 80°, (the temperature of the atmosphere at the time the experiment was made) and the fourth to a temperature of about 52°, being that of our pump water when first drawn. That portion of blood exposed to a heat of 125°, coagulated in a very short time; that exposed to a heat of 98°, was several minutes longer in coagulating; that exposed to a heat of 80°, coagulated at a still later period; while that exposed to a heat of 52°, remained fluid nearly three times as long as either of the other portions, and, when it did at length coagulate, exhibited a sizy covering.

The explanation of this experiment is obvious and easy. The blood in the different cups coagulated with a celerity proportioned to the stimulus of heat, acting on its excitability. The whole experiment is perfectly analogous to that made by Mr. John Hunter, on the muscle of a sheep's neck. The sizy covering of that portion of blood exposed to fifty-two degrees of heat, is to be attributed entirely to the slowness of its coagulation. Its long fluidity, as mentioned on a former occasion, gave time for the red globules to fall to the bottom of the cup, leaving the lighter coagulating lymph at top.

EXPERIMENT V.

BELIEVING that blood (other circumstances being alike) always coagulates with a celerity proportioned to the degree of vitality it possesses, and concieving arterial blood to possess a higher degree of this principle than venous, I made, for my satisfaction on these points, the following experiment.

Into two glass tumblers, of the same size and form, were drawn, at the same time, six ounces a piece of blood from the carotid artery and jugular vein of a calf. These portions of blood were exposed only to the action of the atmosphere. That drawn from the artery coagulated first by nearly two minutes. On repeating this experiment several times, I found no material difference in the result. The arterial blood always coagulated from one to two minutes sooner than the venous. If we attend the shambles of a butcher, while he is slaughtering calves or sheep, we may observe, that when the large vessels of the neck are divided, the florid blood issuing from the arteries never fails to coagulate sooner than that from the veins.

EXPERIMENT VI.

When animals (to use a common expression) are *run to death*, as sometimes happens in the chase of the fox and the stag, as well as in the savage pastime of *bull-baiting*, the muscles can neither be made to contract nor the blood to coagulate. Considering this circumstance as arising from the complete extinction of life in every part of the system, in consequence of the excessive stimulus of exercise, I concieved that a similar effect might be produced on the blood, by subjecting it to the action of an excess of stimulus, after its emission from the vessel that contained it. To determine this point the following experiment was made.

I took two eight-ounce phials and drew into each of them four ounces of blood, from the same orifice in the arm of a healthy person. The phials were carefully closed by stoppers, and one of them placed on a table, where it was suffered to remain at rest, while the other was briskly and somewhat forcibly agitated in the hand for the space of an hour, and then placed on the same table and disturbed no further. The blood in the first phial coagulated in the usual time, while that in the other, which had undergone agitation, remained perfectly fluid till it became putrid, which event occurred in it three days sooner, than it did in that which was allowed to coagulate.

There is no difficulty in explaining this experiment. The stimulus of agitation, like that of excessive exercise to the entire animal, totally destroyed the vital principle of the blood, and in that way disqualified this fluid for either coagulating, or resisting, in any measure, the process of putrefaction. The same result was obtained in several repetitions of this experiment.

EXPERIMENT VII.

This experiment was made with a view to determine the difference between the celerity in the coagulation of the blood of old and of young persons.

Into a small glass jar was drawn four ounces of blood, from a free orifice in the arm of a woman about eighty-four years of age. At the same time, in the same room, and into a similar jar, was drawn an equal quantity of blood, from a similar orifice in the arm of a female, aged twenty years and a few months. Both these persons were in good health. The two portions of blood, without undergoing any agitation, were set along side of each other on the floor of the room. That drawn from the young woman coagulated first by several minutes. On repeating the experiment with the blood of other similar subjects, the result was in substance the same. The youthful blood always coagulated first. In explanation of this experiment, it is necessary only to remark, that the excitability of the blood, like that of the muscles, is less obedient to the action of stimuli in old people than in young. The blood, as well as the solids of persons far advanced in years, is in a state of exhaustion, or partial paralysis.

EXPERIMENT VIII.

If the limb of a living animal be first exposed to a degree of cold sufficiently intense to freeze it, and immediately afterwards to a medium of a high temperature, mortification and death of the part are known to ensue. To ascertain what effect a similar treatment would produce on the blood, the following experiment was made.

A tin tube, half an inch in diameter, being first reduced to the temperature of Zero, was filled with blood as it flowed from the arm of a person in health. The tube with its contents was then immersed in a freezing mixture, composed of salt and pounded ice, having its open end closed by a cap made for the purpose. Long before it had time to coagulate, it was converted into a column of ice. The tube, at the expiration of twenty minutes, was withdrawn from the freezing mixture, and suddenly plunged into water, raised to the temperature of 126° of Farenheit. The blood when thawed, which event took place in a short time, was thin and dark coloured, precisely similar to dissolved blood, drawn from a person in the last stage of a malignant fever. It was now poured from the tube into a glass tumbler, in order that whatever phænomena should occur in it might be more accurately observed. It remained liquid nearly an hour, but at length coagulated so far as to acquire a consistence somewhat similar to that of molasses. It continued, however, an uniform mass, without separating into serum and crassamentum, and putrefied sooner, by several days, than another portion of blood taken from the same person, at the same time, and allowed to coagulate in the natural way. On repeating this experiment several times, the result was, in no instance, materially different from the foregoing.

There appears to be no difficulty in explaining the phænomena which here occurred. In consequence of the extremes it underwent, and the violent treatment to which it was exposed, the vital principle of the blood, though not entirely destroyed, was so far weakened, as to be unfit for the performance of its usual functions. The fibrous portion, therefore, was unable to contract so strongly as to press the serum from among the cruor; hence, the tenderness and uniform consistence of the whole mass; and the *vis conservatrix*, or power of self-preservation, being also exhausted, there was nothing to resist the approach of putrefaction.

With a view to inquire into the existence of a further analogy between blood and muscular flesh, I took a bit of muscle from the neck of a calf, just knocked down, and divided it into two portions of equal length. One of these was laid on a table as a standard of comparison, and the other buried in a freezing mixture, composed of salt and snow. The latter portion began to turn pale, and was soon frozen perfectly stiff, without suffering any perceptible diminution of its length. But the other lost of its length by contraction, I think, not less than three quarters of an inch, and became firm and resisting to the touch. The section of muscle was now withdrawn from the freezing mixture, and thrown into water heated to the 126th, degree. As soon as it was thawed it was taken out of the water, when it began to contract perceptibly, but never lost so much of its length as the other, nor did it ever acquire such a degree of firmness, but continued more soft and yielding to the touch. These two sections of muscle were now placed on a table, and suffered to remain in that situation, subject to the action of the same causes, in order to determine their liability to putrefaction. The result of this experiment was precisely analogous to that just related, respecting the blood. The portion of muscle that had been frozen became putrid first by upwards of two days.

The striking similarity here manifested, between the phænomena exhibited by frozen blood, and muscular flesh, placed under similar circumstances, could not have resulted from dissimilar causes. The principle of life must have been their common source.

It is a fact well known to cooks and epicures, that the process of freezing encreases the tenderness of poultry, venison, and other kinds of flesh. This it does by destroying in them the last remains of the vital principle. For a similar reason, the flesh of animals that have fallen dead, under the fatigues of the chase, is very tender and liable to a speedy putrefaction. In this case, complete *exhaustion* or *perfect death*, is the consequence of an excessive degree of fatigue.

EXPERIMENT IX.

Living bodies have seldom the same temperature with the medium that surrounds them, whether they be inhabitants of the air or the water. They have each a given or natural temperature of their own, which it is one of the known properties of life to preserve, to a certain extent, without much increase or diminution, whatever may be the degree of heat or cold to which they are exposed. Thus, vegetables and some cold blooded animals are cooler in summer, and warmer in winter, than the surrounding atmosphere, the vital principle aiming, under all circumstances, at the preservation of a permanent or uniform temperature. The present experiment was made with a view to ascertain, whether or not this *heat-maintaining* power, is possessed by the blood.

For this purpose, equal quantities were taken of blood fresh from a vein, and of a solution of gum arabic in water, with some salt added to it, of the consistence and temperature of fresh blood. These were put into two tin cups, and exposed to a temperature of about 40°. In each cup was placed a thermometer, sensible and well graduated. The mercury, which stood at first at about 94°, began to fall rapidly in the cup containing the solution of gum arabic, but much less so in that which contained the blood. This difference, between the heights of the two columns of mercury, was seldom less than twelve or fifteen degrees, so that when the thermometer in the solution stood at 50°, that in the blood was as high as 65°. At or about this degree the temperature of the blood became stationary, and could not, by the medium in which it was placed, be reduced lower till it had undergone coagulation. The solution of gum arabic, on the other hand, soon fell to the temperature of the surrounding medium.

This experiment was repeated frequently, and with various viscid fluids, without exhibiting any material difference in the result. No fluid, whatever might be its viscidity or composition, was found so retentive of heat as the blood.

To what cause shall we ascribe this extraordinary power of retention in the blood? Not to its greater viscidity; for some of the fluids used as tests were even more viscid than it; nor can we attribute it to the process of coagulation; for the blood did not coagulate, till after the other fluids were reduced to the temperature of the medium in which they were placed. It would seem, then, that this heat-retaining power must be ascribed entirely to the principle of vitality possessed by the blood. To the action, if not to the very existence, of this principle in the blood, the presence of heat appears to be necessary. Its powerful retention of heat, therefore, must be regarded as an effort at self-preservation.

EXPERIMENT X.

To ascertain the effect of the galvanic influence on blood, was the purpose for which this experiment was instituted.

It occurred to me that, admitting the blood to be possessed of life, this influence might operate on it in a two-fold manner. 1st. When fluid it might accelerate its coagulation; and 2dly. After coagulation it might excite it to contractions, that is, such motions, as arise from the application of a stimulus to a substance possessing the principle of irritability. For determining these points the following measures were adopted.

Into two small tin cups were drawn about three ounces a piece of blood, from the arm of an adult in perfect health. The blood in one cup was set aside as a standard of natural coagulation, while that in the other was subjected to the influence of the galvanic fluid, discharged from an apparatus of considerable power. The effect, with regard to accelerating the process of coagulation, was such as I had anticipated. The galvanized portion of blood coagulated several minutes before the other. Such was the issue of many repetitions of this branch of the experiment. But the latter and most interesting branch was yet to be tried.

In order to determine this, two fresh portions of blood were drawn, and suffered to coagulate. One portion of coagulum was then set aside, while the other was subjected to the galvanic influence, and carefully inspected during the operation. As no microscope was at hand, I was obliged to depend on the naked eye. Notwithstanding the most close and attentive inspection, no movements were observed in the galvanized coagulum, but such as were attributable to *mechanical impulse*. For, in this experiment, it was necessary to bring the two poles of the apparatus into contact with the blood. It was observable, however, that that portion of coagulum acted on by the galvanic influence, began to diminish in size, and press out its serum sooner than the other, which had been suffered to remain at rest. But, as this diminution of bulk, and the discharge of serum, appear to result from a *power of contraction*, I conclude that coagulated blood does contract, when exposed to the galvanic influence, though each act of contraction is in itself too inconsiderable to be perceived by the naked eye. This second branch of the experiment was, like the first, repeated several times, without any material variation in the result.

The present is not an improper occasion to observe, that the application of any stimulus whatever to blood, that has undergone coagulation, accelerates the contraction or diminution of the coagulum. Thus, for example, if we take two portions of recently coagulated blood, and expose one of them to the action of the atmosphere, at the temperature of 75°, and the other to the action of water, raised to the temperature of 125°, the coagulum of the latter portion will never fail to contract, or be diminished in bulk, and press out its serum, in a much shorter time than that of the former. This experiment I have often repeated, and always with the same issue. It can be explained only by admitting, that coagulated blood is, like muscular flesh, susceptible of the stimulant impression of heat. For it will be observed, that a temperature of 125° is not sufficiently high, to produce contraction in the fibrina of the blood, on principles purely chemical.

EXPERIMENT XI.

Having ascertained (see experiment 9) that blood newly drawn parts from its heat much more slowly than other fluids of a similar consistence, when raised to the same temperature, I was desirous of determining what were its properties, with regard to the reception of artificial heat above its natural standard.

For this purpose I drew into a small tin cup about four ounces of blood, in which, when a thermometer was placed, the mercury stood at about 95°. Into another similar cup was poured an equal quantity of a solution of gum arabic in water, with a portion of salt added to it, of about the same consistence, and raised to the same temperature. These cups were placed along side of each other, in water heated to the one hundred and thirtieth degree, each cup holding a thermometer corresponding to the other, in its graduation and sensibility.

In less than two minutes the thermometers began to manifest different temperatures, that in the blood being very perceptibly the lowest. This difference continued to increase, till it amounted at length to several degrees. When the blood began to coagulate, its temperature was about four or five degrees below that of the solution of gum arabic in water, and the difference was still increasing. After its coagulation the experiment was considered as terminated; because, so widely different was its consistence from that of the solution, that no satisfactory inference could be drawn, from any further difference, that might occur in their respective temperatures. This experiment was, like all the preceding ones, repeated a sufficient number of times, to ascertain the certainty of the above result.

EXPERIMENT XII.

Though this experiment did not succeed completely to my wishes, owing to the extreme difficulty of conducting it, yet the result of it was such, as clearly to demonstrate the correctness of the principle, on which it was instituted. The following are the considerations that led to its performance.

The blood of animals that are destroyed by lightning, (provided death be instantaneous) will not coagulate. This I conceived to be owing to the complete extinction of life, in both the solids and fluids, by the violence of the shock. I had already ascertained that electricity, applied in a moderate degree, acts on the blood as a powerful stimulus. It appeared probable, therefore, that if the shock were sufficiently increased, it would act like lightning on the entire animal, and so completely paralyze the blood, as to prevent its coagulation.

To bring the correctness of this inference to the proper test, I drew, from the arm of a young gentleman in health, into two similar vessels, nearly similar quantities of blood. One of the vessels was set aside, in order that the blood in it, might serve as a standard of natural coagulation. Through the blood contained in the other, three very powerful shocks of electricity were passed, immediately after its issuing from the vein. The blood in the first cup coagulated in about the usual time, while that in the other, remained quite fluid more than five times as long, and, even then, underwent but a very imperfect coagulation. It retained, to the last, the appearance of blood, closely bordering on a state of dissolution. This difference of coagulation I was able to attribute to nothing else, but the partial extinction of life, in that portion of blood, through which the electric fluid was passed.

I have thus faithfully, and as concisely as possible, related the principal experiments, to which I subjected the blood, in the course of my investigation. As their drift is neither obscure nor equivocal, and as I have subjoined to each a few explanatory remarks, it would be useless to make them the subjects of any further comment. It will be found, on the most careful examination and comparison of them with each other, that they give rise to no discordancy of facts. The more attentively they are scrutinized, the more perfectly will they be found to harmonize with each other, and the more striking will appear their concurrence, in support of the doctrine of the *vitality of the blood*. As far as I am capable of understanding and unfolding it, such is their true and only interpretation.

An experiment was performed in the year 1801, by Dr. James Hutchison, and afterwards published in his Inaugural Dissertation, which seems alone sufficient to establish the doctrine of the vitality of the blood. I shall relate it in the words of the ingenious author.

"A small quantity of blood," says he, "was obtained from a vein. I received it on the back of my hand; in a few minutes a thin coagulum was formed, which was immediately applied to an ulcer; a piece of oiled linen was placed over it, and the whole secured moderately tight with a bandage. It was examined in twelve hours afterwards; and I observed that it adhered, in many parts, to the ulcer. In twenty-four hours the adhesion was more complete, and those parts which had not united, had become quite putrid; I therefore removed them. In twelve hours more the ulcer was again examined, and the surface appeared very uneven, though perfectly healthy; the granulations being much more eminent, where the coagulum had adhered."

As we cannot conceive it possible for a vital and permanent union to take place between a piece of living and a piece of dead matter, we must admit that the portion of blood used in this experiment was possessed of a principle of life. The only thing wanting is, that the experiment should be further established by frequent repetitions.

LECTURE III.

GENTLEMEN,

Having finished the two principal divisions of my subject, I shall proceed to the statement of a few further facts and considerations, in support of the doctrine of the vitality of the blood. These shall be drawn from certain states and phænomena of the living system, with which you must all be acquainted, and the inferences from which you are therefore prepared to understand.

Under this head of our inquiry, as under the two foregoing ones, my object will be accuracy rather than originality; for I do not know, that I have any thing positively new to offer. The utmost limit of my pretensions will be, to make, perhaps, a new and uncommon use, of old and common materials. That I will repeat what has been already advanced by other inquirers, is very probable. But not having, at present, any book before me, and not remembering to what source (whether reading, or personal observation and reflection) I am indebted for the facts and opinions I may use, I shall not, for the mere purpose of appearing learned, distract your attention or burthen your memories, either by numerous quotations, or frequent references to authorities. From whatever quarter my arguments may have been originally received, I consider myself alone responsible for their correctness and authenticity.

To remove, as far as practicable, any existing prejudices that might act as barriers against their admission, and thus prepare the way for what may be afterwards offered, I shall endeavour, in the first place, to show, that it is neither unreasonable in itself, nor inconsistent with our knowledge of nature, to believe in the doctrine of the vitality of the blood.

It is asserted by the opposers of our doctrine, that blood cannot be vital, *because it is a fluid*. But when duly examined, this assertion can have no weight, for it is founded on a ground hypothetical and visionary. It is predicated on a circumstance, of which we have no accurate knowledge, namely, *the nature or essence of life*. It amounts to a positive declaration, that life and fluidity are incompatible with each other; or that, *to be fluid*, and *to be dead*, are synonimous expressions. Though it is *possible* that this declaration may be true, yet we are obliged, in the present state of science, to regard it as a mere arbitrary assertion, calculated to prove nothing but a want of reflection in those who use it. Such a declaration should never be hazarded, except when it relates to objects fully and familiarly known to us, both in their nature, and in all their relations.

We know but very little of the nature of *fluidity*, and, as already observed, nothing at all of the nature of *life*. On what shadow of ground, then, can any one pretend to affirm, that they are necessarily incompatible with each other? This is like declaring, that *matter* is in its nature inconsistent with *spirit*, and *spirit* with *matter*, when, in reality, we have not the slightest knowledge of the nature of either. With as much reason might I reverse the assertion, and maintain, that life and fluidity are in their natures essential to the existence of each other, and therefore, that blood must be vital because it is fluid. In support of this, I might refer to the writings of Bonnett, Spalanzani, and others, who have taught us, that certain species of animalculæ lose every attribute of life, if suffered to become dry, but are immediately reanimated on being immersed in water. But such reasoning, being altogether sophistical, and not calculated either to enlighten or convince, ought not to be employed in a discussion like the present.

As soon as we shall have acquired a perfect knowledge of the nature and mutual relations or affinities of life and fluidity, and not till then, we will be able to determine, *a priori*, whether they are incompatible with each other or not. In the present state of science, there is nothing to justify an attempt to decide on this subject. Silence, therefore, is most safe and becoming, in a matter where facts are wanting to direct the judgment.

It is again asserted, that *life* can reside only in, or be attached to, organized matter; and that the blood being destitute of organization, cannot constitute a proper receptacle or residence for this principle. Others consider life as nothing more than a mere quality of matter, resulting entirely from organization. Organization, say both these parties, is essential to the existence of life; but the blood is not organized; therefore life cannot reside in the blood.

These objections, being in their nature, nearly the same with the preceding one, may be satisfactorily answered on the same principle. Our knowledge of the nature and affinities of life is too imperfect, to enable us to say, *a priori*, whether it be in any measure dependent on organization or not. We are in possession, however, of innumerable facts, which strongly favour a belief, that it is not dependent on any such state or arrangement of matter. In the eggs of many species of animals, no organization has ever been detected. The contents of some of them are even more fluid than the blood itself. Yet, no one pretends to deny, that in these masses of matter, rude and inorganic as they are, the principle of vitality does actually exist. Nor is it till after the excitement of this principle, by means of heat, that the first lineaments of organization make their appearance. Here, then, instead of life being dependent on, or the mere result of, organization, organization is evidently the result of the principle of life. In other words, the principle of life is the efficient *cause*, while organization is only an *effect*. To contend, then, that the former is in any measure dependent on the latter, is to argue in opposition to the order of nature.

But we have still further facts to prove, that certain bodies, to all appearance destitute of organization, *are alive*, and that mere organization is not alone sufficient, either to confer on, or retain in, matter, the attributes of vitality. It is well known, that certain tribes of marine animals exist, and are possessed of life, which, on the most minute inspection, do not exhibit a vestige of organization. Of this description are the several species of the genus *molusca*, which, from their soft consistence, and inorganic form, are denominated by sailors the "sea jelly." Compared to these animals, *coagulated blood* may be considered as a substance regularly and even firmly organized. Indeed the fibrous portion of red blood, when separated from the serum and globules, exhibits more of the genuine characters of organization, than many aquatic animals, found in our own creeks and rivulets. There appears no just ground, then, to deny to the blood the possession of life, merely on the plea of its being a mass of inorganic matter. Unorganized it certainly is, and must be, while circulating through the vascular systems of animals. But, when drawn from them, it is capable of *assuming an organic form.* In this respect, it even surpasses in vital action, the *solid parts* of animals. Without any aid from them, it is capable of modifying and greatly altering its form and appearance; whereas, without its co-operation, they are unfit for any vital function.

If, as some contend, life be nothing more than a quality of matter, resulting from the circumstance of organization, *death* and *disorganization* must be synonymous terms. For the death of the body is nothing more than the destruction or removal of the cause of its life. Without disorganization, death, agreeably to this hypothesis, cannot take place. But, as far as the observations of physicians, and the dissections of anatomists have gone, the organization of the dead body, previously to the commencement of putrefaction, is oftentimes as perfect as that of the living. Added to this, several of the causes which destroy life, are of such a nature, as not to be capable of producing disorganization. Of this description are *cold water* and *mephitic air*. The former of these, if taken copiously into the stomach, when the system is much heated, and the latter, if received into the lungs by respiration, are known to produce sudden death, without leaving behind them, even in the organs which they primarily affect, the smallest vestiges of disorganization. Similar remarks may be made, with respect to death from *drowning* and *excessive cold*, in which cases the organization remains unimpaired.

Though we may not add much to our knowledge of the subject under consideration, we will perhaps become somewhat better acquainted with ourselves, by a brief inquiry into the cause, why organization has been generally considered as essential to the existence of a vital principle? or, in other words, why life has been attributed only to organized, and not to fluid, forms of matter? These are questions, the true solution of which, appears but little calculated to flatter our vanity.

Were the operations of our minds, and the origin of many of our opinions, subjected to a rigid analysis, we would find much difficulty in supporting our claim to the proud title of *rational beings*. It is at least doubtful, whether we would not be obliged to exchange it in most cases for that of *creatures of habit*. But be this as it may, habit, whether right or wrong, certainly possesses a very extensive influence over both our corporeal and intellectual actions. By habit, in the present instance, I mean artificial associations, formed in our minds, and which, when formed, we are apt to consider as natural, between certain external appearances and particular inherent or essential qualities. These associations or habits, when contrary to reason and confirmed by time and repetition, are denominated prejudices. Thus, for example, an ignorant clown who has never seen charcoal but in the form of a mass of black matter, entertains a firm belief, that it cannot exist any other form. Show him a diamond, and he will consider you either silly or mad, if you assert that it is nothing but a mass of charcoal. Nor, will he think less disrespectfully of you, should you endeavour to persuade him, that this apparently inert substance, bears any part in producing the briskness and exhilarating quality of cyder, ale, and champaign. So firmly, in his mind, are the ideas of *blackness* and *solidity*, associated with that of charcoal, that unless he can discover the two former, he will obstinately persist in denying the presence of the latter.

An ignorant native of the torrid zone, who has never visited any of the higher latitudes, has no conception of the existence of water, in any other form, than that of an azure coloured fluid. Tell him that in distant countries, it is converted, sometimes into a firm glass-like substance, called ice, and at other times into a white flaky one, denominated snow, and if he does not plainly contradict you, he will, at least, give you to understand that he thinks you in an error. You must present water to him in the liquid state, in which alone his limited observation has given him an opportunity of seeing it, before you can enable him to recognize it as such. So firmly are the ideas of water and fluidity associated in his mind, that nothing can induce him to separate them from each other.

Talk to an ignorant Laplander, or Kamschadale savage, of those happier regions, where the sun never disappears but for a few hours at a time, where the rigours of winter are unknown, where an unceasing verdure covers the plains, and a blended luxuriance of fruit and foliage marks the circle of the year, and he will openly charge you with dealing in falsehood, or at least remain incredulous of your tale. In his limited view of things, so necessarily are the ideas of frost, snow, and continued darkness, associated with a certain portion of the year, that he cannot admit, nor even conceive a possibility, of their being separated. As he has never known a year to pass away without finding himself surrounded, during a part of it, with all the horrors of an inclement winter, he believes this to be the established order of nature, in every country, as well as in his own. Such are the unfounded associations, formed by these simple children of nature, in consequence of the limited sphere of their personal observation, and their want of information from other sources. Yet this is a true history of the human mind, in relation to all subjects which it has not sufficiently investigated.

It seems to be owing to wrong associations like these, and, perhaps I may add, to a too limited range of thought, that most persons attach the attribute of life only to organized, and not to fluid, forms of matter. The ideas of *organization* and *vitality*, they consider as inseparable, because they have never seen those actions, which are considered as particularly characteristic of life, performed by any but organized bodies. Life itself, or the vital principle, is too generally confounded with what is nothing more than a peculiar mode, operation, or phænomenon of life. In other words, life, in the general and proper signification of the term, is seldom distinguished from what is nothing more, than a *particular vital action*, rendered evident to the senses by means of motion. Where motion, and that of some *definite kind*, is not manifested, life is not supposed to exist. Hence the idea of definite motion, that is, motion calculated to answer some given end, is, in most minds, associated necessarily with that of life. But it is only by organized bodies that such motion can be manifested; for organization is essential to its performance. It is, therefore, to organized bodies alone, that common observers attribute the principle and properties of life.

Perhaps the following considerations may further illustrate this intricate subject. Fluids cannot perform muscular motion, masticate, swallow, or digest food, nor manifest any emotions or passions, because they have no organic structure calculated for such purposes. But it by no means follows, that they have not life, the common source of all these functions. Organization and vitality are necessarily associated, *only* in imagination. The principle of life, like the matter of heat, seems capable of existing in two states, a *latent* and a *sensible* or *active* one. If it can remain, for years, latent and inactive in the seeds of vegetables, and the ova of insects, why may it not also remain latent in the fibrina of the blood? Let us once admit the position, that life may exist without any perceptible motion, (than which nothing can be more obviously true) and the spell is dissolved, which imposed on us a belief, that there is a necessary connexion between organization and vitality.

It appears, then, that organization is not necessary to the existence, but only to the modification, or definite operations and functions of life. Animal organs are not to be regarded as *sources* of life, but as *instruments* necessary for action and motion. The principle of vitality, for example, cannot operate so as to move a limb, without the organization and structure of muscles; it cannot produce secretion, without the organization of glands; it cannot produce digestion, without the organization of the stomach and its appendages; it cannot produce vision, without the organization of the eye; nor can it produce thought and volition, without the organization of the brain. The several organs of the body, then, are nothing but so many forms of apparatus, destined for the accomplishment of particular purposes. And the wisdom of nature is most strikingly manifested, in the difference of structure and appearance, which obtains among them, according to the different functions they are intended to perform. So forcibly has this circumstance impressed the minds of physiologists, that some of them seem to have considered each organ as possessing a species of life peculiar to itself. This is particularly the case with respect to professor Blumenbach, of Gottingen. Hence, the term "*vita propria*," which occurs so frequently, in the physiological writings of that author. If I have not misapprehended his ideas, he considers the animal body, as a very complicated system, composed of numerous separate pieces of machinery, each one put in motion by a different spring, or principle of action, peculiar to itself.

How foreign is this view of the subject, from the uniform and beautiful simplicity of nature? And how far beneath the elevated and philosophical ideas of the poet, relative to the enlivening principle of the universe? Roused to admiration, and almost to ecstacy, by the grandeur of the prospect around him, where, owing to peculiar arrangements and dispositions of matter, he beheld millions of effects, bursting from the operation of a single cause, he boldly exclaims:

> "All are but parts of one stupendous whole,
> "Whose body nature is, and God the soul;
> "That *chang'd through all*, and yet *in all the same*;
> "Great in the *earth*, as in the *etherial frame*;
> "*Warms* in the *sun*, *refreshes* in the *breeze*,
> "*Glows* in the *stars*, and *blossoms* in the *trees*;
> "Lives through all life, extends through all extent,
> "Spreads undivided, operates unspent;
> "*Breathes* in our *soul*, *informs* our *mortal part*,
> "*As* full, as perfect, in a *hair*, as *heart*;
> "As full, as perfect, in *vile man* that *mourns*,
> "As the rapt *Seraph*, that *adores* and *burns*."

To the eye of an accurate and philosophical observer, the same simplicity manifests itself in the principle which enlivens the animal system. It is the same principle that pervades all the organs of the body, enabling the lungs to perform the office of respiration, the liver to secrete bile, the stomach to digest food, the eye to see, the ear to hear, and the skin to secrete the matter of perspiration. The reason why these several parts or organs assume, when under the influence of suitable stimuli, different modes of action, and are subservient to different purposes is, not because each one possesses a *species of life*, but a *kind of organization and arrange-*

ment, peculiar to itself. It is not, as professor Blumenbach would have it, owing to a "*vita propria*" but to an "*organizatio propria*" that we perceive these different parts subservient to the performance of different functions. It is thus that the stroke of a bow produces a base or a treble sound, according to the thickness or length of the string, which it causes to vibrate.

Having, as I trust, satisfactorily obviated the objections raised against the vitality of the blood, on the score of its being *fluid* and *inorganic*, the way is now open for whatever I may have further to offer, in support of the doctrine. In addition to what I have already advanced on this subject, I believe the blood to be possessed of life, for the following reasons.

1st. Because it is the great distributer of life to, and its supporter in, every member and organ of the body. If the current of blood to any part of the system be entirely obstructed, death and mortification will inevitably ensue. It is to be lamented, that in operations for aneurism, as well as in various accidents, to which we are liable, the truth of this assertion is, too frequently confirmed. We are in the habit of removing warts, polypi, and other preternatural excrescences, on this principle. By obliterating, by means of ligatures, the cavities of the vessels that supply those excrescences with blood, they soon perish, and fall off, in a gangrenous state. By an operation similar in its principle, opaque spots are oftentimes removed from the lucid part of the cornea. If the arteries conveying blood to these spots be divided by a lancet, and prevented from uniting again, the spots will perish for want of their vital fluid, and be removed, most probably, by the action of the absorbents. When the nerves, passing to any part of the body, are compressed, divided, or otherwise destroyed, though sensibility and mobility, are injured by the operation or accident, life itself still remains, provided the circulation of the blood be unobstructed. Hence we see, that *blood* is essential to the existence of life, in every member, organ, and even excrescence, of the body, though the *nervous influence* is not. The latter only contributes to the activity and perfection of life, while the former is necessary to its very existence. But if blood conveys life to, and preserves it in, the several organs and parts of the system, it must unquestionably be itself a living fluid.

I know it may be said, that blood preserves life in parts to which it flows, by acting simply as a stimulus, and not by carrying the principle or matter of vitality along with it. But if this were the case, life might be preserved in these parts, at least for a time, by some other stimulus, besides that of blood. This, however, cannot be done. If the current of blood, to the leg or arm, be *completely* obstructed, warm poultices, fomentations, and other stimulants are applied to no purpose. Death and mortification ensue, as certainly and perhaps as speedily, as if no applications whatever were employed.

2dly. If an extraordinary exertion of life be required to be made by any member or part of the animal body, the economy of nature is such, as to supply that part with a preternatural quantity of blood, to enable it to make and sustain, the exertion required. This is particularly illustrated in the healing of wounds, the erection of the penis, and the phænomena exhibited by the gravid uterus, and the female mammæ during the secretion of milk.

The healing of a wound requires an unusual exertion and expenditure of vital energy. Soon, therefore, after a muscle or other part is cut, lacerated, or punctured, a swelling occurs, which is nothing else than an accumulation of blood, around the injured spot. Vessels which were before invisible, and which contained nothing but a colourless lymph, are now enlarged, and become the receptacles of red blood. Nor does the healing process ever go on favourably without more or less of this accumulation or swelling. If the lips or sides of a large wound continue pale and placid, the symptom is a bad one, and poultices, fomentations, and other means, are employed, to solicit a flow of blood into the part. In such a case, the nerves and absorbents appear to bear no part in producing an augmentation of the vital energy. They preserve very nearly their usual appearance, without sustaining either an increase or diminution of size, throughout the whole process of cure. It is the blood-vessels alone, that are altered very materially both in their state and appearance. Their diameters are expanded, their tone is increased, they become more vigorous in their action, and receive a preternatural influx of blood. There is, then, the most solid ground to believe, that this preternatural accumulation of blood is intended to strengthen, and does strengthen, the vital energy of the part, which it could not do without being itself possessed of life. As no other component part of the body, whether solid or fluid, is present in an unusual quantity, the blood must operate as the sole cause of the increase of vital energy and action. Hence, in pale, leucophlegmatic, and emaciated habits, where this fluid is in small proportion, and of weak qualities, wounds heal with much difficulty. And hence, in the latter stage of malignant fever, when the blood is in a state of exhaustion bordering on death, the smallest scratch proves sometimes incurable. I believe it may be laid down as an axiom in surgery, that without a sufficient accumulation of blood, to augment the vital energy in the wounded part, no curative process can ever go forward.

In the *erectio penis*, and its natural concomitants, there is a vast expenditure of vital energy. To prepare the part for this expenditure, a great accumulation of blood is effected. This accumulation appears to act in a twofold manner, viz. *mechanically* and *vitally*. By its mechanical

operated on the uterus only as a stimulus, increasing its *action*, without at the same time augmenting its *power*, it would produce indirect debility or exhaustion, which is the very reverse of an accumulation of vital strength. For how can indirect debility be more certainly induced, than by the long continued impression of a preternatural stimulus? Instead of blood, suppose the uterus were subject to the constant and long continued action of a preternatural degree of heat, which is nothing but a pure stimulus. Would this tend to strengthen its vital energies? or would it not rather, either rouse it to active *inflammation*, or wear it down by degrees into *indirect debility?* One or the other of these latter results, would unquestionably take place.

The same remarks which have been made relative to the uterine system, in a state of pregnancy, may be applied to the *mammæ*, during the period of lactation. While secreting milk, these glands require a greater accumulation, and are subject to a greater expenditure, of life, than at any other time. Besides the increased action necessary in performing their secretory process, they acquire also a peculiar sensibility, and vital appetency, which they did not possess before. Indeed their whole appearance, and functions are expressive of a preternatural accumulation of life.

To what cause is this accumulation of both irritative, and sensitive life to be attributed? Not to an enlargement in the size, an increase in the number, or any other alteration in the state, of the nerves, lymphatics, or solid fibres. No such changes, with respect to these parts, take place. The only perceptible alterations that occur in the mammæ, are confined exclusively to the sanguiferous system. The blood-vessels are not only expanded in their dimensions, but I believe increased in number, to make room for a preternatural influx of blood, To this influx, then, are we compelled to look, for that accumulation of life, and that increase of action, which prevail in the mammæ during the time of lactation. The objections which were urged against the blood's acting as a *simple stimulus*, on the uterine system, during gestation, are equally applicable in the present instance. Exhaustion, or indirect debility, would be the natural issue in either case.

Various other phænomena might be enumerated, in which, a temporary accumulation of life is produced, by means of a local accumulation of blood. The erection of the papillæ of the tongue, occasioned by the sight, and taste of grateful food, the turgescence of the lips arising from voluptuous kisses, and the protrusion, or erection, as it may be termed, of the nipple of the female breast, resulting from gentle titillation, appear to be all of this description. The turgescence, or swelling, in these cases, bears a strong analogy to the erection of the penis. Its final cause is pleasurable sensation. It is itself produced by an accumulation of blood, and produces, in its turn, an accumulation of both irritative, and sensitive life,

From what has been here advanced, the blood would seem to be nature's flying squadron, which she dispatches at pleasure, from part to part of the system, accordingly as the functions, or exigencies of one organ or another, demand a temporary reinforcement of their vital energy. For these purposes, the blood is peculiarly fitted, by its fluidity, and consequent aptitude for motion. Nor is its vitality less essential to the offices it performs in the economy of the body.

If with Girtanner, Humbold, Davy, and other philosophers, we view life as a mere chemical phænomenon, resulting from the affinities and action of certain kinds of matter, we may thence derive even further evidence, in confirmation of the vitality of the blood. In considering this point, we must take analysis for our guide. As far as that has hitherto thrown light on the subject, the *fibrina* of the blood appears to consist of the same component parts, and those in nearly the same proportion, with the muscular, or irritable fibre. If, therefore, in the *latter* substance, life be the result of the mutual action of these parts on each other, or of the action of some extraneous matter, on the whole of them as a compound, must not, in the former, the same causes produce invariably the same effect? Unquestionably they must, else every principle of reason is fallacious, and all rules of induction are at an end. In examining the doctrine on chemical principles, no shadow of objection against the vitality of the blood, can be drawn from the circumstance of its fluidity. Perhaps even the reverse of this is true. For, other circumstances being alike, chemical agents are most powerful in their operation, when reduced to a fluid, or an aeriform state. If, therefore, life be a phænomenon purely chemical, it is likely to exist in a state of even greater vigour in the blood, than in the solid parts of the body. This consideration not only justifies, but adds weight to, the declaration of Mr. John Hunter, that he was at a loss to determine, whether the body would die sooner without the blood, or the blood without the body. For my own part, I am at present inclined to believe, that, in whatever way the matter is to be explained, the blood, particularly in warm blooded animals, is more tenacious of life than the solids. I think it will appear, that the fibrina continues longer to contract, on the application of stimuli, than a bit of muscle, cut from an animal at the same time the blood was drawn.*

* I would not have it inferred, from any thing here advanced, that I am, as yet, an advocate for the hypothesis of *chemical life*. The doctrine of the vitality of the blood, stands in no need of aid from that speculative source. If it did, I would certainly abandon it. For, notwithstanding the fashionableness of the hypothesis in Europe, and the ascendency it has gained over some minds in this

It might, perhaps, be considered improper in me, to close these observations, without taking some notice of the opinions of certain medical philosophers, who have lately appeared in opposition to the doctrine of the vitality of the blood. The most distinguished of these, whose writings have fallen into my hands, are professor Blumenbach, and Dr. Crumpe of the Royal Irish Academy. As these authors have not attempted to bring their inquiries to the only genuine test of truth, I mean *experiment*, I cannot attach to their opinions, in this instance, the same respect which I am bound to do in many others.

Whoever will read, with attention, the short treatise "*De vi vitali sanguini neganda*" must I think, close it with a sentiment of regret, that it has nothing in it worthy of the professor of Gottingen. Strike from it the name of Blumenbach, and it will dwindle into a very feeble performance. Did it contain a single experiment, I would endeavour to answer it; but, being from beginning to end, a mere tissue of hypothesis, and written in a style of Latin, not very intelligible to common medical readers, and therefore, not likely to mislead public opinion, I do not think it necessary to make it the subject of any further remarks.

As Dr. Crump's Treatise on Opium, has acquired for its author considerable reputation, and is, no doubt, familiarly known to most of you, any errors it may contain on the subject of the vitality of the blood, present a higher claim to our attention. Under these impressions, I cannot suffer the following sentiment to pass unnoticed.

"Separate," says the doctor, "a muscle from an animal, and it will contract on being irritated; *draw a cup of blood, and no irritation can rouse it into action.*" This latter assertion is so directly opposed to the result of experiment, that I know not how to reconcile it with even a very

country, it will require stubborn facts to convince me, that man with all his corporeal and intellectual attributes, is nothing but a *hydro-phosphorated oxyde of azote*. On all subjects, however, I hope and believe, that I am open to the impression of facts and reasonings. But they must be such as I can understand. I am sorry to add, that every thing on the subject of *chemical life*, that has hitherto reached me, either through the press, or otherwise, is, to me, wholly incomprehensible. I can readily assent to the beautiful extravagance of the poet, when he exclaims,

"That very law which moulds a tear,
"And bids it trickle from its source,
"That law preserves the earth a sphere,
"And guides the planets in their course."

But, when the chemist declares, that the same laws which direct the crystallization of spars, nitre, and Glauber's salts, direct also the *crystallization* of man, he must pardon me if I neither understand him, nor believe him.

moderate share of knowledge on the subject. Had Dr. Crumpe ever seen a portion of fresh-drawn blood, exposed to the influence of electricity, to the action of air or water raised fifteen or twenty degrees above its own temperature, or even to the stimulus of simple agitation, he could never have declared that fluid to be unsusceptible of irritation. For a full refutation of the error, into which our author has here fallen, I need only refer you to the results of my first and fourth experiments. I trust it is there satisfactorily proven, that blood is no less susceptible of irritation, than muscular fibre.

Dr. Crumpe again contends, that in cases of incised wounds, the blood which is effused, and undergoes coagulation between the divided parts, does not connect them together, by becoming itself an organic and vascular substance, but by serving as a mere receptacle and mechanical support, for the elongated and inosculating vessels, that shoot out from the fresh surfaces on either side. "It [the blood] appears to me," says he, "to be only the medium through which the divided, inflamed, and irritated vessels extend their extremities." He adds, "*Can an instance be shewn, where effused or coagulated blood, so situated as not to be in reach of these vessels, ever became vascular?*" To this interrogation, I think I can answer decidedly in the affirmative, and hope to convince you, by the following fact, that I do not risk my opinion on hypothetical grounds.

In the summer of the year 1800, I was obliged to submit to the extraction of a tooth from my lower jaw. The hæmorrhage from the lacerated vessels, though not very copious, proved tedious and troublesome. Various expedients were devised to suppress it, without effect. It at length occurred to me, that if the blood were allowed to fill up the alveolus, and retained there by pressure, till coagulation should take place, the thrombus would effectually close the mouths of the divided vessels. The experiment was made, and succeeded to my wishes. The hæmorrhagy, which had continued during nearly three days and nights, was now at an end; and as the coagulated blood proved neither inconvenient nor disagreeable, it was suffered to remain in the socket of the jaw. The weather being warm, I examined it carefully, several times a day, lest it might become putrid and offensive. But instead of this, I observed it on the fourth or fifth day after coagulation, beginning to assume the appearance of *flesh*. Nor did this incarnation commence at the *circumference*, but in the *centre* of the coagulum, at the greatest possible distance from any vessels that might, by elongation, have been protruded from the adjacent gums. From this central point, the process continued to extend, till what had been at first nothing but congealed blood, became a piece of perfect flesh, similar in texture and appearance, to that of the gums. It resembled, for a time, a piece of flesh ingrafted, or introduced, in the form of a plug, into the jaw. In this case, the blood was not exchanged for, but actually converted into, an organic substance. Should this fact ever come to the knowledge of Dr. Crumpe, I hope he will have the candour to admit, that, at least, in one instance, effused and coagulated blood has become vascular, and that in a part to which the surrounding vessels could not at first reach. As the incarnizing process began in the *centre* of the coagulum, the phænomenon can be explained only by admitting the blood to be possessed of a *vital and self-organizing power*. Another fact, analagous to the foregoing one, has taken place in my own person, within the last five months. Blood lodged in the alveolus was certainly converted into flesh. I suspect this to be a very frequent occurrence after the extraction of teeth.

The only further error of Dr. Crumpe, I shall here notice, is, one into which a physician of extensive reading (as I believe him to be) ought not to have fallen. Speaking of the order, in which the different parts of an animal embryo are originally evolved, the doctor asserts, that "the *heart* is generally the *first part* observed, and always in motion." Respecting this assertion, I shall only briefly observe, that abundant evidence of its fallacy, may be collected from the writings of Dr. William Harvey, whose knowledge on the subject of the order or succession of animal evolution, will not be called in question. The following sentence expresses his opinion, on the point under consideration: "*Quantum*," says he, "*ex accurata inspectione discernere licuit, fit sanguis antequam punctum saliens efformatur.*"

If, then, it be true, that the blood is the primogenial or first-born part of the animal embryo, and even, as Harvey alleges, the common parent or source of all the parts subsequently formed, it is not only an error, but a palpable absurdity, to deny it the possession of a principle of life. For how can that, which is in itself destitute of life, give origin to vital organs and membranes? It does not appear to be an abstract principle of life, operating on the blood as a *raw material*, but the blood itself, acting as a living fluid, that gives being and life to the solid and organized parts of the body.

In the great work of generation, or what I have already termed *animal evolution*, the first, and most simple *vital act*, appears to be, the formation of *blood*, out of what may be denominated the rudiments, or raw materials of animal nature. With this fluid, when formed, the vital principle, whatever may be its essence or origin, seems to incorporate itself, confering on it the attributes of vitality, and, by its aid, goes on to produce, in proper order of succession, the various solid parts of the body. To these solids, thus originally formed and fashioned by itself, the blood continues afterwards to be the faithful dispenser of life and sustenance; while they, by their peculiar and appropriate modes of action, preserve it in a renovated and healthful state. Hence, the beauty and perfection of the animal circle! The blood gives original existence and life to the solids: the solids, by their energies and action, preserve the blood in health, and continue it in motion; while it, again, on each return to the several organs, which it visits in its round, comes fraught with what is necessary to replace their expenditures, whether of substance, strength, or vitality.

FINIS.