MEDICINE IN MODERN TIMES.

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OR

DISCOURSES DELIVERED AT

A MEETING OF THE BRITISH MEDICAL ASSOCIATION

AT OXFORD.

BY

DR. STOKES, DR. ACLAND,

PROFESSOR ROLLESTON, REV. PROFESSOR HAUGHTON,

AND DR. GULL,

WITH A REPORT ON MERCURY BY

DR. HUGHES BENNETT.

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PREFACE.

The following Addresses were delivered in Oxford on August 5, 6, and 7 of last year, before the Medical Association of Great Britain and Ireland, several Visitors from foreign Countries, and special Delegates from the United States.

The Association consists of more than 4,000 medical men; of these between 500 and 600 attended the Oxford Meeting.

The Addresses may serve to show some of the aims of the Association as understood by the several speakers. Although such Essays do not commit the profession to any opinions or objects, they nevertheless are of some public interest as illustrating what in the opinion of several independent thinkers would be likely to be cordially received by their professional brethren on the subjects set before them.

A Report, which was not an Address in the ordinary sense, is added, because it was presented to a Special General Meeting, and because it is an example of one method of medical enquiry. The exact value of this method is as little understood by many in the present day, as it may perhaps be overrated by a few. In a sketch of Medicine this kind of investigation could not but be recognized as an important form of modern research.

H. W. A.

OXFORD, February, 1869.

CONTENTS.

VALEDICTORY ADDRESS,

DELIVERED IN THE HALL OF CHRIST CHURCH,

BY

DR. STOKES,

ON LEAVING THE PRESIDENT'S CHAIR,

AUGUST 5, 1868.

B

I.

VALEDICTORY ADDRESS.

My term of office, as Annual President of the British Medical Association, having expired, I have to resign the chair to one whose life and labours are a guarantee that he will do it honour. I need not ask you to believe with what sincerity I have appreciated the importance of the position, which, owing to your too flattering estimation of me, I have held since the occasion of the meeting in Dublin, where, as here, you were received and honoured in the halls of a great University.

To be chosen to preside over a society of more than four thousand educated gentlemen, all zealous for the advancement of human knowledge,—many of them distinguished in the paths of literature and science, and not a few the practical doers of Christ's work upon earth, is a distinction of which any man might be proud, for which any man should be thankful. For, though the place of meeting influences the selection of President, the character of him who is to preside influences, at least among other things, the choice of the place of meeting. Here, and for the third time, you meet in

Oxford, the heart of England, whose history is that of the country, the free, the enlightened, the religious—the conqueror in the arts of peace and of war.

Let me, before bidding you farewell, say a few words as to the future of this great Society—now the most numerous body working for the benefit of science in the world, and which will doubtless attain to larger dimensions. So far, we have been an united body, which is to be attributed to our federal constitution, with an independent local action, and a representative and imperial executive. How long this strength-giving union may last no man can predict; nor, on the other hand, can any man say to what an amount of influence for good this Association may attain. But it is plain, that its durability and usefulness will depend on its being made the instrument for the public good, rather than the machinery to advance the immediate worldly interests of the profession. And every one of us must lay it to heart that a great issue rests within his hands. The man among us who by his unselfish labour adds one useful fact to the storehouse of medical knowledge, does more to advance its material interests than if he had spent a life in the pursuit of medical politics. Far be it from me to say that there are not great wrongs to be redressed. It is impossible, in any country, that evils of custom and of administration, private wrong, corporate shortcomings, hard dealings, unfair competition, and scanty remuneration for public and private services, should not occur. But these evils being admitted, how are they to be lessened, if not removed? Is it by public agitation and remonstrance, addressed to deafened or unwilling ears? is it by the demand for class-legislation? or is it, by the efforts of one and all, to place medicine in the hierarchy of the sciences —in the vanguard of human progress; eliminating every influence that can lower it, every day more and more developing the professional principle, while we foster all things that relate to its moral, literary, and scientific character? When this becomes our rule of action, then begins the real reform of all those things at which we fret and chafe. Then will medicine have its due weight in the councils of the country. There is no royal road to this consummation. On the one hand, the liberal education of the public must advance, and the introduction of the physical sciences into the Arts courses of the Universities has given the death-blow to empiricism; and, on the other, that of ourselves must extend its foundations, and we must trust far less to the special than to the general training of the mind. When medicine is in a position to command respect, be sure that its rewards will be proportionally increased, and its status elevated. In the history of the human race, three objects of man's solicitude may be indicated: first, his future state; next, his worldly interests; and lastly, his health. And so the professions which deal with these considerations have been relatively placed: first, that of divinity; next, that of law or government; and, as man often seems to love gold more than life, the last is medicine. But, with the progress of society, a juster balance will obtain, conditionally that we work in the right direction, and make ourselves worthy to take a share in its government, not by coercive curricula of special education, not by overloaded examinations in special knowledge, which are, in comparison to a large mental training, almost valueless; but by seeing to the moral and religious cultivation, and the general intellectual advancement of the student. Doubtless, such a revolution, which, could men only read the signs of the times, is slowly, though surely, coming, will lessen the number of a certain order of candidates for licence to practise. Doubtless, also, while the funds of special corporations will be diminished, University education will be extended; and the whole character of medicine will be changed, greatly to the advantage of its social position in the country, and the interests of science and the public at large.

These principles have, from the first, influenced the Medical Council, whose efforts have been so much directed to the promotion of general education, and who, while administering an imperfect law to the best of their ability, and persevering in what they believe to be the right course, have been exposed to depreciating observations. As every one knows, the Council has no direct coercive powers in the matter of education, and I believe that, at least as yet, it is better that it should not have such powers; but I know that I speak the sentiments of the existing members of that body, that whether its constitution be or be not changed, they look to the profession at large for moral support and for counsel.

Our invitation to this metropolis of ancient and modern British thought, which, with its sister Universities of Cambridge and of Dublin, has so effectually subserved the real interests of medicine, is a graceful compliment to the Association, and an evidence that this great University will still more foster the cause of medical science.

Putting aside the success of your labours at Dublin, in a scientific point of view, your meeting of last year deserves a long remembrance. It was the first occasion on which the members of all ranks of British and Irish professional men met to know one another, to unite in the common cause of the advancement of knowledge, and to learn, on a great scale, how the mutual cultivation of science will efface national prejudices; for it is only in this way that those national dislikes and distrustings which become hereditary feelings, transmitted from one generation to another, which separate peoples and delay the peaceful federation of the world, can ever be removed. The star of knowledge, while it illuminates the path to wider and still wider discovery, yet is like unto that which guided the sages of the East to the cradle at Bethlehem; for its benignant light is the herald of peace and good-will among men.

I now respectfully and gratefully bid you farewell, and may all good things be yours.

GENERAL RELATIONS OF

MEDICINE IN MODERN TIMES,

ADDRESS

DELIVERED IN THE HALL OF CHRIST CHURCH,

BY

DR. ACLAND,

ON TAKING THE PRESIDENT'S CHAIR,

AUGUST 5, 1868.

II.

THE GENERAL RELATIONS OF MEDICINE IN MODERN TIMES.

THE meeting which was held last year by your Association in Dublin was attended by circumstances never to be forgotten. The great interest of that delightful metropolis, the fervent hospitality of our hosts, the pre-eminence of our President, bequeathed to his successor a task from which any man, however fortunately placed, might reasonably shrink.

Two motives only have induced me to accept the honour most graciously offered, of following him as your President: the one, the strong desire to help in carrying on his and your work of last year; the other, the conviction (more than justified by the great kindness already shown to me) that you will only require such hearty service as it is in my power to render. To all, whether to your distinguished Officers, to my valued co-adjutors in this place, to those present from this or other countries, I look for a continuance of that kindness and support. Confident of this, I anticipate a useful and happy meeting, though we are shorn of some of the splendours of Dublin, and remote from the exhilarating buoyancy of Irish life.

It has been said that one chief art in a President's Address is to advance no serious opinions, and to provoke no criticism. A speaker cannot, however, speak with advantage unless he express thoughts which awaken a response in the head and the heart of his hearers. The test of the success or failure of his attempt must be their approval or condemnation.

Pleasant and bright as I hope our meeting will be, full as it should be of the double objects of such a gathering,—the advance of our common pursuits and the calling forth the kindly feelings of a common profession,—I much mistake your wishes if you do not desire that something more than an agreeable retrospect, more also than words of hearty welcome, should occupy us for a brief space to-night.

We are living in a critical period of our country's history; in a new era in the history of Man. Every part of our social fabric is undergoing scrutiny, revision, and reform. Government, trade, institutions, laws, the artificial usages of society, the character to be given to our children by the method of their early training, are not only being criticised, but are most of them being changed—changed with unexampled rapidity; and the change is, some think, a tendency to absolute perfection, or, according to one philosopher, a last plunge down the falls of Niagara. The facility with which ideas are communicated through the whole human family, distinguishes our age from all that precede it. Our own profession is not exempt from these influences: even if it were, we are part of the body politic, and as wise men we might do well to look forth from the fretted shelter of this ancient Hall, itself a memorial of the ferment of the Reformation, and, scanning the clouds as they drift along, take the bearing of our own course in the stream of time.

The accomplished authors of the special addresses which you will hear at your general meetings, Professor Rolleston, Professor Haughton, and Dr. Gull, will bring before you abundant illustrations of the present state of three great departments of the science and the art we profess. While waiting for these addresses and for the other more technical communications which you will hear, I propose to take a general survey of the position occupied by our professional knowledge in relation to other branches of knowledge, and to consider the objects which ought to be held in view when we discuss the temper required of us by our times, and the training proper for the formation of that character and temper.

No better illustration of what is meant by Medical character and temper can perhaps be found than in the words deep graven in the hearts of true Physicians for a hundred generations:—

ὁ καιρὸς ὀξύς· ἡ δὲ πεῖρα σφαλερή· ἡ δὲ κρίσις χαλεπή.

How may every age—some more than others, but yet every age—for itself exclaim, 'Yes! opportunity fleets by and is lost. Old experience is a quicksand. Sound judgment is hard, above all things.'

The traditions of our present resting-place may well induce us to ask, in the language of this grey old aphorism:—

Are we losing opportunities now?

Is our experience fallacious?

Is not judgment in science and in art still hard?

Are we attempting what cannot be done? or spurning what can even now be accomplished?

What will our children's children say of us and of our day?

Will they bless us for the training we gave them, and the example we set? Or will they say that our conclusions were baseless or rash, the tasks we bequeathed to them unnecessarily difficult?

Now, in judging of the Medical character, we set aside of course all reference to individuals. We form an ideal character. And yet the ideal cannot be considered wholly in an abstract way. We have to judge of it in its relations,—first, to the condition of SCIENCE, and secondly, to the constant properties and the variable accidents of HUMANITY.

In the present age SCIENCE is advancing, and the means of its progress are increasing with altogether unexampled rapidity. No bounds can be set to its possible conquests. A profession dependent on Science must vary with that on which it depends; and if it does not advance with the advance of Science, that fact proves it to be in error.

HUMANITY has its constant properties and its variable accidents: its constants of need of food, of warmth, and of clothing; its constants of sickness and disease;

its constants of relative poverty and relative wealth; its constants of yearning after good, and exposure to misery and guilt. But Humanity has also its variable accidents of climate, of fashion, of ease, of luxury, of degradation; not like death unavoidable and irremediable, but accidents, terrible accidents—such as, however formidable and perilous, to a certain extent may be avoided or can be remedied.

Reflection will show to what a vast range of subjects Medicine is linked, by these two circumstances,—the progress of Science and the fluctuations of Humanity. How numerous are the points of Science which touch us more or less closely! How intimately are we bound up with the sufferings and the sorrows, physical and mental, of mankind!

It is this necessary law of connection with Science as a whole, and with Man as a whole, which makes both the difficulty and the glory of our work in the body politic; and it is this connection which pre-eminently now, at this period of the history of our country and of human knowledge, makes a revision of our present state desirable and indeed unavoidable.

Let us then consider, first, the relation of Modern Medicine to Modern Science.

It was just now said that Medicine is necessarily linked to the existing condition of Science. This statement must be examined more closely. Medicine is at once in advance of the exact Sciences so called, and behind them. It is in advance of them, inasmuch as it clinically observes as facts some things which

Science has not yet fully explained; and it believes with strong conviction what can at present be neither demonstrated nor ignored. It is behind them, inasmuch as they claim for every fact of Science an exactness to which Medicine cannot always pretend.

But Medicine too long consented to regard itself as an Art as distinguished from Science. It cannot wholly fulfil its function till it asserts, and has entirely substantiated, its claim as one of the band of Sciences that precisely interpret the phænomena of Nature.

Now the existing relations of pure Science and Medicine are both direct and indirect. In the *direct* relations it follows the lead of Science without hesitation; in the *indirect* relations it cannot afford always to wait for positive instructions. Let us look for a moment at each of these relations.

The relations are *direct* in the case of all those means of modern invention and research which are applied daily, in ways of which half a century ago there was no conception, to the discovery and the explanation of physiological and pathological phænomena. To the interpretation of sounds heard within the body, Laennec and a host of subsequent observers brought precise acoustical observation and experiment, and showed us how to map out the condition of internal parts, the action of which we hear but cannot see: so that though we are not always able to say absolutely what is the nature of an abnormity, producing an abnormal phænomenon, we can at least say what it is not. By applications of optical instruments Czermak, Desormeaux, and Cruise have laid open to us many organs of the body before inscrutable—the pharynx, the vocal chords, the trachêa, the vagina, the uterus, the bladder; so that the actual but hidden causes of many phænomena are no longer matter of argument, but of sight and demonstration. The secrets of the eye, I need not say, are now disclosed by means of the physical contrivances of Helmholtz and others, assisted by the many elucidations contributed by our own countrymen. Ogle and Allbutt in this country, and others on the Continent, are working out the further proposition that some states of the eye are not only important in themselves as local abnormities, but as being pathognomic of other suspected conditions in other and distant organs. The sense of touch is so supplemented by the skilful apparatus of Marey that the wave phænomena of the pulse and heart are registered; and thereby, through indirect but clear induction, we can fathom the secrets not only of the circulatory apparatus, but of nerve action and nerve lesion behind and beyond. And am I to add in this company that the very romance of zoölogical evolution is brought to enlarge our knowledge of the parasites that infest our bodies, modifying sanitary regulations in a remarkable manner, or that in every practitioner's hand the microscope and the test tube answer in a moment questions once unanswerable, though on them hang issues of life and of death?

It were tedious to you, and unbecoming in me, to tell of these things in detail—for they are of the alphabet of Modern Medicine in its every-day work. They are named only to recall in the argument a few out of many instances of the *direct* application of scientific appliance and method to the progress of medical knowledge by means of light, sound, and direct touch. The exploration of the nervous system by electrical agencies, by manometers and the like, through the recent labours of many, but of late especially of M. Duchenne de Boulogne, and the registration of changes of temperature in evidence of chemical alterations and in proof of corresponding alterations in the organism, should be cited as among the less developed but equally certain advances of physical enquiry as applied to disease.

The indirect relations of Medicine to the other branches of Physical Science are more remarkable still, but being also more remote are less familiar.

Foremost it should be remembered what is the effect of the *temper* of Modern Science on Modern Medicine. Now it must at once be granted that opinion and authority in Medicine have ceased to have any value as such, except where the authority is derived from high moral and intellectual qualities combined with large experience. But this is a personal question, like the personal equation of astronomical observers, and does not affect the *framework* of the Art. The framework of Medicine simply considered as an Art now depends on accurate data, on experiment, on observation, and direct induction therefrom. But though this be true, yet Medicine as an Art cannot always go hand in hand with Medicine as a Science. The physician, with emergencies constantly before him, cannot wait to act till Science has established her conclusions with absolute certainty. In so far as Medicine is purely scientific it is not in harmony with the average mind of mankind; in so far as it is empirical it is at variance with pure Science. Science being organised knowledge, and admitting no uncertain element, objects to the probabilities which guide the master of the Art to his conclusions, and lead him to act with a promptitude incomprehensible and appalling to the uninitiated. Just as the scientific navigator, who is furnished (as every navigator ought to be) with the instincts of the empirical seaman, trusts those instincts in a gale as readily as his accurate observations in clear weather; so the true clinical physician decides first, and afterwards puts together in logical arrangement the reasons for his decision. In so far as he does this he abandons the order observed in pure Science. Still he must do so in many cases if he is to act at all. If he cannot do so, he is apt to become first sceptical, then indifferent.

That such scepticism and such indifference may be a real danger in the practice of Medicine, and is some counterpoise to the many advantages which Modern Medicine may derive from her scientific character, is hardly to be doubted.

This subject of Medical Scepticism is too grave to be here passed over without some consideration. Healthy criticism of existing belief is one thing. Mere destructive criticism with no honest purpose of getting at the truth is another. The former is a necessary quality in a man of full power. The latter is the frequent sign of idleness in youth, and of carelessness in advanced years. What is certain in respect of Medicine critically considered as a Science and as an Art may be thus stated. There is a true Medicine and a false Medicine. Like the wheat and the tares, they now stand together. The true is that which is based on unalterable laws of Nature; the false that which is the result of ignorance, unconscious misinterpretation, or wilful error,—ignorance of Nature, unintentional misunderstanding of her laws, wilful falsification of facts to subserve some temporary purpose. From these two, the true and the false, come all the traditions of our Art. To winnow the one from the other, to extrude the uncertain from the proved, to add to what is known, regardless of the effect on previous beliefs, is the special duty of the time in which we are now placed. If this duty were completely done we should possess the real history of an Art three thousand years old. It is a history not without parallel in other departments of human thought. There was a time when the priest and the physician were one, and when the art of healing was looked on as a supernatural gift. It is so esteemed even now among savages. Cures wrought by a higher intelligence, being above the conception of the 'rude untutored mind,' seem emanations from the attributes 'of the Unknown God.' The impostor priest could be also impostor physician. Trading on the weaknesses of his fellow-men, he would dogmatise on the ailments of the body and their cure, as well as on the diseases of the mind and their remedies.

The destruction of such dogmas, groundless though they may be, is a slow and dangerous process, as all students of history can tell. But the time is come when every opinion and conclusion has to be sifted;

and another danger has come rapidly upon us—that of reckless negation of the accumulated experience of our race. What are our fathers to us? Are we not better than they? This is with some the modern version of the well-known lamentation:—

'Ætas parentum pejor avis tulit
Nos nequiores, mox daturos
Progeniem vitiosiorem.'

But still in the traditions of the past there is a mass of practical wisdom. Nothing is more admirable than the caution and care—the generally scientific spirit, and often the truly scientific method, with which the best men, such as Morgagni, Sydenham, and Hunter, observed and reasoned. It is conceivable that this caution was due in great measure to the uncertainty of the ground they trod, and the want of precision in the means they possessed. We are superior to our fathers in the means at our disposal, and in the positiveness with which we can up to certain points enunciate our results. But we ought not to overlook the fact, that with these positive gains we are subject to contingent losses, and that in an epoch of details and comparatively facile methods of enquiry the great qualities of patience and reserve may be lost to those who are not themselves original investigators.

With the exception implied by the above remarks, the temper of Modern Science indirectly rules the progress of the Healing Art. It is of consequence to appreciate what that temper is.

It would be difficult more aptly to describe it than by the words of Newton:—'The main business of natural philosophy is to argue from phænomena without feigning hypotheses, and to deduce causes from effects, till we come to the very First Cause, which certainly is not mechanical.' To discuss this simple phrase, and to expand it into its full significance, would be to recapitulate the history of a great portion of Modern Science. There probably is no part of it to which some modern thinkers would not take exception. But it cannot fail to raise in every mind a splendid and affecting image of the boundless field of Physical Philosophy. It will suggest to one a countless host of loving worshippers; to another it reveals a crowd of stern enquirers ardently groping in dim cold twilight. Each in his own sphere, each tinged with the special hue of his own nature, in Physics and Biology, all alike are searching for a True Cause. From the causes of twining in the delicate tendril to the causes of variation in the human species, from the causes and local conditions of atmospheric changes to the causes and physical consequences of the combustion of a fixed star, the biologists and physicists of the day are seeking a True Cause: and, each in his way appreciated by hundreds of fellow-workers and ten thousands of more or less intelligent followers, is making a step towards the First Cause which, Newton says, 'is certainly not mechanical.' And what have they reached? First, the conviction, clearly expressed many years ago, of the exquisite interdependence between our entire Fauna and Flora in the chemical circulation of matter on our globe; and next, the generalisation at once so simple, so overwhelming, that all action of which we are immediately cognisant is but the result of the operation of solar heat upon and through interdependent and correlative existences; that all things in this system are capable only of interchange; that there is no destruction of what exists; no creation of new energy.

The theorem of the Conservation of Energy has not, as may be supposed, brought direct fruit to Medicine, but indirectly it has already told in more ways than one.

It makes more and more doubtful the existence of a 'Vital Principle,' controlling the ordinary laws and affinities of matter.

It tends to bring the phænomena of living bodies more and more within the domain of pure physical necessity.

It helps to lessen the improbability of the hypothesis of Darwin, by showing how deeply mutual correlations run into the very structure of the universe; and it increases the probability that living beings placed in similar conditions will move in similar lines, and conversely.

But it explains nothing whatever of the origin of things: nothing of the nature of Will. However true it may be that the solar energy was stored up myriads of ages ago in the coal-fields, however true it may be that, in the processes of vegetable life and decay, the sun's energy is constantly being first captured and then liberated for further use in other organisms, yet nothing of this emits the smallest spark of information on the *true cause* of organisation or of the working of a single organic cell.

It is clear (however careful we may be not to idolise new words) that the idea of Conservation of Energy must now find an entrance into every conception of organic change. Even in Medicine we are thus more and more drawn to the conviction that the same result follows the same cause in similar organic conditions; and that while health consists in the regular performance of an elaborate series of physical changes, diseases properly called chemical (as opposed to mechanical or surgical diseases) follow a definite course, which we should be able to estimate if we could know all the conditions; a supposition (it may be added) which is impossible because of the factor of Will which has to be taken into account.

These fundamental ideas seem at first sight to belong so little to the work of every-day life or to the practice of an Art, as hardly to have any relation to them. Yet very little reflection shows how the profession which has always assiduously pursued, indeed has been the chief promoter of, natural knowledge, cannot separate itself from the indirect, any more than from the direct influence of Science; and so has to follow these apparently remote speculations. Who would suppose that the question of spontaneous generation so keenly debated from a very early period to our own year and day, need have any immediate bearing on practice? Yet see how the observations of Pasteur are connected with the questions of infection, nay more, of suppuration, and (as shown to be probable by Professor Lister) of surgical treatment. It would indeed be a great point if we could prove that no germs, carriers of disease, spontaneously originate, but must always immediately come from a progenitor cell. If so, there would be just a hope that some diseases might be effectually and finally stamped out. What we do know of zymotic diseases does not favour this expectation. At the same time it has to be borne in mind that both the success and the failure of vaccination disclose the existence of properties which it would have been fantastic to expect, but which experiments proved to exist.

What the powers of 'Nature' are in producing and regulating morbid products, and what the powers of man may be, is becoming every day more apparent in such enquiries as those on the relations of vaccination to syphilis, which are now being discussed by Ballard and Seaton. If we take them into consideration together with the researches into the origin of tubercle by Villemin, Sanderson, and Wilson Fox, the enquiries into the origin of the Cattle Plague and the whole state of our knowledge of the nature of what is called infection, we have presented to us certainly one of the most remarkable series of biological and pathological investigations that has been ever recorded, and to an intelligent and cultivated person unacquainted with the state of human knowledge in respect of biology, one which must, at first, seem almost incredible.

Yet any one, however little informed upon such subjects, reflecting on these few general illustrations, cannot fail to see the vastness of the subjects now comprised under the head of Modern Medicine; the firmness of the Scientific basis on which it is placed; and the peculiar difficulty which it encounters as being a Science complicated by an Art, and that Art one which is not only entangled with all the disturbing elements of progressive human society, but also an Art operating on the most complicated of chemical processes, namely, the so-called vital actions.

Further, he would probably say, 'There are here intricate processes affecting human health, depending on actions partly physical and involuntary, partly mental and voluntary—what really are the powers by which Medicine can influence them?' If such a question were proposed, the answer would be, 'There are two methods by which the art must work; first, the method of Pure Science, with no other object but the attainment of knowledge and truth, the method which works by observation and experiment, in physics, chemistry, anatomy, and physiology, by the study of agents and, where necessary, by vivisections; and secondly, the Empirical method, or the method which attempts to cure by rules derived from tradition, probability, and tentative experiment.'

Now, by the first of these methods we are able to ascertain the law or course of action of the most complicated vital processes, as appears from the researches into tubercle and syphilis to which I but now alluded. But this method is unable to explain all the relations of phænomena in any, even the simplest organism, for it cannot be said that we at present understand the simplest vital process. This is put with much force by a great French chemist: 'Car nous n'en connaissons aucun d'une manière complète, puisque la connaissance parfaite de chacun d'eux exigerait celle de toutes les lois, de toutes les forces qui concourent à le produire, c'est-à-dire la connaissance parfaite de l'univers.'—Berthelot, Chimie Organique, ii. 810.

Still it is a great thing to see the laws or course of action of living bodies being gradually developed and laid down. We know, for instance, that certain diseases will run a special course in a certain family. What is this but the law that living matter acts in a definite manner under definite conditions, and that when we can predicate the conditions we can predict some at least of the results? This law is the scientific basis of all curative medicine in individual instances, and of all preventive or state medicine in communities.

The application of this law to vital as well as to inorganic phænomena strikes a blow at many ancient prejudices which assumed and sometimes fostered the notion of exceptional and erratic procedures, that is to say, of procedures for which no reason could be given. Viewed calmly, it is the ground for all hope of future progress in Therapeutics; and for the following reason among others. In the present state of knowledge we are always on the verge of the most amazing results, and we do not know when or where the outcome will be. As in a siege, we advance by a series of zigzags and parallels; and these must be begun at a great distance from the fortress. While we are ignorant of the nature of some of the commonest chemical changes that we know to be going on every second of our lives in our own bodies, some enquirers are quietly but minutely discussing the chemical actions by which acid and alkaline magmas modified the constitution of the earth's crust, and gradually produced the chemical conditions which made the evolution of organisms, as at present constituted, possible in our planet. And slowly but surely the siege of the fortress of knowledge advances. Latterly it has shown sign of progress in a new and unexpected direction. Chemistry which used to be chiefly analytical has now become enthusiastically synthetical. There are virtually no limits to the substances which can be made. Berthelot makes a calculation of the number of combinations with acids of certain alcohols. He says if you gave each a name, allowing a line for the name, then printed 100 lines in a page, and made volumes of 1,000 pages, and placed a million volumes in a library, you would need 14,000 libraries for your catalogue. He therefore properly calls such bodies infinite, instancing the synthetical construction of the alcohol and aldehyde series, of the organic acids, of the amides, of urea, and the millions of possible bodies which loom in the future,—certain to be made, waiting to be made, the possessors of qualities suspected but unknown.

I almost hesitate to observe that bodies of this kind have important relations to the properties of the nervous system in man. Chloroform and the various amides employed by Richardson have made this familiar to all. The beautiful experiments by Bernard upon amygdalin show the question to be still more intricate and vast than Berthelot puts it in the passage already cited.

It is manifest, therefore, that the possible agents for affecting the human body are infinite, and the instances which I have partially touched on of the mutual relations of glanders, tubercle, and irritation, of syphilis and vaccination, show what might antecedently have been expected—the equally infinite problems which may be experimentally discussed and solved in the higher animal organism, problems equally affecting the classifications of Pathology and Therapeutics. Science is indebted to this Association for the example it sets in the appreciation of this vast question; and especially to Professor Bennett for the able and patient manner in which he is now conducting for it difficult researches on the action of an important remedy, the first, I hope, of a long series of such detailed and rigorous enquiries. A more useful expenditure of money can hardly be conceived.

Of the second, or the Empirical method, to which I but now alluded, it is not to the present purpose to speak.

Having said thus much on the relations of Modern Medicine and Modern Science, in the hope of vindicating our profession from one-sided attacks founded on the notion that it is wanting in scientific precision, I leave this slight sketch of a vast subject, in order to consider the present relations of Modern Medicine to Humanity; in other words, its relations to the wants of Man in the complex state of modern society.

If the philosophic basis of Medicine has been changed during this century by its relations to Science, its object or aim has been as much modified by its relations to Man as he is, and as he is becoming, under the exigencies of fast-growing population and the agency of a fast-improving social organisation.

It would not be true to say that till modern times no great crowds of men have been densely congregated into unhealthy masses, nor to say that attention had never been given to their sanitary state. There were great armies before the Christian era; there must have been great crowding in ancient Rome; a careful and detailed sanitary code was imposed on the Jewish people at the time of the Exodus; Rome paid no small attention to sanitary works. But neither permanent populous cities nor sanitary codes were the rule. In our times, on the contrary, one of the peculiarities of modern life is shown from statistics to be the tendency to increase of population in great towns: so that in England between 1841 and 1851 * there was an increase in the population of towns of over 100,000 inhabitants, of 23 per cent.; and in the following decennial period, 1851 to 1861, there was, in France, in towns of similar magnitude, taken collectively, an increase of 50 per cent.

In our day Preventive and Public Medicine has become a great branch of Medical Science. Imperfectly as yet carried out in this country, it is more fully developed in several Continental countries, and, of late, in a noble manner in the United States. It is here and there carried to great perfection, as in various departments of armies; it has made great progress also in navies, and in almost every part of civil life. Still we

* Quetelet, Bulletin de Comm. centrale de Statistique, tom. x. 1866.

cannot say that the evils incident to modern civilisation have been as yet met by that clear-sighted and systematic superintendence by which alone they can be subdued.

It would far exceed the limits which are necessarily imposed upon me to attempt even the slightest sketch of the exact position of Sanitary Science in this kingdom. Its literature has become voluminous. Its general principles are recognised. Its need is felt. Nay, its value is by some injuriously exaggerated. What it requires now is proper administrative organisation. The admirable paper of Dr. Rumsey read last year in Dublin has been followed by not unimportant results, which will be the subject of a special Report by Dr. Stewart, and therefore need not be here discussed. It will suffice to say that we have reason to hope that we may shortly see the relations which ought to subsist between this department of the Science of Medicine and the community at large investigated systematically by a Royal Commission.

To anticipate the conclusions of such a Commission would be no becoming occupation. But this may be confidently expected, that one result would be the elevation of the duties of an Officer in State Medicine to that of a recognised profession, as in several special instances it has already become. At present it is not uncommon for a young man to be charged with wasting his powers if he devotes himself to improve the public health. Hereafter, charge of the public health must be made as much a matter of honourable ambition in the body politic, and must become as much an object of special education and training, as the business of any other recognised branch of the civil service. The Government will have to define the duties to be discharged by Public Health Officers or other Officers of Public Medicine, and the General Council of Medical Education will be able to direct the education of those who aspire to the performance of the duties so defined.

What those duties and what that education shall be, it is not our province to-day to consider. It is enough to say, that without a sound general education and without intelligent interest in the great problems that wait on dense population, viz. a fluctuating labour-market, the rights of capital and of labour, the duties of property, the principles of morality, and the nature and aims of physical, moral and intellectual education, in other words, for what purposes the bodies of the people are to be trained, no man can discuss with safety the large questions which must be answered by State or Preventive Medicine. If he be not so prepared, he will be liable to take part unwittingly with dogmas dictated by ignorant selfishness, or unintentionally to support oppressive enactments suggested by imaginative philanthropy, in ignorance of the amount of the burdens which can in the long run be borne by the toil of a people, for the purpose of civil administration.

I have not here touched on the problems arising from the condition of great towns. They are become part of the literature—I had almost said the sensational literature—of the day. The admirable Reports of Mr. Simon, under the sanction of the Privy Council, the graphic sketches by Dr. Farr in the Registrar-General's

Returns, the papers by the Metropolitan Officers of Health, the Returns from Ireland by Dr. Burke, from Scotland by Dr. Stark, Professor Gairdner, and others, and the paper by Dr. Morgan, have filled the public mind first with amazement, then with alarm. Long familiar with poverty, and the sorrows and penalties and crimes which hang about it, as well as the brightness and patience which called forth the words 'Blessed be ye poor,' I had not learnt the intricacy of these problems till in the work of the Cubic Space Committee (wherein I joined the able President of your Council) I found myself set face to face with them, and had to consider what was the significance to the State of a child born in a workhouse of a prostitute, brought up during childhood in the workhouse, cast forth into the purlieus of the city, becoming pregnant and returning poisoned with syphilitic sores, at an immature age to bear a syphilitic infant: nor did I see the magnitude of the problems till I found that, not in one instance but in thousands, not in one district but in many, is this process being carried on. Where and how these frightful evils can be stopped is known only to Him who can tell the causes which laid desolate whole kingdoms of Asia, and left us to wonder at the ruins of cities whose very names are unknown. But it rests upon us, more perhaps than on any other class in the community, to see to it that no remedy which can be applied, however partially, is neglected, and that no means by which the comfortable and indifferent public can be roused to appreciate the task before them shall be left unused.

Still less do I presume to handle now the relation of Physiological and Medical knowledge to the habits of some among the higher classes—to the conditions of modern society which over-stimulate nervous action, the late hours, the exhausting effort, the wholly unhealthy existence. This is a subject which needs much tenderness and skill, and longer time than you can spare. But it is a subject on which the advancing knowledge and culture of Modern Medicine will have not a little to say; and which, it may be hoped, will be so said as to be heard.

There is one other relation of Modern Medicine which it would be improper to pass by, although it is one which an over-prudent man would instinctively avoid—its relation to spiritual beliefs.

The reason why an over-prudent man would avoid all allusion to such beliefs, is that he dreads to entangle himself in the maze of angry controversy which not only surrounds but almost fills the ecclesiastical world; controversy, not between creeds permanently opposed, as the creeds of Buddhism, of Islam, and of Christendom, but feuds in the bosom of each separate religious system.

The reason why we cannot, if we would, avoid considering our own relation to spiritual beliefs, lies in the two fundamental facts, that we are ourselves men like other men, and that we stand in a closer and more real relation to man, as man, than does any other class of the commonwealth.

It has indeed been said, 'Ubi tres medici, ibi duo

Athei.' The recent attacks by the Cardinals in the French Senate on the Faculty of Medicine show that the charge conveyed in this aphorism is not forgotten in France. Signs of the same notion are not wanting in this country. What is the fact? The fact seems to be that the members of the Medical Profession are in their lives not less religious than the average of the society in which they live. As a body they are calm, earnest men, who mingle little, perhaps too little, in the questions of the day, and seldom with violence. Religious enthusiasm is rare with them; fanaticism is generally absent; and on the whole it may be said that as a Profession they stand aloof from religious discussion. Self-interest operates in some degree; usage operates to some extent; but there is a deeper reason for their standing aloof, which religious teachers would do well to lay to heart. There are none who know so much of the reality of man's nature, its phænomena, its conditions, its pains, its privileges. To the Physician the bodily nature is bared in its beauty and in its hideousness, in its formation and growth, and in its decay and dissolution. Man's relation to other living forms, his likeness or his unlikeness to irresponsible, unreasoning, or half-reasoning brutes, are vital questions to those whose minds are filled with ideas of anatomical homologies, of the relations of functions to organs, of the laws of hereditary transmission, and of the evolution of mental attributes as well as of corporeal organisation in the animal series. The Physician sees in the body of man the material structure by which alone the known operations of the mind of man are possible in this world, the organs by which alone he can work his earthly work, whether it be the work which he shares in common with the beasts of the field, or the work through which he can enter into conscious relation to his unapproachable Creator: the frame by which, while bound down in an earthly charnel-house, he lifts his eyes and strains his heart with yearning ineffable towards a higher nature, and obeys the upward-tending impulses of affections strong unto death, affections so pure and so divine, as to lose in the love of others even the consciousness of self.

All this, and much more, our Profession sees as phæ-nomena. It knows that 'the child is the father of the man;' that 'the sins of the fathers are visited upon the children to the third and fourth generation;' that man, though in one sense 'lower than the beasts which perish,' is yet 'the paragon of animals, in apprehension how like a God, in action how like an angel.' These, and all the contrasts which poets and preachers paint, are present to us under all phases, in every circumstance of race and creed, of temptation caused either by want or by luxury and power, or temperament engendered by any of these conditions, modifying, as you all know, both disease and the remedies it requires.

I forbear from enlarging on this difficult and perilous topic here. But I shall have to recur to it briefly under the last head of my address. It need only now be said that the connexion which Medical Men must have with the future culture of the country is becoming more and more intimate. The general public, heretofore indifferent to Physical Science, are becoming daily more alive to its importance, inasmuch as they are in many instances themselves scientific, and judges of those who are so.

If these then be the relations of Medicine to Science, and to the bodily wants and the mental condition of man, and such the character of the Physician, what is to be the preparation for his duties? There is no better answer than this of Strabo: Ἡ δὲ [ἀρετὴ] ποιητοῦ συνέζευκται τῇ τοῦ ἀνθρώπου· καὶ οὐχ οἷον τε ἀγαθὸν γενέσθαι ποιητὴν μὴ πρότερον γενηθέντα ἄνδρα ἀγαθόν. 'The value of a poet is bound up with that of the man. He cannot be a good poet who is not a good man.' On which Joseph Henry Green, who quotes the passage, says: 'I anticipate no objection when I state that the process for attaining or approximating to this great moral result constitutes in its scope or end *a liberal education.*' What that is, and how to be attained, is held by all thinking men to be one of the problems which our age has to solve, in and for the interests of our country. May not grave mistake arise herein? At all events, in the present transitional condition of this and other questions of social economy, it were a waste of time for an Association such as ours to undertake the investigation of this difficult subject. But so far may be said, that the object of academical education for *our* Profession is *from early life to discipline all the faculties.* It is obvious that the Physician should be many-sided—he should be capable of sympathy with every form of good; he should have all his senses, eye, touch, hearing, disciplined to nice precision and exactness, both in perception and thought.

When a man says there is in the present age of the world only one education worth possessing, the education provided by 'the Scientific Method,' he seems to have forgotten the more genial parts of our nature, the relations of man to man; and the more tender parts of our nature, sympathy with goodness, imagination, generosity, devotion. Are not these essential to the highest success in our profession, quite as much as the intellectual efforts of the more specially scientific observer? We may take one instance: the bearing of the so-called Fine Arts on the development of certain qualities of mind. It is not on account of the accuracy which is required for their successful cultivation, for that belongs more or less to all scientific work. No youth can be a draughtsman or a musician to any good purpose without this; but music and drawing not only discipline the sense of sight, touch, and ear in an eminent degree, but have a peculiar effect on the imagination and intellect. There is a refinement of observation and a tact acquired by the study of masters of music, architecture, painting, and sculpture, which add a charm to the life and character of a man, whatever be his profession, such as is hardly attainable in any other way. Not, of course, that I would wish all men to be so educated as if they were to be artists or musicians. Mathematics are valuable to train the mind to habits of accurate thought. The mathematics may be forgotten, may vanish in all their details, but the accuracy and precision given by their study may remain. So the practical dexterity of the eye and hand and ear in drawing and in music may be lost, but the delicate perception of form and colour and the relations of colour, of sound and the relations of sound, and the effect produced upon the mind by the study and cultivation of the Arts therewith connected, may remain and tinge with a higher character the whole nature of the man. The development in excess of one part of our nature distorts the harmony of that nature. Many properties, which in excess are noxious, are, when held in subordination, ennobling. It would be out of place, I repeat, to attempt a full analysis of this educational question. It is indeed here entered upon simply by way of protest against the view that accurate Science can do all that is needed for training the Medical Student. Of all men, allow me to repeat, he most needs the harmonious development of all the good qualities of his nature, with 'scientific accuracy' and method at the root. In the present period of our history, I might say in the history of the human family, the principles which should regulate the training of our children have to be discussed with largeness of view, and carried out with extreme caution. There is no part of a statesman's duty just now for which he is more gravely responsible than that of preparing the children of the nation for the struggle, intellectual, religious, and material, which is certainly in store for them; there is no greater political treachery than that a statesman should pander to popular clamour by joining any temporary educational cry which he does not believe to be founded on permanent truths of morality.

It would be trespassing too much on your good-nature to ask you to listen to the proofs that an acquaintance with the mental constitution of man, with the ways of ennobling its impulses, and with that mixed knowledge and discipline which are called religion, is more especially necessary for our Profession. I, therefore, assume that you generally consider every scheme of preliminary education faulty which does not admit this, and will only state briefly what present circumstances seem to require of caution under that admission. Granted that for the intellectual training of a medical man, religious discipline and psychological knowledge are required, how are they to be imparted? and of what kind should they be?

If those who have investigated the subject were agreed as to the nature and origin of human families; if the unity of our race were conceded; if there were no variations in character dependent on family and inheritance; if there were no questions as to the future state, nor disputes concerning our relation to the Infinite; if no questions had arisen within the pale of Christendom as to the scheme of redemption, nor outside that pale as to the evidence of that Christian Faith; then indeed the student preparing for Medicine would find some definite course of mental philosophy and religious instruction established in all colleges from San Francisco to Calcutta. Till that day of united conviction arrive we must be content to take some general position that all can accept. Nor is this difficult. All will agree that we must, 1st, Study the phænomena of human nature as now known to us without regard to the origin of man; and 2ndly, Study the principles of laws which ought to regulate the will and affections of man for the good of himself and society—in other words, the principles of universal morality.

Nothing less than this is necessary for the youth who are to follow our profession, nothing more can we now enforce. We have in England to educate for the empire, that is to say, for persons of every creed. Our education as Physicians cannot in this respect be limited by any one form of religious belief; and however much I may deplore, in the pathetic words of Faraday, 'The people will go astray when they have this blessed book to guide them,' we cannot deny that under existing circumstances the results of mental science, deduced from every source, must be to some extent made part of the higher education of our profession without any regard to the bearing they are supposed to have upon generally accepted religious opinion.

The reason is a practical one and plain. It is our business to deal with the characters of men, to observe the action and reaction respectively of body and mind, to trace out how character is affected by physical alterations in the brain; how this may be modified by physical means, by discipline, by food, by the physiological agents called drugs. And our youth must and will follow the researches which are made in these several directions. It will be useless to denounce the enquiries which tend to explain the relations between thought and material organisation. That bundle (as it were) of qualities, good and evil, which we call Mind, does, as far as we know, require for its manifestation the continuity and integrity of a complex organisation. That organisation varies with the qualities which are exhibited. The mental organisation of animals inferior to man is as various as their bodily structure. In truth, we have as good right to call the bodily organisation the material part made for the action of mind, as the mind the consequence of the bodily organisation. The distinctive properties of the mind of man furnish the most notable illustration of the origin of Force. The absence of any one of these powers, and especially of the Will, shows the greatness of their presence. There are phænomena such as those of aphasia, such as the innumerable facts of pathological analysis observed in the insane, such as the remarkable results obtained by the researches of Claude Bernard, which become part of the common stock of knowledge, and must find their place in any theory of humanity which is to claim an acknowledgment from the intelligent physiologist of the future.

This deep, this profoundly interesting subject might be pursued to great length; it is commended to your serious attention with these words of Bacon:—

'All depends on keeping the eye steadily fixed upon the facts of nature, and so seeing their images simply as they are. For God forbid that we should give out a dream of our own imagination for a pattern of the world.

'Rather may He graciously grant us a true vision of the footsteps of the Creator imprinted on His creatures.'—Nov. Org. pr. 1.

'Very meet it is, therefore, that we be sober-minded, and give to faith that only which is faith's.'—Nov. Org. 65.

It is not to be desired that we should part to-night without some interchange of thought on the relation of this University as a place of national education to the topics we have been considering.

The functions of an University have lately been clearly stated by Mr. Mill in his address at St. Andrews. His statements coincide with those repeatedly expressed in this place by the best thinkers on the subject. His opinion is of special value, as no one will suspect him of too great leaning towards ancient studies, or the gentler parts of human culture. He says:—

'The proper function of an University in national education is tolerably well understood. At least there is a tolerably general agreement about what an University is not. It is not a place of professional education. Universities are not intended to teach the knowledge required to fit men for some special mode of gaining their livelihood. Their object is not to make skilful lawyers, or physicians, or engineers, but capable and cultivated human beings.... What professional men should carry away with them from an University, is not professional knowledge, but that which would direct the use of their professional knowledge, and bring the light of general culture to illuminate the technicalities of a special pursuit.'

He reviews the several parts of human knowledge by which this function is to be performed. For my own part, I know nothing apparently so damaging to the reputation of the study of classics as a means of forming completeness of character, as its active antipathy in former years to the cultivation of Science, and its passive indifference to the promotion of Art.

As long as this temper existed it implied an ignorance of both, and as respects Art a narrow appreciation of the true domain of Literature.

The causes of this temper need not be discussed by us: we are not the persons to do it in this place. The causes do not lie entirely in the effect of classical training, but in the narrowing tendency of exclusive devotion to one class of pursuits in a limited society. It was addition to the old studies, not substitution, that was needed. The addition is in the course of being made. May the substitution be averted. Your assembly in this Hall is a proof of the fact of the addition. This meeting is a testimony, if any were needed, that these ancient foundations are opened to the Professions, opened on conviction and with hearty good-will: the union of the ancient thought with modern method will be effected in this place.

Yet we may ask, why the Professions should be welcomed by the University? why this union should be desired by them? The answer is plain. Not through the guidance of the people by a few superior minds, not through the laying down rules of fashion by concurrence, not through the dogmas of authority by compulsion, but by the culture of practical life, by the moral elevation of the working people of every class, are the great traditions of this country to be maintained.

Not by Peers, nor Commons, not by Employers, nor Artisans as such, nor by all combined,—but by love of knowledge, of truth, and of uprightness; by a wide view of the needs of man, religious, moral, material; by a small estimate of our own powers, but a large one of our duties; by a just sense of the narrow field to which our own vision is limited, and of the shortness of the time during which to each of us that vision shall last;—by all these qualities uniformly diffused according to the capacity each may have, are Peers, Commons, Employers, Artisans, to keep alive the force of their common country.

And if these thoughts seem to belong to the arena of the political world, and not to the quiet recesses of a Scientific Assembly, remember that if your young men who are to be engaged in professional life, if the sons of your commercial men come hither, you will find their characters tempered through life by the processes to which they have been submitted. If they find here the traditions and the practice of general culture, of love of good, of pursuit of all knowledge, pure or applied; if they learn precision when precision is needed, method when method; if they are taught to indulge imagination where only imagination avails, fancy where only fancy; if they see us here resisting authority when there should be enquiry, but bowing humbly before that which is not for man to know, not ashamed of reverence and hope, nor afraid of faith; if here they may learn to be industrious and contented, of manly yet of tender heart,—then the Professions may send their youth to a place the country has reason warmly to cherish, if not wholly to approve.

I know nothing more hopeful than the prospect before us. Those who control the fate of this city of a thousand years can still say that the religious basis of the place is not yet undermined by levity, nor suffering from the dry-rot of unfounded assertions;

that Science is daily enlarging its borders by the addition of new institutions and new men, and that Art is asserting its just claims: so that those who desire to see the youth of England possessed of great opportunities may yet find them here.

One word more and I have done. The picture I have drawn is not intended to describe what is to be sought, or what is to be had in this or the other place of education, but what is to be sought in all. What we may aim at here we wish others to attain. If they be first, then we will follow; if they be last, we hope that our speed may quicken with their advancing tread.

You will perceive, I doubt not, that I address you not as an Oxford man, but as President of your Association when it visits Oxford. While you are here I hardly have the functions of host. As your representative I have asked freely for the aid you seemed to desire, and in your name I thank all who have granted it. In the name also of your Oxford friends, and as official head of the ancient and honourable Faculty of Medicine in her University, I heartily welcome you to whatever in the lotus-growing stagnant depth of the Long Vacation you may find still living here. You bring your own life; from England, Scotland, Ireland, from the United States of America, from France and Germany. Live it among us. And in exchange for our welcome to the banks of Isis give us your thoughts and your counsels, to the end that we may all return refreshed and strengthened to our common and happy toil.

RELATION OF PHYSIOLOGY

TO

MEDICINE IN MODERN TIMES,

READ BY

PROFESSOR ROLLESTON,

IN THE DIVINITY SCHOOL, AUGUST 5, 1868.

III.

PHYSIOLOGY

IN RELATION TO MEDICINE IN MODERN TIMES.

The fact that my connection with this place has put me into the honourable office of giving this address will, I hope, justify in your eyes my adoption for it of an arrangement, and my choice for it of a set of topics, which that local position has suggested to me. If I were to say that I had chosen for the subject of this address the bearings of the studies which it is the business of my life to teach here, upon the interests of the medical profession, I should be giving it too ambitious a title; it is but with some and with few of these bearings that I propose or feel myself competent to deal. I shall limit myself, firstly, by selecting only such topics as, having been pressed forcibly upon my own attention in my own peculiar course of labour, have come to assume, in my own eyes at least, a considerable importance, and have seemed, in consequence, not unlikely to prove possessed of interest for others also; and I shall limit myself, secondly, by abstaining from going over ground which has, within my own knowledge, been occupied by persons who have on previous occasions stood in the position which I now occupy before you.

Let me throw the heads of my address into a few short phrases, and say that I propose, with your permission, to speak firstly of the bearing of certain portions of the very extensive range of subjects, comprised under the titles Anatomy and Physiology, upon certain points and problems which come before the attention of the medical practitioner in the course of his actual duties; and secondly, of the illustration which some of the conclusions recently come to in Biological Science cast upon the validity of certain principles which are ordinarily looked upon as authoritative canons for the regulation of the reason in medical, and, indeed, in other investigations. Under the term Biological Science are included, besides pure Physiology, Human and Microscopic Anatomy, Comparative Anatomy also; and in this place, as your visit to the Museum will have convinced you, we give considerable, but as we hope, not undue prominence to this latter branch of study. I propose to speak of the bearings of Biology on Medicine in each, but, owing to our local speciality just alluded to, specially in the last mentioned of these four departments. And I must ask you to bear in mind that the very constant reference which I shall make, if not in my address, at all events in my notes, to the works and writings of others, is in like manner to be explained by my wish to have a distinctive colouring given to this address by the local peculiarities of the great educational centre in which we are assembled. For one of the most distinctive peculiarities of this ancient University is the formation within its precincts of such a library of modern science as will shortly have no superior, and but few rivals, in the world. This we owe to the well-advised administration of the funds of that famous physician, Dr. Radcliffe; and it is from a wish to make a sort of acknowledgment of the obligation which medical and other sciences owe to him and his trustees, that I shall so constantly, at least in print, refer to the chapters and pages of the innumerable books which their enlightened munificence has put here at the disposal of the student. It is not, I can assure myself, from any irritable anxiety to impugn or depreciate the work of others, that I have so constantly consulted and specified their pages; nor, I trust, have I allowed myself to be tempted into the unpardonable fault of using, or rather abusing, a great opportunity by making upon it a petty personal display. Rather have I felt it to be my duty to occupy this hour as you occupy your lives, in doing what it may be possible to do for the good of humanity. Your presence and your example make me feel that any other course would be but impertinence; and I have therefore kept constantly before my eyes Bacon's sentence in condemnation of all empty parade of useless erudition—*Vana est omnis eruditionis ostentatio nisi utilem operam secum ducat.*

The title of Niemeyer's work on medicine, the seventh edition of which has recently appeared and come into my hands, will furnish me with an excellent text for my first head—The Connection and Inter-dependence of Medicine and Physiology. That title runs thus:—Lehrbuch der Speciellen Pathologie und Therapie mit besonderer Rücksicht auf Physiologie und Pathologische Anatomie, von Dr. Felix von Niemeyer. Siebente vielfach vermehrte und verbesserte Auflage. Berlin, 1868. From this work I will take my first illustration of the nexus and connection of which I have to speak; and I believe that, though I am obliged to dissent from the explanation therein given of the facts I shall refer to from its pages, the explanation which they seem to me to bear, or rather demand, shows even more clearly than the one there given, the intimacy of the alliance which is now becoming so close between the experimentalist and the practitioner. In writing (vol. ii. p. 334) of a form of neuralgia, the pain of which those who have suffered from it themselves, or, indeed, have seen others suffering from it, will allow is not exaggerated by the application to it of the words 'fast unerträgliche,' Niemeyer remarks* that the physiologists Dubois-Reymond and Dr. Möllendorff refer its origin to the existence of a dilated state of the *arteria carotis cerebralis*. This state of dilatation these authorities explain by a reference to certain facts in the physiology of the cervical sympathetic as discovered now some seventeen years ago by Bernard, and elucidated still further by Waller, Budge, and Brown-Séquard. And in like spirit, or at least by a reference to certain anatomical facts in the arrange-

* Incorrectly however; Dubois-Reymond himself explaining hemicrania as due to a tetanus of the muscular coat of the arteries. See Reichert u. Dubois-Reymond's Archiv. 1860. Brown-Séquard, Journal de la Physiologie, iv. 13. 1861.

ment of blood-vessels, which he supposes to become dilated and distended, the great Göttingen anatomist, Henle (cited by Niemeyer, vol. ii. pp. 319–339), explains the causation of certain neuralgiæ. The neuralgia which is apt to haunt the sixth, seventh, and eighth intercostal spaces of the left side, he has suggested may be explained by the peculiar arrangement of the left or smaller azygos vein in that region; and the greater relative frequency of neuralgia of the first than of the second and third divisions of the fifth cerebral nerve, he refers to the greater quantity of dilatable veins with which it is beset in passing from the inner to the outer surface of the sphenoid. Now I venture, though with some diffidence, as I find myself in opposition to such names and authorities, to dissent from the explanations thus given of these facts, which I suppose must be acknowledged, with perhaps an exception as to the relative statistical frequency of neuralgia of the first division of the fifth, to be only too real facts. By laying the physiology of the cervical sympathetic alongside of the natural history of an attack of neuralgia, we shall be enabled, I believe, to see that there are stages in each corresponding with stages in the other, but that it is a stage of spasm in the one, and not a stage of relaxation and congestion, which corresponds with the stage of pain in the other. Stimulation of the upper cervical sympathetic produces, more or less immediately, contraction of the blood-vessels of the head and dilatation of the pupil, and diminution of the temperature. This is the first line of operation, resulting in what Brown-Séquard (Lectures on the Physiology and

Pathology of the Nervous System, 1860, p. 142) calls 'Decrease of Vital Properties.' But after a while the reverse of all this takes place, and the vessels dilate. Now whether this be so in consequence of exhaustion, as is ordinarily said (e. g. Funke, Physiologie, vol. ii. p. 772), or not, as Dr. Lovén (who says in Ludwig's Arbeiten Phys. Anst., Leipzig, 1866, p. 11, that the sympathetic is not so easily tired) thinks, is of no consequence, or of little consequence, as the fact of the sequence of events is accepted as I have stated it on all hands. Indeed, similar alternations of alteration in the calibre of vessels take place, as is well known, spontaneously, as the phrase goes—whether rhythmically or not, still chronometrically in relation to the needs of the animal and its tissues; in the arteries of the rabbit's ear (Funke, loc. cit. ii. p. 771, citing Schiff and Callenfels), in the veins of the bat's wing (Wharton Jones, Phil. Trans. 1852), in the arteries of the frog's web (Lister, Phil. Trans. 1858, p. 653); and the occurrence of these latter alternations makes the occurrence of the former more intelligible. Now, a similar alternation from a stage of contraction of blood-vessels, of coldness of skin, of shivering, of total absence of heat, redness, swelling, or tenderness, to one of increased circulation, swelling, heat, and tenderness, constitutes two stages in an attack of neuralgia, homologous with the two described as occurring in irritation of the sympathetic. It is rare, I believe and Dr. Anstie teaches (Stimulants and Narcotics, 1864), for pain, as opposed to tenderness, to persist after congestion; and pain in tissues differs as much from tenderness as remorse in a conscience differs from tenderness in that organisation; and the two things are well-nigh equally exclusive the one of the other in both cases. Perhaps the mere apposition of the two sets of occurrences side by side is sufficient to justify my conclusion that the congestive stage of the physiological experiment is not the homologue of the painful one of the morbid history, mere apposition being sometimes sufficient to decide us on more difficult homologies, at least in the negative. But I may add, that the argument from the *juvantia ac lædentia*, as the older physicians and physiologists phrased it, gives some confirmation to the view which teaches that spasm and starvation go in company with pain; relaxation and congestion only with tenderness. I will put the facts before you as premises; you will piece them together into an argument for yourselves. Chloroform* is the greatest of *juvantia* in neuralgia; chloroform, indeed, and ether, in equal parts, as recommended† by Mr. R. Ellis, may be safely entrusted to a safe person for self-administration; and, if taken persistently as well as prudently, may keep the attack in suspense until the enemy, from weariness or chronometric obligations, retreats or withdraws.‡ But this great reducer of neuralgia, this great

* For action of chloroform, see further, Chloroform, its Action and Administration, by A. E. Sansom, M.B. 1865. Asthma, by Hyde Salter, M.D. 1868. p. 216.

† See Lancet, February, May, June, 1866, and pamphlets published by Hardwicke and Brettell.

‡ The anæsthetic effect of bisulphide of carbon in various kinds of headache, as pointed out by the late Dr. Kennion (Medical Times and Gazette, July 18, 1868, p. 77), may, perhaps, be similarly explained.

and blessed producer of 'indolence,' as Locke called it, is also the great reducer of muscular spasm, as we know from its action and our employment of it in cases of hernia, and, indeed, of tetanus. Now, if it relaxes muscles which we can see in the limbs and trunk without the aid of a microscope, we may think it not improbable that it will do the like by muscles which we cannot see without the aid of that instrument, in the arterioles. Chloroform, secondly, has the reverse action to that of the sympathetic, in dilating the blood-vessels of the head;* as, indeed, also has alcohol, itself too a producer, though less directly, of the 'indolence' we desire, as well as of much that we deprecate. And chloroform, thirdly, antagonises the sympathetic in its very obvious action on the pupil.

It may be bold in me to venture further in this direction; yet, as a member of the British Medical Association, and as a reader of our admirable Journal,

* I find that both Mr. Durham (Guy's Hospital Reports, iii. vi. 1860, p. 153) and Hammond (On Wakefulness, p. 25) are agreed that, in animals under chloroform, the veins of the brain become distended. I do not, however, lay any great stress upon this fact; firstly, because the veins may become distended under the influence, not so much of the chloroform, as of the more or less partial hindrance to respiration which its inhalation implies; and, secondly, because we have, as yet (Funke, vol. ii. pp. 769-773), no very distinct evidence for the production of effects on the veins by the sympathetic. But, taking the facts as given, we must allow that venous fulness, though inferior, doubtless, to arterial replenishment, is still, as the growing prostrate of the aged, the rank hairs shooting up round old ulcers, and the cock s spur transplanted to the cock's comb, show, a more or less favourable condition for growth and nutrition; whereas pain is correlated always with malnutrition and ordinarily with atrophy, and is now always spoken of as a 'depression' rather than as 'an exaltation of function.'

I may perhaps be allowed to say, before I return to my own more immediate subjects, that the account given by Dr. George Johnson in that periodical for March 21st, 1868,* seems to me to indicate that epilepsy itself is but a frightful caricature of neuralgia, and of the results of vaso-motor irritation and contraction. The presence of dilatation of the pupil in all those sets of cases may be thought perhaps but a slight indication in the direction of identity of cause. High spirits and great vivacity are not rarely, in both diseases alike, precursors of an attack; while counter-irritation, which both Schiff and Setschenow† are agreed in considering a strong and universal reflex depressant, is not rarely, in both diseases alike, both, *ex hypothesi*, dependent on reflex vascular constriction, a preventive.‡

* See Brown-Séquard, Lectures, l. c. p. 179, *ibique citata.*

† Setschenow's words are these (Neue Versuche, p. 23, 1864): 'Es giebt endlich bei meinen Gegnern einen Versuch, an dessen Richtigheit ich keinen Grund zu zweifeln habe, an welchem die einseitige Trigeminus-Reizung eine starke allgemeine Reflex Depression hervorrief.' These words seem to furnish something like a rationale of the picking of the nose in helminthiasis, as also of much of that counter-irritation of the fifth nerve at its periphery which so-called 'nervous irritable' persons practise on themselves in the way of 'tricks.' Malgaigne practised similarly on his patients, as certain savage races do upon themselves with their labrets, ear- and nose-rings.

‡ These views I came to entertain without any knowledge — or perhaps I should rather say without any conscious recollection—of those which Dr. Radcliffe had put before the world in his lectures delivered at the College of Physicians, and published in 1864 in his work on Epilepsy, Pain, and Paralysis. I have not altered what I had written in consequence of my consultation of this most valuable work, to which I resorted after seeing a reference to it in Dr. Anstie's book. This latter work I have already quoted in the text, and I found it most useful and suggestive to me. I believe, indeed I hope, that what I have written is

I will now proceed to give, in the second place, an account of a physiological experiment worked out for us, from time to time, in the laboratory of Nature, which throws not a little light on a question which, I learn from Dr. Wilson Fox's work on Dyspepsia (p. 141), is still a matter of debate among pathologists—the question, to wit, 'of the influence of perverted innervation in causing inflammatory, or sometimes even still severer morbid changes.'* It is well known that stags,

more or less in accordance with Dr. Radcliffe's views. But it is as much inferior for purposes of consultation, and indeed in other points of view also, to what Dr. Radcliffe has written, and I have read of his, on the same subject, as a skull when just removed, and that in a somewhat fragmentary condition, from a barrow, is to the same skull when pieced together and reconstructed, as you may see many such skulls in the Museum, by Mr. Robertson.

* The following references to authorities on this vexed question I herewith append:—

Lister, Philosophical Transactions, 1858, p. 627.
Beale, Philosophical Transactions, 1865, part i. p. 447.
Virchow, Archiv, vol. xvi. 1859, p. 428; Cellular Pathology, Chance's translation, pp. 311, 312.
Paget's Surgical Pathology, Turner's edition, p. 237.
British Medical Journal, 1866, p. 402.
Anstie, Lancet, 1866, vol. ii. p. 548.
Simon, Holmes's System of Surgery, vol. i. p. 62.
Bernard, Leçons Physiol. Pathol. Syst. Nerv. vol. ii. p. 518.
Brown-Séquard, Lectures on the Physiology and Pathology of the Nervous System, p. 143.
Budge, Handbuch der Physiologie, p. 794.
Funke, Handbuch der Physiologie, vol. ii. p. 776.
Donders, Spec. Physiologie, p. 140.
Billroth, Die Allgemeine Chirurgische Pathologie und Therapie, 1868, p. 72.
Niemeyer, Lehrbuch der Pathologie und Therapie, 1868, ii. pp. 320, 340, 428.

after injury to the testes, have corresponding changes wrought out in the corresponding horns. Such a specimen I can show you from the Christ Church Museum, founded by Dr. Matthew Lee. In a curious old work dedicated to him, in company with two others of the King's Physicians, by Dr. Richard Russell,* and styled 'The Economy of Nature in Acute and Chronical Diseases of the Glands' (pp. 21–24), five cases of injury to the testicular gland in stags are recorded. In such cases as these after the injury to the testis, the horn may or may not be shed annually, and it may never thenceforward lose its 'velvet;' but it never becomes the dry lowly vascular

Handfield Jones, Functional Nervous Disorders, p. 11; Lectures in Medical Times and Gazette, 1865.

Samuel, Moleschott's Untersuchungen, Band ix. p. 18.

* It may be interesting to record here, in passing, that Dr. Richard Russell lived at Reading, that he was a friend of Dr. Chapman and of Dr. Frewen of this very place, and that the copy of his work to which I have referred, and which exists in the Christ Church Scientific Library attached to the Christ Church Museum, and deposited with it in the University Museum Buildings, did in 1760, eleven years before Dr. Russell's death, belong to Dr. Chapman. Now these facts and dates render it not improbable that this very specimen may have been given by Dr. Russell to Dr. Chapman, possibly together with this copy of his book. It is unfortunate that so much should be left to speculation; but this digression may be justified by the moral which it conveys, to the effect that we are bound, when receiving a specimen into a museum, to put on record forthwith, for the benefit of our successors, a note of its history and donor. Dr. Russell was the author of several other works besides the one I have quoted. Their existence has escaped the notice of Dr. Munk, in his interesting volumes, The Roll of the Royal College of Physicians, vol. ii. p. 132. Their titles are: 1. De Tabe Glandulari sive de usu Aquæ Marinæ in Morbis Glandularum Dissertatio. In 1 vol. 8vo.; pret. 5*s*. 2. A Dissertation concerning the Use of Sea-water in Diseases of the Glands; to which is added an Epistolary Dissertation to R. Frewen, M.D. In one volume, 8vo.; price 5*s*.

weapon of offence which, in a fortnight or three weeks from the present time, we shall see the bucks polishing their hard leather-coated horns into against shrubs and trees. It remains vascular and spongy within, and coated outside with a hairy skin, which may be prolonged into pendulous outgrowths. Being sensitive and fragile, and bleeding easily, it acts as a second sexual disqualification; and, as I am speaking of this correlation of growth, I may be allowed to add, that its reality is further testified to by its absence after similar lesions in reindeer, where both bucks and does are alike horned. Now, I submit that the unilateral correspondence of malnutrition, such as we have here, is as good an instance and exemplification of Pflüger's first law of reflex action, the law of unilateral (*gleichseitig*) transmission of stimulus, as any unilateral or homolateral twitching of any muscle can be in response to any one-sided stimulus. Only the reflex action shows itself in the way of nutrition—a sort of reversed hemiplegic nutrition, it is true—and not in that of movement nor in that of secretion.*

Let me, as in the former case, lay alongside of the

* See Otto, Neue seltene Beobachtungen Samml. vol. ii. p. 10; Elsaesser, Diff. Sex. Mamm. præter partes sexuales, p. 36. Since writing the above, I have seen a note to page 22 of Mr. Paget's Surgical Pathology, edited by Professor Turner, in which the fact that no disturbance of nutrition is effected by mere transplantation of the testis *in cocks*, is brought forward to show that no mere nervous disturbance can account for these alterations of nutrition. I do not think that these negative results, obtained from experiments on half a dozen *birds*, can outweigh the positive facts of unilateral correspondence in malnutrition which have been so frequently observed in mammals. (See Hunterian Catologue, Osteological Series, vol. ii. p. 591.)

physiological experiment a parallel to it from pathology. This I will do by the help of Budge, who, at pages 794–795 of his Handbook of Physiology, gives us the two following short histories, which have come under his own observation. His words run thus in translation: 'After a long continuing stagnation of blood at the end of the small intestine and the beginning of the large, in consequence of which exudations and adhesions of the peritoneum ensued, the entire right half of the body became weaker than the left, was tired sooner by exertion; the right foot became cold sooner, under the same circumstances, than the left; the right ear became much more rapidly the seat of vascular dilatation than the left; and other similar phænomena developed themselves. After a great abscess under the right gluteus maximus, and an immense loss of pus, the right hand and the entire right arm became not only evidently thinner to the eye than the left, but also actually smaller.' These cases are decisive as to the interference of the nervous system in the process of nutrition; and, though organs and structures, such as the epithelial and the cartilaginous, both physiological and, I suppose, morbid, may and do exist and grow in animal bodies being as devoid of blood-vessels and nerves as though they were found in vegetables, still any arguments based upon these undoubted facts can be met at once, if so we care to meet them, with the more or less accepted physiological axioms, 'The interdependence of parts augments with their development; the solidarity of organs increases and is more intimate with each superaddition of a fresh factor to the entire economy.' But these cases do not, of course, touch the question of the way in which this nervous influence comes to act on the tissues, whether mediately through the blood-vessels, or immediately on the tissues with which they are supposed, in the case of the salivary glands by Pflüger, though not, as I apprehend, by Dr. Beale (Phil. Trans. 1865, p. 447), to become continuous. Nor does such an experiment as that of Bidder, in which a salivary gland, under nerve-stimulation, picked out two-thirds of the entire quantity of iodide of potassium in the circulation, to one-third picked out by the substance of its fellow, not so stimulated. For a more innervated gland is also a more vascular gland; and of the two antecedents, greater nerve-current and greater blood-current, we have no right from this experiment to say that the one rather than the other is the cause of this particular consequent. And much probability will come to attach itself to Virchow's views, according to which innervation is not proven to increase nutrition directly, but works only mediately by its influence on the blood-vessels, in the minds of persons who may be averse to multiplying laws by cases such as these. We go to a case, as I suppose most of us may, like myself, have gone, and we frequently find one side of the body hot, and the other cold. This latter, the friends will tell us, is the paralysed part; we find that it is not; and Bernard's experiments, and Brown-Sequard's (l. c. p. 146), enable us to understand why this is so. An excellent case to the same effect, showing how increase of vital properties may take place in the entire absence of any connection with the upper part of the cord or brain, may be given from a paper of the late Sir B. C. Brodie's, in the twentieth volume (1837) of the Medico-Chirurgical Transactions. 'A man was admitted into St. George's Hospital, in whom there was a forcible separation of the fifth and sixth cervical vertebræ, attended with an effusion of blood within the theca vertebralis, and laceration of the lower part of the cervical portion of the spinal cord. Respiration was performed by the diaphragm only—of course, in a very imperfect manner. The patient died at the end of twenty-two hours; and, for some time previously to his death, he breathed at long intervals; the pulse being weak, and the countenance livid. At length, there were not more than five or six respirations in a minute. Nevertheless, when the ball of a thermometer was placed between the scrotum and the thigh, the quicksilver rose to 111° of Fahrenheit's scale. Immediately after death, the temperature was examined in the same manner, and found to be still the same.'

The larger size of a horse's hoof, the nerves of which had been divided, should probably be similarly explained by the greater afflux of blood which would set in thither temporarily until the continuity of the nerve was re-established. (Ogle, Med. Times and Gazette, Nov. 3, 1866.) And, finally, such an occurrence as the inflammation of skin, cartilage, or cornea, after its own sweet will, and not in the line of an irritated nerve passing through it or near it (Virchow's Cell. Path., Chance's translation, p. 299), seems to speak plainly enough to the self-sufficiency of animal cells to respond to what Niemeyer calls 'Insulte,' without appealing to any higher powers for assistance; just, in fact, as though they were as little animal, as truly vegetable, and as independent of any cranio-spinal centre as the gall-producing oak or willow.

But, in spite of all this, I am inclined to think that the direct action of nerves on cells is a *vera causa*; and, even if our highest microscopic powers do succeed in proving that nerve-tissues are never continuous with any other tissues in any part of their distribution, it must still be recollected that such intervals as may be demonstrated will be, if not insensible, at all events infinitesimal; and nerve-force may well be sufficient to act across such gaps as these. (See Dr. Radcliffe's Lectures on Epilepsy, 1864, pp. 13 and 330.) I can appeal for my justification to Professor Lister's experiment, recorded in his paper on the Cutaneous Pigmentary System in the Frog (Phil. Trans. 1858, pp. 636–639), in which certainly the nerve-system is shown to have some control over the molecular movements of concentration and diffusion quite independently of the blood-vascular system. The cessation of the circulation in a frog's web entails the concentration of the pigment; therefore Professor Lister took a pale frog—i. e. one in which the pigment was already concentrated; and, tying a ligature above the ankle, so as to eliminate the condition of cessation of the blood's circulation, he then eliminated the condition of nerve-influence from the craniospinal axis by amputation above the ligature. *Cessante causa, cessat et effectus*; the nerve-force is removed; and the pigmentary diffusion which it had held in check is set up and continues, until superseded by the *post mortem* concentration which ordinarily takes place, and produces that lightening of the dark hue usually seen in the frog after death. This experiment, which I have not given in full, nor in Professor Lister's own words, is a very striking one; and I hope I may remark, without offence to any representatives of the German Fatherland, to which physiology owes so much, that much that has been recently written and worked at there might have been spared, had Mr. Lister's papers been as well known to them as they will be to their successors. They seem to me to mark an era in the literature and in our knowledge of the essence of inflammation.

Here, if I may be allowed to digress somewhat, I would remark that Professor Lister's suggestion made in 1858 (loc. cit. pp. 619 and 640) as to the probability of the existence in the limbs of a ganglionic apparatus co-ordinating molecular and other movements at the periphery, sometimes independently, sometimes subordinately to the cranio-spinally placed nerve-centres, may seem to have found a justification in Professor Beale's demonstration in 1865 of the ending of the muscular nerves in nucleate reticular plexuses. Assuredly, the discovery of these net-works bearing nuclei does away with the necessity for any further carrying on of the apparently interminable discussions as to the existence of an 'idio-muscular' as opposed to a 'neuro-muscular' contractility. But I will take this opportunity of saying, that there are not wanting purely physiological considerations, which though not by any means amounting to demonstration, do nevertheless lend some little probability to the 'neuro-muscular' explanation of those movements which take place in muscles separated from all connection with central nerve-organs. Firstly, these movements are, within my experience, more marked and frequent in the muscular tissues of young animals; and the history of the development of nerves would lead us to expect to find a greater degree of independence in the peripheral nerve-system, than we should look for in the adult organism; for nerves do not grow from cells in the direction of what we know in the adult state and under low powers of the microscope as their branches; but as Von Hensen has shown (Quain's Elements of Anatomy, 7th edit. p. clxiv.) two nerve-cells are connected by a fibre, and it is by the withdrawal of the one cell from the other, and the elongation, so to say, of the interconnecting fibre, that the peripheral and central ganglionic systems respectively assume their adult relations. And just so, I may add, in certain annelids and lamellibranchiata, we have, as we not rarely do have in the lower animals, a stereotyped though but partial adumbration of what is but a single scene in the moving diorama of the development of the higher; and we find the peripheral nerve-system studded with eyes or other sensory organs, and possessed of a prominence and importance relatively to the central nerve-ganglia which is only temporarily seen in the development of more perfect creatures.

Secondly, many of the cases of death in adults, in which this irritability is found to exist most commonly and markedly, are cases in which, from very various reasons, the functions of the intracranial nerve-centres are put into abeyance at a very early stage in the process deathward. Such are (see Nysten, cited by Brown-Séquard, Proceedings of Royal Society, 1862, p. 211) cases of decapitation, of asphyxia, and of sudden hæmorrhage from a large artery. Now we know that movements do continue in a portion of intestine which has been deprived of its mesentery, and we ascribe the production of these movements to the presence in the walls of the intestine of the plexuses demonstrated to us by Meissner and Auerbach; and, if we may ascribe like effects to like causes, we may ascribe the *post mortem* twitchings of muscles to Professor Beale's neuro-muscular apparatus. In like manner we should expect from similar reasons, and we do find, as a matter of fact, this same neuro-muscular irritability greatly prominent in the small-brained cold-blooded vertebrata, and in hibernating mammals. In all of these animals alike, the central nerve-system is small relatively to the entire mass of their bodies; whilst in birds, or at least in the more highly organised of the class—for birds, like other bipeds, differ as to the mass and use of the brain (see Parker, Zool. Soc. Trans. v. 1862, p. 207)—the brain may hold a more favourable relation to the entire mass of their bodies than in any other class of animals; and in birds, as is well known, with some few reptile-like exceptions, such as the peewit, muscular irritability ceases almost with their last act of expiration.

Whilst speaking of this condition occasionally found in the muscles after death, I am tempted to say a few words of the empty condition of the arteries which is almost constantly found after death. I observe that

Von Bezold has explained this well-known phænomenon as being due to a last nervous impulse communicated to the small peripheral vessels *from the brain.* His words are (Untersuchungen aus dem Physiologischen Laboratorien in Würzburg, Heft ii. pp. 358, 359, 1867): 'Sicher ist aber ein ungemein wichtiges Moment hierbei die Innervation der Muskeln in den kleinen Gefässen des Körpers. Man stelle sich vor, dass in der Agonie, in Todes-Kampfe, das vasomotorische central Organ im Gehirn noch in Krampf-zustande versetzt wird, welche mit Pausen der Erschöpfung abwechseln... Ausserdem ist gezeigt worden, dass jenes letzte Ueberpumpen des Blutes aus den Arterien in die Venen, bei den Saugethieren wenigstens, unter dem Einfluss einer letzter Thätigkeit des Gehirns geschieht.' Surely all these 'Vorstellungen' would have been spared, if Professor Von Bezold had been acquainted with Mr. Lister's papers, or even with those points in them to which I have referred. Indeed, that his view is untenable, is clear from a consideration of the fact that the circulation can be kept up, and will, like the muscular irritability, persist in a decapitated animal for a long while after death, if artificial respiration be put in play. The empty state of the arteries *post mortem* is most probably to be explained by the action of the peripheral nerve-system on the arterioles; though Dr. Alison would have explained it by the attraction *a fronte* force of the tissues around the capillaries; but Von Bezold's view of the source of the nervo-muscular action of the peripheral vessels is, I apprehend, more untenable and less plausible than most theories which have 'had their day and ceased to be.'

The following short history seems to me to be a good instance of the action, or rather of the want of action, of the peripheral nerve-system upon the arterioles. A man, who came some years ago under my own care, had had a bullet pass through his arm just above the elbow, so as to sever the musculo-spiral nerve. The scars of exit and entrance were in the lower third of the arm. Under ordinary circumstances, the soft parts of the lower arm maintained their normal consistence; but their power of resisting changes of temperature was greatly impaired, as well of course as the sensibility of the parts supplied by the injured nerve. I recollect seeing the swollen state of the inner side of the hand one cold raw morning when the man was on sentry duty, and had his hand chilled down by the musket he had to carry. Now, I apprehend that this turgescence is to be explained by saying, that the local or peripheral nerve-system of the affected parts was competent under ordinary circumstances to regulate the calibre of the arteries; but that its activity was liable to be depressed, as under the circumstances related, into actual abeyance, in the absence of any possibility of any assistance being supplied to it from the cranio-spinal nerve-axis. Thus, under the depressing effect of cold, which seems to work here much as it does in checking the regeneration of artificially amputated parts in snails and in salamanders (Müller's Physiology, by Baly, 2nd edit., i. p. 444; Bonnet, Œuvres, tom. v. i. pp. 328, 329), the peripherally placed ganglionic system was put into abeyance; and turgescence of the vessels it ordinarily supplied with 'tone' ensued. Just similarly in mammals the skin of which has been covered with an impermeable varnish, and in which death is as much due to the chilling down which the destruction of the non-conducting power of their hairy integument entails, as to the penning-in of its various acrid and volatile and other secretions, œdema and vascular congestion are to be observed in the skin, as well as in other organs (Ranke, Physiologie, p. 456). The flame of mammalian life, like the flame of inorganic combustion of carburetted hydrogen, can only be sustained at a high temperature; a certain reduction is as fatal to the one as it is to the other in the Davy lamp, and the vitality of the more exposed peripheral is more easily depressed than that of the more protected central nerve-system.

I should be paying but a poor compliment to the judgment which has provided a microscopic exhibition for the instruction and entertainment of this evening, if I were to dwell at any length upon the relations borne by Histology to Medicine and Surgery. And, secondly, if I were to dwell in the least adequately upon the importance of a knowledge of Microscopic Zoology to the diagnosis, and what is better than the diagnosis, and even than the therapeutics, the prophylaxis of diseases of all kinds, from those which are considered trifling or contemptible by most men, except those who suffer from them, up to those which excite world-wide anxieties, such as trichiniasis or cholera, I should have to extend my address to a length you would shudder to think of. Upon one single point I will make a few remarks; and the purport of these will be to show how the manipulation of such an instrument as a catheter may find, if we are to do justice to our patients, its regulative condition in the manipulation and revelations of the microscope. I had myself recently come to suspect that the determining condition of the triple phosphatic alkalescence of the urine was to be looked for and found in the presence of some of those organisms which Pasteur has proved and hygienists have believed to be the real causative agents of fermentations and putrefactions. One accepted view of the causation of this most mischievous metamorphosis is, that the coats of the bladder, in consequence of altered innervation, as after spinal injuries, act upon the urine as so much dead matter acts on blood in causing its coagulation, or as the tissues round about the capillaries act when they are in an abnormal condition upon the rows of blood-corpuscles within those canals; and by this 'catalytic' agency break up the urea and throw down the ammoniaco-magnesia phosphates. Another view ascribes the like effect to the 'fermentative' working of the abundant catarrhal mucus, which is in some cases flaked off from the inner walls of the bladder. Now, neither of these views suggests a *vera causa* for the effect for which they profess to account. Blood coagulates when in contact with non-vitalised matter, and blood-corpuscles arrange themselves in *rouleaux* under similar circumstances. But Mr. Lister, who has shown us so much which bears on this matter, has shown us also, and that in the last number but two (July 18, 1868) of our Journal, that urine will remain for an indefinite period undecomposed in a properly constructed, which happens to mean a properly contorted, receptacle, even though that receptacle be as little vitalised as glass. And Niemeyer has shown, what I dare say many who are now honouring me with their presence have observed, but, I think, not recorded, that urine often retains its acidity through protracted cases of vesical catarrh, and in spite of cumuli of clouds of 'fermentative' mucus; which are, therefore, as little of *veræ causæ* as is 'catalysis' itself. But the presence of vibrios in the urine, and that before it leaves the bladder, is a *vera causa*, i.e. a present condition, and therefore possibly a cause, or connected with the cause, of the phænomena to be investigated (see Beale, On the Urine, p. 196); and the idea that the alkalescence in question depended upon them, an idea which I had not the time to find an opportunity for verifying for myself, I find has been verified for the benefit of others by Niemeyer, with the assistance of Traube and Teuffel. 'In the course of last year,' says Niemeyer (l. c. ii. 66, 1868), 'I arrived, partly by means of an observation of Traube's, partly by means of experiments and observations of my own, which have been published by Teuffel in the Berlin Klinische Wochenschrift, at the conviction that it was not the vesical mucus, but lower organisms, which probably get into the bladder by means of the introduction of badly cleaned catheters, and excite there the alkaline fermentation of the urine.' Now, whether the vibrios find their way into the bladder exclusively on dirty catheters or not, I apprehend that the addition of some carbolic acid to the oil used for lubricating these instruments, whether they be guilty or not of what is here laid to their charge, will be a piece of practice calculated to prevent the alkalescence which the vibrios cause by preventing these vibrios themselves from entering on their evil activity. Mr. Lister's paper, just alluded to, will show that this is an experiment which may very safely be tried; if carbolic acid can be safely introduced into a wounded pleural cavity, assuredly we need not hesitate about the passing of it into a bladder. Thus many scientific researches, undertaken in the first instance for the elucidation of speculative truth, and for the rectification not of unsound organs and functions but of unsound theories and explanations, and prosecuted throughout by the aid of the most refined methods and instruments, come ultimately to bear upon such matters as catheterisation and alkalescent urine. I would not, however, be thought to undervalue the worth of researches carried on at whatever cost with the sole object of procuring correct notions as to the way in which processes, even wholly beyond our power of modification, have been and are being carried on. It is a great and positive gain when we get rid of one false hypothesis, one single false formula which by frequent repetition has attained to the dignity of a philosophic axiom, and acquired a sort of prescriptive right to 'warp us from the living truth.' The Chemists, as I am informed, are conspiring to effect what the old Greeks would have called a 'Catalysis' of the kingdom of 'Catalysis' itself, and its banishment to the Limbo of Vanity, there to herd with Phlogiston and many other and younger as well as older unsubstantial Idola Theatri;

and though these alterations of theory may not as yet have affected the oxygen we breathe, nor even have enabled us as yet to regulate with any greater precision the processes of fermentation with which we have for so many ages had an empirical familiarity, they have given us at least a warning as to maintaining always that proper diffidence as to the all-sufficient validity of our theories, by whomsoever promulgated or endorsed, which is so constantly of avail in actual practical work. The phænomena, let me add, to account for which the hypothesis of Pangenesis has been recently (Darwin, Animals and Plants under Domestication, 1868, ii. p. 403) put forward provisionally, are and will, we may believe, always remain, beyond our control; but there will be no one, I suppose, who will not feel an interest in observing how the revelations in the all but infinite divisibility of 'germinal matter,' which we owe to Professor Beale, may come to bear upon the explanation of the marvellous phænomena of reproduction and hereditary transmission. Nor can I leave this subject without remarking that it is in great probability upon the self-multiplication of such infinitesimal particles as this hypothesis of Pangenesis postulates that processes, to the naked eye the very reverse of Genesis, have been found to depend; and the Blue Book on the Cattle-Plague (Third Report of the Commissioners, &c., p. 151) will show you that here, too, we are dependent on the employment of the very highest powers of the microscope; and it is scarcely necessary to add that its employer was in this case Professor Beale.

If I have been short in speaking of the advantages which the histology of modern days has conferred upon its therapeutics, I might be shorter still in dealing with my third head—the dependence, namely, of the healing art upon the facts of Anthropotomy—that is to say, upon the naked-eye knowledge of the structure with which it has to concern itself. Some little, however, I must say with your permission. Some persons are inclined to think that there is some sort of antagonism between the interests of microscopic and those of naked-eye anatomy; and hints more or less obscurely expressed may be found to this effect here and there in writings even of the present day. It is in much the same spirit that persons are found to say that the sending of missionaries to the heathen abroad entails so much curtailment of similar work at home, and that others will say that the starting of any fresh charitable institution necessitates the subduction of so much from the funds available for those already on foot; and that others again will say that the encouragement of natural science is 'inimical' to the progress and cultivation of literary and classical studies. Now, all these views depend upon the radically false assumption that intellectual and moral activities are limitable and measurable by certain quantitative conditions, just as a man's expenditure is or ought to be limited and measurable by his balance in the bank. This analogy is a wholly fallacious one, but it has nipped many an excellent project in the bud. A truer analogy is furnished us by the history of those infinitesimal scraps of germinal matter of which I was just now speaking, which are hard to destroy even with floods of carbolic acid and copperas, and which possess a faculty of self-multiplication wholly unparalleled within my experience in the history of the metallic objects of which we were just now speaking. Activity and earnestness, in fact, which are some of the best things, resemble some of the worst in being eminently contagious. The example of a strenuous labourer in one field, spreads into the weedy acres of his slothful brethren on the right and left; and the improvement of the microscope has but been accompanied by a more thorough and accurate working out of human dissection. Let us leave metaphor and general statements, and come to facts. I have in my possession a work written for the use of anatomical students in the University of Edinburgh—a place then, as now, at least on a level with the most advanced centres of such education elsewhere in Great Britain. Its title is, 'The Anatomy of the Human Bones and Nerves, with a Description of the Human Lacteal Sac and Duct, by Alexander Monro, M.D., late Professor of Anatomy in the University of Edinburgh. A new edition, carefully revised, with additional Notes and Illustrations, by Jeremiah Kirby, M.D., author of Tables of the Materia Medica. 1810.' The date of its appearance takes precedence, therefore, by a dozen years at least, of the first appearance of an achromatic combination; and if the development of microscopic zeal had really been injurious to the diffusion of thorough anthropotomical knowledge, we should find here in perfection that precision and fulness which *ex hypothesi* are the exclusive fruits of individual attention and undistracted concentration. Now, a few weeks ago, I was pursuing some anatomical researches into the homologies of the shoulder-joint muscles, and by the suggestion of one or two of my friends, amongst whom I may mention Dr. Boycott, I took up the line of argument for homological identity which innervation furnishes. Being deep in the country, I was reduced to consult, in the absence for the moment of other books, the work I have just mentioned, for a small matter in the composition and decomposition of the brachial plexus. This is what I found to satisfy my enquiry in a book expressly treating, you will please to recollect, of the nerves, and written by one of those 'famous old anthropotomists' who were not distracted by 'microscopische Spielereien.' 'The fourth cervical nerve, after sending off that branch which joins with the third to form the phrenic, and bestowing twigs on the muscles and glands of the neck, runs to the armpit, where it meets with the fifth, sixth, and seventh cervicals, and first dorsal, that escape in the interstices of the *musculi scaleni*, to come at the armpit, where they join, separate, and rejoin in a way scarcely to be rightly expressed in words; and, after giving several considerable nerves to the muscles and teguments which cover the *thorax*, they divide into several branches, to be distributed to all parts of the superior extremity. Seven of these branches I shall describe under particular names.' (p. 291.) These seven branches have the particular names of Scapularis, Articularis, Cutaneus, Musculo-

Cutaneus, Muscularis, Ulnaris, and Radialis. Of such little trifles as the connection of the second and third cervical sympathetic ganglia which give off heart-nerves, with the arm-nerves, upon which connection the pain down the inside of the arm in heart-disease Niemeyer (ii. 338) supposes may, and the older anatomists would have said *must* depend; as the subclavius nerve and its connection with the phrenic, and so with the shoulder-tip pain in liver-disease, we have just as little mention made as we have of the nerves supplying those small muscles, the pectorales. Surely knowledge is not like a volcanic archipelago, where the upheaval of one mass of solid ground entails the submergence of another; rather it resembles some vast table-land which is rising, and now and then at accelerated rates of progress, out of the waters, and has, in these days of the subdivision of property and of labour, its broad and continuous surface seized upon, partitioned out by enclosures, and put under cultivation by various occupants so soon as ever its outlines are recognisable.

My last topic in this division of my address is the connection which Comparative Anatomy has with Medicine and Surgery, and the bearing which a cultivation of this department of Biology has, or is likely to have, upon the interests of the profession. Of the benefits which Comparative Anatomy receives at the hands of medical practitioners there is little occasion to speak; or rather Mr. Parker's volume on The Shoulder Girdle, just published by the Ray Society, may speak for me; it is only less vast than valuable, and will constitute the commencement of a new epoch in the science. But what I have to speak to is, not the benefits which Comparative Anatomy receives, but those which it can confer. And I believe that the educational working of this study is perhaps the particular line along which the best fruits for the profession, and for the public, may reasonably be looked for. Any study which forces its students into that most valuable knowledge—the knowledge of when a thing is proved, and when it is not—is *ipso facto* an ally of real medicine, and a deadly enemy of quackery. A person who has in any way become acquainted with what reasoning and reasons are in one subject, will be apt to look for similar reasons and similar reasoning when he has to deal with another, and especially and rightly if that other be a closely allied subject. And when natural knowledge shall have become more widely and generally diffused, an end which we may hope to help towards accomplishing by means of our School of Physical Science, quackery, with its painful spectacles of reputation and confidence unfairly withheld and more unfairly bestowed, will cease to flourish in its present rank exuberance. A worker in Biology gains reputation accordingly as he is acute enough to observe and generalise for himself, and accordingly as he is conscientious enough to make himself master of and duly acknowledge the labours of others. It cannot be said that learning, talent, and labour are equally certain to secure prominence either for a medical doctrine or a medical practitioner. The medical doctrine obtains currency, acceptance, and popularity, and the confidence of an ill-educated public, by virtue ordinarily either of the effect on the imagination which its paradoxical character secures for it, or of the effect on the ear of the alliterative ring of the phraseology in which it is embodied. The success of persons, again, in the medical profession, and in some other walks of life too, may depend on personal qualities quite other than any connected with diligence, attainment, or ability—upon, say, certain peculiarities of manner, either in the way of polish or of that of roughness. The greatness of the stake, his own health, for which a man is playing when he adopts a particular doctrine or doctor of medicine, no doubt disturbs the balance of such powers of judgment as he may have, much as in Gessler's hopes the placing of an apple on the head of Tell's son disturbed the steadiness of the father's hand and eye. But habits of thought, as of other things, may be acquired by a proper course of education, and habits, like drill, steady a man under emergencies; and a scientific training enables a man to set about forming a right judgment in a right way and upon proper and legitimate grounds, even when nothing less than life itself is at stake. If a knowledge of such a subject as Comparative Anatomy, and of its external aspect, scientific Zoology, is a knowledge which will give the layman more power of forming right decisions, it is perhaps needless to labour long at showing that this self-same knowledge may be of the like service to the professional man. A sort of practical proof of its value is furnished us by the fact, that in Edinburgh as in Germany a dissertation on some subject of Comparative Anatomy is often accepted as a thesis for the degree of Doctor in Medicine. By such a regulation we have the obvious fact recognised, that the same sort of skill in the employment of methods, the same familiarity with organs, tissues, and functions, the same reasoning powers, are employed in investigating the problems of life wheresoever existing. The second aphorism of the Novum Organon applies to the one as to the other line of investigation, that of Human, and that of Brute Biology:—*Instrumentis et auxiliis res perficitur*; and alike in both, *nec manus nuda nec intellectus sibi permissus multum valet.* Comparative Anatomy, finally, besides thus benefiting the public firstly, and the profession secondly, is of use to Human Biology and Medicine, as such, inasmuch as it casts so much light upon the problems which the more highly evolved organs, functions, and other relations of our own species render in a much higher, and, indeed, sometimes in the highest degree, difficult or impossible to investigate. Answers to what are riddles in Human Anatomy and Physiology are often to be found given in very simple language in the structures and functions of the lower and lowest animals. Of such hints furnished by the brute creation towards the proper solution of certain problems which concern each and all of us in dealing with our own species, I will herewith, by your permission, give a few. Of the use of rest towards the repair of injuries I presume there is little doubt, but the best established teaching is all the better for the support of a few concrete examples. Now, in what animals do we find the greatest capacity for repair of injuries, and for the reproduction of lost parts and limbs? Precisely in those in which the whole of life is carried on at the lowest rate, and in the nearest approximation to rest which is compatible with animality,—in those animals, to wit, which breathe water, and have but its scanty percentage of dissolved oxygen to sustain their animal functions. The metamorphoses which an animal may have undergone, or may have to undergo, have very little directly to do with its power of recovery from injury, or of regenerating a lost limb. No animals go through more complex metamorphoses than do many of the Crustacea, and nearly all the Echinodermata; yet assuredly no other class has a larger capacity for the reproduction of lost fragments of their bodies. Now the latter of these classes is exclusively, and the former all but exclusively, aquatic. The more perfect, again, an insect's metamorphosis, i. e. its power of building up tissues and organs, the more perfect ordinarily, or rather the more profound, has been its quiescence as a pupa. Indeed, the very exception here proves the rule, and proves it to be a good one; for such hemimetabolous insects as, like May and dragonflies, come, in their imago state, to differ almost as much from their larval forms as the imagos of many holometabolous insects do from their larvæ, are during those preparatory stages as completely aquatic as any crustacean (Westwood, Introduction to Entomology, vol. ii. pp. 29, 38; Carus and Gerstaecker, Handbuch der Zoologie, p. 29). I am aware that there is such authority as Mr. Paget's (Surgical Pathology, ed. Turner, p. 123) and Mr. Darwin's (Animals and Plants under Domestication, vol. ii, p. 15) in favour of regarding the power of repairing injuries as standing in an inverse ratio to the amount of metamorphotic change through which an animal has gone; and I must therefore take the more pains to show that my explanation, to the effect that this happy power depends mainly upon the peacefulness and quiet with which the various processes of life are carried on ordinarily, and after the mutilation, is the truer one. My opponents' case would rest on such facts as these which follow. I will give them first, and then show how they really support my views. The larvæ or tadpoles of the tailless Batrachia, but not the adults, says Dr. Günther (Darwin, loc. cit., and Owen, Comparative Anatomy of the Vertebrate Animals, vol. i. p. 567), are capable of reproducing lost limbs. So with insects, says Mr. Darwin, l. c., 'the larvæ reproduce lost limbs, but, except in one order' (the Orthoptera, and amongst them the Phasmidæ *), 'the mature insect has no such power.' There is, however, one common property which lies at the bottom of the power of repair both in the larval forms and in the perfect adult animal, both in the invertebrata and in the vertebrata specified. This common property is the *comparative insignificance of the apparatus for aërial respiration:* in all alike—in the larva of the anurous amphibia, in the larva of the butterfly, and in the orthopterous insect—the lungs or the tracheæ, as the case may be, contrast to disadvantage with those

* There seems to be some little doubt whether even a Phasma can regenerate lost parts after its last moult, and some authorities would not consider it adult till after such ecdysis. The crustacea, however, moult many times after attaining the adult state, i. e. a state in which they can reproduce *the species.*

of their congeners, or adult representatives, which have come to differ from them in having lost the power of reproducing lost parts. But active respiration is a prerequisite for activity of function and rapidity of rate of vital processes: and the absence of this is, according to my argument, the cause of the presence of the reparative power. The lungs are of course all but wholly in abeyance in the tadpole, and the tracheæ have no vesicular dilatations developed upon them in the caterpillar forms of any insect, nor in the adults of the non-volant Orthoptera. In the Phasmidæ, the curious 'walking-stick' insects, we observe just the same sluggishness, combined with great tenacity of life, which we observe among mammals in the Bruta. Let me add some more facts in further illustration of my position. The Myriapoda, which Mr. Newport has shown to possess this power of repair up to the time of their final moult, are so little like the more typical insects, as to have been classed with the Crustacea, by no less an authority than Von Siebold. Any one, again, who will compare the simple non-cellular lung of the adult Batrachian newt *Salamandra Aquatica*, which possesses an unlimited power of repair *as an adult*, but *not in its young stages* (Bonnet, Œuvres Hist. Nat. v. Pt. i. p. 294), with the lung of the adult frog, will have little difficulty in understanding how their power of repair differs out of all proportion more than the amount of the metamorphotic changes they severally go through. The land Salamander, *Salamandra Terrestris*, has, so far as I know, escaped the hands of Spallanzani and Bonnet; its adult lung being little inferior in extent and development of spongy matter to that of the adult anura, I should expect the power of regeneration to be reduced to zero as in them. If the teaching of Comparative Anatomy has forced me to differ from the teaching of Mr. Paget, there are other facts in the same region of research which, as it seems to me, put one of his other many valuable doctrines in a clearer light than even his own clear enunciation of it. 'Each man's capacity,' says Mr. Paget (Lancet, Aug. 24th, 1867), 'for bearing a surgical operation may best be measured by the power of his excretory organs in the circumstances in which the operation will place him.' Now, I am inclined to ascribe the very considerable, and indeed, on my views, somewhat exceptional powers of reproduction which two sets of air-breathing terrestrial animals, the pulmonate snails and the earth-worms, possess, to the great development of their excretory apparatus. Living, as they do very ordinarily, in atmospheres laden with carbonic acid from decaying vegetable matters, they must get rid of the products of their waste and wear in the shape of fluid solutions; and the alkaline secretion with which the bodies of both are so abundantly slimy, furnishes just the required medium. When injured or mutilated, these animals can withdraw themselves pretty completely from the atmospheric oxygen by shedding out this secretion, and it at the same time disembarrasses their system from any excess of carbonic acid which may be generated within it. Thus they can attain the most perfect possible condition for repair and regeneration, the minimum of activity of all save the excretory organs; and I submit that it is possible that these two conditions may be connected as cause and effect, just as in the reverse direction a defeat of surgical skill may be connected with the presence of a fatty kidney or liver, or the excitability of a nervous system. It is going perhaps too far to attempt to explain the much greater power of repair which Amphibia possess as compared with either Pisces below or Reptilia above them to the larger size, and consequent smaller aggregate surface and less perfect aërating power of their blood-cells, and to the transpirability of their naked skins, which execute such important depuratory work for them, and are so closely connected and correlated with their lungs, livers, and kidneys. It is curious, however, and interesting to remark that the older anatomists, in commenting on the very obvious solidarity of these latter organs, went on, in their ignorance, I imagine, to a great extent of the nature of amyloid and other degenerative changes in such cases, to observe that it was illustrated by the 'fact' that, as the lungs grew smaller, so the kidneys grew larger in phthisis*. (See Funk, De Salamandræ Terrestris Vitâ, 1827, and Meckel, Pathol. Anat. vol. i. 613, 646.)

Verloren, as quoted by Donders (On the Constituents of Food, translated by W. Daniel Moore, M.D., p. 24),

* For accounts of experiments as to regeneration of lost or destroyed parts, see Darwin, Animals and Plants under Domestication, vol. ii. p. 15, *ibique citata*. Owen, Comp. Anat. of the Vertebrata, vol. i. p. 567. Newport, Phil. Trans. vol. cxxxiv. 1844, *ibique citata*. Paget, Surg. Path., ed. Turner, p. 123. Spence Bate, Ann. and Mag. Nat. Hist. for the current month, Aug. 1868, citing Mr. Lloyd of Hamburg, p. 118. McIntosh, Experiments on Carcinas Mœnas, p. 28.

has shown how the history of insects bears on the question which we are this afternoon to have expounded to us of the 'Relation of Food to Force;' and the very title of Bischoff's and Voit's work, The Laws of the Nutrition of the Carnivora, shows how this subject, to which I shall no further allude, but leave it to the able handling to which it has been entrusted, is dependent on the life, and the modes of life, of the lower creation. But I would say that it was from a study of the structures of the class of animals last mentioned—viz. the Carnivora—that I first came myself to be convinced that the uterine mucous membrane would, if properly looked for, be found in all animals alike to stretch after delivery over the area previously occupied by the placenta; and assuredly there is no one of the many complex and hard to be investigated problems of human physiology to which we are more bound to be thankful for light whencesoever obtained; and this, though the light come, as, in justice to Dr. Matthews Duncan and Dr. Priestley, I must say it did, in the way of illustration and confirmation rather than in that of discovery. (See Zoological Society's Transactions, 1863, p. 289; Dr. Duncan's Researches in Obstetrics, p. 206.)

These facts of the structural and functional arrangements of the lower animals have been used recently to illustrate some other points of uterine pathology and therapeutics in our own species. In a work by Dr. F. A. Kehrer, of Giessen, in two parts, the former of which was published in 1864, and treats of 'Die Zusammenziehungen des weiblichen Genital-canals,' and the second of which was published in the present year, 1868, and treats of 'Die Vergleichende Physiologie der Geburt des Menschen und der Saugethiere,' I find no little light thrown upon the question of relative position, whether as cause or effect, in which early and late abortions respectively stand to imperfect involution of the uterus. And I find also *in loco* a very distinct admonition as to the inexpediency of allowing fear of decomposition to terrify us into what is called 'meddlesome midwifery.' These extracts I think you may be interested to hear; I will simply quote them, and leave you, who are so well able to do it, to make the application of them for yourselves. 'Finally, let it be remarked that in rabbits in the earlier stages of gestation I saw the fœtus with its fœtal envelopes protrude entirely out of the os uteri, whilst the placenta still remained firmly attached in the uterus; a phenomenon which indicates either that in the earlier stages of pregnancy the placenta materna is less lacerable, or that the motor power of the uterus is a relatively smaller one, and one which finds its analogy in the occurrences which take place in abortions and premature deliveries in the human subject.' Heft i. p. 52. In his second Heft, p. 166, Dr. Kehrer, in speaking of the retention of the placenta being sometimes followed by symptoms like septic poisoning and sometimes not, has words to the following wise effect:—'What chemical changes may be set up in the retained placenta is clearly dependent hereon, whether the after-birth is shut off from contact with the air by the genitalia or not; for, if air find access to it, the membranes of the ovum putrefy; if air be excluded, a process of decomposition, probably identical with one of maceration of the fœtus, but wanting further chemical investigation, is set up. The occurrence of the one or the other eventually is so far of importance, as thereupon hangs the after-effect of a retention of the placenta upon the general health. In fact, we observe in women, just as in the animals mentioned, sometimes only insignificant symptoms, sometimes emaciation and sickness; sometimes, as after the absorption of putrilage from the decomposing membranes, a violent, even a fatal pyæmic fever. In the face of these facts, it seems to me to be rational in ruminants, in which the cotyledon can only be detached from the uterus by considerable violence, and scarcely even then, completely to *avoid all introduction of the hand into the cavity of the uterus after delivery*, just with the object of keeping it free from the ingress of air, and to leave the separation of the placenta rather to the natural forces. We shall thus best avoid putrefaction being set up in the cavity of the uterus, and so expose the animal the less to the risk of pyæmia.' I have not quoted from the recently published works of Dr. Matthews Duncan and of Dr. Graily Hewitt, though I have specified in my notes the pages of those works which bear upon what I have just quoted from a foreign source. I have forborne to do this, not because I think those works less, but because I think them more valuable, and I presume they will be in your hands as they have passed through mine*.

* Dr. Graily Hewitt, The Diagnosis, Pathology, and Treatment of Diseases of Women. Second edition, 1868, pp. 32, 342, 393. Dr. Matthews Duncan, Researches in Obstetrics, pp. 276, 284, 285. Cazeaux, Traité des Accouchemens, pp. 334, 349.

The human anatomist who has once seen in the lower animals the structures which represent, as it were, in exaggeration or caricature, the human costo-coracoid membrane; the tuberculum pubis, with the homologue of the clavicle which is attached to it as Poupart's ligament; or the supracondyloid process of the humerus—is not likely to forget their existence when, with either scalpel or bistoury in hand, either for the ligature of an artery or the setting free of a hernia, he has to deal with their representatives in the human frame. But, if I am right in thinking that the ciliary muscle in the eye would not have secured for itself the notice which it has done of late years, had it not been for the much more obvious manifestation of a similar, if not homologous, muscular apparatus in the eye of a bird, I apprehend that I am justified in saying that every surgeon who performs Mr. Hancock's operation of cylicotomy for glaucoma is under obligations, whilst so doing, to Comparative Anatomy and Sir Philip Crampton. I need not speak of the bearing of the discovery of this muscle in the human eye by Mr. Bowman upon the physiology of its adjustment to clear vision at varying distances. Pure Physiology, again, unassisted by Comparative Anatomy, has made out much of pure function; but, much as has been attempted in the way of experiment with infusions of pancreatic substance, and with the introduction of cannulæ into the duct of the gland, I am inclined to think that a comparison of the relative size of the gland in the carnivora and the herbivora respectively, in a dog, say, and in a rabbit, points as unmistakably as any of the lines of experiment just referred to—which, by the very nature of the case, are greatly beset with several sources of fallacy—to the fact that this salivary gland is concerned as much with the digestion of albumen and fat, as with that of starchy substances. With the remark that Hyrtl's discovery (Wien. Zool.-Bot. Ges., 1861, cit. Henle, Handb. der Anat. ii. 310) of the diverticular character of the glomeruli in the kidney of the selachians and amphibia bears not a little upon the existence of a similar arrangement between the vasa recta and the renal arterioles in the human kidney, whereby, as by the direct communication shown by Schrœder van der Kolk to exist between the arteries and veins of the pia mater, the capillary circulation may be skipped, and the tissues in relation with it left at rest, I leave this part of my subject, and begin the concluding portion of my address.

In this part of my address I propose to consider, mainly by the light of recently attained biological results, the value of two great rules for the conduct of the understanding, each of which has a legitimate sphere of application, but the former of which enjoys, it seems to me, more and the latter less than its deserved prominence. The first of these two regulative principles has received the endorsement of Newton, and it stands as his first 'Regula Philosophandi,' at the commencement of the Third Book of the Principia. It was known in the days of the schoolmen as the 'Razor of Occam,' and in later days it has been styled the 'Law of Parsimony' or 'Economy.' Newton enunciates it as follows:—'Causas rerum naturalium non plures admitti debere, quàm quæ et veræ sint et earum phænomenis explicandis sufficiunt. Dicunt utique philosophi: "Natura nihil agit frustra;" et "Frustra fit per plura quod fieri potest per pauciora." Natura enim simplex est et rerum causis superfluis non luxuriat.' I know that this Regula has great influence on the minds of many biologists, and I believe that this its influence is by no means always for good. This is not a subject in which authorities ought to count for much; but I may say that, while the names of Aristotle, Malebranche, Maupertuis, St. Hilaire, Goethe, Bichat, and Dugald Stewart may be quoted in approval of this rule, the names of Bacon, Mill, and De Candolle may be brought forward by those who repudiate it or curtail its application. Our motto, however, is 'Nullius addictus jurare in verba magistri;' and our business is to ask, not what men have laid down, but how Nature operates. Can a phænomenon have more than one cause, or can it not? Is it possible, for example, and to put the question in a concrete and most practically interesting point of view at once, that a fever which we know can spread by infection or contagion, can also originate spontaneously? I rather incline, though but doubtfully, and after an imperfect examination of imperfect data, to anticipate that a negative answer to this latter question will turn out some day to be the true one; but I do not know that there is anything in the analogy of Nature to compel us to incline towards this negative conclusion *à priori*. Such a phænomenon, at all events, as a living animal, is often enough produced by two or more distinct processes, within the limits of the same species: as, for example, from ova of different character, summer ova or winter ova, impregnated or unimpregnated ova; by fission or gemmation; through two different series of metamorphotic changes. And such a phænomenon as the production of a particular tissue may depend—in the case of adipose tissue, for example—upon the employment in Nature's laboratory of one or the other of two different chemical compounds. Pain may be, as Dr. Handfield Jones has shown paralysis is, produced in one case by the impact of shock upon nerve-centres, in another by the curtailment of their supply of blood. In each and all of these cases, the maxim which has many a time been sonorously enunciated in these Schools, 'Entia non sunt multiplicanda præter necessitatem,' would, if listened to, have closed our ears and eyes to at least one-half of the truth. That Bacon would have classed this maxim with his Idola Theatri, I think I am justified in saying, for that in the very next section (Section xlv.) of the Novum Organon to that in which he treats of these delusive notions, I find these words:—'Intellectus humanus ex proprietate suâ facile supponit majorem ordinem et æqualitatem in rebus quam invenit;' and if I am told, as by Mr. Mill (Logic, vol. ii. p. 379, ed. 1846), that Bacon, in the actual practice of investigation, acted as though there were no such thing as Plurality of Causes, I need only answer that herein his practice did not differ from his precepts at all more widely than does the practice of many other writers, of many practising, of many teaching doctors, differ from theirs. I have a satisfaction in quoting the living De Candolle, who enjoys one of the first and best deserved scientific reputations of the day, in repudiation of Maupertuis' famous principle of 'least action.' De Candolle writes thus in his Geographie Botanique, vol. ii. p. 115: 'Nous aimons à croire aux moyens simples, peut-être uniquement à cause du peu de portée de notre esprit.'

What, then, is the legitimate application? where does Nature really bind herself to the observance of a 'Law of Parsimony'? In, as I think, three distinct lines of her operations.

Where an organ can be diverted from one and set to discharge another function, there Nature will spare herself the expense of forming a new organ, and will adapt the old one to a new use. She is prodigal in the variety of her adaptations, she is niggard in the invention of new structures (Milne-Edwards, cit. in Darwin's Origin of Species, p. 232). The complicated arrangement of co-operating muscles whereby the bird's third eyelid is drawn across to moisten and wipe its eyeball without undue pressure on the optic nerve, is manufactured, if so we may express ourselves, out of the *suspensorius* muscle, which in other animals has but the simple function of slinging up the eye. The scarcely less complex and beautiful arrangement of the bird's *levator humeri* is the result of a modification of a subclavius muscle. (See Trans. Linn. Soc. vol. xxvi. 1868.)

Secondly, where, by availing herself of the inorganic forces always at work and available in the circumambient medium, whatever that medium may be, or where, by the employment as in what is called 'Histological Substitution,' a lowly organised or vitalised tissue, such as elastic tissue, she can spare herself the manufacture of such expensive structures as muscle, there Nature adopts a line of practice which we call a Law of Parsimony. Where a suspensory muscle for the eye can be dispensed with altogether, as where there is a more or less closed bony orbit, as in ourselves, and an air-tight cavity formed by it together with the soft tissues lining it, there atmospheric pressure is trusted to to steady the eye in the socket, as it refixes the tooth loosened by inflammation, and holds the head of the femur in the acetabulum. The eye of the burrowing mole, on the other hand, loses its *recti* and *obliqui* before it verges itself into total extinction; but this very *suspensorius* it retains after the wreck of its other property, as its guardian in the undivided undifferentiated temporo-orbital fossa.

Thirdly, where matter that would otherwise be wholly refuse, and to be rejected, can be utilised, there Nature exemplifies this law by her 'utilisation of waste substances.' The transverse colon, with its various contents, aids and ekes out the elastic recoil of the lungs in expiration; and by its near approximation to the stomach, has, as Duverney long ago pointed out, the shock of the ingestion of fresh food propagated directly to it as a warning against sluggishness in the discharge of its own function. The air we use in speech, as Mr. Paget has pointed out, we could not use for breathing.

Many other instances of the 'Law of Parsimony' might be given; but I know not of any which cannot be reduced under one or other of these three heads; I know of none, that is, which can be in any way held to negative the tenability of a law of 'Plurality of Causes.'

The second great principle for the regulation of the understanding of which I wish to speak, is one which, I believe, possesses less currency and notoriety, and is less observed than it deserves. This canon bids us, in considering a complex phænomenon, to be most careful that we omit none of the circumstances which can by any possibility be of the essence of the case. And as the possibilities of Nature are all but infinite—as, for example, the investigator of problems of Geographical Distribution knows well that a 'secret bond' may bind up together, and that inextricably, the interests of organisms removed as far as possible to all appearance from each other in the scale of life; as a fly or a plant may, by its increasing and multiplying, make half a continent uninhabitable or inhabitable by the highest mammals; I apprehend that in biological and medical problems, by the phrase 'all the circumstances which can by any possibility be of the essence of the case,' we mean practically, '*all* the circumstances of the case,' without any qualifying limitation. But we will let Descartes, to whom the enunciation of this rule is usually and, so far as I know, rightly assigned, enunciate it for us in his own words. These run thus (Œuvres, tom. xi. 1826, ed. V. Cousin, p. 23): 'Règle Septième pour la Direction de l'Esprit. Pour compléter la science, il faut que la pensée parcoure d'un mouvement non interrompu et suivi tous les objets qui appartiennent au but qu'elle veut atteindre et qu'ensuite elle les resume dans une énumération méthodique et suffisante.' Some of the very greatest advances which have been made of late in practical diagnosis have been made in the spirit of this recommendation. The application of a chemical test to the urine for information as to the expediency of giving or withholding wine in the case of a sinking life, would have seemed to Swift, could he have had any idea either of such a procedure or of the employment of a sphygmograph for the same object, more absurd than any of the follies he ascribed to the philosophers of Laputa. But as Archbishop Whately, a name to be greatly honoured here, and, indeed, wherever else liberality, and fearlessness, and ability are held in respect, has well pointed out, the absurdities of Laputan aspirations are less wonderful than the actual attainments of modern science. And to these results science has attained, for that her votaries have known that what may seem to Swift, and such as Swift, to be but curious and dilettante, otiose, or even disgusting, may turn out ultimately to be essential elements in problems, the solution of which promote directly and greatly the interests of man, and the glory of Him to whom nothing is common nor unclean. Could anything have seemed at first sight to be more impertinent, more otiosely curious and trifling, than to enquire during an epidemic of cholera what was the nature of the subsoil in the area it was ravaging, to what depth it was porous, and at what level the water was, and had been previously, standing in it? Yet, as I think, Von Pettenkofer has at last fought out and won his battle on these points (see Zeitschrift für

Biologie, Bd. I. Heft. iii. und iv. 1865; Bd. II. Heft i. 1866; Supplemental Heft, 1867, p. 54; Bd. IV. Heft iv. p. 400); and the distinguished President of our 'Public Medicine' section, Mr. Simon, who is as little prone as most men of science to take up over-readily with any new wind of doctrine, has told us (Report of the Privy Council for 1866, pp. 366 and 457) that certain of his carefully observed cases of the distribution of this disease seem to illustrate and find their explanation in Von Pettenkofer's theory. I have pleasure in adding that I see, by papers published by the illustrious Professor of Munich in the Allgemeine Zeitung* for June last, that he has been able to show that, amongst all the other circumstances of the case at Gibraltar and at Malta, there were still to be found, all guessing and objections notwithstanding, the porous subsoil and the retreating ground-water, as factors in the complex constituting an area or arena for cholera. Let us attend to and note always all the circumstances in every complex phenomenon which we have to investigate, but let us not betake ourselves over-hastily to the process of eliminating antecedents, until we be quite sure that they do not enter as factors into its causation.

I may say, in conclusion, that attention to this seventh rule of Descartes might have saved such students of Natural Science as have fallen into materialism from falling into it. The Physiologist, as

* See also Allgemeine Zeitung, December 8, 1868, No. 343. 'Ueber das Verhältniss der "amtlichen Choleraberichte" zum Boden und Grundwasser.' Von Dr. Max V. Pettenkofer.

such, has nothing to do with the data of Psychology which do not admit of being weighed or measured, nor of having their force expressed in inches or ounces. This language, which I long ago employed myself (Nat. Hist. Rev., April 1861; Med. Times and Gazette, March 15, 1862), coincides with an utterance which I am glad to see in Mr. Herbert Spencer's recently issued first number of his new edition of Principles of Psychology. There (part i. chap. i. p. 48) Mr. Spencer says, 'It may safely be affirmed that Physiology, which is an interpretation of the physical processes which go on in organisms in terms known to natural science, ceases to be Physiology when it imports into its interpretations any psychical factor, a factor which no physical research whatever can disclose or identify, or get the remotest glimpse of.' But, I apprehend,* if the Physiologist wishes to become an Anthropologist, he must qualify himself to judge of both sets of factors. There is other science besides Physical Science, there are other data besides quantifiable data. Schleiden, a naturalist well known by name to all of us, compares the Physical Philosopher (Materialismus der neueren deutschen Naturwissenschaft, p. 48), who is not content with ignoring, without also denying the existence of a science based on the consciousness, to a man who, on looking into his purse and finding no gold there, should not be content with saying, 'I find no gold here,' but

* In the "Anatomical Memoirs of John Goodsir," edited by his successor, Professor Turner, and published subsequently to the delivery of this Address, some remarks to the same effect as these may be found. Vol. i. pp. 268 and 292.

should go further and say, 'there is no such thing as gold either here or anywhere else.' It is interesting to note that here in Oxford, till within a few years of the present, we narrowed the application of the word 'Science' in what seems now to be a curiously perverted fashion. For, ignoring all the physical world as entirely as though we had been already disembodied, we used the word to denote and connote only Logic, Metaphysics, and Ethics. By a 'student of science' in my undergraduate days was meant a student of the works of Aristotle, Kant, and Sir William Hamilton. The wheel has since made somewhat of a circle; our nomenclature, like much else belonging to us, is altering itself into a closer correspondence with the usages and needs of the larger world outside; the so-called 'student of science' of the year 1850 is now said to go into the 'School of Philosophy;' and 'the student of science,' as our terminology runs in the year 1868, will be found at the Museum studying the works of Helmholtz, Miller, and Huxley. I do not say this by way of triumph, but rather in that of regret, little disposed or used though I am, and hope always to remain, to regret or deprecate change as such. For there is a philosophy of both subjects, and a science also in both; and I would hope that both the one and the other might still retain a lien on the two words and the two things, nor suffer its rival to establish a claim for sole possession by its own default in exercising a right of usage.

Advocates of the dignity of man are wont to regard, or to profess to regard, with something like horror, doctrines which would hint that either his bodily structures or his mental faculties, his 'more pure and nobler part,' may have attained their perfection in the way of gradational evolution. But it is not clear to me that the horror expressed for these conclusions is much more legitimate than the arguments with which they have been assailed by Prime Ministers and others, in the Sheldonian Theatre close by, and elsewhere within our precincts. For dignity rests upon responsibility—a man is worthy or unworthy, accordingly as he can or cannot make a good answer when called upon by a voice, either from within or without, to account for his conduct or for his character. And just as a man is responsible for the employment of the wealth he possesses to the Government under which he is suffered to enjoy that wealth, no matter in what way he may have become possessed of it, whether by the hereditary transmission of a family estate, or in any other of several feasible and conceivable ways, so is a man responsible for the employment of his corporeal and mental faculties, howsoever he may have been allowed to become seized of them, to that larger and largest Government under which he has his being. I believe, however, that, if men would take as much and the same care in these psychological questions as the physiologist does in his experiments and observations, to overlook none of the conditions and circumstances of the entire complex of phænomena upon which they undertake to decide, they would come to see that above, and often behind, but always beside and beyond the whirl of his emotions and the smoothly fitting and rapidly playing machinery of his ratiocinative and other mental faculties, there stands for each man a single undecomposable something—to wit, himself. This something lives in his consciousness, moves in his will, and knows that for the employment and working of the entire apparatus of feelings and reasoning it is individually and indivisibly responsible. Its utterances have but a still small voice, and the turmoil and noise of its own machinery may, even while working healthily, entirely mask and overwhelm them. But if we withdraw ourselves from time to time out of the smoke and tarnish of the furnace, we can hear plainly enough that, howsoever the engine may have come together, and into its present being, the engineer, at all events, is no result of any processes of accretion and agglomeration. Science, business, and pleasure are but correlatives of the machinery in its different applications and activities; *we* are something besides all this, manifesting ourselves to others in the decisions of our will, and manifesting ourselves to ourselves in our aspirations and consciousness of responsibility.

> 'And e'en as these are well and firmly fixed,
> In dignity of being we ascend.'

I have heard this line or argumentation likened to an attempt to defend Sebastopol by balloons. 'Whilst you are in the clouds, your city will be taken beneath your feet.' But a position, though airy, may yet be impregnable. There are those present who will recollect how the highest placed forts of that town were never taken, but continued to the last to answer shot for shot, and shell for shell, to the Allies. The attacking forces knew not the strength of those north forts till they entered them, but when they entered them, they entered them as friends.

APPLICATION OF PHYSICS

TO

MEDICINE IN MODERN TIMES.

ADDRESS DELIVERED BY THE

REV. PROFESSOR HAUGHTON,

IN THE DIVINITY SCHOOL, AUGUST 6, 1858.

IV.

ON THE RELATION OF FOOD TO WORK, AND ITS BEARING ON MEDICAL PRACTICE IN MODERN TIMES.

MAN, like other animals, is born, grows, comes to maturity, reproduces his like, and dies; passing in his lifetime through a cycle of changes that may be compared to a secular variation, by a metaphor borrowed from the science of Astronomy, while, in his daily life, he passes through a smaller cycle of changes that may be called periodic.

From the time of the publication of Bichat's celebrated Essay on Life and Death, it has been admitted that man and other animals possess a double life, animal and organic, presided over respectively by two distinct, though correlated centres of nervous force; of these, one thinks, moves, and feels; the other merely cooks; receiving the food supplied, changing and elaborating it into elements suitable for the use of the animal life. In the lower forms of animals, the organic life becomes almost coextensive with the whole being of the creature, which simply digests, assimilates, and excretes; but barely feels or moves. In the higher forms of animals, and more especially in man, the animal life dominates over the organic life, which becomes its slave, and exhibits the remarkable phænomena of mechanical force, of geometrical instinct, of animal cunning, and finally, in man himself, produces intellectual work, rising to its highest form in the religious feeling that recognizes its great Creator, and bows in humility before Him. It is a simple matter of fact, and of every day observation, that all these forms of animal work are the result of the reception and assimilation of a few cubic feet of oxygen, a few ounces of water, of starch, of fat, and of flesh.

The general question of the relation of Food to Work would involve a consideration of the possibility of throwing a bridge across the gulf that separates the organic from the animal life, so as to connect the products of nutrition (taken in its widest sense) with the work of every kind accomplished by the animal life, whether mechanical or intellectual. We resemble the spiders of the heather on a summer morning, that float their gossamer threads into the air from the summit of a branch, in the hope that some stray breath of wind may fasten them to a neighbouring tuft, and enable the hungry speculator to extend the range of his rambles and his chance of food. Already a few feeble threads connect the chemistry of our food with the mechanical work done by our muscles; when these shall have been securely fastened, from the higher vantage-ground thus acquired, our little bridge of knowledge may possibly be extended to embrace the phenomena of the geometrical instinct of the bee, or the cunning of the beaver; and our successors may even dare to speculate on the changes that converted a crust of bread, or a bottle of wine, in the brain of Swift, Molière, or Shakespeare, into the conception of the gentle Glumdalclitch, the rascally Sganarelle, or the immortal Falstaff. At present such thoughts would be justly regarded as the dreams of a lunatic, and I must crave your indulgence for having mentioned them. The history of science is, however, filled with such dreams; some never realized; others converted by time into realities so commonplace, that the genius of their originators is habitually forgotten or underrated.

During childhood and youth, the food that we eat is used for the double purpose of building up the tissues of the bones, muscles, brain, and other organs of the body, and of supplying the force necessary for Work done, whether mechanical or intellectual. In adult life, the first use of food almost disappears, for the bones, muscles, brain, and other organs have already reached their full developement, and act simply as the media of communication between the Food received and the Work developed by it.

Let us take, as illustrations, the muscles and brain, regarded as the organs by means of which Mechanical and Intellectual Work is done. These organs resemble the piston, beam, and fly-wheel of the Steam Engine, and, like them, only transmit or store up the force communicated by the steam in one case, and by the products of the food conveyed by the blood in the other case. The mechanical work done by the steam engine must be measured by the loss of heat experienced by the steam in passing from the boiler, through the cylinder, to the condenser; and not by the loss of substance undergone by the several parts of the machinery on which it acts. In like manner, the mechanical or intellectual work done by the food we eat is to be measured, not by the change of substance of the muscles or brain employed as the agents of that work, but simply by the changes in the blood that supplies these organs—that is to say, undergone by the Food used, in its passage through the various tissues of the body, before it is finally discharged in the form of water, carbonic acid, or urea.

The Divine Architect has so framed the animal machine, that moves and thinks, that the same blood, which by its chemical changes produces movement and thought, also repairs the necessary waste of the muscles and brain, by means of which movement and thought are possible; just as if the steam that works an engine were able, without the aid of the engineer, to repair the wear and tear of its friction and waste spontaneously; but no greater mistake is possible in Physiology than to suppose that the products of the changes in the blood, by which Mechanical or Intellectual Work is done, are themselves merely the result of the waste of the organs, whether muscles or brain, on the exercise of which that Work depends.*

* The very skill with which provision is made for the repair of the waste of the organ used as the instrument of Work may mislead the

The ancients, who derived all their knowledge from observation, and not from experiment, were well aware of the double duty imposed upon food in early life—of producing both the secular and the periodic variations of the body; or, in other words, of promoting growth, and of developing work. Their practical knowledge is summed up by Hippocrates in the aphorism—

'Old men bear want of food best; next those that are adults; youths bear it least, more especially children; and, of these, the most lively are the least capable of enduring it.'*

The food consumed in twenty-four hours, including air and water, undergoes a series of changes of a chemical character before leaving the body, in the form of one or other of its excretions. Some of these

observer into supposing that the work itself may be measured by the waste of its instrument. Thus, it has been shown by Mr. A. Macalister, of Dublin, that the heart, which has imposed upon it the necessity of working day and night without ceasing, during life is furnished with double the usual supply of blood through the coronary arteries, which are injected twice for every single beat of the heart. If, indeed, it were possible to assume that all muscles wasted equally for equal quantities of work, and also to measure separately the products of that waste, we might then assume the waste of the organ as the measure of its work. Neither of these assumptions, however, can be admitted, for it can be shown that different muscles act under different conditions, more or less advantageously, so that equal wastes would represent unequal works; and also it is impossible to separate in practice the products of waste of muscles from those of the general changes of the blood.

* *Γέροντες εὐφορώτατα νηστείην φέρουσι, δεύτερον οἱ καθεστηκότες ἥκιστα μειράκια, πάντων δὲ μάλιστα παιδία, τουτέων δὲ αὐτέων ἃ ἂν τύχῃ αὐτὰ ἑωυτῶν προθυμότερα ἐόντα.*—Aph. I. 13.

changes develope force, and others expend force, but the algebraic sum of all the gains and losses of force represents the quantity available for work. This work must be expended as follows:—

1. The Work of growth (*secular*).
2. The Work of maintaining Heat (*periodic*).
3. Mechanical Work (*periodic*).
4. Vital Work (*periodic*).

During childhood and youth the work of growth is positive, for a certain proportion of the food used is employed in building up the tissues of the body instead of being expended in actual work; it is, in fact, 'stored up' in the body, as *vis viva* is stored up by the fly-wheel of machinery, and constitutes a reservoir of force that may be called upon at an emergency requiring sudden expenditure of force, as in case of illness; or to supply the gradual wasting of old age. In adult life, and in old age, the work of growth ceases completely, except so far as is necessary to repair, from day to day, the small wastes of the organs employed in Work; so that nearly the whole of the food employed is expended on the periodic work of the body. Hence we can readily see the reason for the aphorism, which asserts that food is more necessary for the young than for the old, and more required by those of a lively disposition, either of mind or body, than by others.

Hippocratic Doctrine of Innate Heat.

Hippocrates was well aware of the connexion between food and animal heat, although he erroneously regarded the animal heat as an innate property of the body that caused an appetite for food, instead of being itself produced by food; if we transpose his cause and effect, *mutatis mutandis*, all his maxims as to animal heat are true. Thus, he says—

'Growing animals possess most innate heat, hence they require most food; . . . . but the old have least heat, and therefore require the least fuel.'*

'The cavities of the body are naturally warmest in Winter and Spring; . . . . in these seasons therefore most food must be given; and since there is more innate heat, more nourishment is required; as may be seen in youths and athletes.'†

These maxims, when translated into modern language, express the well-known fact, that the chemical changes of food that take place in the body produce animal heat, and that the necessity for food to supply mechanical work is greatest with the young and active, while the necessity for the production of animal heat is greatest in the cold seasons of the year. The direct connexion of food with mechanical work is expressed in the following maxims:—

'There should be no labour when there is hunger'‡—and its converse,

* Τὰ αὐξανόμενα πλεῖστον ἔχει τὸ ἔμφυτον θερμὸν, πλείστης οὖν δεῖται τροφῆς, ... γέρουσι δὲ ὀλίγον τὸ θερμὸν, διὰ τοῦτο ἄρα ὀλίγον ὑπεκκαυμάτων δέονται.—Aph. I. 14.

† Αἱ κοιλίαι χειμῶνος καὶ ἦρος θερμόταται φύσει. . . . . ἐν ταύτῃσιν οὖν τῇσιν ὥρῃσι καὶ τὰ προσάρματα πλείω δοτέον. Καὶ γὰρ τὸ ἔμφυτον θερμὸν πλεῖστον ἔχει, τροφῆς οὖν πλείονος δέονται. σημεῖον αἱ ἡλικίαι καὶ οἱ ἀθληταί.—Aph. I. 15.

‡ Ὅκου λιμὸς, οὐ δεῖ πονέειν.—Aph. II. 16.

'Let labour precede meals.'*

On principles such as those just given, the training of the athletes was conducted; and they were compelled to undergo a regular course, commencing with blood-letting and active purgation,† and consisting of systematic muscular exercise suited to the nature of the contest intended, accompanied by a dietary, of which the chief ingredients consisted of biscuits and pigs' kidneys, washed down by a minimum of water. It is, truly, not much to be wondered at, that those who survived the training were formidable in the boxing-ring or racecourse.

The relation of animal heat to respiration is referred to by Hippocrates, in a remarkable maxim:—

'Those persons have the loudest voices who have most [innate] heat, for they inspire the largest quantities of the cold air; and the product of two great quantities must be itself great.' ‡

Galen believed the heart to be the centre of 'innate heat,' but he was well aware that increase or diminution of respiration caused increase or diminution of heat, and was intimately connected with it. Thus he says:

'Since, therefore, the heart is, as it were, the hearth and fountain of the innate heat, with which the animal is pervaded,' &c. §

* Πόνοι σιτίων ἡγείσθωσαν.—Epid. VI. Sect. iv. 28.

† Ἕλκεα ἐκφύουσιν ἢν ἀκάθαρτος ἐὼν πονήσῃ.—Epid. VI. Sect. v. 32.

‡ Οἷσι πλεῖστον τὸ θερμὸν, μεγαλοφωνότατοι, καὶ γὰρ ψυχρὸς ἀὴρ πλεῖστος. δύο δὲ μεγάλων μεγάλα καὶ τὰ ἔκγονα γίνεται.—Epid. VI. Sect. iv. 22.

§ Ἐπεὶ τοίνυν ἡ καρδία τῆς ἐμφύτου θερμασίας, ᾗ διοικεῖται τὸ ζῶον, οἷον ἑστία τέ τίς ἐστι καὶ πηγὴ κ.τ.λ.—De usu partium, Lib. vi. cap. 7.

'The necessity for respiration is the greatest and most imperious guard of the innate heat.'*

'Those persons in whom the innate heat has been much cooled, breathe but little and slowly.'†

Lavoisier's Theory of Animal Heat.

The doctrine of 'innate' heat, taught by Hippocrates and Galen, ruled in Medicine for 1500 years after Galen's death; until it received its death-blow from the genius of Lavoisier, who demonstrated, in his celebrated memoir read before the French Academy of Sciences in 1783, that the source of animal heat is to be found in the combustion of the carbon of the body by the oxygen of the air received into the lungs by respiration. Lavoisier's experiments were repeated and confirmed in 1822 by Dulong and Despretz; and have formed the starting-point for all modern investigations on the relation of food to work. As already stated, the work done by food in the body may be divided into

1. The Work of Growth.
2. The Work of Animal Heat.
3. Mechanical Work.
4. Vital Work.

Lavoisier arranged his experiments so as to exclude almost all the foregoing kinds of work, except that of

* *Ἡ χρεία τῆς ἀναπνοῆς ἡ μεγίστη μὲν καὶ κυριωτάτη φυλακὴ τῆς ἐμφύτου θερμασίας ἐστίν.*—De diff. Resp. Lib. i. cap. 4.

† *Ὥσπερ καὶ ὅταν μικρὸν εἰσπνέωσι καὶ βραδέως, οἷς ἱκανῶς ἔψυκται τὸ ἔμφυτον θερμόν.*—Ibid. cap. 20.

animal heat. A Guinea-pig was placed under a bell-glass inverted over a surface of mercury, and a current of fresh air was allowed to circulate through the apparatus, being passed at its final exit through tubes containing caustic potash, which arrested the carbonic acid produced by the animal. In this manner it was easy to ascertain the carbonic acid excreted, by the increase in weight of the tubes of caustic potash during the experiment.

Lavoisier found that his Guinea-pig, in ten hours, burned, on the average, 3.333 grms. of carbon; and this quantity of carbon he estimated from other experiments as capable of melting 326.75 grms. of ice at the freezing temperature. The same Guinea-pig was then placed in an ice calorimeter, and left in it for ten hours, during which time the heat of its body was found to have melted 402.27 grms. of ice at the freezing temperature.

If we use, instead of the coefficient of combustion of carbon employed by Lavoisier, that now generally adopted from the experiments of Favre and Silbermann, the quantity of melted ice represented by $3\frac{1}{3}$ grms. of carbon would become 364.78 grms., instead of 326.75 grms. We are, therefore, entitled to say that the heat of combustion of expired carbon determined by Lavoisier is equal to

$$\frac{36478}{402.27} = 90.68 \text{ per cent.}$$

of the animal heat developed, which is regarded as 100 parts.

Two years later, in 1785, Lavoisier laid before the Royal Society of Medicine of Paris an account of further experiments, also conducted on the breathing of Guinea-pigs, by which he showed, that of 100 parts of oxygen absorbed by those animals, 81 only reappeared in the form of carbonic acid, and 19 parts disappeared altogether. Lavoisier considered that these 19 parts of oxygen were employed in the body in the combustion of hydrogen, the product of such combustion being water.

If we use Lavoisier's data just given, and the known atomic weights of carbon, oxygen, and hydrogen, we shall have, for 81 parts of oxygen in the form of carbonic acid, and 19 parts of oxygen in the form of water, the following quantities of carbon and hydrogen consumed by the respiration of his Guinea-pig in the same time:—

$$\text{Carbon} = \frac{6 \times 81}{16} \quad \text{Hydrogen} = \frac{19}{8}.$$

Multiplying these numbers by the Heat Coefficients of Favre and Silbermann, we find—

$$\text{Heat produced by Carbon} = \frac{6 \times 81}{16} \times 8080.$$

$$\text{Heat produced by Hydrogen} = \frac{19}{8} \times 34462.$$

It has been already shown that the heat developed by the combustion of carbon in Lavoisier's experiment amounted to 90.68 per cent. of the heat emitted by the animal; hence the heat produced by the combustion of the hydrogen will amount to

$$90.68 \times \frac{19 \times 34462}{8} \times \frac{16}{6 \times 81 \times 8080} = 30.24.$$

By adding together the heats due to the carbon and hydrogen, we find that Lavoisier's experiments, when fairly interpreted by the data of modern science, give the following results:—

Heat produced by the combustion of carbon and hydrogen	120.92
Animal heat	100.00

Finally, in 1789, Lavoisier published further experiments, by which he showed conclusively that the consumption of oxygen by the body is notably increased by three causes—

1. By a lowering of the external temperature.
2. By the act of digestion.
3. By muscular exercise.

The experiments of Lavoisier were repeated in 1822 by Dulong and Despretz, and their results, when corrected, like those of Lavoisier, by using the modern heat coefficients of carbon and hydrogen, are as follows:—

The mean of Dulong's experiments on 16 animals and birds is 90.6 per cent. of the animal heat given out—the lowest number, 85.5, belonging to a kitten 60 days old; and the highest number, 99.4, belonging to a puppy 50 days old.

M. Despretz obtained an average of 92.3, from 16 mammals and birds; his highest number being 101.8, derived from an old female rabbit; and his lowest number being 84.2, derived from 4 owls.

The foregoing experiments left no doubt remaining in the minds of men of science as to the substantial truth of Lavoisier's doctrine of animal heat; and led immediately to a number of supplementary experiments, amongst the most remarkable of which were those of Regnault and Reiset.

Regnault directed his attention especially to the distribution of the oxygen absorbed by animals, between the carbon and hydrogen of their blood, or tissues, which had been laid down by Lavoisier in the proportion of 81 to 19. He found that the proportion was not a fixed one, but varied with the food in a very instructive manner.

The average of his experiments on 14 animals, including worms, lizards, and insects, as well as birds and mammals, was—

Oxygen combined with carbon . .	81.7
Oxygen combined with hydrogen .	19.3

a result nearly identical with that found by Lavoisier. The highest proportion of oxygen combined with hydrogen occurred in the case of chickens fed on meat, and amounted to 32 per cent.; and the lowest proportion occurred in the case of rabbits fed on bread and oats, and amounted to 1 per cent. only.

Still more recent experiments, made with improved apparatus and methods by Pettenkofer and Voit, in Munich, show, like those of Regnault, that the proportion of the oxygen employed in forming carbonic acid, to the whole oxygen absorbed, varies with the food, ranging in the case of a large dog from 52.4 to 148.2, according as the animal was kept altogether without food, or fed upon a mixed diet of meat and sugar. These investigations have also shown that, under ordinary conditions, it is probable that a dog consumes nearly all the oxygen absorbed in the formation of carbonic acid.

Before leaving the subject of animal heat, it is worth while to estimate its amount in a manner that will bring it into comparison with ordinary mechanical work.

In Lavoisier's experiment with the Guinea-pig, 402.27 grms. of ice were melted in ten hours; from this fact we find, assuming the latent heat of ice at 142° F., and 772 as Joule's coefficient for converting British heat units into foot pounds,

Mechanical work equivalent to the daily animal heat of Lavoisier's Guinea-pig =

$$\frac{402.27 \times 24 \times 142 \times 15.432 \times 772}{7000 \times 10}$$

$$= 233310 \text{ ft. lbs.}$$

As the average weight of a Guinea-pig is 4 lbs., the preceding amount of work, representing animal heat, would be sufficient to raise the weight of the animal through a vertical height of

$$\frac{233310}{4 \times 5280} = 11.05 \text{ miles.}$$

Ranke has shown, by experiments made upon himself, under various conditions of food and fasting, by means of Pettenkofer and Voit's apparatus, that his daily excretion of carbonic acid varied from 660 grms. to 860 grms., showing a mean of 760 grms. His weight was 67 kilos., from which fact, and the assumption that an English mile is 1600 metres, we obtain, employing the constants already given, the height through which the combustion of 760 grms. of carbonic acid would raise the weight of 67 kilos. in 24 hours—

$$= \frac{760 \times 6 \times 8.080 \times 423}{22 \times 67 \times 1600} = 6.609 \text{ miles.}$$

The extreme values of the carbonic acid excreted, viz. 660 grms. and 860 grms., would correspond to the heights of 5.74 miles and 7.48 miles respectively.

Dr. Edward Smith has estimated the daily excretion of carbon from the lungs, in the case of four persons, as follows:—

	Body Weight.	Carbon.
Mr. Moul	173 lbs. . . .	6.735 oz.
Dr. E. Smith . .	196 „ . .	7.85 „
Prof. Frankland .	136 „ . .	5.60 „
Dr. Murie	133 „ . .	6.54 „

In order to convert the preceding data into vertical miles through which the body weight is lifted, we must multiply the ounces of carbon by the following coefficient, and divide the product by the body weight.

$$\text{Coeff.} = \frac{8080 \times 9 \times 772}{16 \times 5 \times 5280} = 132.91$$

log. (coeff.) = 2.1235473.

We thus obtain, for the heights through which the carbon consumed would lift the observers—

Mr. Moul	5.17 miles.
Dr. E. Smith	5.32 ,,
Prof. Frankland	5.47 ,,
Dr. Murie	6.53 ,,

Pettenkofer and Voit succeeded in producing a range of carbonic acid excreted by a large dog, weighing 33.3 kilos., from 289.4 grms. to 840.4 grms.; the minimum corresponding to the 10th day of fasting from solid food, and the maximum corresponding to a diet of 1800 grms. of meat, 350 grms. of fat, and 1410 grms. of water.

It may be easily shown by a calculation similar to the foregoing, that these excretions of carbonic acid correspond to the mechanical works of lifting the weight of the dog through vertical heights of 5.03 miles, and 14.62 miles respectively.

Combining together the preceding results, and expressing them all in the natural units of the weights of the animals lifted through a height, we find—

Work due to Animal Heat.

MAN.

1. Dr. Ranke (fasting)	5.74 miles.
2. Dr. Ranke (well fed)	7.48 ,,
3. Mr. Moul	5.17 ,,
4. Dr. E. Smith	5.32 ,,
5. Prof. Frankland	5.47 ,,
6. Dr. Murie	6.53 ,,
Mean	5.952 miles.

This result agrees very closely with the calculation already made from 760 grms. of carbonic acid, in the case of Dr. Ranke; viz. 6.609 miles.

Work due to Animal Heat.

ANIMALS.

1. Guinea-pig	. . . . . .	11.05 miles.
2. Dog (fasting)	. . . . .	5.03 "
3. Dog (over-fed)	. . . . .	14.62 "
Mean	. . . . . .	10.233 miles.

Source of Muscular Work.

As soon as it was satisfactorily established by Lavoisier and his successors that the natural combustion of carbon and hydrogen in the blood was sufficient, or somewhat more than sufficient, to account for the animal heat, it became a matter of great interest to physiologists to ascertain, if possible, how much of the work developed in the blood by chemical changes is employed in producing animal heat, how much in mechanical work, external and internal, and how much in vital or mental operations.

At the outset of this enquiry, it received a misdirection from the conjecture thrown out by Liebig, that the excretion of nitrogen (in the form of urea) gave necessarily the measure of the wear and tear of the muscular tissues themselves, which are composed of proteinic or nitrogenous compounds. This conjecture led to Liebig's celebrated classification of food into Heat-producing and Flesh-forming foods, which has been unhesitatingly received until lately, in this country, by physiologists and physicians. Before investigating the truth or falsehood of Liebig's theory, it is worth while to state the most recent results obtained as to the muscular work per day of which man is capable.

From numerous observations, of which some were made by myself on the daily labour of hodmen, paviours, navvies, and pedlars, I have obtained the following mean:—

Daily labour of Man = 353·75 ft. tons = 109549 kil. met.

This quantity of work is the exact equivalent of the work done by a man of 150 lbs. weight in climbing through one mile of vertical height, and is, as I have already shown, about one-sixth part of the work expended in producing and maintaining animal heat.

I was led to believe, from investigations made to determine the quantity of urea excreted in various diseases, that a certain minimum quantity, equivalent to 2 grs. per pound of body weight, was excreted quite independently of muscular exertion, and I proved that death was preceded in many chronic diseases by a fall in the urea excreted to 2 grs. per pound. These investigations were made chiefly on patients dying of advanced kidney disease, in which the excretion of albumen had nearly or altogether ceased, and on patients dying of phthisis.

Pettenkofer and Voit found that the excretion of urea in a dog reduced from 33.3 kilos. to 29 kilos. by 10 days' fast became 8.6 grms. And, since

29 kilos. = 63.8 lbs.
8.6 grms. = 132.7 grs.

Excretion of urea = 2.08 grs. per lb. of body weight.

Ranke obtained a precisely similar result from observations made upon himself, after long fasting, continued for several days.

If these views be well founded, it is plain that part only of the urea excreted can be regarded as due to muscular exertion, for 2 grs. per pound. (or 300 grs. for a man weighing 150 lbs.) must be set aside as a constant due to vital work, independent of muscular work altogether. Hence it would follow, supposing the muscular exertion to be measured by the increased excretion of urea produced by it, that the urea will not increase as fast as the muscular exertion, but it ought to increase regularly, although at a slower rate. With a view to settle this important question, I devised the following observations upon myself in the month of July, 1866, which prove conclusively that an increase of muscular exertion, amounting to fourfold, is not accompanied by any corresponding increase in the excretion of nitrogen, in the form of urea.

I had previously ascertained by repeated experiments, extending from 1860 to 1865, that my excretion of urea (under ordinary conditions as to exercise, which never amounted to five miles per day), ranged from

465.09 grs. per day, to
537.47 grs. per day.

501.28 mean.

This quantity of urea I regarded as my natural physiological average, and it was so well established, that I thought I should obtain an important result by comparing it with the average found from several days of unusual muscular exertion. I accordingly walked for five consecutive days in the hilly districts of Wicklow, noting carefully the horizontal distance travelled each day, and the vertical height traversed up and down. The vertical heights were reduced to horizontal distances, on the assumptions (which are well founded) that 20 is the proper coefficient for converting one into the other, and that the work of descent is half the work of ascent.

During the five days of observation the work done, expressed in horizontal miles of walking, was as follows :—

First Day.

		Miles.
Miles walked		11.4
Height ascended . . .	1800 ft. =	10.2
		21.6

Second Day.

		Miles.
Miles walked		12.0
Height ascended . . .	2400 ft.=	13.7
		25.7

Third Day.

Miles walked		11.6
Height ascended . . .	1400 ft.=	8.0
		19.6

Fourth Day.

Miles walked		9.3
Height ascended . . .	1400 ft.=	8.0
		17.3

Fifth Day.

Miles walked		10.4
Height ascended . . .	1600 ft.=	9.1
		19.5

From the preceding statement it follows that the average work done each day was 20.74 miles of horizontal walking—the result of which upon the urea excreted was to be compared with the result already mentioned, as a physiological constant determined under circumstances in which the daily muscular work never exceeded 5 miles of horizontal walking.

In order to determine the urea, I collected each day all the urine passed, and kept one-fifth part of it; and at the close of the fifth day examined the mixture formed from the five days' urine. It was found to contain 501.16 grs. of urea per day—a result practically identical with the physiological quantity previously found by me under totally different conditions, viz. 501.28 grs. I was much surprised at this result, for I had previously believed in the theory laid down by Liebig, which attributed the excretion of urea to the disintegration of muscular tissue.

It might be objected to the preceding reasoning, that the combustion of proteinic compounds represented by 501.28 grs. of urea excreted is actually sufficient to produce the mechanical force necessary to maintain the muscular exertion of walking 20 or 21 miles per day.

1°. The urea excreted bears to the proteine consumed the proportion of 24 to 79; as appears from their chemical compositions—viz.,

Urea . .	$C_2 H_4 N_2 O_2$ . .	60
Proteine .	$C_{36} H_{27} N_4 O_{12}$ . .	395

2°. In 100 parts of proteine there are 53.7 parts of carbon, and 7 parts of hydrogen; the total heat due to the combustion of 1 grm. of proteine is, therefore,

	Heat Units.
0.537 grm. of Carbon	4.3389
0.070 grm. of Hydrogen . . .	2.4123
	6.7512

This number, 6.7512, represents the maximum quantity of heat units* that could be produced by the com-

* Heat unit = 1 kilog. of water raised 1°C.

bustion of one grm. of proteine; but the term depending on hydrogen in it should be reduced to $\frac{5}{9}$ths of its amount, in consequence of the hydrogen already combined with oxygen in the proteine. Hence we find—

Combustion of 1 *grm. of Proteine.*

Carbon	4.3389	heat units.
Hydrogen	1.3402	"
	5.6791	

3°. In 100 parts of urea there are 20 parts of carbon, and $6\frac{2}{3}$ parts of hydrogen; the total heat, therefore, due to the combustion of 1 grm. of urea is

0.20 grm. Carbon	1.6160
0.067 grm. Hydrogen	2.3089
	3.9249

The term depending on hydrogen, in this result, should be reduced to $\frac{1}{2}$, in consequence of the hydrogen already combined with oxygen in the urea.

Hence we find—

Combustion of 1 *grm. of Urea.*

Carbon	1.6160
Hydrogen	1.1544
	2.7704

4°. From the three preceding statements it is easy to see that, for every gramme of proteine consumed, 0.8416 heat units are contained in the urea excreted; so that

The Digestion of 1 *grm. of proteine gives out* 4.8375 *heat units.*

It is easy to see that 501.28 grs. of urea excreted correspond to 1650 grs. of proteine in the food, or to 106.92 grms.; and the total work due to the digestion of this quantity of food may be found by multiplying it by the '*Digestion coefficient*' already found, and by 423, which is Joule's coefficient for converting heat units into kilogrammeters. Hence we have—

Work due to production of 501.28 *grs. of urea*

$$= 106.92 \times 4.8375 \times 423$$
$$= 218786 \text{ k. m.}$$
$$= 704 \text{ ft. tons.}$$

This amount of theoretical work produced by nitrogenous food is double the work actually done during the walking excursion.

The average work was 20.74 miles horizontal per day, which may be considered as the exact equivalent of lifting my weight (knapsack and clothes included = 150 lbs.) through one mile of vertical height. Hence the work actually done by me was

$$\frac{150 \times 5280}{2240} = 354 \text{ ft. tons.}$$

This amount of muscular work accounted for almost exactly half the whole theoretical work supplied by the food that goes to form urea, viz. 704 ft. tons.; but it has been already shown that 2 grs. of urea per pound of body weight is required to maintain the vital work, including circulation and respiration; this would give (since I weighed 128 lbs.) 256 grs. of urea, required for vital work; or almost exactly half of the 501.16 grs. excreted; so that one-half of the available work might be considered as expended on vital work, and the other half as expended on external muscular work. This supposition, however, requires us to believe that the muscles act without loss by friction. This is not admissible, for I have elsewhere endeavoured to show that there is a loss in the force applied by the muscles of various animals, in consequence of the friction of their tendons, amounting, on the average, in man to 35 per cent.*

Hence it may be regarded as certain that the available force represented by 501 grs. of urea is not sufficient to account fully, both for vital work and for the external mechanical work expended by me during the experiments just described.

The foregoing observations and calculations were made in the month of July, 1866, but I did not then publish them, as I found afterwards that I had been anticipated by Dr. Fick and Dr. Wislicenus of Zurich, in a paper published in June in the Philosophical Magazine, on the urea excreted during an ascent of the Faulhorn. Professor Frankland, in a paper published in the same journal, in September, 1866, corrected some erroneous reasoning that found its way into Fick's and Wislicenus' paper, and further supplied, from direct experiment, the *Digestion coefficient* of proteine, which had been obtained by me from calculation.

* The loss of force caused by friction, in the tendons of the foot varies in different animals, as follows:—

1. Lion . .	59	3. Leopard .	45.5	5. Dingo . .	33.
2. Jaguar . .	49.2	4. Jackall . .	49.2	6. Wolf . .	348

The actual value of this important constant was found by him to be—

Actual value of digestion coefficient of Proteine	4.3155
Calculated value	4.8375

My only object in now publishing an account of the independent experiment and calculation made by myself, is to confirm the certainty of the important fact first proved experimentally by Fick and Wislicenus, that the force due to the urea excreted in a given time is not sufficient to provide the actual work that may be done by the muscles in the same time.

Liebig and his followers, misled by a preconception of the simplicity of nature, assigned to nitrogenous food the duty of providing the force necessary for the production of muscular work, by supplying the waste of muscular tissue; while they supposed the farinaceous and fatty foods to provide the amount of animal heat required by the body.

The opponents of Liebig have fallen into the opposite error, and deny that nitrogenous food contributes any portion of the force employed in muscular work.

The truth, as is usual, lies between the two extreme hypotheses, and we are now compelled to admit that a given development of force, expressed in animal heat, muscular work, and mental exertion, may be the effect of several, perhaps many, supposable supplies of digested food, farinaceous, saccharine, fatty, and albuminous.

Just as a given algebraical function may be equated to a given constant, by the use of a certain definite number of values of its variable quantity, so may a given effect of work in the animal body be produced by certain definite, though very different, combinations of various kinds of food, the digestion of which follows each its own law, and developes its own amount of force. The number of roots in our equation of life increases the difficulty of solving it, but by no means permits the acceptance of the lazy assumption that it is altogether insoluble, or reduces a sagacious guess to the level of the prophecy of a quack.

Lavoisier supposed in his earlier investigations that animal heat was developed by the combustion of carbon and hydrogen in the lungs; just as in earlier times it was supposed to be produced spontaneously in the heart, which was imagined to be so hot as even to burn the hand that should imprudently venture to touch it.

In like manner, Liebig and his followers supposed the muscular work to be developed in the substance itself of the muscles that were its instruments.

Both of these doctrines are now justly repudiated by physiologists, and the view, proposed in 1845, by Dr. Mayer of Heilbronn, and recently developed with much ability by Mr. C. W. Heaton, of Charing Cross Hospital, in the Philosophical Magazine for May, 1867, that the blood itself is the seat of all the chemical changes that develope force in the body, has gained favour among physiological chemists, and also met with acceptance among practical clinical observers.

Thus the human mind revolves in cycles, and the physicians of the nineteenth century are preparing to sit at the feet of Moses, and learn that the blood of an animal really constitutes its life; while South African theologians are disposed to reject his authority, because he happened to confound a Rodent with a Ruminant.*

Whatever be the kind of food employed, its effect in the production of force must be ultimately measured by the quantities of carbonic acid and water produced by its combustion, and there is no more convenient measure of the production both of carbonic acid and water than urea, so far as it goes. I shall prove shortly that every four grains of urea excreted represent five tons lifted through one foot; and I have shown by the preceding investigation that the work represented by urea is not sufficient to account for vital and external work, much less for animal heat. The investigations of Dr. Edward Smith, on the excretion of carbonic acid, enable us to show that the carbonic acid alone is sufficient to account for both vital and external work, and also for the production of animal heat. This may be proved as follows:—

Dr. Smith has given results, from which may be deduced the quantities of carbonic acid excreted per minute, during the four following conditions:—

1. Lying in the horizontal position, and nearly asleep.
2. Fasting, and in sitting posture.
3. Walking at two miles per hour.
4. Walking at three miles per hour.
5. Working on the Treadmill, ascending at the rate of 28.65 feet per minute.

* No reasonable person can fail to perceive the ignorance of the great Lawgiver, who will apply to him the test first proposed by Swift for Homer; Moses, like the author of the Iliad, was profoundly unacquainted with the discipline and doctrines of the Church of England.

	Carbonic Acid per min.
1. Sleep and rest	5.522 grs.
2. Sitting	7.440 „
3. Walking at two miles per hour	18.100 „
4. Walking at three miles per hour	25.830 „
5. Treadmill	44.973 „

The foregoing quantities of carbonic acid per minute may be converted into vertical miles per hour for the body weight, by multiplying them by the following coefficient :*—

$$\frac{60 \times 6 \times 8080 \times 9 \times 772}{22 \times 196 \times 5280 \times 5 \times 7000} = 0.025263$$

$$\log. = \bar{2}.40420.$$

Performing this calculation we find—

	Carbonic Acid.	Body weight lifted through miles.
1.	5.522 grs.	0.1400 mile.
2.	7.440 „	0.1887 „
3.	18.100 „	0.4591 „
4.	25.830 „	0.6551 „
5.	44.973 „	1.1406 „

It is easy to calculate that the external work done in the cases 3, 4, 5, was as follows :—

	External Work.
No. 3. Walking two miles per hour .	0.1000 mile.
No. 4. Walking three miles per hour	0.1500 „
No. 5. Treadmill	0.3256 „

* Dr. Edward Smith's weight was 196 lbs.

Subtracting these amounts of work from the applied work, due to the production of carbonic acid, we find, as the quantities left for Vital Work, including circulation and respiration, and for the production of Animal Heat, per hour:

	Vital Work and Animal Heat.
No. 3.	0.3591 mile.
No. 4.	0.5051 „
No. 5.	0.8150 „

As I have already shown the work due to animal heat per day to be six miles; it follows that the work of animal heat per hour is 0.2500 mile.

Deducting this amount from the foregoing, we find for the Vital Work done, under the three different conditions—

	Vital Work.
No. 3. Walking at two miles per hour	0.1091 mile.
No. 4. Walking at three miles per hour	0.2551 „
No. 5. Treadmill work	0.5650 „

This result proves, in a striking manner, the great disadvantage under which an increased amount of muscular work is done, in a given time; and it is quite in accordance with other results obtained by me from totally different experiments.

No two classes of animals can well differ more from each other than the Cats and Ruminants, one of which is intended by nature to eat the other. They differ in all respects as to food, the Cats requiring a supply of fresh meat and blood for their health, and the Ruminants being exclusively vegetable feeders; yet in both classes we find a great developement of muscular power, and of rapid action of muscles, qualities alike necessary to the pursuer and to the pursued. There can be no doubt that muscular work is developed in the Cats from the combustion of flesh, and in the Ruminants, mainly, if not exclusively, from farinaceous food. It is, however, worthy of remark, that the muscular qualities developed by the two kinds of food, differ considerably from each other. The hunted deer will outrun the leopard in a fair and open chase, because the work supplied to its muscles by the vegetable food is capable of being given out continuously for a long period of time; but in a sudden rush at a near distance, the leopard will infallibly overtake the deer, because its flesh food stores up in the blood a reserve of force capable of being given out instantaneously in the form of exceedingly rapid muscular action.

In conformity with this principle, we find among ourselves an instinctive preference given to farinaceous and fatty foods, or to nitrogenous foods, according as our occupations require a steady, long-continued, slow labour, or the exercise of sudden bursts of muscular labour continued for short periods. Thus Chamois hunters setting out for several days' chase provide themselves with bacon fat and sugar; the Lancashire labourers use flour and fat, in the form of apple dumplings; while the Red Indian of North America almost transforms himself into a carnivore, by the exclusive use of flesh food; he sleeps as long, and can fast as long as the Puma or Jaguar, and possesses stored up in his blood a reserve of force which enables him, like a cat, to hold his muscles for hours in a rigid posture, or to spring upon his prey, like a leopard leaping from a tree upon the back of an antelope.

If the preceding view of the muscular qualities developed by the two kinds of food be correct, important inferences suggest themselves as to the food that should be employed in relation to several kinds of work. Of these inferences, I shall select two examples:—

1. The nurses of one of our Dublin Hospitals were formerly fed chiefly upon flesh food and beer, a diet that seemed well suited to their work in ordinary times, which was occasionally severe, but relieved by frequent intervals of complete rest. Upon the occasion of an epidemic of cholera, when the hospital duties of the nurses became more constant, although on the whole not more laborious, they voluntarily asked for bacon fat and milk, as a change of diet from the flesh meat and beer; this change was effected on two days in each week with the best results as to the health of the nurses, and as to their power of discharging the new kind of labour imposed upon them.

2. I have been informed, on competent authority, that the health of the Cornish miners breaks down ultimately, from failure of the action of the heart and its consequences, and not from the affection of the lung's called 'miner's phthisis.' The labour of the miner is peculiar, and his food appears to me badly suited to meet its requirements. At the close of a

hard day's toil, the weary miner has to climb by vertical ladders through a height of 100 to 200 fathoms before he can reach his cottage, where he naturally looks for his food and sleep. This climbing of the ladders is performed hastily, almost as a gymnastic feat, and throws a heavy strain (amounting from one-eighth to one-quarter of the whole day's work) upon the muscles of the tired miner, during the half hour or hour that concludes his daily toil. A flesh-fed man (as a Red Indian) would run up the ladders like a cat, using the stores of force already in reserve in his blood; but the Cornish miner, who is fed chiefly upon dough and fat, finds himself greatly distressed by the climbing of the ladders—more so indeed than by the slower labour of quarrying in the mine. His heart, over-stimulated by the rapid exertion of muscular work, beats more and more quickly in its efforts to oxidate the blood in the lungs, and so supply the force required. Local congestion of the lung itself frequently follows, and lays the foundation for the affection, so graphically, though sadly, described by the miner at 40 years of age, who tells you that 'his other works are very good, but that he is beginning to leak in the valves.'

Were I a Cornish miner, and able to afford the luxury, I should train myself for the 'ladder feat' by dining on half a pound of rare beefsteak and a glass of ale, from one to two hours before commencing the ascent.

The excretion of nitrogen by the Cats and Ruminants is very different, as might be expected from their food. I have ascertained that the urea discharged by a Bengal Tiger and a Sheep, daily, is as follows:—

Bengal Tiger . . .	4375 grs. of urea.
Sheep	256 „

It is worthy of remark, and serves to throw light on the meaning of the excretion of nitrogen from the body, that causes but slightly connected with muscular exertion in the Ruminants increase amazingly the excretion of urea. Thus I have found the following excretion of urea from a Ram during the rutting season:

Ram (rutting season) . . . . 1493 grs. of urea.

This amounts to a *sixfold* increase of urea, which cannot possibly be accounted for by the food consumed at the time, but requires us to assume a certain storing up of force, represented by nitrogenous compounds, which has been going on for a considerable period previous to the rutting season. A similar and equally remarkable storing up of phosphates and carbonates takes place, previous to the rutting season, in the Ruminants that shed their horns, which in the *Cervus Megaceros* often weigh 90 lbs.

These remarkable phenomena remind us of the maxim of the wise Hippocrates, who recommends moderation in the use of the gifts of the Golden Venus, as well as in those of Ceres and Bacchus—

πόνοι, σιτία, ποτὰ, ὕπνος, ἀφροδίσια μέτρια, with which may be compared its converse in the Latin proverb—

Sine Cerere et Baccho, friget Venus:

or, as the old proverb says;

When the wolf comes in at the door, love flies
out at the window.

Application of Theory to Diseased Conditions of Body.

The relation of food to work, complicated enough in health, becomes more so in disease, and the problem to be solved by rational theory becomes still more difficult. I cannot attempt even to sketch an outline of this part of my subject considered in general, but shall content myself with asking your attention to three remarkable examples of disease which illustrate the principles I have attempted to lay down.

These diseases are—

A. Typhus Fever.
B. Cholera Asiatica.
C. Diabetes mellitus.

A. *Typhus Fever.*—In Typhus fever a prominent symptom is the remarkable elevation of temperature, accompanied by an increased excretion of urea and carbonic acid, by the kidneys and lungs, indicating (as no food is taken) an increased morbid metamorphosis of the blood and tissues. The temperature commonly rises to 104° F., representing an increase of upwards of 5° F. above the normal temperature.

If we knew the cause of this increase of temperature, or rather of the increased metamorphosis of which it is the sign, we should know the cause of *Typhus* fever, and learn to combat the disease on rational grounds. At present the cause is unknown, and therefore the physician is forced to treat the symptoms as they appear, instead of attacking the cause of the disease. Let us examine for a moment the terrible significance of the symptoms.

Your patient lies for nine or ten days, supine, fasting, subdelirious; the picture of weakness and helplessness; and yet this unhappy sufferer actually performs, day by day, an amount of work that might well be envied by the strongest labourer in our land.

The natural temperature of the interior of the body is 100° F., while the temperature of the corresponding parts in Typhus fever is at least 105° F. This seems at first sight a small increase—only 5 per cent. of the whole; but it is in reality $2\frac{1}{2}$ times as great as it appears, and actually amounts to $12\frac{1}{2}$ per cent., or one-eighth part of the total animal heat. For the total quantity of heat given out by the heated body is proportional (from Newton's law of cooling) to the elevation of its temperature above the temperature of equilibrium, towards which it tends. If we suppose this equilibrium temperature to be 60° F., then the quantities of animal heat given out in Typhus fever and in health will be in proportion of 45 to 40, showing that the animal heat of Typhus exceeds that of health by one-eighth of its amount.

We have already seen that the work due to Animal

Heat would lift the body through a vertical height of six miles per day; and it thus appears that an additional amount of work, equivalent to the body lifted through nearly one mile per day, is spent in maintaining its temperature at Fever Heat.

If you could place your fever patient at the bottom of a mine, twice the depth of the deepest mine in the Duchy of Cornwall, and compel the wretched sufferer to climb its ladders into open air, you would subject him to less torture, from muscular exertion, than that which he undergoes at the hand of nature, as he lies before you, helpless, tossing, and delirious, on his fever couch.

The treatment of this formidable disease in former times consisted of purging, vomiting, and bleeding the patient, with the view of eliminating an imaginary poison, and so helping nature to terminate the disease.*

In modern times, thank God, the physician either does not interfere at all, or adopts the rational process of retarding the disintegration of the tissues consumed to supply the fever heat, by furnishing in their stead, fuel, in the form of wine and beef tea, sufficient to maintain the increase of temperature imperiously required.† This practice may be justly considered

* *Νούσων φύσιες ἰητροί.*—EPID. vi. Sect. v. 1.

† It is not intended by this to assert that a high temperature, 104° to 108° F., must be maintained, in order that the disease may terminate favourably, for the very contrary is the fact. The blood, in Typhus, as in other pyrexies, is a fluid possessed of greater oxidising power than it has in health; in consequence of this, an increased metamorphose of tissues takes place, accompanied of course by an elevation of temperature, which measures precisely the oxidising power

rational, because the condition of the circulation admits of its application, and it is considered good, because it has been rewarded with success, in the hands of the skilful clinical physician. In concluding this sketch of the prominent symptom of Typhus fever, and as an illustration of the eagerness with which every possible combustible in the body is made use of, I may mention, on the high authority of Dr. Stokes, of Dublin, that the very urea excreted by the kidneys is not permitted to leave the body without first paying its tax to fever, by being burned into carbonate of ammonia, thus rendering the urine of an advanced case of bad Typhus fever eminently alkaline.

of the blood, and the risk to life in Typhus is directly proportional to the rise in temperature. The indications of the sphygmograph are similar to those of the thermometer, a '*full dicrotic*' pulse corresponding to a temperature of 103° F., and the pulse of '*death agony*,' with the heart's first sound gone, corresponding to a temperature of 109° F. There is no case on record of recovery from a condition marked by such a pulse and temperature.

The effects of alcohol, administered in fever, when the temperature does not exceed 105° F., are twofold—immediate and secondary. The immediate effect is to supply a hydrocarbon to the blood, which is decomposed by it in preference to the body tissues. The secondary effect of alcohol is to change the blood itself, which thus loses its oxidising qualities; in consequence of which the temperature falls, the hyperdicrotic character of the pulse disappears, and the destructive metamorphose of the tissues becomes lessened. The statement here given of the effects of alcohol given in Typhus, to the exact amount required by the condition of the blood, in narcotic doses, is borne out by clinical observation, and is independent of any theory as to the cause of Typhus.

It is not at all improbable that the theory of contagious disease, that each such disease owes its existence to a special living organism, and not to an organic poison, may ultimately prove to be correct.

B. *Asiatic Cholera.*—This remarkable disease presents, as every one knows, three distinct stages, viz.,

1. The premonitory stage of diarrhœa.
2. The stage of collapse.
3. The stage of consecutive fever.

The stage of collapse exhibits the following symptoms:—vomiting or purging; muscular cramps; suppression of bile and urine; lowering of body temperature to 95° F.; extreme prostration of strength; extremities pulseless; and face Hippocratic.

When death occurs during collapse, the following symptoms are usually found, on careful examination of the corpse. The temperature rises to 103° F.; the muscles give out their characteristic susurrus CCC, and exhibit spontaneous movements; the whole train of symptoms producing the effect of a ghastly attempt at resurrection.*

In this disease we have phænómena respecting animal heat, the very reverse of those found in Typhus fever; the body performing one vertical mile short of its daily work, instead of one mile in excess. The prostration of strength resulting from this deficient combustion is so great, that death is often caused by bringing the patient to hospital in a cab instead of upon a stretcher, by his walking up a dozen steps into his ward, and sometimes even fatal results have followed a sudden effort to sit up in bed to vomit.

* It is startling, on making a post-mortem examination of a cholera patient alone, and by candle-light, to witness, on the first free incision of the scalpel, the hand of the corpse raised slowly from its side and placed quietly across its breast.

The rise of temperature after death, and the continuance of muscular susurrus and motion, tend to prove that the impeded circulation which is the prominent symptom in Cholera collapse, is due to constriction (probably vasomotor nervous) of the capillaries—in consequence of which the muscles are deprived of their supply of freshly oxidised blood, the result of which is necessarily contraction, and cramp, which produces the excessive agony that characterises this disease.

All authorities on Cholera, whether their object be to 'impede' or to 'assist' Nature, are agreed that medicines, whether astringent or purgative, are not only useless, but dangerous in the stage of collapse.

It is useless to give alcoholic fuel to restore the loss of animal heat, for there is no circulation to cause the oxidation of the hydrocarbons.

It is equally useless and more dangerous to give opium to check the remaining purging that exists; for if vomiting have ceased, your acetate of lead and opium pills lie, as if in the stomach of a corpse, and at the termination of collapse, your patient enters upon the consecutive fever, with perhaps a dozen grains of opium in his stomach, placed there like an explosive shell by your ill-timed zeal, and rapidly passes into a comatose condition, from which he never for a moment rallies. His death is always accredited by the Registrar to cholera morbus, and not to opium.

Purgatives and emetics* in Cholera collapse effect the

* When mustard is used, its conservative effects as a stimulant sometimes counteract its destructive effects as an emetic.

same object as opium, but with greater rapidity. In the stage of blue collapse the chances of life and death are almost exactly equal, and the slightest additional loss of force turns the wavering beam on the side of death. The effects of a brisk purgative or emetic (if they act) upon a patient, unable to climb a dozen steps, or sit up for a quarter of an hour, without fatal syncope, may be easily imagined; and the use of them cannot be justified by any arguments borrowed from right reason.

A remarkable though transient improvement takes place in Cholera collapse by the injection of warm water (brought to the specific gravity of serum by the addition of mineral salts) into the veins or bowels: the patient loses the cramps, feels that he is about to recover, speaks to his friends, and often transacts whatever business is necessary; but speedily falls back into collapse. The improvement in his condition is altogether due to the temperature of the fluid injected, which supplies for a brief period the deficient animal heat, permits a partial oxidation of the blood, restores the capillary circulation in the muscles, and so destroys their cramp; and by supplying the deficient work required, removes for the moment the fatal prostration of strength. Any one who has witnessed the remarkable effects of warm liquids thus injected in Cholera collapse must feel that recovery would be certain, if the improvement could by any possibility be made permanent.

Our hopes for the future, as to the treatment of Cholera, lie, as I believe, in the direction of supplying to the body, directly, its lost animal heat. I have witnessed the happiest results from an injection of warm salt water into the bowels, assisted by hand friction of the surface with turpentine and chloroform, and the application of bags of hot salt along the spine: in cases treated in this manner, we may expect to witness cessation of muscular cramp, restoration of perspiration to the skin, with increase of capillary circulation, and finally, to reward our efforts, a return of the excretions of urine and bile; when these reappear, all vomiting and purging cease, and our patient is almost cured.

After recovery, the contrast between the Cholera and Fever patient is as great as it was during sickness. The Fever patient has been overworked for 9 or 15 days without a suitable supply of food, and when convalescent, experiences a complete exhaustion of strength that lasts for many weeks. The Cholera patient, on the other hand, has been prevented from working, by constriction of the capillary vessels, caused by the absorption of the Cholera poison,* and feels, on recovery, much like a man that has been half drowned, while the Fever patient resembles a man that has been half starved: the one is able to return to his work in the course of a few days, the other, only after the lapse of as many weeks.

There are two popular superstitions prevalent among medical men respecting Nature, which yearly slaughter hecatombs of victims; viz., that Nature is simple in

* Whatever this may be, its period of incubation is 49 hours; that of strychnine is 22 minutes.

her operations, and beneficent in her intentions; she is often both simple and beneficent, but at other times she is unquestionably both complex and malevolent.

An Egyptian fable informs us, that the votaries of the Goddess Nature were divided in opinion as to whether she was transcendently beautiful, or hideously ugly; and that, in order to keep up this difference of opinion which suits her purposes, she always wears a thick veil over her face.

> 'For, with a veil that wimpled everywhere,
> Her head and face were hid, that mote to none appear;
> That some do say, was so by skill devised,
> To hide the terror of her uncouth hue
> From mortal eyes that should be sore agrised;
> For that her face did like a Lion show,
> That eye of wight could not endure to view;
> But others tell that it so beauteous was,
> And round about such beams of splendour threw,
> That it the sun a thousand times did pass,
> Nor could be seen, but like an image in a glass.'

Before trusting Nature in the matter of Cholera, and proceeding to help her, it would be well to enquire whether she intends to cure the patient by her evacuations, or to put him into his coffin. For myself, I greatly mistrust her, and would wish to ask, previous to assisting her, whether she is really my Mother, or only my Stepmother. Our experience in Dublin has shown, that no more effectual mode of shortening life could be devised in Cholera than the 'eliminant' treatment; and it was accordingly abandoned as soon as tried in that city.

It is much to be regretted, that an authority so deservedly held in high repute as that of Sir Thomas

Watson, can be now quoted in favour of the treatment of Cholera, by the maxim, *similia similibus curantur.* So far as Dr. Watson has informed us, his change of opinion rests upon the statements of others, and not upon his own experience. He has suddenly become an advocate of the castor oil, rhubarb, calomel, and eliminant treatment of Cholera, and writes as follows:—

'When I last spoke on this subject in these lectures, I stated that the few recoveries which I had witnessed had all taken place under large and repeated doses of calomel, but I could not venture to affirm that the calomel cured them. At present, I am much disposed to believe that by its cleansing action, the calomel may have helped the recovery; and after all that I have since seen, heard, read, and thought upon the matter, I must confess that in the event of my having again to deal with the disorder, I should feel bound to adopt, in its generality, the evacuant theory and practice.'

Sir Thomas Watson omits to add, that the cases here referred to were only six in number, of whom three died, and three recovered; which is exactly what might have been expected if he had not interfered at all.

Cholera from Bengal visits these islands, at intervals of about 17 years, and it is much to be feared, that on its next outbreak hundreds of patients will be sacrificed, in obedience to the dogma that asserts it to be our duty to assist Nature.

C. *Diabetes mellitus.*—This disease furnishes us with one of our best proofs that all the chemical changes, by means of which work is produced, take place in the blood and not in the tissues of the body; and, at the same time, an examination of its phænomena explains satisfactorily the regimen and diet which has been found, by experience, most suitable to the diabetic patient. I shall illustrate the disease by a case which was placed under my control, by Dr. Stokes, some years ago.

A young man (æt. 20) named Murphy, suffered from fever (Enteric?) in November, 1859, and on recovering, became diabetic; he was admitted into the Meath Hospital, in October, 1860, where he remained, under my observation, until his death on the 12th January, 1861.

He was allowed, for nine weeks, to eat as much as he liked of certain kinds of food, which were varied, week by week, to suit his wants, my object being to obtain, if possible, the natural constants of the disease, undisturbed by external interference; the only medicine used by Dr. Stokes's order being opium, to produce sleep, and a little kreasote occasionally, to promote digestion. As the details of this experiment have been fully published, I shall confine myself to the final results. His food and excretions were analysed from week to week, so as to determine the total quantities of sugar-forming and urea-producing food, as well as the sugar and urea actually excreted.

During six of the nine weeks, the sugar excreted was in excess of the sugar ingested; and the means of the whole nine weeks' daily excretion and ingestion of sugar were—

Sugar excreted	. . . . . .	9773 grs.
Sugar ingested	. . . . . .	9321 "
Diff.	. . . . .	452 grs.

During two of the nine weeks of observation the urea excreted was in excess of the urea ingested; and the means of the whole nine weeks' daily excretion and ingestion of urea were—

Urea excreted	. . . . . .	1182 grs.
Urea ingested	. . . . . .	1349 "

The foregoing facts illustrate strikingly one of the prominent symptoms of Diabetes, viz., the canine appetite; the quantity, both of sugar-producing and urea-forming food consumed is more than double what is necessary to maintain a vigorous labourer in perfect health. An examination of the excretions explains the other prominent symptom of Diabetes; viz., the complete prostration of strength in the patient, notwithstanding the great amount of food consumed.

In a state of health, food produces three excretions only, viz., urea, carbonic acid, and water; in Diabetes, the farinaceous foods appear in the excretions as sugar, and not as carbonic acid and water; and the work necessary to maintain animal heat must be provided altogether at the expense of flesh food, which is the very form of food least fitted to maintain it.

The Diabetic patient resembles a racing steamboat on the Mississippi, whose supply of coals is exhausted, and whose cargo furnishes nothing better than lean pork hams, to throw into the furnace, to maintain the race. It cannot be wondered at that our poor patient, under such disadvantageous conditions, fails to keep in the front.

Let us compare together the minimum of work necessary to keep Owen Murphy alive, with the work actually supplied to him by the food digested.

1. I have already stated that Dr. Ranke found 660 grms. of carbonic acid excreted daily, in the extreme fasting condition, when he weighed 67 kilos. Now, since

$$\begin{aligned} 660 \text{ grms.} &= 10185.35 \text{ grs.} \\ 67 \text{ kilos.} &= 147.71 \text{ lbs.}, \end{aligned}$$

we find 69 grs. per lb. of body weight, as the minimum excretion of carbonic acid, consistent with continued life.

This quantity of carbonic acid represents a work generated by its production that would lift its corresponding pound of body weight through a height of

$$69 \times \frac{6}{22} \times 8080 \times \frac{9}{5} \times \frac{772}{7000 \times 5280} = 5.716 \text{ miles.}$$

Under ordinary conditions, the greater part of this carbonic acid and work is produced by the digestion of farinaceous food; but since, as we have seen, the farinaceous food is excreted as sugar in the Diabetic patient, and therefore does no work at all, the whole of the foregoing work must be done by the digestion of other kinds of food.

I have already shown that it follows from Lavoisier's experiments (confirmed in a remarkable manner by those of Regnault), that the work done by the combustion of carbon in the body is to the work done by the combustion of hydrogen in the proportion of 9068 to 3024, almost exactly 3 to 1; hence we have the work done, by Owen Murphy, as a minimum in health—

Due to carbon	. . . .	5.716 miles.
Due to hydrogen	. . .	1.905 „
		7.621 miles.

This result is somewhat in excess of the truth, for the same reason that the calculated *digestion coefficient* of proteine is in excess of that found by Frankland from experiment; for the combustion coefficients of carbon and hydrogen, in organic compounds, are slightly less than when free. If we are permitted to reduce 7.621 miles in the same proportion as in the digestion of proteine, viz., 48 to 43, we shall find—

Owen Murphy—minimum of work consists of body weight lifted through . . . 6.83 miles.

Let us now compare this minimum with the work actually performed by him when suffering from Diabetes, by the digestion of flesh food and production of urea.

2. I have already shown that the work produced by the formation of 501.28 grs. of urea is 704 ft. tons, by calculation from the composition of proteine and urea; this result should be reduced in the proportion of 48375 to 43155, in order to obtain the work given by Professor Frankland's experiments. Making this reduction, we find that 500 grs. of urea correspond to

626.3 ft. tons of work, or 100 grs. urea to 125.26 ft. tons; or, in other words—

Every four grains of urea excreted correspond to five tons lifted through one foot.

Owen Murphy excreted, on an average, 1182 grs. of urea, daily, during nine weeks—which, by the foregoing rule, are equal to

$$1475 \text{ ft. tons} = \text{Murphy} \times x;$$

where x represents in miles the height through which the patient could be lifted by the work done per day; and is equal to

$$x = \frac{1475 \times 2240}{93.56 \times 5280} = 6.69 \text{ miles.}$$

This result is almost exactly equal to that already found as the minimum consistent with continued life, and explains in the most satisfactory manner the complete prostration of the patient, notwithstanding the consumption and digestion of more than double the usual quantity of flesh food.

In corroboration of the foregoing conclusion, I may mention that Murphy's temperature was found to be constantly 2° F. below that of other patients (chronic) placed in the same ward, and, in other respects, under similar conditions.

His unfavourable symptoms (so long as his powers of digestion were not impaired) were invariably alleviated by the free use of flesh food and fat, the latter being, instinctively, preferred by him; so much so, that during the delirium that preceded his death for 24 hours, he raved incessantly about 'fat, roasted fat, which the angels of heaven were preparing for him.'

I have studied many other cases of *Diabetes mellitus*, and found similar results in all; but I feel it to be unnecessary to describe them, as one well-ascertained train of phænomena, carefully observed and recorded, is quite sufficient to establish the order of Nature.

Conclusion.

I have now, Mr. President and gentlemen, to apologise for the length of time during which I have spoken, and to thank you for the patience with which you have listened to me. I am well aware how much I am indebted to your kindness, for I laboured under two serious disadvantages in addressing you: in the first place, I had undertaken a task beyond my strength; and again, my address is made, shortly after you had, like myself, been charmed and instructed by the luminous, learned, and eloquent oration of Professor Rolleston. I felt confident, however, that I possessed one advantage that he did not; I was a stranger in Oxford, and believed that my faults in matter and style would be leniently criticised; in this expectation, I am happy to say I am not disappointed; and again I thank you for your kindness. Two other advantages I share with him, which have contributed to his address as much as to my own—a profound respect and reverence for all honest labourers in search of truth, whether they have preceded us by 20 years or by 2000 years; and an unwavering confidence and faith in the future that lies before the Science of Medicine. We traverse a sea, mapped with imperfect charts, but assured of a safe guide in our compass and stars; but we cannot afford to neglect a single rock or shoal, buoyed for us by the skill and care of those that have preceded us. Let us follow their example, and mark with conscientious care, for our successors, the dangers we ourselves discover and escape.

Assembled, as we are, within the halls of the University of Oxford, the centre and heart of all that is intellectual and religious in the life of England; an University that borrows its accurate Logic, as well as its refined Ethics, from the lips of Aristotle; that reverences Euclid as the fountain and source of its elegant Geometry; and sits at the feet of Homer, Pindar, and Æschylus, to learn its poetry; we need not fear that Hippocrates and Galen will ever want admirers and students; but the Oxford of to-day has taught us, what many did not anticipate, that she is equally ready and skilful, as she has proved herself to be in cultivating Literature, to devote her vast intellectual energies to the encouragement and development of the Natural Sciences, based upon the solid and only permanent foundation of Mathematical research. The efforts made within the last few years by Oxford to encourage within her walls the Mathematical and Natural Sciences, have won for her the respect, and warmed towards her the hearts of all that search for truth in the study of Nature. Our brothers in Oxford, like the Athenians at Syracuse, have gone on board the fleet, while we watch them from the shore, sympathizing in the sea fight; as they win, we shout; when they fail, we weep.

Long may the union of the far distant, but never-to-be-forgotten Past, with the living Present, that now exists in Oxford, continue! No science, no profession, can benefit so much by it as that of Medicine.

CLINICAL OBSERVATION

IN RELATION TO

MEDICINE IN MODERN TIMES,

ADDRESS DELIVERED BY

DR. GULL,

IN THE DIVINITY SCHOOL, AUGUST 7, 1868.

V.

RELATIONS OF MEDICINE AND CLINICAL OBSERVATION.

I DEEPLY feel the honour and responsibility which you have put upon me, by placing me here to-day as your exponent of the present position of Clinical Medicine. My task is difficult from the distinguished character of my audience, and from the imperfections of our knowledge in the subject of which I have to speak. I feel, indeed, as one about to undergo the ordeal by fire, the difficulty and delicacy of my task being so great that I dare not hope to escape adverse criticism.

You have been listening to learned discourses on the physics and physiology of living things, wherein the lecturers have been able to instruct and satisfy the mind with details more or less capable of demonstration; whilst I have to admit that my subject lies on the confines of human knowledge, and that too often the highest effort of the clinical student is to arrive at some feeble probability, in the presence of uncertain, or even delusive, evidence.

Clinical Medicine, though a special department of knowledge, is so intimately connected with other sciences that, when the claims of these are satisfied, it might seem that nothing would remain to it. This appears to me the present error of our schools. It would not, however, be too much to assert that, were it possible to conjoin in one human intelligence all that is now known of other sciences, such knowledge would be compatible with entire ignorance of the department of clinical medicine. As the physiologist must yet assert, that the phænomena of living tissues are not explained by their chemical composition, or, as the chemist himself has equally to admit, that mere isomerism may be no clue to chemical qualities, so the clinical physician knows that the phænomena of disease are not explained by the knowledge of healthy textures, nor by the action of healthy organs. Clinical work is a work by itself; and yet, if I may use the comparison, only so far by itself as one form of organic life may be considered separate from another. It stands apart, but has the most intimate relations to all that surrounds it. It is elucidated by the light of physics, chemistry, and physiology, yet is not comprehended by them as they now stand. In ages gone by, Hippocrates had to vindicate the study of disease from the inroads of superstition; at the present day we have to guard it against assaults on the side of science, and need to watch lest we betray it by accepting a too chemical or physical limit to our thoughts.

We should all contemplate with great satisfaction such inroads of the collateral sciences upon medicine, as that at length medicine might have no separate existence; but this consummation appears to be, as yet, far distant, and must be so acknowledged. Happily, such is the extension of the human mind into Nature, that almost daily new regions are discovered; and the boundaries of the old are so extended as to require fresh subdivisions in order to bring them within the domain of thought. Formerly, the physician might have been able to comprehend all that then constituted the allied sciences of medicine; but that can never again be possible. His duty lies, therefore, in giving an exact and scientific character to the department which remains to him,—to investigate its phænomena with that concentration which is necessary in every physical enquiry, and with all those aids which are afforded in increasing perfection by modern science.

It is not, however, to be overlooked, that even Science herself is apt to have her moments of dogmatism, and, by throwing the light of some particular enquiry full in our eyes, to blind us for the time to that which lies beyond. How often has medicine been thus diverted from her difficult path. A discovery in physics has made us for the moment no more than galvanic batteries, or a discovery in chemistry mere oxidising machines. To-day, however, we go to bedside work untrammelled by any exclusive theories of this kind, ready to investigate diseases in every way that investigation is yet possible, and forming our judgment in no narrow spirit of a foregone conclusion. We have no system to satisfy; no dogmatic opinions to enforce. We have no ignorance to cloak, for we confess it; but we have to bring into the court of enquiry all possible evidence, and to decide upon it by the light of science and experience. They whose work lies more open to experiment and demonstration are apt to forget the difficulties we have to encounter, and the mental labour required for dealing with them with any measure of success. They would have us postpone these difficulties to a more convenient season, until, by the advancement of other branches of science, their investigation could be undertaken with less risk of failure.

However gratifying and proper this delay might be, were the end of our knowledge contemplation only, and were there no motives to present action, yet, as we are in the midst of human suffering, and have some knowledge for its relief, it is plainly the duty of some, and worthy of the highest intellects, to apply themselves to this work, even though by so doing they may forego the immediate rewards which pure science so liberally affords.

Whilst thus asking for the unstinted recognition of clinical medicine as a scientific department, I am not forgetful of the obligations imposed upon us, nor that in it there lie problems, as yet, far beyond a scientific solution. No one can hope, even as the sciences now stand, and much less as they shall further advance, to obtain a foremost knowledge of them and of medicine at the same time. Yet such knowledge is to us of daily necessity. We must, therefore, refer our physiological difficulties to the physiologist, and our chemical questions to the chemist, and still admit that there remains an unlimited area of study for us, in tracing the causes and relations, and in recognising the presence, of disease. I would not be understood to say that the physician should neglect the sciences of chemistry and biology, and devote himself to the limited study of morbid phænomena. If any should desire to do so, the attempt would prove its impossibility. The interchangeable relations of things are such as to make necessary the most discursive operations of the mind, if it would successfully enquire into that which is most special. I desire only that that which is our proper work should have our entire energies, strengthened and directed by every collateral aid.

Whilst the biologist traces downwards the relations of the various forms of living things, and, breaking away, one by one, the barriers of separation between them, at length views them all as springing from a common germ, the student of clinical medicine, working in another direction, seeks opposite results. To him, one form of life absorbs and centralises all other forms. His object is to see the facts of human organisation in their most special relations. The very perfection of his work lies in this. It is not even man in general; it is the individual man upon whom his attention has to concentrate. The stock whence he may have sprung, the circumstances of his birth, the time he may have lived, the diseases he may have undergone, the habits he may have acquired,—are all subjects to be brought into the focus of thought at the bedside, and made to clear up the problem of disease.

Medicine is a specialism; but of no narrow kind. We have to dissect nature; which, for practice, is better than to abstract it.* Every form of life has to us a

* Nov. Org.

value, but in an order the reverse of the generalisations of natural history. We desire to know what limits, specialises, and perverts. We study in order to distinguish, and not to classify.

Yet it is not only the individual that as a whole we have to isolate for the purposes of clinical study; we have further to enquire into the life of his several organs and tissues. These have each their own life, and correlative with it their own tendency to disease, and their specific power and mode of repair. To clinical medicine, therefore, the body becomes a pathological museum. In every part we recognise certain proclivities to morbid action; and the purpose of our study is to trace these tendencies to their source on the one hand, and to their effects on the other. Histology and anatomy are daily widening this fundamental department of medicine; and we may be sanguine that an acquaintance with the morbid changes to which the same parts are liable, where present in the lower animals (comparative pathology), will afford further valuable aid, as by it we shall have in some degree a dynamic test of the general tendency to these morbid states, in addition to that furnished by human pathology. A knowledge of these intrinsic tendencies to pathological change in the several organs prepares us beforehand to recognise their occurrence where, without such knowledge, the signs and symptoms which are present would convey no information. To know, for instance, that the brain, from the early period of adult age, in persons otherwise healthy, is prone, without obvious exciting cause, to the formation of tumour in its substance—to be aware of the probability of insidious *ramollissement* of the central commissures in younger subjects—often enables us to suspect these conditions, and to give weight to what might otherwise seem some unimportant ailment. As I name these two instances, I feel sure your clinical reminiscences will supply the proof of what I state, and afford a sad page of suffering, death, and error.

There would appear to be some textures of the body endowed with an almost complete immunity from disease, whilst others are equally liable to it—a difference of which at present we cannot give a sufficient account. Both stand as glaring instances to warn us against the adoption of some of our current theories. For instance there is, perhaps, no part of the body organised for more rapid cell-life, or that is more extensive or more vascular, than the mucous membrane of the jejunum; yet, with the exception of the choleraic and diphtheritic processes, it is almost exempt from primary organic changes—so much so that at the bedside, in considering the probabilities of disease in the abdomen, we have, on the one hand, to exclude this part from our consideration (except in mechanical obstruction), and on the other to direct special attention to certain seats of lesion whence the trouble is likely to have sprung. Contrast the limited area presented by the lesser curvature and pyloric region of the stomach, the duodenum, cæcum, and rectum, with the enormous extent of the valvulæ conniventes, and compare the frequency and character of the lesions in the one with those of the others, taken together, and it is obvious how much our diagnosis of abdominal disease depends upon our acquaintance with the tendency of certain textures in the abdomen to morbid changes.

The investigations of morbid anatomy have thrown a flood of light upon the so-called acute idiopathic diseases. Formerly such affections were supposed to be of common occurrence; and the treatment of the day was adapted to their apparent violence. But how rarely now do we meet with a case of acute inflammation of the membranes of the brain, or of the peritoneum, or, indeed, of any other texture, which we cannot refer to some chronic lesion, or to some distinct cachexia; the only idiopathic part of the case being that which was formerly overlooked or unrecognised — some chronic tissue-change, unnoticed in the storm of acute disease to which it had given rise.

Oken has said that all the tissues are nervous, and bone is hardened nerve. I need not discuss this assertion on the present occasion; no doubt modern physiology and pathology are advancing the evidence that whatever is living has nerve-quality in it. The highest expression of this quality is conscious intelligence; the lowest it is, from the nature of the case, at present impossible to mark. This much, however, it appears important to recognise clinically—that morbid brain-force may give rise to a variety of disorders, apparently distinct from their original cause. There is a neuro-pathology from the brain to the tissues, as there is a reverse order of disturbance from the tissues to the brain. If we trace the history of morbid brain-force through the various members of a family, we shall often recognise a great variety of related phænomena, which in nosological classification, are separated and considered as distinct. The intellectual disturbance in one may appear as epilepsy in a second; as mere dyspepsia and so-called acidity in a third; in a fourth as some peculiar neuralgia; in a fifth, if a female, in many varieties of capillary disturbance, such as amenorrhœa, vicarious menstruation, hæmatemesis, or even hæmoptysis; in a sixth, some part of the intestinal tract, the colon chiefly, may appear to be the recipient of the morbid nerve-process, and the patient be tortured with fears of a tumour, which—though a mere phantom, is yet calculated to mislead the unwary. Nor does this list exhaust the catalogue of these strange vagaries. It would seem as if sometimes this morbid brain-action expended itself upon the voluntary muscles, which, if of the abdomen, may be shaped into forms that defy diagnosis and bewilder the most cautious.

It might be thought unnecessary for me to point to this strange field of pathology, which has long been recognised as in part connected with hysteria, but I have reason to think our views on the subject are still wanting in distinctness, and that the term 'hysteria,' as now understood, does not include all I here intend. These morbid conditions occur as essentially, if not so frequently, in the male as in the female, though the form of them may be determined by the sex. It would seem that, in different individuals, different portions of grey matter became the seat of the same kind of morbid action; the equivalent of mental disorder in one occurring as some visceral disorder in another, and so on. And, besides these mere functional disturbances, the history of medicine and my own individual experience supply instances of actual tissue-changes which admit of no explanation until thus looked at; and, I need not add, such cases are entirely distinct from feigned and fictitious disorders.

The flatulent dyspepsia of the student, the tears of the distressed, the dry mouth of the anxious, and the jaundice of fright, daily remind us how far the cerebral influence extends, and physiology will hereafter teach us to trace the steps whereby these effects are produced. As there is no explanation of laughter when the axillary nerves are tickled, so there seems to be none of the morbid fears which oppress those who are the subjects of some affections of the colon, and who weary our patience with their doleful complaints. Yet surely we have no more ground to deny the reality of the one than of the other, though we must at present refer both to some ultimate fact of our natural history. 'As face answereth to face' by mysterious sympathy, so do these and other peripheral impressions excite or depress, in an equally mysterious way, the subjects of them.

I cannot turn from this hasty glance at the idiopathic pathology of the tissues without mentioning how much clinical medicine has gained by recognising the relations of the tissues to time. The wise man says, 'There is a time to be born, and a time to die.' What the physiological limit of the latter may be has not been determined, but at the bedside it has to be recognised and its concomitants distinguished from the phænomena of disease.

Abercrombie was amongst the first to point out that the paralytic affections of age were due to senile changes in the tissues; and more recently, the convulsive affections of otherwise healthy but aged people, have been included in the same category. The epileptic attack of the old man is an evidence of failing power, as his paralytic seizure is an evidence of failing tissue. Though the actively growing organs of the child contrast in a striking manner with the same in decay in the old, there is yet, in some respects, a similarity between the diseases of the two periods of life, like the tints of the rising and the setting sun. In the first period, the organism has not acquired its forces; in the latter period, it is losing them. Infantile convulsions, and senile convulsions; infantile diarrhœa, and senile diarrhœa; infantile eczema, and senile eczema; uric acid deposits in childhood, and uric acid deposits in age, may afford illustration of the truth of my statement.

Time moreover, acting differently upon different parts of our organism, often performs a kind of pathological dissection, exposing the inherent weakness of entire organs, or parts of them, and giving rise to diseases for which at present we have often no name but that which designates some prominent symptom. This process of decay, due to time only, occurs at almost every period of life, according to the constitution of the individual. The fatty degeneration of muscular fibre, occurring in the children of certain families, affords an illustration of this, and we have yet more striking ones in the progressive muscular atrophy, which occasionally exhibits itself from primary changes in the nervous system, in equally young subjects. Senile changes may thus occur in childhood, as in the ephemera old age comes on in a day. My thoughts have been specially directed to this subject of late, whilst passing in review the facts of locomotor ataxy. The condition of the nervous system which most commonly gives rise to this form of unsteadiness of gait is plainly one of decay, like baldness, or greyness, or the occurrence of the arcus senilis. It occurs to individuals of particular families, in which other forms of nerve-degeneration are prevalent. It is limited almost entirely to males, at the middle or after the middle period of life; and, if we may venture to draw general conclusions from the few observations that have been made post mortem, is connected with fatty degeneration of the posterior columns of the cord; not, however, limited to these, but associated with changes of the like kind in other parts of the cord, and in the brain itself.

My friends, Mr. Lockhart Clarke* and Dr. Hughlings Jackson, endorse the opinion first put forward by Duchenne, and subsequently maintained by Trousseau, that this locomotor ataxy is due to disease of the spinal marrow only; but I am disposed to think that they arrived at this opinion against clinical evidence, and perhaps biased unconsciously by prevailing theories of the relation of the nervous centres to each other.

It is only fair to English pathologists here to state, that this form of disease has long been known to them.

* Mr. Lockhart Clarke writes to me that he has long objected to the theory which limits ataxia to lesions of the cord only, and that he has fully stated this in the *Lancet* of December, 1865, p. 729.

Matthew Baillie regarded it as the most common form of paraplegia, and had an anatomical explanation of its peculiarities, which, imperfect as it is, proves that he had investigated its post-mortem anatomy. His theory was that the malady arose from a morbid effusion of cerebro-spinal fluid, which, when the patient was erect, gravitated into the lower part of the theca vertebralis, and, by pressing upon the cord, rendered the patient unable to steady his movements; whilst, from the same cause, these became free again when he was placed in a recumbent position. I quoted these observations of Baillie in the year 1849,* and took occasion then to call the malady encephalic paraplegia. It is probable that this was too restricted a term, as is also that of locomotor ataxy; and that in our further consideration of the disease, we must recognise a diminution of nervous power from failing nutrition of a more general kind, as well as special lines of more unequivocal decay, chiefly marked in the posterior columns of the cord. In support of this opinion I may state that ataxy alone occurred but three times in fifty cases referred to by Trousseau. And yet notwithstanding this, Trousseau drew the characters of the disease which he wished to typify from these three cases, and not from the forty-seven others in which there was evidently disease of parts of the brain, as well as of the cord. If, however, all that has yet been done still leaves the question of the lesion occasioning this form of paralysis in dispute, one suggestive fact remains—namely, the singular isolation of the posterior columns of the cord by the degenerative process. This fact appears to indicate that the affected structures have

* Gulstonian Lect. 1849.

their own vitality, and probably, therefore, a separate function from that of adjacent parts. Todd maintained the existence of this separate function on other grounds, and concluded that the posterior columns were mere commissures. This theory seems the more probable from the facts now alluded to, inasmuch as textures fulfilling only such a function may be expected to have a lower vitality than others which are more essential. This supposition is strengthened by what is observed under extreme inanition, it having been proved by the well-known experiments of Chossat that the nervous centres resist atrophy more than other tissues. Related to these intrinsic morbid changes, whether local or general, are the cancerous and tubercular affections, and the universal liability of the tissues to that perverted process of nutrition which we call inflammation. Although, for practical purposes, it is convenient to intensify the differences between cancer, tubercle, and the inflammatory process, we have abundant evidence of intimate relations between them. Thus, the children of parents dying of cancer are not rarely tubercular, and those of the third generation are liable to various forms of chronic inflammation; whilst in the same families are healthy individuals, in whom we can discover no evidence of any special morbid tendency. It is not improbable that that which seems most special to us in cancer and tubercle may depend more upon gradation than change of diathesis, and that both are allied to more common degenerations.

In tubercular phthisis this has long been felt and acknowledged. Those who have given most attention to the subject (and I could name no one whose experience would have more weight than that of the late Dr Addison) have had to confess that the larger their observation, the greater their difficulty in drawing a line of demarcation, limiting tuberculous from simply inflammatory productions. No doubt many of the errors of prognosis in phthisis, some wilful, some unwitting, arise from assuming a distinction which does not exist, except in extreme cases where, as the logicians would say, the *quantum* passes into the *quale*. Although it has been observed that the scrofulous diathesis of early life may show its special characters in age, still we have, on the other side, frequent proof of a change to that which is malignant; and experience will, I am sure, confirm my statement that the tubercular peritonitis of childhood corresponds with great strictness to the cancerous peritonitis of age. It is probable that cancer is nearer to the simple degradation of tissues than tubercle. It appears to be more independent of an external exciting cause; and although in its structure there is an appearance of vital activity, such activity has no correlation with the healthy organic processes, and is a mere eddying off and separation from the organic cycle. A closer enquiry into the local origin of cancer, and of the mode whereby the system becomes infected by it, are yet desiderata. The tendency to infect the body generally may, after all, not be dependent upon a more marked cancerous diathesis, but upon local circumstances. Something analogous appears to exist in the infecting and non-infecting chancre; for though, according to some authorities, one is essentially local, and the other as essentially infecting, yet it appears certain that even the infecting kind has not always the same infecting power.

In passing from these idiopathic or intrinsic morbid conditions to such as arise from accident or *ab extra*, the fevers chiefly claim our notice. In the consideration of these, small-pox stands first; since through vaccination we have it clearly demonstrated, at least for this one form of fever, and it seems but fair to infer the same of others, that recovery and subsequent immunity are produced by a process of impregnation and assimilation, and not, as is still by some maintained, of elimination. The old theory of depuration, though true of gross chemical poisons, as lead, or mercury, or arsenic, appears to have no application to those operations which take place in the body, in contagious diseases, as the effect of organic poisons. After any one of these the organism is not restored to its former condition, as if any poison had simply been cast out; but there is notoriously a residual and permanent effect, which has been induced under the superficial disorder; this effect being shown by permanent indisposition to a repetition of the same morbid process. Unfortunately for science, the phænomena of fermentation have been assumed in explanation of what takes place under these circumstances; and by the theory of zymosis we are carried back to the days of ignorance, when concoction and maturation were made to explain whatever was obscure. We discover, however, at the bedside nothing in the phænomena of febrile disease proper to zymosis. As well might we call the evolution of the germ after impregnation by that name. The physiological disturbances induced by any one of the fever-poisons—namely, the excess of heat, the rapid waste, the quick action of the heart, the altered functions of secretion and excretion, &c., the so-called symptoms of the disease—are but the outward effects common to the class, and only so far peculiar to each as they may vary in time and intensity. That which really constitutes the specific character of each fever is the attendant tissue-change which, when completed according to the special poison, is followed by convalesence.

If this be in any degree an approach to a true conception of these diseases, it follows that the object of medicine must be rather to limit the violence of attendant symptoms, than, with our present knowledge of therapeutics, to aim at arresting or neutralising their specific processes. Had any one formerly been asked the remedy for small-pox, he would hardly have seriously supposed the answer to be the poison of small-pox itself. Yet so it has proved; and from this experience we have learned that help may come precisely in the opposite direction to that looked for in our theories, and we obtain a striking proof of the truth of Bacon's aphorism, 'Natura non nisi parendo vincitur.'

Yet, notwithstanding this teaching, pathology still persists in looking in another direction; and therapeutics are governed by the idea that disease is an entity which must be combated and cast out. I fancy that the habit of calling these and similar affections blood diseases insensibly fosters the idea of depuration.

Now, though I am not disposed to stir up a discussion between *Solidists* and *Humoralists*, I cannot but express my conviction that the susceptibility to the various contagious fevers does not lie in the blood, except so far as it may be a channel through which the poisons reach the tissues; and that it is in these, and especially in the nervous tissues, that the true fever processes begin and end. The facts of habit, such as those of taking opium or using tobacco, the facts of acclimatisation, and of the commoner experience of our life, whereby the nervous system becomes accustomed and indifferent to continued sources of irritation, render such an opinion the more probable; and a confirmation of it is gained by that enduring effect which ensures against a repetition of morbid actions.

Were I to give liberty to my imagination, I might perhaps trace here a much more extensive law of our nervous organisation, whereby that which is new excites and that which is old becomes indifferent. For my own part, the views which have been put forth as to syphilisation have, on these grounds, seemed to deserve the fullest consideration; and, though I have not original observations to set against the adverse conclusions of some who have made the subject a matter of experimental enquiry, I feel that we cannot easily set aside the experience of those who have asserted its success. Analogy is in its favour.

I cannot conclude these general remarks on some difficulties which now occupy our minds in respect to pathology without alluding to the vexed question of rheumatic fever. Is this state due, or is it not, to a *materies morbi?* Further, have we any grounds for assuming that such *materies morbi* is lactic or acetic acid? I put these questions thus explicitly, because it seems to have been settled, upon mere authority, that they may both be answered in the affirmative. I say authority alone, not forgetting the experiments which have been made on animals in proof of this theory, since such experiments appear to me to prove only this, that the acids named, entering the blood, may cause endocarditis and some other pathological changes simulating those of rheumatism; but I cannot therein recognise the rheumatic state, as I am acquainted with it at the bedside. There are, so far as I know, no analyses of the blood in rheumatism which show that it differs from normal blood in respect of its acidity. The theory of an acid *materies morbi* appears to be supported chiefly upon the excessively acid secretion of the skin in this disease, and the increased acidity of the urine. But neither of these can be considered in any degree characteristic; for not only in the worst forms of the rheumatic process is the secretion of the skin not acid but alkaline, but in conditions of the system totally dissimilar from rheumatism—as, for instance, in phlebitis, and especially in that form following injuries to the head, as well as in arterial embolism—we often meet with excessive acid sweating, misleading to a false diagnosis those who believe this to be characteristic of rheumatism. Notoriously, a proper function of the skin is to secrete, and probably to form in itself, lactic and acetic acid. Under different kinds of irritation this formation becomes excessive; but I know of no facts to show that this excess indicates a special pathology, or may be regarded as a salutary process, whereby the system is relieved of a *materies morbi.* In so supposing it, we seem to be misled by the same fallacies as, before the time of Sydenham, misled practitioners in the treatment of eruptive diseases. I confess to a strong sympathy with those errors; and, though my reason is convinced that they were errors of the most dangerous kind, I cannot but excuse them, and admire the genius and courage of Sydenham, which enabled him to detect and correct them.

If, failing the evidence of the cutaneous secretion, the rheumatologist adduces proof of an acid diathesis from the character of the urine, must not the force of his argument be abated by the admission that urates replace urea in a large number of other morbid conditions, so that we cannot attach any specific value to this fact, the general significance of which will be better appreciated by the physiologist, who will see in the presence of uric acid and the urates a debased condition of organic waste, common to the life of inferior organisms?

To pass, however, from these desultory observations, which the present position of pathology has suggested, the subject of diagnosis claims a few words. We must all admit that diagnosis ultimately rests upon an exhaustive pathology. Without a knowledge of what is possible in disease, diagnosis must be defective, and is, therefore, in that degree defective at the present day, since it is plain that we are unacquainted with many pathological states, if, indeed, we be fully acquainted with any. Moreover, a knowledge which seems exhaustive to-day, may, in the changing circumstances of the world, be defective to-morrow. Without raising the question whether disease has within historical periods changed its type, it may be maintained, as was long ago pointed out, that the pathological tendencies of the body do vary with the *genius anni*, as witness the changes in epidemic disease. Within our present experience, cholera has afforded us new pathological questions, which are not yet solved; and strangely side by side with it—to make the contrast more impressive—diphtheria has revived; laying before us, as if to teach us how feeble our pathological science is, two opposite conditions, two diseased mucous surfaces—one digestive, one respiratory—from the former of which, by mysterious inversion of its normal forces, the salines and water of the blood may be fatally diffused, and from the latter, as by a kind of morbid polarity, the fibrine only poured out.

Although the perfection of diagnosis cannot be reached till we have a perfect pathology, we have to confess that it falls behind the pathological knowledge we at present possess, as the revelations of the post-mortem tables abundantly confirm.

And this brings me to the second principle of diagnosis, a knowledge of the probable in disease. Of this, experience alone can inform us, and experience of the most varied kind. But when so varied, and supported by a large knowledge of pathology, it often enables us, as by prophetic insight, to diagnosticate conditions, which neither direct physical examination, nor the most systematic arrangement of symptoms, could explain. These suggestions are amongst the best fruits of experience—of that experience which is able to arrive at causes, and from causes anticipate effects. The advancement of diagnosis depends upon the capacity of medicine to make these anticipations with increasing certainty, which, though in a sense *anticipationes mentis*, are truly *interpretationes naturæ*. To illustrate these remarks, what could I better quote than the clinical history of thrombosis and embolism? What mysterious obscurity, until recently, involved the phænomena of which embolism was the cause; but, thanks to Virchow and Kirkes, when once the pregnant fact of embolism in vein or artery is recognised, not only present phænomena arrange themselves in order, but we are able to anticipate the possible occurrence of others, and by anticipation often to prevent them. Symptoms and physical signs may supply us with abundance of clinical facts; but, until the one great fact be recognised in such cases, we can make no step in diagnosis.

This instance by no means stands alone; for whereever, from the nature of the case, we are unable to make a complete physical examination, as must always occur in diseases of the brain, and as too often occurs in diseases of the abdomen, pathological inference, or pathological anticipation, has to supply a meaning to other portions of the evidence. In brain diseases this method of interpretation comes largely into play; and the neglect of it has much to do with the obscurity in which these diseases are still involved. It is often impossible to form any opinion whatever of the lesion under which a patient with brain disturbance may be labouring, from an enquiry however acute, and however complete, into the mere statical facts, as they at the moment present themselves. The attempt to do so is perhaps more likely to lead to error than to truth; a fact which, if I be right in the statement of it, shows of how little value mere symptoms are in the diagnosis of such cases.

Abercrombie felt this to its full extent, and one of the objects of his treatise on cerebral disease was to make it clear, and to warn us against future attempts in that direction. A perusal of his writings leaves upon the mind the impression that the most diverse affections of the brain may, at the bedside, present the same symptoms; that in the most extensive lesions there may be no symptoms at all; or that the whole catalogue may appear without any lesion. But the feeling of despair which such a perusal formerly produced is now in great part dissipated by the success with which the enquiry can be made in the direction pointed out. Admitting that we shall never diagnosticate cerebral lesions by their symptoms; partly because different lesions produce the same symptoms if the seat be the same; partly because there appear to be surplusage portions of brain-tissue, as for instance in the hemispheres, where lesions cannot make their presence known; and partly because in that monster disturbance epilepsy, we have a variety of states simulating organic lesion,—we betake ourselves again with renewed energy to the study of morbid anatomy and pathology, which first caused the confusion by disturbing our ignorance, feeling assured they will at last afford us a full clue to the difficulty. To a large extent they have already afforded this clue. To begin with the last fallacy I have named, I may remark that we are better acquainted than formerly with the various forms of the epileptic state. A better pathology has prepared us to recognise in this condition a great variety of effects. Todd drew attention to epileptic hemiplegia; and in the same subjects there occurs also a remarkable form of coma, which has often led to the supposition of effused blood, or tumour, or abscess — suppositions which have been falsified by the recovery of the patient. Whilst our notion of epilepsy included nothing more than a convulsive state with unconsciousness, numerous errors in diagnosis must have occurred from this source alone. It now represents to us a condition of disturbed nerve-force, in which may occur not only the common phænomena of epilepsy, but coma without convulsion, paralysis following convulsion, sudden and transient mania, or an approach to it, as well as, according to Trousseau, some strange forms of neuralgia. A knowledge, therefore, that a patient is liable to epilepsy, or comes of a family in which such a state has occurred, must make us pause in our diagnosis, and thus save us from a precipitate or erroneous conclusion. The proneness of the aged to epilepsy is a fact probably not sufficiently borne in mind in the diagnosis of cerebral disorders. As to the second fallacy, when disease is situated in what may perhaps be called without misuse of the term, the surplusage of the cerebrum or cerebellum, we are often led to suspect its presence, and as often correctly to infer its nature and avoid the third fallacy, from a knowledge of the fact of surplusage, and of what is probable under collateral circumstances, though the symptoms of organic disease may be apparently of an insignificant kind. For instance, headache with occasional bilious vomiting in a young and healthy adult—tumour (?); the same symptoms, with chronic suppuration about the ear, or in some distant part—abscess (?); nearly the same symptoms with syphilitic cachexia—syphilitic affection of the brain (?). This is the merest outline, but is true to nature. May I say, once for all, that any peculiar shape of head, large or small, has, like the epileptic brain, long been known to increase the difficulties of cerebral diagnosis. Further, how much have we gained in the diagnosis of cerebral disease by the known tendency of renal cachexia to induce chronic or subacute cerebritis, and of embolism to plug the vessels?

In turning from the diseases of the brain to affections of the chest, we find that we are able to combine our knowledge of the possible and the probable with direct physical signs; and consequently the diagnosis of chest-affections has very steadily advanced. Old errors, however, still linger even here, and a true dynamic estimate of lung-lesions is yet a desideratum. This must be supplied by improved interpretation of physical signs through attendant physiological conditions. The word 'phthisis,' which has now too often a specific value, will dilate so as to include a whole genus of chronic affections, which, when duly recognised and classified, will afford more secure grounds of prognosis, and spare us the perusal of worthless records of so-called consumption cured.

In the diagnosis of abdominal diseases, we want an increase in the number of cardinal facts; such, for instance, as that the enlarged gall-bladder changes its shape by contraction of its muscular coat; or of the two characteristic notches in an enlarged spleen; or of the peculiar position of the colon in enlarged kidney, &c. At present our diagnosis is mostly one of inference, from our knowledge of the liability of the several organs to particular lesions: thus we avoid the error of supposing the presence of mesenteric disease in young women emaciated to the last degree through *hysteric apepsia*,* by our knowledge of the latter affection, and by the absence of tubercular disease elsewhere. We infer alcoholic changes in the liver from the aspect of the face, yellowish and mottled by venous stigmata, even without any direct knowledge of the state of the liver, or the habits of the patient. We suspect a cancerous disease of the peritoneum in the aged from the pain with ascitic effusion.

It is obviously to an increased perfection of physical diagnosis, aided by pathology, that we must look for the advancement of medicine. The feelings of the patient, the so-called symptoms, are of little value taken by themselves; often, in fact, their mere number and variety are a proof of the absence of disease, and it is admitted on all hands that they need the interpretation of physical enquiry.

The eye, the ear, the touch—and chemistry supplementing the other two senses,—are impressed into our daily service, and we may hope that what the ophthalmoscope has effected for the eye, these other means

* I have ventured to apply this term to the state indicated, in the hope of directing more attention to it.—W. W. G.

may do for other parts. To chemistry we owe much; but there are two yet unsatisfied claims clinical medicine has to make upon it. We want analyses of the *residuum* of the urine, in which we may hope to discover new elements for diagnosis; and we further want ready clinical means for recognising what has been discovered by more elaborate processes. Our hope in the direction of chemistry is unlimited, seeing that there must be changes in the urine largely corresponding to the changes in the organs and textures whether healthy or morbid.

To return, however, to physical diagnosis; the fidelity with which the characters of a disease are often marked in its tissue-changes must excite equally our wonder and attention. It may be no more than the tint of the morbidly vascular part—a condition apparently the most trivial and accidental; and yet it returns with unerring certainty under the like conditions, as our guide to diagnosis. The multiplied generations of the vaccine vesicle from one lymph, maintaining to this day, in all respects, the characteristics of that which first arose under the hand of its immortal discoverer, is one of the strongest evidences that I could adduce of such fidelity, and may well encourage us to the investigation of physical signs as an evidence of the pathological causes to which they are due.

But our diagnosis is not always of a single morbid state. There may be grafted upon some special diathesis, as of tubercle or of gout, the effects of alcohol, syphilis, mercury or miasm. No doubt even more complicated instances could be given, but this will suffice to show what diagnosis must embrace before we proceed to treatment.

As health is our object, or as near an approach to it as circumstances admit, *hygiene* and *therapeutics* claim the last and highest place in our thoughts. Happily, at this day, hygiene has gained strength enough to maintain an independent position as a science. To know and counteract the causes of disease before they become effective is evidently the triumph of our art; but it will be long before mankind will be wise enough to accept the aid we could give them in this direction. Ignorance of the laws of health and intemperance of all kinds are too powerful for us. Still we shall continue to wage an undying crusade; and truly we may congratulate ourselves that no crusade ever called forth more able and devoted warriors than are thus engaged.

The diseases of the young are in large part preventable diseases.

Epidemics carry off in great proportion the healthy members of a community.

It is futile, if not worse, to speak as some do of leaving diseases to work out their own ends, as agents of a moral police. Medicine allows no such prerogative to our judgment. It is enough for us that diseases prevail, to stimulate our best efforts for their prevention, without our asking a question beyond.

Nothing can stimulate science more to the investigation of therapeutics than the feeling that the diseases calling for treatment prevail in spite of our best efforts to prevent them. Where hygiene fails, properly commences the work of therapeutics; but it is painful to find ourselves occupied in making feeble and often useless efforts to combat the effects of a poison which might perhaps have been stamped out in its beginnings.

The strength of modern therapeutics lies in the clearer perception than formerly of the great truth that diseases are but perverted life-processes, and have for their natural history not only a beginning, but equally a period of culmination and decline.

In *common* inflammatory affections, this is now admitted to be an all but universal law. By time and rest, that innate *vix medicatrix*,

> 'Which hath an operation more divine
> Than breath or pen can give expressure to,'

reduces the perversions back again to the physiological limits, and health is restored. To this beneficent law we owe the maintenance of the form and beauty of our race in the presence of so much that tends to spoil and degrade it. We cannot pass through the crowded streets and alleys of our cities without recognising proofs of this in the children's faces, in spite of all their squalor and misery; and, when we remember what this illustration in all its details reveals, we may well take heart, even where our work seems most hopeless. The effects of disease may be for a third or fourth generation, but the laws of health are for a thousand. Bearing this in mind, I have often had occasion to remark in practice how little we can estimate the reparative powers, however able we may be to discover disease. This is, perhaps, never more striking than in some chronic affections, which, having resisted all our efforts to cure, may have been abandoned in despair, or at length placed under some indifferent treatment. Under these circumstances, with what interest have most of us day by day watched the lessening deviations of disease, until the balance of health has been again all but restored, unstable though the equilibrium thus gained may, from the nature of the case, eventually prove.

Therapeutics were at one time directed only by two ideas—of *strength* and of *weakness*. *Sthenic* and *Asthenic* expressed in general terms the morbid conditions requiring treatment. Of the same import, but of older date, were the thoughts derived from the then current theory of *Phlogiston*; and the terms *Phlogistic* and *Antiphlogistic* still linger in medical treatises. From a better physiology, however, we have learnt that perverted functions in disease, however exaggerated, are due to failure and not to excess of the vital powers.

Organic strength lies nowhere but in the living circle of nutrition and function.

A rapid pulse and active delirium, like the increase of the animal heat, are signs of deficient *balance-power* —a power which we have been so slow to recognise in living organisms, that we have not yet an accepted expression for it. How different seems to us at the present day the value of the symptoms which were formerly considered indicative of strength!

In an increase of temperature we see but increased waste. Every degree of rise in the thermometer indicates to us a corresponding decline in that nervous control which regulates the functions in health; and this decline is the more important, if it be true in complex organisms, as it is in simple machines, that this combination which limits the mere working forces is the highest and most characteristic. The terms 'strength' and 'weakness' are valueless, as expressive of conditions so complicated as those of disease. They are deduced for the most part from the feelings of the patient, and a few superficial phænomena. They are empty idols, impressive only by the extent of their emptiness. Surgical treatment was greatly advanced by the imaginary discovery of the *sympathetic powder*, which being placed upon the instrument inflicting the wound, the injured part, by time and rest, was allowed to recover under the simplest means. We need now to import into medicine a large part of the best surgical principles so deduced. The surgeon is contented to place a wounded part under the conditions of physical and physiological rest, and after attention to hygienic conditions, the *res non naturales* of our forefathers, to abide the result. This, no doubt, expresses the largest part of our treatment of common acute disease. We now know that we cannot directly control the morbid processes in pneumonia, pleurisy, or pericarditis; we know further that the means formerly considered essential to the cure of these diseases, tested by better clinical observations, were either useless or pernicious; that instead of favouring the plastic processes in inflammation, whereby the normal decline of the disease was promoted, the effused material was often more or less degraded and spoilt by the treatment employed, and remained in the affected parts, either as a foreign body, or in different degrees approaching thereto.

And this must always have been so, had we continued to regard these effusions as simply foreign products; but as soon as we perceived their physiological relations, and that they had a course of life like the tissues from which they sprung, they took a different aspect, and it became our duty, often without much interference, to stand by, and watch this course to the end. With an audience like the present, so capable of supplying the proper safeguards to these expressions, I am not likely to be misunderstood, as if the duties of the physician were of a negative kind. There is a sufficient sphere for our activity, in ways too numerous for me to mention: in the relief of symptoms where the lesion may be left to its natural course—in the treatment of the lesion itself, where we have means adapted to it, and of these we may have many—in maintaining the health when the degeneration or lesion is incurable. Time would fail me, if it were otherwise proper in this place, to enumerate and enlarge upon the valuable applications of medicine. The discovery of disease—the alleviation of its symptoms—the obviating its inroads—the placing our patient under favourable conditions to bear it—the guarding him against what would be injurious—and the administration of remedies often in themselves effectual for its removal—are surely services of no unimportant kind.

There is probably no human work which daily confers greater good upon society than does ours; and when we consider that from the ranks of our profession the chief cultivators of modern sciences have sprung, whether we speak of botany, comparative anatomy, chemistry, physiology, biology, hygiene, or social science, we may feel some justifiable pride and be encouraged in spite of all failures to go on, assured that our future must be one of ever-increasing usefulness and honour.

THERAPEUTICAL RESEARCH

IN RELATION TO

MEDICINE IN MODERN TIMES,

BEING

THE REPORT OF THE EDINBURGH COMMITTEE ON THE ACTION OF MERCURY ON THE BILIARY SECRETION,

READ BY

DR. J. HUGHES BENNETT,

IN THE DIVINITY SCHOOL, AUGUST 7, 1868.

VI.

REPORT ON THE

ACTION OF MERCURY ON THE BILIARY SECRETION.

It may be remembered that, in an Address I had the honour of delivering two years ago at Chester, I urged upon the British Medical Association the propriety of doing something, in its collective capacity, towards solving one or more of those doubtful points in practical medicine connected with the action of medicines or with the treatment of diseases. The proposal, having met with the general approbation of the meeting, was committed to the Council; and, in the middle of the following October, I received a letter from the Secretary, informing me—'That, in accordance with a resolution passed at the last annual meeting, the sum of £25 be granted, out of the funds of the Association, to a Committee, to be appointed by, and under the direction of, Professor Hughes Bennett, to investigate the Action of Mercury on Animals.' After hesitating for some time, in consequence of the inadequate sum voted, and after communicating with the President, who explained that although small there was no further balance available, I finally accepted the commission. In doing so, I was actuated by the feeling that this first effort of the Association to investigate an important medical question, with the aid of its funds and by a combined effort of its members, ought to be prosecuted at all risks; I also felt confident that an earnest effort in this direction would ultimately receive the support of the Association.

My next step was to secure the co-operation of gentlemen who were capable in various ways of assisting the enquiry; and ultimately I secured the cordial co-operation of Dr. Christison, Professor of Materia Medica and Therapeutics, Edinburgh; Dr. Maclagan, Professor of Medical Jurisprudence, Edinburgh; Dr. James Rogers, formerly of St. Petersburgh; Dr. William Rutherford, Assistant to the Physiological Department of the Edinburgh University; Dr. Fraser, Assistant to the Materia Medica Department of the Edinburgh University; and Dr. Gamgee, Assistant to the Medico-legal Department. The Physiological, Materia Medica, and Medico-legal Laboratories of the University were also to be placed at our disposal.

The Committee thus formed appeared to me to unite in itself all those elements required to give confidence in the results arrived at; viz. 'The skill of the anatomical operator, the analytical power of the chemist, and the varied knowledge, theoretical and practical, of the histologist, physiologist, physicist, pathologist, and therapeutist, as well as of the physician whose knowledge of diagnosis is unimpeachable.' In the Committee thus constituted, it appeared to me every confidence might reasonably be placed, as 'the energy and sanguine character of youth were sufficiently tempered with the caution and reasoning power of age, while every necessary instrument, appliance, and chemical, together with a public hospital, were at its disposal.'

The first meeting of the Committee was held November 16th, 1866. On proceeding to consider by what method the action of mercury on the biliary secretion was to be accurately ascertained, the conclusion was arrived at that no kind of examination of the fæces could yield trustworthy results. Supposing that the chief and characteristic constituents of the bile found their way into the alvine evacuations unchanged, imperfection in the analytical methods at our disposal render their quantitative analysis impossible. The plan of ascertaining bile-acids indirectly by means of nitrogen and sulphur determinations of the alcoholic extract, while most unsatisfactory in the case of pure bile, is still more so when applied to the alcoholic extract of fæces. The method of Professor Hoppe-Seyler of Tübingen, who calculated the amount of bile-acids from the effect which their solutions exert upon the ray of polarised light, presents such complexity and difficulty as to render its systematic employment in any series of analyses altogether inapplicable. As to the colouring matters of bile, there is no direct method known by which they can be estimated. But it was further argued, that did we even possess proper means of estimating the bile-products, it is only a small portion of such as are secreted by the liver which can be found in the alvine discharges. Bidder and Schmidt ascertained that the amount of unoxidised sulphur in them only represented one-eighth part of the total sulphur which the liver secretes, and that, of the other constituents of the bile, the larger proportion is absorbed. Indeed, the utter impossibility of detecting all the constituents of bile in the fæces is admitted by one of the most reliable physiological chemists of Europe, viz. Professor Hoppe-Seyler. That, under the influence of purgatives, unchanged bile is occasionally discharged from the bowel is true; but this furnishes no proof of any increase of that secretion; for, under ordinary circumstances, it is decomposed and partly absorbed in the alimentary canal, and any cause which increases the rapidity of its passage there, must render absorption and decomposition less complete.

As it was evident that no accurate information concerning the amount of bile secreted by the liver was to be obtained by an examination of the fæces, the Committee arrived at the conclusion that the formation of biliary fistulæ in living animals, and collecting the bile directly through such fistulæ from the gall-bladder, was the only means open to them of determining how far mercury influenced that secretion.

HISTORY.

It next became necessary to ascertain what had been made out by previous observers, as to the amount of bile secreted by the liver, under varied circumstances, through biliary fistulæ. For literary researches into this matter, the Committee are greatly indebted to Dr. Rogers. He informs the Committee, in his report on this branch of the enquiry, that efforts to establish biliary fistulæ, and to collect the bile, have been attended with extreme difficulty in the hands of all experimenters, and have led to a large mortality among the animals operated on. Of 18 dogs operated on for this purpose by Professor Schwann of Louvain, 10 died within a week after the operation, from its immediate effects; 6 in from eight to eighty days from inanition, although the appetite remained good; and in 2, the choledic duct was re-established. Some years afterwards, he operated on 12 other dogs, so that the total number operated on amounted to 30; and Bidder and Schmidt inform us that of these one lived four months, and another a whole year, after the operation. The last-mentioned authors say, 'We shall not take into account the unsuccessful cases, of which the number at the commencement of our investigation of this subject was not very small.'* Again, they say, 'After ten or twelve unsuccessful attempts to establish permanent biliary fistulæ in cats, we were obliged to have recourse

* Verdauung's Säfte und der Stoffwechsel, 1852, p. 125.

to dogs.' Dr. Flint, in his paper on a New Function of the Liver, does not mention the number of dogs in which he performed the operation; but it is evident that a great number perished. He says, 'All the experiments made during the winter 1860–61 were unsuccessful, no animal surviving the operation more than three days.' After a number of trials during the following winter, which were not more successful than the previous ones, he succeeded at last with one animal. There is every reason to believe that, had other experimenters informed us of their failures, the number of these would have been equally great. In the few cases which have succeeded, however, it is important to remember that a large amount of valuable information regarding the bile has been obtained, that never could have been arrived at without them.

The results arrived at may be divided into—1, the amount of the biliary secretion in health, and the circumstances which influence it; 2, the special effect of mercury on the secretion of bile.

1. *Previous Researches to Determine the Amount of Bile Secreted in Dogs, and the Circumstances which influence it.*

Haller.*—In Haller's Physiology, reference is given to several cases in which attempts had been made to ascertain the quantity of bile secreted in a given time by experiments on living dogs. The description of

* Physiologia, tom. vi. p. 605.

them, however, is so very vague and general, that they possess little interest for the physiologists of the present day. Van Reverhord found the quantity of bile secreted by a dog in twenty-four hours to be 6 oz.; and Haller, estimating the secretion in the human subject at four times that in the dog, suggested 24 oz. to be the quantity secreted daily in the healthy human adult. He likewise alludes to an interesting case of a man in whom a biliary fistula was formed in consequence of a wound of the gall-bladder. Tacconus, who saw the case, estimated the amount of bile discharged by the fistula at 4 oz.; but whether the expression 'eodem tempore' refers to six or twenty-four hours, it is impossible to say—probably to the former.

Schwann.*—It was not till 1844 that any serious attempts were made to investigate this subject by Professor Schwann of Louvain. He made several interesting experiments, by means of biliary fistulæ, to ascertain the utility of bile in the animal economy. Unfortunately, he does not appear to have carried out his intention of ascertaining accurately its amount.

Blondlot.† — In 1846, Blondlot succeeded in establishing a biliary fistula in a dog of middle size, in which he gives approximately 40 to 50 grammes as the amount of bile secreted in twenty-four hours. His estimate, however, was not made with great precision; for he only collected the fluid for short periods at a time, and could not, therefore, ascertain its exact amount in twenty-four hours.

* Müller's Archiv. 1844, p. 127.
† Essai sur les Functions du Foie, 1846.

H. Nasse.*—Heinrich Nasse of Marburg published in 1851 an interesting memoir, giving an account of a series of experiments performed on one dog in which a biliary fistula had been established, and which lived afterwards five months and a half. His object was to ascertain the influence of the quantity and quality of the food on the biliary secretion. As we have not succeeded in obtaining the original work, the result of his researches will be subsequently tabulated as obtained from the abstract given of them in Canstatt. †

Bidder and Schmidt. ‡ — In 1852, Bidder and Schmidt, in their work on the Digestive Fluids, gave an account of the most elaborate experiments yet made to determine the amount of the biliary secretion. They succeeded in establishing biliary fistulæ in four dogs. In one dog, the daily observations extended from Feb. 17th to April 15th, when he was killed. The bile was collected by holding a balloon-shaped glass over the fistulous opening for fifteen minutes at a time; and this was repeated daily from six to ten times successively. The varying amount of biliary secretion obtained at one period was corrected by the results obtained at other periods, and the average amount calculated from a large number of observations. This method, though excellent for determining the amount of the secretion at different periods of the digestive process, is, as regards the daily quantity,

* Commentatio de bilis quotidie a cane secreta copia et indole. Marburg, 1851.

† Canstatt, Jahresbericht, 1856, 1st Heft, p. 87.

‡ Verdauung's Säfte und der Stoffwechsel, 1852.

evidently unsatisfactory. Besides, as the dog did not consume the same amount of food under these varied circumstances, that might vitiate the result. To simplify the Tables, and render calculation easier, they estimated the amount of bile secreted at so much per kilogramme weight of dog. Thus, if a dog weighing 5 kilogrammes secreted 100 grammes of bile in twenty-four hours, it would be said that 20 grammes of bile were secreted for each kilogramme of dog in twenty-four hours. They estimate the average amount of bile per kilogramme in twenty-four hours at 19.999.

The following Table gives the average amount of biliary secretion in the four dogs, with the average amount of food per kilogramme taken hourly and daily. One kilogramme weight of dog gives six grammes.

	1	2	3	4	5	6
In 1 *Hour.*						
Fresh bile	0.539	0.663	0.696	1.023	1.198	0.824
Dry residue	0.040	0.035	0.029	0.049	0.057	0.042
In 24 *Hours.*						
Fresh bile	12.936	15.912	16.704	24.550	28.750	19.99
Dry residue	0.960	0.840	0.696	1.176	1.268	0.988
Daily amount of food per kilogramme weight of dog		32.49 flesh; 1.74 bacon and butter.	17.85 flesh; 7.87 milk.	79.51 flesh; 8.32 bread.	66.42 flesh; 8.59 bread (rye).	

The first column gives the quantity of bile obtained from recently-formed biliary fistulæ; the four following ones the quantity obtained in cases of fistulæ of some standing; and the sixth gives the average amount of the different observations.

Bidder and Schmidt found that the amount of bile secreted in a given period varies much in different species of animals. Thus, for every kilogramme of animal there is produced on an average:—

CAT.	DOG.	SHEEP.	RABBIT.	GOOSE.	CROW.
			In 1 *Hour.*		
0.608	0.824	1.059	5.702	0.491	3.004 Fluid
0.034	0.042	0.056	0.103	0.034	0.219 Solids
			In 24 *Hours.*		
14.50	19.990	25.416	136.84	11.784	72.096 Fluid
0.816	0.988	1.344	2.47	0.816	5.256 Solids

It appears remarkable that the rabbit should secrete five times as much bile as the other larger animals do, and that the crow should secrete so much more than the goose; but from the manner in which the bile-collections were made, little confidence can be placed in these results. They found that the amount of the biliary secretion was much influenced by the quantity and quality of the food and drink. Taking from six ounces to ten ounces of water produces a rapid increase of the secretion, attaining its greatest measure in from forty-five to sixty-one minutes after it has been taken, and diminishing as rapidly. They found that, when the food of cats consisted almost exclusively of fat, the secretion of bile was reduced to about the quantity furnished by fasting animals. Blondlot (p. 62) says that the use of fat increases the amount of bile; and Ritter and Nasse say that the addition of fat to the food increases the secretion—at least, when the supply of flesh at the same time is not great. Bidder and Schmidt also ascertained that the quantity of the biliary secretion varies at different periods of the digestive process, and that it attained its maximum thirteen to fifteen hours after a meal. On this point, it may be here observed, that Arnold supposed it to reach its maximum two to four hours after solid food was taken; Kölliker and Müller generally from five to eight hours; and Dr. Flint from two to eight hours. Dr. Dalton, from observations made on a case of duodenal fistula, thinks that the biliary secretion is at its maximum an hour after feeding. Ritter and Nasse, like Arnold, remarked two maxima in the course of the day: the first occurring during the first or second hour after feeding; the other so much the earlier the more scanty the supply of food.

In Bidder and Schmidt's tabulated observations on the first dog, it will be seen that the greatest amount of fresh bile was secreted between six and seven hours after a meal. It is true that the greater amount of dry biliary residue was found in one of the collections made from fourteen and a half to fifteen and a half hours after feeding; but, in another quantity collected at the same period after feeding, the amount both of fresh bile and dry residue was much less than in that collected between six and seven hours after a meal. Again, of two quantities collected respectively on the 2nd and 6th of November, from fourteen to fifteen hours after feeding, the amount of fresh bile in the first collection was only about the half of what was secreted from three to four hours after a meal; and in the second collection it was about half of that secreted from four and a half to five and a half hours after a meal. The tabulated observations on the third dog seem to give more support to Bidder and Schmidt's opinion; but the quantities of biliary secretion given for different periods after feeding are too fluctuating to permit the amount of bile secreted at any given stage of digestion to be accurately estimated. The observations of Bidder and Schmidt themselves, therefore, do not support their own conclusion; and, as this is opposed to those of other experimenters, it must be concluded that the amount of bile secreted varies considerably in the same animal, and at the same period of digestion, even independently of food and drink.

Arnold.*—In 1854, Dr. Arnold published a work on the Physiology of the Bile, and afterwards made some additional experiments on the subject in 1857. The apparatus he employed consisted of a canula 4½ centimètres long and 4 centimètres wide, attached by a screw to an elastic caoutchouc bag 10 centimètres long and 1 centimètre broad. Fifteen millimètres above this attachment, and at right angles with the canula, was a metallic plate, 12 millemètres in diameter. This plate was placed between the skin and the muscles; and the wound healed perfectly over it, preventing all escape of bile between the soft parts and the canula. The distal extremity of the bag had a cork-stopper, by taking out which the bile collected in it could be removed. The operation was performed in the usual manner on a healthy dog of

* Zur Physiologie der Galle, 4to. Mainz, 1854.

middle size, weighing 9.250 kilogrammes, on the 18th of June, 1853. The common duct was first tied close to the duodenum, and again half an inch from the gut. The portion between the two ligatures was then excised. Although, after the operation, the dog was exhausted, and vomited its food more than once, on the following day he appeared to be quite well. The bile flowed freely through the canula until July 1st, when it ceased. Another and wider canula, with a broader border, was then inserted. This also, subsequently, was so forced forward by the contraction of the wound, that no bile could flow, and the canula was withdrawn. The apparatus first inserted was then employed, and answered perfectly, as the canula was firmly fixed in its place by the wound healing over it, so that not a drop of bile escaped at its edges. From the 18th of June until the 6th of July, the dog was fed on bread, milk, flesh, and potatoes. It lost 375 grms. in weight during this period, without any perceptible derangement of digestion. The fæces were pultaceous, without any trace of bile-pigment; had a putrid odour; and contained a considerable quantity of fat, but no trace of muscular fibre. To prevent him from licking the bile, he was muzzled. From July 6th to August 2nd, he was fed entirely on flesh. From the 6th to the 9th of July, he ate daily 500 grammes of fat flesh. During this period, the fæces were like clay, soft, and contained a quantity of fat. The weight of the body diminished rapidly, so that on the 8th it was 8.203 kilogrammes, and on the 9th 7.750 kilogrammes. He was then lean, but lively, and had 750 grammes of flesh, pretty free from fat, divided into three portions, which he ate morning, midday, and evening. During the period he lived on flesh his hair fell out largely, and could easily be pulled out in tufts without causing pain. There was also a large development of gas in the intestines, with borborygmi and liquid fæces. About the 20th of July they assumed their natural consistence, and were brown externally, though of an ash-colour internally. Each time the fistulous opening was interfered with or irritated, the fæces became softer and more liquid, and their odour more cadaverous; while, when consistent, it was less penetratingly putrid. From August 3rd to September 1st, the food consisted of old rye-bread, of which there was consumed, on an average, daily 470 grammes, and was commenced before the dog refused all animal food. During this period, its weight increased to 8 kilogrammes, the emaciation disappeared, and the falling off of the hair diminished. Indeed, the hair in a few months grew abundantly, so that it presented a black shining coat, as before the operation. The appetite returned, and he ate greedily. The digestion was good; the fæces of firm consistence, of a yellowish-grey colour, like that of the bread, and less offensive than when he was fed on flesh. Their quantity also was increased as three to two. Their average daily weight was 320 grammes; whereas, when fed on flesh, it was 210 grammes. He also drank more. When the diet was bread, he drank daily the average quantity of 450 cubic centimètres; when fed on flesh, only 340 cubic centimètres.

The quantity of bile secreted on the average, when fed upon 750 grammes of flesh and upon 470 grammes of rye-bread, is shown in the following Table :—

Daily food.	Weight of dog.	Bile secreted daily.	Bile-solids daily.	Bile secreted daily per kilo.	Bile secreted hourly per kilo.
750 gr. flesh	7.750 kilos.	90.295 gr.	2.892 gr. to 3.056 gr.	11.65 gr.	0.486 gr.
470 gr. rye-bread.	7.812 kilos.	63.024 gr.	1.662 gr. to 2.634 gr.	8.067 gr.	0.336 gr.

From hourly observations it appears that the largest quantities of bile were secreted during the first hours after getting food; drinking water also increased the secretion. The dog caught cold September 1st, and died September 3rd, from peritonitis. On September 4th the body of the animal weighed 7.512 kilogrammes. On dissection, it was found that where the ductus communis choledochus had been cut off, a new one three lines long was formed, having on one side of it a small collection of pus containing the ligatures. It was therefore believed that in a short time the common duct would have been re-established. Round the plate of the canula a newly-formed mucous membrane was discovered, continuous with the gall-bladder.

The following are the more important conclusions drawn by Arnold from the whole enquiry (p. 19) :— 1. Cutting off the bile from the intestines, if a sufficiently increased quantity of food can be digested, is not injurious to an animal. For two dogs of the same weight, one with and the other without fistula, the first will require five-eighths more flesh or three-fifths more bread than the second. 2. The quantity of bile secreted is influenced by the quantity and quality of the food. A dish of bread gives rise to a less secretion of bile than one of flesh. 3. From experiments on dogs with biliary fistulæ, in consequence of the increased diet they require, no conclusion can be drawn as to the quantity of bile likely to be secreted in healthy dogs in proportion to the amount of food they take. 4. The quantity of bile secreted in proportion to the weight of the animal is estimated too highly by Bidder and Schmidt, and also by Nasse; because all animals with biliary fistulæ require much more than their ordinary food to keep up their usual weight, while the quantity of the diet influences the amount of bile secreted. 5. Besides food, the drinking of water considerably increases the secretion of bile. 6. The nature of the food does not much influence the solid constituents of bile. Arnold admits, however, that in this respect Nasse's observations may be more accurate than his own. 7. The secretion of bile, apart from the influence that it exerts on the absorption of fat, plays an important part in the process of nutrition. 8. In Arnold's dog, the biliary secretion was most copious during the first hours after taking food. After the fourth hour it began to diminish until the twenty-fourth hour, when it was least, but the diminution was not regular. His manner of collecting it, however, like that of Bidder and Schmidt, is objectionable—viz., at varying periods of fifteen minutes, half an hour, and an hour. Instead, therefore, of determining how much bile flowed in twenty-four hours, this was made to appear by multiplying so many times the half hour or hour collections.

Kölliker and Müller* made some experiments on the bile during the years 1853 and 1854, of which they published an account in the Würzburg Abhandlungen for 1855. They succeeded in establishing biliary fistulæ in three dogs. For one kilogramme of dog, they found in twenty-four hours—

Hours after food.	Fresh bile.	Dry residue.	No. of observations.
1 to 2	1.450	0.051	8
3 to 5	1.407	0.047	5
6 to 8	1.514	0.048	8
16 to 22	1.320	0.051	7

Another dog gave the largest quantity four to five hours after feeding, and least after nineteen to twenty-one hours. A third dog gave the maximum five to six hours after a meal, and not much less after sixteen or seventeen hours. They found, as other observers had done, that the quantity of food consumed has a decided influence on the quantity of the biliary secretion. When, for example, a dog ate $18\frac{1}{2}$ ounces of flesh in twenty-four hours, the bile collected amounted from 5.3 to 6.6 grammes in an hour, but when it ate $33\frac{1}{2}$ ounces of flesh, it was increased from 7.5 to 7.8 grammes. It is important to observe that the calculations of Kölliker and Müller, like those of Bidder and Schmidt, were derived from collections of bile made during a quarter of an hour, half an hour, and occasionally one hour, and the amount per day was estimated from the averages of these. In no case was it collected continuously for twenty-four hours.

* Würzburg Abhandlungen für 1855, Band v.

Scott.*—Dr. Scott appears to have been the first who collected all the bile secreted by a dog during twenty-four consecutive hours. We must refer to his paper for a description of the method he adopted for collecting it, and for the account he gives of his interesting and carefully-conducted experiments and analyses. He avoided the error liable to occur in calculating the amount of bile secreted in twenty-four hours, from quantities obtained during a part only of that period. He estimated the amount of fresh bile given off in twenty-four hours at about 23.15 grammes; of dried residue, at 1.13 per kilogramme.

Dalton.†—Dr. Dalton of New York attempted to ascertain the amount of bile which passed into the duodenum from the choledic duct by means of a duodenal fistula. But as in this manner it is obviously impossible to determine the amount of the entire quantity given off by the liver, no account of his researches need be given.

Flint.‡—The last experimenter we need cite is Dr. Flint of New York. As his object, however, was rather to ascertain the amount of cholesterine secreted by the liver, than to determine the quantity of biliary secretion, he does not give us much information on this point. In one dog, the bile was collected for thirty minutes at a time during various periods of the day, and was found to be secreted at its maximum four hours, and at its minimum twenty hours, after feeding.

* Beale's Archives, vol. i.

† Physiology, p. 190, 3rd edit.

‡ American Journal of Medical Sciences, vol. xliv., p. 366.

The dog weighed 10 lbs.; and there were collected, in the twenty-four hours, 243.233 grains—an amount which gives an index of the quantity secreted during that period. He further says that, disregarding slight variations, which might be accidental, it may be stated in general terms, that the maximum flow of bile from the liver is from the second to the eighth hour after feeding, during which period of time it is about stationary.

We here subjoin a Table containing the results of the experiments of different physiologists who have investigated the subject of the biliary secretion. With the exception of the results of those of Dr. Flint and Dr. Scott, the Table, of which the arrangement is slightly changed, is taken from Canstatt's Jahresbericht for the year 1863, No. 1, p. 141. The weight is given in grammes:—

Name of observers.	Amount of bile secreted in 24 hrs. per kilogramme weight of dog.		Food taken in 24 hours per kilogramme weight of dog.	Quantity of bile secreted in 24 hrs. for 100 grammes of food.	
	Fresh bile.	Dry residue.		Fresh bile.	Dry residue.
Nasse, 1851	19.2	0.685	155 flesh	12.3	0.44
	22.8	0.700	208 „	11.1	0.337
	23.1	0.784	260 „	8.9	0.30
	24.0	0.765	At will	...	...
	28.4	0.760	„	...	...
	17.7	0.446	100 flesh and 100 br.	...	...
	17.9	0.400	130 „ „	...	...
	12.2	0.500	87 bread	13.9	0.575
Bidder and Schmidt, 1852	15.9	0.840	32.4 flesh, 1.7 fat	49.3	2.608
	16.7	0.696	17.8 „ 7.8 milk	83.5	3.48
	24.5	1.176	79.5 „ 8.3 bread	25.7	1.23
	28.7	1.268	66.4 „ 8.5 „	35.1	1.54
Arnold, 1854-57	11.6	0.373	96 flesh	12.0	0.585
	8.1	0.215	60 bread	13 4	0.357
Kölliker and Müller, 1853	32.7	1.034	...	...	...
	32.6	1.290	98 flesh	...	...
	26.1	1.013	92 „	28.56	1.694
	21.5	0.748	54 „	22.85	0.792
	36.1	1.162	64 „	56.50	1.816
	53.6	1.683	94 „	56.7	1.79
	32.1	...	37.9 bread, 90 cubic centimètres of milk	...	...
Scott, 1858	23.11	1.128	58.6 flesh, 10.3 milk	35.0	1.6
Flint, 1862	11.98	0.440	...	...	...

II. *Previous Researches to determine the Influence exercised by Mercury on the Biliary Secretion.*

Nasse.*—Professor H. Nasse was the first who attempted to ascertain, by experiment on the dog with biliary fistula, the influence of mercury on the secretion of bile. It is stated in Canstatt that the result of his

* Canstatt's Jahresbericht, 1852, Heft i. p. 156.

experiments was, that calomel increased the absolute quantity of the bile, but diminished its solid constituents.

Kölliker and Müller administered to one of their dogs which had a biliary fistula, 4 grains of calomel at ten o'clock on the morning of the 28th. Five half-hour observations made after midday gave an average of 3.823 grammes of bile excreted, an amount a little above that of previous averages. On the following day, however, four half-hour observations gave on an average 3.267 grammes,—that is rather less than the usual average.

On the 21st and 29th days the dog took again 4 grains of calomel, but the biliary secretion, instead of increasing, diminished. Seven observations of half-an-hour each, from the 28th to the 31st day, gave an average of only 2.183 grammes, and the bile at the same time was of a brownish colour, and so thick that at last it scarcely dropped from the canula. This circumstance was undoubtedly owing to the dog's health, which was bad. It had lost weight, had diarrhœa, greyish coloured, and later even bloody stools. For several days at this period the animal took only a little bread and milk.

Dr. Mosler,* in his investigations, proposed to himself the question, 'What substances introduced into the blood appear in the bile?' In some of the experiments a solution of the substance to be tried was injected into the blood, in others the medicine was given by

* Virchow's Archiv, Band xiii. S. 29, 1858.

the mouth, and the bile afterwards tested, to ascertain if it contained any trace of the substance administered. With regard to mercury, he tells us, that on the 23rd of May, at seven o'clock A.M., 5 grains of calomel in a little bread and milk were given to a dog, which had a completely healed biliary fistula. All the bile secreted till three o'clock P.M. was collected by means of a sponge and tested for mercury, but not the slightest trace of it could be discovered. At 4 o'clock P.M. 10 grains of calomel were administered to the same animal, and for greater accuracy a small tube with a caoutchouc bag attached was introduced into the fistula, and kept there till next morning. No trace of mercury was found in the collected bile, and no striking increase of the biliary secretion was remarked. After this experiment the animal was dull, ate less than usual, and had thin very offensive stools. To make a trial of the drug in smaller doses, Dr. Mosler gave the same animal one grain of calomel every hour from the 25th to the 26th of May, so that altogether 25 grains of calomel were given,—no trace of mercury could be found in the collected bile. To another powerful dog with biliary fistula he gave on the 19th of August, at nine o'clock, three pills, each containing 3 grains of calomel. Next morning at six o'clock A.M., three similar pills were given, and at nine o'clock two more —so that the dog had 30 grains of calomel in eighteen hours. The bile discharged from the fistula was carefully collected by a sponge, from three o'clock on August 11th till the same hour on August 12th. Compared with the quantity collected during twenty-four hours on the day previous to that of the experiment, there was no striking increase of bile, nor did it contain any trace of mercury. He repeated this experiment with 24 grains of calomel with the same negative result. Dr. Mosler concludes from these experiments that, when mercury is administered in the form of calomel, either in the small or large doses, it does not pass so rapidly into the bile, nor produce the marked increase of the biliary secretion that medical men imagine. It is much to be regretted that Dr. Mosler did not measure the bile passed during these experiments, which would have given far more value and precision to his observations.

Scott.—The only other experiment made to determine the influence of mercurial preparations, or rather of calomel on the biliary secretion, with which we are acquainted, are those of Dr. Scott, who deserves great credit for the careful and scientific manner in which he has carried them out. He made four trials with calomel, in which he estimated the amount of increase or decrease of the biliary secretion by taking the average of two days previous and of two days subsequent to its administration.

In the first trial, 3 grains of calomel were given to the dog at three o'clock P.M. on the 13th of June.* The daily average amount of bile secreted on the 11th and 13th of June was 1960 grains, and that of bile secreted

* The bile secreted during twenty-four hours was always collected on the morning of the day indicated. The amount obtained on June 12th was not used in calculating an average, as a considerable quantity was lost in collecting it.

on the 14th and 15th, 1358 grains, showing an average diminution of 602 grains for each of the two days subsequent to the administration of the calomel.

In the second trial, 6 grains of calomel were administered at eleven o'clock A.M. on the 16th of June.* The amount of bile secreted during twenty-four hours, and collected on the morning of the 16th, was 1639 grains, and of that secreted during the subsequent twenty-four hours, and collected on the 17th June, was 518 grains, indicating a diminution of 1121 grains in the biliary secretion during twenty-four hours after the administration of the calomel.

In the third trial, 12 grains of calomel were given at 4.30 P.M. on the 3rd of July, the average daily secretion of bile for two previous days (2nd and 3rd of July) amounting to 3044 grains, and that for two subsequent days (4th and 5th of July) to 2720 grains, showing a diminution of 323 grains on the average daily quantity of bile secreted after the administration of the calomel.

In the last trial, 12 grains of calomel were given at 5.45 P.M. on July 7th; the daily average amount of biliary secretion on the two preceding days (the 6th and 7th) being 2658 grains, and on the 8th and 9th July being 1724 grains, showing a diminution of 933 grains in the daily average quantity of bile secreted after the administration of the calomel.

We subjoin a Table of the daily amount of fresh

* The amount of bile collected on the 15th was not used in making an average, probably because Dr. Scott supposed the secretion of the previous twenty-four hours was still under the influence of the calomel.

bile, collected for several days. The † before the dates indicates the days on which calomel was administered:—

Amount of Bile secreted in Twenty-four hours, in Grains.

June 11	1628.00	July 1	2168.051
„ 12	1767.700	„ 2	2941.239
†„ 13	2293.527	†„ 3	3148.400
„ 14	1819.636	„ 4	2560.300
„ 15	896.680	„ 5	2881.500
†„ 16	1639.968	„ 6	2644.300
„ 17	518.701	†„ 7	2672.900
„ 18	1810.450	„ 8	1963.500
„ 19	817.717		

Dr. Scott concluded that all the trials gave but one result; viz., 'a diminution in the amount of bile and bile-solids secreted after the administration of large doses of calomel.' We are of opinion, however, that the diminution is not nearly so great as he has made it appear: thus, for example, if, in the first trial, we set aside the results of June 12th (as Dr. Scott has done), and only take the amount of bile secreted during the twenty-four hours previous, and subsequent, to the administration of calomel (as Dr. Scott has done in the second trial), the amount of decrease will be considerably less than he has calculated it to be. But the number of Dr. Scott's observations are far too few, and not sufficiently long continued, to allow us to draw any definite conclusion from them—indeed, he himself has fully admitted the truth of this remark.

It must, I think, be evident, from this notice of all that has been previously accomplished, that no exact information has yet been obtained as to the influence of mercury on the secretion of bile, or as to any other action it may exercise on the liver.

III. *Investigations of the Committee during Two Years on Dogs with Biliary Fistulæ.*

THE Committee have completed the operation for biliary fistulæ in 33 dogs, but, from various causes which it is unnecessary to detail, satisfactory observations could only be arrived at in eight of these. They have been numbered consecutively from one to eight. Considerable improvements have been made in the mode of operating and in the method of collecting the bile, so that such an amount of exact information has been arrived at as will, in the opinion of the Committee, set at rest the important question as to whether mercury does or does not influence the secretion of the liver.

I have placed in your hands the printed Tables, giving the results of the Committee's investigations. From these it will be learned at a glance the amount of bile secreted in the dogs operated upon, before and after the administration of the drug.* All the facts

* The Tables are placed together at the end of the Report.

were obtained and recorded by Drs. Rutherford and Gamgee, occasionally assisted by Dr. Fraser. Now and then the operations, collections of bile and examination of the animals were superintended by other members of the Committee, and from time to time general meetings were held, in which the progress of the enquiry was carefully considered. On these two gentlemen, however, it may be said, devolved the labour of the enquiry, the amount of which will appear in the sequel. They kept careful minutes of their proceedings, and drew up and are entirely answerable for the accuracy of the Tables and calculations now in your hands. It is from their notes, from the minutes of the meetings of the Committee kept by myself, and from the Tables, that the Report has been compiled. Of this, all I can venture to give at present is a short abstract.

I pass over a minute description of the method of operating and applying the apparatus which has been furnished to me by Dr. Rutherford. It contains most valuable hints for future experimenters, with ample directions how the difficulties both of the operation and of making perfect collections of bile are best to be avoided. In every case the dog was allowed to recover fully from the operation before observations were commenced; the appetite was seen to be good, the amount of the various items of food and the dog itself carefully weighed, while the changing appearance of the stools, as well as the free exit of bile through the canula, externally gave satisfactory evidence that the whole biliary secretion could be measured and analysed with the greatest exactitude. From the first it became evident that the amount of bile secreted varied considerably from day to day even in health. An average, therefore, of several consecutive days was obtained in every case, before endeavouring to determine the influence of mercury upon it.

Before examining the Tables themselves, it should be stated that the metric system of weights is adopted throughout, with a view of assimilating the results of the Committee with those of foreign observers. It should therefore be remembered that—

1 gramme = 15.434 grains
28.3 grammes = 1 oz.
1 kilogramme = 2.2 lbs.

We have not followed this rule with regard to the doses of medicine, considering the English weights would be more intelligible to British practitioners.

In the first series of observations on a Retriever Dog that weighed 18.5 kilogrammes (Table I), it was thought necessary to secure six days' continuous collections, in order to possess a correct average. Unfortunately, on the 13th, 14th, and 15th, owing to slipping and other accidents connected with the apparatus, some of the bile was lost. On the six days, however, on which the bile was collected the averages are correctly given, viz., 119.76 grammes of fluid bile, 7.622 grammes of bile-solids, and 1.259 grammes of bile-salts. In this and all the following Tables the amount of bile secreted is estimated with regard to each kilogramme weight of dog, and each 100 grammes of dry food. These estimates, though of great importance physiologically, need not be referred to especially at present.

Table II gives the results of other six days in which the bile was collected perfectly and consecutively, for 24 hours at a time. For the whole period the dog was in perfect health, and ate his allowance of food regularly. The average amount of fluid bile was 131.31 grammes, of bile-solids 4.71 grammes, of bile-salts 1.343 grammes. So that with the same amount of food consumed daily, while the fluid bile was augmented the solid bile was diminished. As it seemed impossible to obtain a better standard of comparison than these six continuous collections, it was resolved now to commence the administration of mercury. Accordingly,

Table III shows how, with exactly the same diet and all other circumstances being equal, the secretion of bile was effected by the daily administration of 5 grains of blue pill. In order to maintain the animal's good health, the apparatus was taken off, and he was allowed to run about in the open air for five days previously. Five grains of pil. hydrargyri were given and collections of bile were made as before on the 10th of July, 1867. On the 11th, the bile escaped from slipping of the apparatus. On the 12th, however, to the 17th inclusive, the six collections were perfect, and show that the exhibition of the drug was accompanied by slight diminution in the secretion of fluid bile, and a slight augmentation in the amount of the bile-solids. Now, as it is the amount of bile-solids which indicates the quantity of secretion, rather than that of fluid bile, which is dependent chiefly on the presence of water, it may be supposed that the blue pill in this case had affected the liver. The slight increase in the average of bile-solids, however, is evidently due to the large amount collected on July 14, viz., 8.09 grammes. But it will be seen that in Table IV, after six days' rest and abstinence from mercury, the amount of bile was increased to 231.9 fluid and 7.55 grammes of solid bile.

Table IV.—After an interval of six days, collections were again made under the influence of pil. hydrargyri, but the dog had become weaker and thinner. Extraordinary variations occurred in the amount of bile obtained during the six following days, which variations, however, were not dependent on alterations in the food or in the mercury given, but on the accidental circumstance that bile was separated by the kidneys, owing to its free exit by the fistula having been interfered with. On the first of these days, for instance, July 23, a day on which no mercury was given, he secreted a much larger quantity of bile than he had ever done before. No results as to the influence of mercury on the secretion of bile, therefore, can be derived from the observations recorded in Table IV.

Tables V and VI give the results obtained before and after giving mercury on a half-bred Collie Dog that weighed 15.6 kilogrammes. As soon as the fistula was healed, bile was collected for seven days to ascertain the average, the animal being quite healthy. This was—for fluid bile, 82.36; for bile-solids, 5.31; and for bile-salts, 1.042. Calomel was then given internally daily, in varying doses, but the secretion of bile diminished, and the animal's strength and appetite rapidly declined. On the fifth day the bile was lost, and on the sixth the amount was very small. The average quantity on the four days it was collected was 60.02. The calomel given in this case evidently injured the health of the animal. It was determined, therefore, in the next case to try minute and frequently repeated doses.

The results are given in TABLE VII. The observations were made on a Retriever, weighing 12.9 kilogrammes, and they distinctly prove that when so administered mercury exerts no influence on the average amount of bile secreted or on the solids of the bile. It is also to be observed, that although during the second four days the animal took food only once, the amount of bile secreted was on the average nearly the same as during the first four days, when he ate well.

TABLE VIII shows that in a healthy Collie Dog, weighing 19 kilogrammes, the bile was collected for five days to ascertain its average amount, which was 67.1 grammes; solids, 3.5. One-twelfth of a grain of calomel was then given every hour until ten doses had been taken on the first day, and twelve doses on the second day. On the third day 10 grains of blue pill were given. This was repeated after two days' interval, and afterwards the small doses of calomel were again given. During the five days mercury was administered in this way the average amount of bile secreted was diminished nearly one-half—being 35.4; and bile-solids, 1.8.

In the observations tabulated in TABLE IX, made on a Retriever weighing 5.1 kilogrammes, we availed ourselves of the commodious process of subcutaneous injection, employing a solution of corrosive sublimate. The collection of bile was so equal on the three first days, and the dog so healthy, that a more extended average was thought unnecessary. Four-fifths of a grain were injected in the evening, and again in the morning, with the effect of diminishing the secretion of bile at once from 104.7 to 78 grammes. There was also a corresponding diminution of bile-solids. The two doses, however, poisoned the animal, which died on the following day. In the next series of observations, therefore, it was determined to use smaller and gradually increasing doses.

Tables X and XI.—In a remarkably strong Retriever, weighing 27.4 kilogrammes, for which we were indebted to Dr. Kelburne King, of Hull, the bile collections were so equable and perfect, as soon as the fistula was established, that after four days mercury was given. During that time the average quantity of bile secreted was—fluid, 127.15 grammes; solids, 6.43 grammes; salts, 1.03 grammes. The sixth of a grain of corrosive sublimate was first injected below the skin, which was gradually increased as seen in Table XI, and continued for ten days, with the effect of causing some diminution, the average quantity of fluid bile at the end of that period being 113.8 grammes; of solids, 5.972 grammes. On the dose reaching a third of a grain twice daily, the animal's health suddenly failed, and on the following day poisonous symptoms were manifested as in Dog 5, Table IX. The secretion of bile was at once diminished from the average of

113.8, during the previous ten days, to 32.70 and 54.60 grammes, when the mercury was suspended, and the dog slowly recovered.

The observations on this animal were very perfect and apparently conclusive. During ten daily collections, although there were marked variations, such as were observed almost invariably during long-continued collections, the amount of bile, on the whole, was diminished. But when the constitutional action of the drug was unequivocal, it suddenly fell down to one-third its previous amount.

Tables XII and XIII.—Still more conclusive observations were made on a strong mongrel Collie, weighing 19.3 kilogrammes, which recovered rapidly from the operation, and retained its health perfectly during an unusually lengthy series of trials with mercury until its poisonous effects were manifested. As seen in Tables XII and XIII, during eight days that the bile was collected before giving the drug, the average amount was 180.2 grammes fluid bile, 5.96 grammes solids, and 1.07 grammes of salts. As in the last case, corrosive sublimate was given in small doses, gradually increased for twelve days, including two in which a third of a grain was given twice daily before symptoms of poisoning appeared. The average amount of bile secreted during these twelve days was—of fluid bile, 150.19 grammes; of solid bile, 3.374 grammes. There also the secretion suddenly fell during the next two days to an average of one-third of the previous quantity.

The two series of observations on Dogs 6 and 7 were so alike, and were so perfectly carried out, as to prevent all possibility of fallacy. They demonstrate that mercury, acting on the general constitution, has no influence whatever in increasing the secretion of bile, and when pushed to the extent of causing salivation or symptoms of poisoning, greatly diminishes it.

The observations in TABLE XIV were made on a Collie Dog, weighing 16.7 kilogrammes. The collections were perfect for six days, and gave an average of fluid bile, 357.4 grammes; of solid bile, 13.11 grammes; and of salts, 3.12 grammes. Large or purgative doses of blue pill and calomel were then tried; 10 grains of blue pill on the first day, and 10 grains of calomel on three succeeding days. This produced daily purgation. The average of four days shows a reduction of the quantity of bile to 272.67 grammes, and it will be observed that the amount diminishes daily as the purgation increases and continues. The mercury was now omitted, and the amount of bile immediately rose on the first day to 293.9 fluid bile, and on the second to 297.5.

This series of observations is most conclusive as to the influence of large doses of blue pill and calomel which produce purgation. Under their influence there was a steady diminution in the amounts of hepatic secretion, and the moment these doses were suspended the quantity of bile again increased.

In another recent observation on Dog 6, in which on one day 10 grains and on a second 15 grains of blue pill were given, the animal was purged, and the bile was diminished from 173.9 grammes to 119.9 grammes.

In TABLE XV we have a third series of observations on Dog 6, the strong Retriever sent to us from Hull. The object was to determine with more precision the influence of food on the secretion of bile. These observations are not sufficient for the purpose, but, as far as they go, the influence of want of food in diminishing the bile secretion is most marked.

All the preceding observations and Tables would be comparatively useless, unless it could be clearly shown that the dog is as susceptible to mercurial action as man. Although in veterinary and other works it is admitted that this animal may be salivated—although Overbeck states that by means of friction with mercurial ointment he succeeded in producing marked salivation, with spongy gums, in three dogs out of five;* and Murray, in his experiments with large doses of calomel, also produced salivation in one dog†—such was the fundamental importance of this matter, in reference to the present enquiry, that it became necessary to ascertain with exactitude in what manner and to what degree mercury acted constitutionally in these animals. This investigation was undertaken by Dr. William Rutherford, who carried it to a successful termination, as will be seen in Table XVI.

In six dogs, three without and three with biliary fistulæ, a solution of corrosive sublimate was injected below the skin, in gradually increasing doses. They were all poisoned by the drug; and, in five, salivation with the usual symptoms were produced. The post-mortem examination of each is given. It should

* Mercur. und Syphilis, Berlin 1861, pp. 110—114.

† Trans. of Med. and Phys. Society of Bombay, 1841, p. 11.

further be observed that Dog 7, the subject of the trials recorded in Table XIII, was also salivated July 10th, had mercurial fætor of the breath, and ulceration of the gums, from which he never fully recovered.

It may be urged that all investigations in dogs or other of the inferior animals can never clear up in a precise manner the therapeutical action of drugs in man. There can be no question that some animals are altogether insensible to remedies which produce powerful effects in others; that different doses are often requisite in them to occasion similar results; and that these may present modifications or peculiarities indicating such distinctions, that no conclusions can be correctly drawn from them. The facts, therefore, demonstrated in Table XVI, viz. that dogs are readily salivated by mercury; that when salivated they present exactly the same symptoms as are shown in the human subject; and that, in poisonous doses, like changes are seen in the dead body, are of great value.

Further, it should be observed that dogs, like men, can be fed on flesh, and on a vegetable or on a mixed diet; and that the latter was used in all the animals experimented on. In this respect they are superior to most others, even to the quadrumana, which, though in conformation most resembling man, are altogether vegetable feeders. So far, therefore, as experiments and exact observation are capable of determining the influence of mercury on the liver, the Committee have no doubt that the dog is superior to all other animals, and that what applies to him is equally applicable to man. On those who object to this reasoning is imposed the task of showing by what other method more exact results are to be obtained. The Committee have given great attention to this question, and have arrived at the most decided opinion, that, in the present state of science, the plan they have adopted is the only one deserving of confidence.

It has been thought that the Committee should have experimented on other animals besides the dog; but, were mercury given to a rabbit, cat, pig, or donkey, it might still be said that the results obtained do not apply to man. Bidder and Schmidt failed to establish biliary fistulæ in cats; and we did not think it worth while to waste time and money in repeating his efforts. In pigs, the bile differs from that of man inasmuch as it contains hyocholic acid and no sulphur, and it may fairly be supposed that mercury might not produce the same influence on it. The donkey has no gall-bladder, and the formation of biliary fistulæ in that animal would therefore be very difficult, if not impossible. We do not see any advantage, then, that could be obtained by experimenting on other animals. On the other hand, the qualitative composition of canine is identical with that of human bile—a circumstance which, combined with the other facts referred to, can leave little doubt that the influence of mercury on both is precisely the same.

The conclusions to be deduced from the preceding observations are many; but only a few of these, bearing upon the normal secretion of the liver and the influence of mercury upon it, need be referred to.

1st. The relation of food to the biliary secretion is not so invariable as previous experimenters, and more especially Bidder, Schmidt, and Arnold appear to think. Thus, on looking at the collections of bile in the healthy animal, before mercury was given, it will frequently be seen, that while eating the same food, and without any other disturbing cause, it was one-half (Tables I and VIII), one-third (Table II), and even one-fifth (Table VII) the quantity collected on previous and subsequent days. Again, it is to be observed that in Table VII, when for four days scarcely any food was taken, the amount of bile secreted was as large as during the four days he ate well. At the same time, there can be little doubt that abstinence from, or considerable diminution of, food, checks the secretion of bile, as shown in Table XV.

2nd. The relation supposed to exist between the amount of biliary secretion and the size or weight of the animal has not been supported by the foregoing observations. For example, Dog 4, Table VIII, secreted 3.5 grammes of fluid bile per kilogramme, and Dog 6, Table X, 4.6 per kilogramme; while Dog 5, Table IX, secreted 20.5 grammes per kilogramme, and Dog 8, Table XIV, secreted 23.4 grammes. Similar variation occurs in the bile-solids. These results show the fallacy of the supposed estimates which have been formed as to the daily average secretion of bile in the human subject.

3rd. Although an animal will live for a certain time without any bile passing into its alimentary canal, it would appear that even when a fistula has been established without accident the health begins to suffer, in periods varying from a few days to a few months. Emaciation comes on, the appetite fails, the excretions assume a peculiarly fœtid odour, and death occurs from inanition. Here much depends on the strength of the dog. Vigorous animals remain apparently well for lengthened periods, although the whole of the bile secreted passes out through the fistula, and the fæces are quite clayey. Dog 6 not only remained healthy during many collections of bile made when mercury was given, but at its conclusion had even gained weight. He is now quite recovered from the poisonous effects of the drug, and is made the subject of a new series of experiments (Table XV). At this moment the animal is in perfect health, although the fistula was made April 24th, and no bile has passed into his intestines for upwards of three months. It is such cases that favour the view of Blondlot and Arnold as to the inutility of the bile for the purposes of digestion.

4th. Various circumstances apparently diminish the amount of bile secreted. The chief of these, as shown by the preceding observations, are starvation, diarrhœa, and mercurial poisoning.

As regards starvation, it was observed that great discrepancies existed in different dogs between the amount of food eaten and the quantity of bile secreted. The results recorded in Table XV, if not conclusive, leave little doubt in the minds of the Committee that deprivation of food, and probably the kind of food, have, as might be expected, a most important influence on the biliary secretion.

Spontaneous diarrhœa was observed on several occasions to diminish the flow of bile. Thus, in Table XII, the bile was lessened from 241.6 grammes to 200 grammes in one day, and to 124 grammes on the following one, apparently from this cause. On another occasion (Table XIV) it was in like manner lessened from 379.1 to 282.7 grammes. Table XIV indicates that purgation caused by calomel is followed by diminished bile secretion. It may be inferred, therefore, that so far from purgatives acting as a stimulant to the liver, and increasing the flow of bile, they have a directly opposite effect. This conclusion requires to be confirmed by direct experiments.*

The influence of unequivocal poisoning and depression of health, as the result of giving mercury, on the secretion of bile, is well shown in Tables IX, XI, and XIII. In Table IX the amount of bile at once came down from 104.7 to 78 grammes; in Table XI, from the average of 113.8 grammes to 32.70 on the first day, and 54.60 grammes on the second day; in Table XIII it came down from the average, 150.19 grammes, on the first day, to 74.20, and on the second to 17.50 grammes.

It is very possible that other physical causes influence the flow of bile, more especially temperature. The idea that warm climates increase the amount of bile, and predispose to bilious diseases, is prevalent in the East; and a few facts observed by the Committee support the idea that cold checks the secretion. This, however, requires to be determined by direct experiment.

5th. As to anything that enables us to increase the amount of bile, beyond the giving food and supporting health, we are unacquainted with it. Perhaps there is no opinion in Medicine more widely spread, and certainly there is none more universally acted upon, than

* Since this abstract was read at Oxford, further investigations by the Committee with Podophyllin, incorporated in the Report read to the British Association at Norwich, establish the accuracy of this conclusion.

that mercury does so; in short, that it acts as a cholagogue. Yet not only have the few experimenters who have directed their attention to this subject invariably observed that mercury rather diminishes than increases the secretion of bile, but the general results of the trials made by your Committee fully confirm this conclusion. We have seen that in whatever form or dose it may be given, such as continuous moderate doses of blue pill, minute and frequently-repeated doses of calomel, or large doses varying from 10 to 15 grains, it utterly fails to stimulate the liver. Its constitutional action has been excited slowly and rapidly by means of corrosive sublimate with a like result. In poisonous doses it produces a marked diminution in the flow of bile. In some efforts, continued for a considerable time, mercurial inunction was attended by an entirely negative result. In all these varied attempts, carefully repeated, under every varying circumstance that could be thought of, no evidence was obtained that mercury acted specially upon the liver at all. The exact measurement of all the bile secreted in eight dogs, first without and then with mercury, tends rather to show, that so far from increasing the flow of bile, it causes its diminution, through its general depressing action on the entire organism. This fact seems now to be so certain and thoroughly established, that the Committee consider it unnecessary to make any further researches on the subject.

Some attempts have been made to ascertain the history of an opinion which for so long a period has exercised such a powerful influence on medical practice. But the literary researches of the Committee on this head have not been very extensive. So far as they have been enabled to determine, it originates in some vague statement made by Paracelsus, or the authors of his time, as to the effects of mercury in what he has called 'icteritia.' On this subject, however, the Committee invite the co-operation of the literary members of the Association.

It may be imagined that calomel, blue pill, or other preparations of mercury, when taken into the stomach instead of a constitutional, may possess a purely local action, and stimulate the duodenal orifice of the common choledic duct. Occasionally the insertion of the canula into the fistulous opening, that is, into the gall-bladder, was followed by a large collection of bile. (See Tables I, IV, V, XII, and XIV.) But frequently an opposite result was obtained (see Tables II, VII, and XI), so that little importance can be attached to supposed irritation. Besides, the introduction of the canula by rendering the fistulous opening more patent, offers a ready explanation of why the flow should be occasionally increased on the first or second day.

Again, it may be supposed that mercurials possess some specific power of exciting the liver topically through the nerves, similar to what pyrethrum exerts on the salivary glands. Such assumptions have no facts for their support. They appear to be negatived by the recent experiments of the Committee, recorded in Tables X to XIII inclusive. In them the common duct was simply tied and divided, with little injury to the neighbouring parts, and the same diminution followed the administration of the drug, as in the earlier experiments, in which a considerable portion of the ducts was excised, with great destruction to the tissues. In the first dogs operated on also, great shock followed the operation, a result which was entirely avoided in the last cases. In Dog 6 the parts around the common bile ducts were dissected after death, and the nerves proceeding from the solar plexus to the liver were found at some distance from it, uninjured, although a fistula had been established. Further, Heidenhain has found that after division of the common bile-ducts in rabbits and guinea-pigs, it is possible to influence the biliary secretion by irritating the spinal cord.* This could only have been effected through the nerve filaments proceeding from the solar plexus to the liver, which do not appear to be very liable to injury on dividing the common bile-duct.

It is unnecessary to dwell upon the importance of the results which the Committee have taken so much pains to arrive at. If the refutation of a widespread error be as important as the establishment of a new truth, the practical advantage of demonstrating that mercury is not a cholagogue cannot be too highly estimated. Although in recent times the administration of mercurials for hepatic diseases has greatly diminished, their employment is still very general, and in India almost universal. Recent cases demonstrate that long-continued salivation and great loss of health have been produced in the attempts to remove old abscesses or other chronic diseases of the organs. There are few of its lesions in which it is still not thought advisable to try small or full doses of the drug.

On this subject, however, it is unnecessary to dwell

* Studien des Physiologischen Instituts zu Breslau, 1868, Heft 4.

at present. The real question is, whether the evidence brought forward is satisfactory, or whether further researches are necessary. On this and many other topics connected with therapeutics, what we require are not unfounded assumptions and vague speculations, but positive knowledge based on unquestionable data.

It would be vain attempting to convey an adequate idea of the great labours, wearisome repetition of observations, numerous disappointments, and loathsome manipulations, which have tested the zeal, endurance, and courage of Drs. Rutherford and Gamgee, on whom the entire labour of the experiments devolved. The difficulties and expense have been greatly increased by the want of a proper locality for carrying on such investigations, and by the necessity of combating the well-meaning but, we humbly think, mistaken notions of those who maintain that physiologists are not justified in experimenting on animals, even with the objects of determining more accurately the use of poisonous drugs and of preserving the life of man. On the other hand, our warmest thanks are due to Mr. Nunneley of Leeds, to Dr. Kelbourne King of Hull, and Dr. Andrew Buchanan of Glasgow, for their kind assistance in forwarding animals to us.

Scientific investigation by the careful accumulation of exact facts must always be a tedious and laborious process. If, however, our advance in this direction be sure as well as slow, if the method of enquiry pursued is one which merits confidence and communicates to the science of therapeutics something of a certain and less of a conjectural character, I trust no one will regret the time, labour, and expense of our operations.

Equivalent Weights { 1 gramme = 15·434 grains.
28·34 grammes = 1 ounce.
1 kilogramme = 2·2 pounds.

TABLE I.

First Series of Observations on Dog 1. Daily amount of Bile secreted without Mercury.

1	2	3				4			5			6			7
Date.	Weight of Dog.	Amount of Food in Grammes.				Quantity of Bile secreted in 24 hours.			For each Kilogramme of Dog there were secreted			For each 100 Grms. of dry food there were secreted			
	Kilos.	Water.	Milk.	Bread.	Meat.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	
1867.						Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	
June 11.	18·5	None	567	170·1	283·5	150·4	8·843	1·221	8·12	0·478	0·066	65·5	3·85	0·53	
,, 12.		,,	,,	,,	,,	125·0	8·400	1·355							
,, 15.		,,	,,	,,	,,	75·7	5·791	0·836	4·09	0·313	0·045	32·9	2·52	0·36	
,, 17.		,,	,,	,,	,,	121·8	6·954	1·280							
,, 18.		,,	,,	,,	,,	115·0	7·290	1·345							
,, 19.		,,	,,	,,	,,	130·7	8·456	1·520							
Mean	.	.	.	.	.	119·76	7·622	1·259	6·47	0·412	0·068	52·18	3·32	0·548	

Note.—The amount of dry food consumed daily during the above period amounted to 229·5 grammes.
,, ,, ,, for each kilogramme of dog amounted to 12·3 grammes.

In columns 5 and 6 the maximum, minimum, and mean quantities only are calculated. The last, however, are estimated from the mean quantities of column 4.

TABLE II.

SECOND SERIES OF OBSERVATIONS ON DOG 1. DAILY AMOUNT OF BILE SECRETED WITHOUT MERCURY.

1	2	3				4			5			6			7
Date.	Weight of Dog.	Amount of Food in Grammes.				Quantity of Bile secreted in 24 hours.			For each Kilogramme of Dog there were secreted			For each 100 Grms. of dry food there were secreted			
	Kilos.	Water.	Milk.	Bread.	Meat.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	
1867.						Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	
June 29.	16·8	None.	567	170·1	283·5	106·2	3·865	1·15							
„ 30.		„	„	„	„	148·0	Not det.	Not det.							
July 1.		„	„	„	„	117·5	4·31	1·116							
„ 2.		„	„	„	„	185·1	6·60	2·010	11·01	0·392	0·119	80·6	2·87	0·87	
„ 3.		„	„	„	„	81·6	2·978	0·824	4·85	0·177	0·049	35·5	1·29	0·35	
„ 4.		„	„	„	„	149·5	5·80	1·615							
Mean	.	.	.	.	.	131·31	4·71	1·343	7·82	0·28	0·079	57·21	2·05	0·58	

NOTE.—The amount of dry food consumed daily during the above period amounted to 229·5 grammes.
„ „ „ for each kilogramme of dog amounted to 13·6 grammes.

In columns 5 and 6 the maximum, minimum, and mean qualities only are calculated. The last, however, are estimated from the mean quantities of column 4.

TABLE III.

THIRD SERIES OF OBSERVATIONS ON DOG 1. DAILY AMOUNT OF BILE SECRETED WHEN PIL. HYDRARGYRI WAS GIVEN.

1	2	3				4			5			6			7
Date.	Weight of Dog.	Amount of Food in Grammes.				Quantity of Bile secreted in 24 hours.			For each Kilogramme of Dog there were secreted			For each 100 Grms. of dry food there were secreted			Amount of Pil. Hydrargyri given.
	Kilos.	Water.	Milk.	Bread.	Meat.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	
1867.						Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	
July 9.	15	None.	567	170·1	283·5	..	..	..	..	..	..	..	..	..	5 Grains.
,, 10.	,,	,,	,,	,,	,,	100	4·27	1·10	..	..	..	..	..	..	Do.
,, 11.	,,	,,	,,	,,	,,	..	..	..	..	..	.	..	..	..	Do.
,, 12.	,,	,,	,,	,,	,,	89	3·729	1·023	..	..	..	..	..	..	Do.
,, 13.	,,	,,	,,	,,	,,	170·6	6·19	1·74	..	..	..	..	..	..	Do.
,, 14.	,,	,,	,,	,,	,,	204·0	8·09	2·29	13·6	0·39	0·152	88·8	3·52	0·99	Do.
,, 15.	,,	,,	,,	,,	,,	139·0	5·86	1·52	..	..	..	..	..	..	Do.
,, 16.	,,	,,	,,	,,	,,	127·6	5·13	1·50	..	..	..	..	..	..	Do.
,, 17.	,,	,,	,,	,,	,,	67·0	2·86	0·73	4·46	0·19	0·042	29·19	1·246	0·31	
Mean	.	.	.	.	.	127·60	5·16	1·38	8·50	0·344	0·09	55·6	2·24	0·601	

NOTE.—The amount of dry food consumed daily during the above period amounted to 229·5 grammes.
,, ,, ,, for each kilogramme of dog amounted to 15·27 grammes.

In columns 5 and 6 the maximum, minimum, and mean quantities only are calculated. The last, however, are estimate[1] from the mean quantities of column 4.

TABLE IV.

FOURTH SERIES OF OBSERVATIONS ON DOG I. DAILY AMOUNT OF BILE SECRETED WHEN PIL. HYDRARGYRI WAS GIVEN.

1	2	3				4			5			6			7
Date.	Weight of Dog.	Amount of Food in Grammes.				Quantity of Bile secreted in 24 Hours.			For each Kilogramme of Dog there were secreted			For each 100 Grms. of dry food there were secreted			Amount of Pil. Hydrargyri given.
	Kilos.	Water.	Milk.	Bread.	Meat.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	
1867.						Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	
July 23.	15	None.	846	282	225·6	231·9	7·55	Lost.	..	..	..	..	..	..	
" 24.	"	"	"	"	"	56·7	1·61	Lost.	..	..	..	..	..	..	5 Grains.
" 25.	"	"	"	"	"	95·0	2·86	0·931	..	..	..	..	..	..	Do.
" 26.	"	"	"	"	"	49·1	1·73	Lost.	..	..	..	..	..	..	Do.
" 27.	"	"	"	"	"	38·2	1·41	0·443	..	..	..	..	..	..	Do.
" 28.	"	"	"	"	"	175·0	4·88	1·66	..	..	..	..	..	..	Do.
" 29.	14·9	"	"	"	"	69·3	3·64	9·691	..	..	..	..	..	..	Do.

NOTE.—This Table is given to show the remarkable variations in the amount of bile collected daily; which variations, however, were not dependent on alterations in the food, or mercury given, but on the accidental circumstance that bile passed out in the urine, owing to its free exit by the fistula having been interfered with.

TABLE V.

First Series of Observations on Dog 2. Daily amount of Bile secreted without Mercury.

1	2	3				4			5			6			7
Date.	Weight of Dog.	Amount of Food in Grammes.				Quantity of Bile secreted in 24 Hours.			For each Kilogramme of Dog there were secreted			For each 100 Grms. of dry food there were secreted			
	Kilos.	Water.	Milk.	Bread.	Meat.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	
1867.						Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	
Sept. 21.	15·6	None.	564	None.	None.	130	7·29	1·41	8·333	0·467	0·09	23·0	12·9	2·51	
„ 22.		„	564	„	„	81	4·48	0·907							
„ 23.		{Not ac ment any	curately in book food ta	noted. is 'scar ken.'	State- cely}	94·15	6·072	1·205							
„ 24.		None.	225·6	None.	None.	94·70	5·750	1·01							
„ 25.	15·6	„	282	„	310·2	78·80	5·92	1·25	..	..	..	73·9	5·60	1·18	
„ 26.		„	197·4	„	225	62·50	5·70	1·08							
„ 27.		Has on	ly taken	a little	milk.	35·50	1·99	0·436	2·27	0·12	0·027				
Mean for six days . .						82·36	5·31	1·042	5·27	0·34	0·066				

Note.—On 25th September the dry food consumed amounted to 105·7 grammes, or 6·7 grammes per kilo. of Dog.
„ 21st September „ „ „ „ 56·4 „ 3·6 „ „

TABLE VI.

SECOND SERIES OF OBSERVATIONS ON DOG 2. DAILY AMOUNT OF BILE SECRETED WHEN CALOMEL WAS GIVEN.

1	2	3				4			5			6			7
Date.	Weight of Dog.	Amount of Food in Grammes.				Quantity of Bile secreted in 24 hours.			For each Kilogramme of Dog there were secreted			For each 100 Grms. of dry food there were secreted			Amount of Calomel given.
	Kilos.	Water.	Milk.	Bread.	Meat.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	
1867.						Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	
Sept. 28.	14·3	None.	846	None.	None.	61·6	3·54	0·67	4·307	0·24	·046	72·8	4·18	0·79	2 grains three times in the day = 6 grains.
„ 29.	„	„	253·8	„	112·8	7·3	..	..	..	..	..	..	..	..	2 grains in the day = 2 grains.
„ 30.	„	„	141·0	„	253·8	85·7	5·69	1·11	5·99	0·39	0·07	164·2	10·9	2·12	2 grains three times in the day = 6 grains.
Oct. 1.	„	„	253·8	„	None.	65·5	3·76	0·85	..	..	..	..	..	..	2 grains four times in the day = 8 grains.
„ 2.	„	„	282	„	„	Lost.	..	..	..	..	..	..	..	..	2 grains six times in the day = 12 grains.
„ 3.	12·24	„	None.	„	„	2·2	..	..	..	..	..	..	..	..	2 grains twice in the day = 4 grains.
Mean quantity during first four days .						60·02	..	..	..	..	..	..	..	..	

NOTE.—On 28th September the dry food consumed amounted to 84·6 grammes, or 5·9 grammes per kilo. of Dog.
„ 30th „ „ „ „ 77·55 „ 5·30 „ „

TABLE VII.

OBSERVATIONS ON DOG 3. DAILY AMOUNT OF BILE SECRETED BEFORE AND AFTER CALOMEL WAS GIVEN.

1	2	3				4			5			6			7
Date.	Weight of Dog.	Amount of Food in Grammes.				Quantity of Bile secreted in 24 hours.			For each kilogramme of Dog there were secreted			For each 100 Grms. of dry food there were secreted			Amount of Calomel given.
	Kilos.	Water.	Milk.	Bread.	Meat.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	
1867.						Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	
Oct. 26.	12·9	None.	564	None.	338·4	91	4·83	1·19	..	..	..	..	..	..	Before calomel was given.
„ 27.		„	„	„	„	111	6·14	1·26	8·60	0·476	0·097	78·7	4·354	0·839	
„ 28.		„	„	„	„	22	1·16	0·27	1·70	0·080	0·002	15·6	0·822	0·191	
„ 29.		„	„	„	None.	58·5	3·04	0·60	..	..	..	..	..	..	
Mean of the above Observations			564	..	253·8	70·62	3·792	0·83	5·47	0·293	0·064	58·9	3·165	0·692	
Oct. 30.	12·2	None.	None.	None.	None.	36	1·88	0·45	2·96	0·154	0·036	..	..	..	Seven pills, each containing $\frac{1}{12}$th of a grain of calomel, were given one at a time, with an hour's interval between each.
„ 31.		„	„	„	„	89·9	4·79	1·15	..	..	..	..	..	..	Seven pills as above.
Nov. 1.		„	„	„	338·4	108·5	5·76	1·36	8·89	0·472	0·111	..	..	..	Fourteen pills as above.
„ 2.		„	„	„	None.	46·9	2·51	0·6	..	..	..	..	..	..	Six pills as above.
Mean of the Second Series of Observations						70·32	3·732	0·89	5·76	0·305	0·072				

NOTE.—On October 26th, 27th, and 28th, the dry food consumed amounted to 141 grammes, or 10·9 grammes per kilo. of Dog.
The mean quantity consumed daily on the first four days amounted to 119·8 „ 9·13 „ „

The amount of food consumed when the mercury was given was so small—taken on one day only—that no calculation has been made from it.

TABLE VIII.

OBSERVATIONS ON DOG 4. DAILY AMOUNT OF BILE SECRETED BEFORE AND AFTER CALOMEL AND PIL. HYDRARGYRI WERE GIVEN.

1	2	3				4			5			6			7
Date.	Weight of Dog.	Amount of Food in Grammes.				Quantity of Bile secreted in 24 hours.			For each Kilogramme of Dog there were secreted			For each 100 Grms. of dry food there were secreted			Amount of Mercury given.
	Kilos.	Water.	Milk.	Bread.	Meat.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	
1867.						Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	
Nov. 3.	19	338·4	282	112·8	338·4	71·2	3·84	0·85	..	..	..	..	..	..	Before mercury was given.
„ 4.		„	„	„	„	61·9	3·34	0·81	..	..	..	..	..	..	
„ 5.		„	564	56·4	„	88·9	4·78	1·14	4·67	0·251	0·06	50·85	2·703	0·652	
„ 6.		„	282	„	„	72·0	3·80	0·90	..	..	..	..	..	..	
„ 7.		„	„	„	„	41·5	2·23	0·514	2·18	0·117	0·027	28·308	1·521	0·350	
Mean . .		338·4	338	78·96	338·4	67·1	3·592	0·842	3·53	0·146	0·044	40·13	2·148	0·503	
Nov. 8.	18·3	338·4	282	56·4	338·4	*76·0	4·13	1·01	4·15	0·225	0·055	51·84	2·817	0·688	10 pills given—each containing 1-12th grain of calomel. One pill every hour.
„ 9.		„	„	„	„	*47·0	2·54	0·56	..	..	..	..	..	..	12 pills as above.
„ 10.		„	„	„	„	*12·5	0·06	0·01	0·68	0·003	0·0005	8·525	0·048	0·006	10 grains pil. hydrargyri given.
„ 11.		„	„	„	„	45·0	2·41	0·54	..	..	..	..	..	..	No mercury given.
„ 12.		„	„	„	„	58·0	3·07	0·754	..	..	..	..	..	..	No mercury given.
„ 13.		„	„	„	„	*24·0	1·33	0·34	..	..	..	..	..	..	10 grains pil. hydrargyri given.
„ 14.		„	„	„	„	*17·8	1·00	0·26	..	..	..	..	..	..	9 pills, each containing 1-12th grain calomel given. One pill every hour.
„ 15.		„	„	„	„	45·0	2·40	Lost	..	..	..	..	..	..	No mercury.
Mean of the five days (marked *) on which Mercury was given.		338·4	282	56·4	338·4	35·46	1·812	0·436	1·93	0·099	0·023	24·18	1·23	0·29	

NOTE.—The average amount of dry food consumed during the first period amounted to 167·2 grms., or 8·8 grms. per kilo. of Dog.
„ „ „ „ 146·6 „ 8·01 „ „

TABLE IX.

OBSERVATIONS ON DOG 5. DAILY AMOUNT OF BILE SECRETED BEFORE AND AFTER CORROSIVE SUBLIMATE WAS GIVEN.

1	2	3				4			5			6			7
Date.	Weight of Dog.	Amount of Food in Grammes.				Quantity of Bile secreted in 24 Hours.			For each Kilogramme of Dog there were secreted			For each 100 Grms. of dry food there were secreted			Amount of Corrosive Sublimate given.
	Kilos.	Water.	Milk.	Bread.	Meat.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	
1868.						Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	
March 9.	5·1	None.	567	113·4	None.	105	4·158	0·934	20·58	0·815	0·183	142·3	11·05	2·48	Four-fifths of a grain of corrosive sublimate were given at 1 o'clock p.m., immediately after the collection of bile was made, on the 11th, and another dose of four-fifths was given at 9 o'clock a.m. on the 12th, and the last collection of bile was made at 1 p.m. on the same day.
,, 10.	,,	,,	141·6	None.	,,	106·5	4·313	0·969	20·88	0·845	0·190	748·0	30·4	6·84	
,, 11.	,,	,,	56·7	,,	226·8	104·7	4·062	0·942	20·52	0·796	0·184	167·8	6·9	1·51	
,, 12.	4·98	,,	850·5	56·7	None.	78	3·178	0·717	15·67	0·638	0·142	65·5	2·67	0·60	

NOTE.—On March 9th the dry food consumed amounted to 73·7 grammes, or 14·45 grammes per kilo. of Dog.
,, 10th ,, ,, ,, 14.16 ,, 2·776 ,, ,,
,, 11th ,, ,, ,, 62·37 ,, 12·22 ,, ,,
,, 12th ,, ,, ,, 119·07 ,, 23·9 ,, ,,

TABLE X.

First Series of Observations on Dog 6. Daily amount of Bile secreted before Corrosive Sublimate was given.

1	2	3				4			5			6			7
Date.	Weight of Dog.	Amount of Food in Grammes.				Quantity of Bile secreted in 24 hours.			For each Kilogramme of Dog there were secreted			For each 100 Grms. of dry food there were secreted			
	Kilos.	Water	Milk.	Bread.	Tripe.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	
1868.						Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	
May 30.	27·4	846	564	225·6	1353·6	136·3	6·97	1·22	4·97	0·25	0·044	26·52	1·31	0·23	
„ 31.	„	„	„	„	„	Lost.									
June 1.	„	„	„	„	„	132·8	not det.	not det.							
„ 2.	„	„	„	„	„	114·0	6·04	0·97	4·16	0·22	0·039	21·55	1·139	0·17	
„ 3.	„	„	„	„	„	125·5	6·28	0·91							
Mean .	27·4	846	564	225·6	1353·6	127·15	6·43	1·03	4·64	0·23	0·037	23·99	1·21	0·19	

Note.—During the whole of this period the dry food consumed amounted to 530·1 grammes, or 19·34 grammes per kilo. of Dog.

TABLE XI.

SECOND SERIES OF OBSERVATIONS ON DOG 6. DAILY AMOUNT OF BILE SECRETED WHEN CORROSIVE SUBLIMATE WAS GIVEN.

1	2	3				4			5			6			7
Date.	Weight of Dog.	Amount of Food in Grammes.				Quantity of Bile secreted in 24 hours.			For each Kilogramme of Dog there were secreted			For each 100 Grms. of dry food there were secreted			Amount of Corrosive Sublimate administered.
	Kilos.	Water.	Milk.	Bread.	Tripe.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	
1868.						Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	
June 6.	27·5	846	564	225·6	1353·6	95	4·89	0·90	3·45	0·17	0·03	17·92	0·92	0·16	$\frac{1}{6}$ of a grain.
„ 7.		846	564	225·6	1353·6	104·5	5·14	Notest[d]	..	..	..	..	..	..	Do.
„ 8.		846	564	225·6	677·8	104·7	5·94	0·93	..	..	..	..	..	..	Do.
„ 9.		846	564	225·6	916·5	143·3	4·90	1·21	..	..	..	..	..	..	$\frac{1}{6}$ grain twice in the day.
„ 10.	27·44	846	564	225·6	1128·0	159·6	7·32	1·41	5·82	0·26	0·05	33·6	1·54	0·297	Do.
„ 11.		846	564	225·6	677·8	104·8	5·83	Notest[d]	..	..	..	..	..	..	Do.
„ 13.		846	564	225·6	677·8	114·45	5·21	0·68	..	..	..	..	..	..	Do.
„ 14.		Not accurately noted.				89·0	5·39	0·80	..	..	..	..	..	..	Do.
„ 15.		761·4	564	225·6	296·1	107·1	7·71	0·87	..	..	..	..	..	..	Do.
„ 16.		846	564	225·6	916·5	115·71	7·39	1·13	..	..	..	..	..	..	$\frac{1}{6}$ of a grain in the evening, and $\frac{1}{3}$ of a grain in the morning.
Mean .	27·47	836·4	564	225·6	888·6	113·8	5·972	0·99	4·14	0·217	0·036	27·4	1·44	0·239	
June 17.		846	564	225·6	789·6	32·70	2·62	0·401	..	..	..	..	..	..	$\frac{1}{3}$ grain twice in the day.
„ 18.	27·6	789	564	None.	70·5	54·60	2·93	0·40	1·97	0·10	0·014	73·6	3·01	0·56	$\frac{1}{3}$ of a grain.
															Total quantity, 4 grains.

NOTE.—On June 6th the dry food amounted to 530·1 grammes or 19·27 grammes per kilo. of Dog.
„ 10th „ „ 473·8 „ 17·20 „ „
„ 18th „ „ 73·9 „ 2·67 „ „
Mean amount of dry food consumed during the whole period . . 380·68 „ or 13·8 „ „

TABLE XII.

FIRST SERIES OF OBSERVATIONS ON DOG 7. DAILY AMOUNT OF BILE SECRETED WITHOUT MERCURY.

1	2	3				4			5			6			7
Date.	Weight of Dog.	Amount of Food in Grammes.				Quantity of Bile secreted in 24 hours.			For each Kilogramme of Dog there were secreted			For each 100 Grms. of dry food there were secreted			Observations.
	Kilos.	Water.	Milk.	Bread.	Tripe.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	
						Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	
June 18.	19·3	850	567	226·8	453·6	263	8·28	0·89	13·62	0·429	0·046	145	3·56	0·49	At the time of commencing observations, Dog was in perfect health. Fæces quite white and semi-solid.
,, 19.		,,	,,	,,	,,	241·6	6·76	1·40							
,, 20.		,,	,,	,,	,,										
,, 21.		,,	,,	,,	,,	200·0	5·34	1·30	..	..	..	..	..	..	Dog does not look very well. A little diarrhœa.
,, 22.		,,	,,	,,	,,	124·0	7·34	1·22	..	..	..	..	..	..	Diarrhœa has ceased. Dog looks well.
,, 23.		,,	,,	,,	,,	160·5	6·24	1·22							
,, 24.		,,	,,	,,	,,	158·6	5·12	0·77							
,, 25.		,,	,,	,,	,,	113·7	5·25	Accidentally lost.	5·37	0·272	..	62·71	2·89	..	
,, 26.	19·7	676·8	846	225·6	676·8	180·3	3·40	0·70							
Mean . .	19·5	763·4	706·5	226·2	565·2	180·2	5·96	1·07	9·24	0·305	0·054	51·84	1·714	0·307	

NOTE.—The amount of dry food consumed daily from June 18th to June 25th amounted to 181·3, or 9·3 grms. per kilo. of Dog.
,, ,, ,, on June 26th amounted to 389·06 grammes, or 19·74 ,, ,,

TABLE XIII.

SECOND SERIES OF OBSERVATIONS ON DOG 7. DAILY AMOUNT OF BILE SECRETED WHEN CORROSIVE SUBLIMATE WAS GIVEN.

1	2	3				4			5			6			7
Date.	Weight of Dog.	Amount of Food in Grammes.				Quantity of Bile secreted in 24 hours.			For each Kilogramme of Dog there were secreted			For each 100 Grms. of dry food there were secreted			Amount of Corrosive Sublimate given.
	Kilos.	Water.	Milk.	Bread.	Tripe.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	
1868.						Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	
June 27.	19·70	676·8	846	225·6	676·8	172·2	4·30	1·60	..	..	..	..	..	..	$\frac{1}{6}$ grain twice in the day.
,, 28.	,,	676·8	846	225·6	676·8	137·0	3·20	1·10	..	..	..	..	..	..	Do.
,, 29.	,,	676·8	846	225·6	676·8	142·8	3·55	1·02	..	..	..	..	..	..	Do.
,, 30.	,,	846·0	846	225·6	676·8	162·7	4·23	1·31	..	..	..	..	..	..	Do.
July 1.	,,	846·0	846	225·6	None	113·3	2·09	0·69	..	..	..	..	..	..	Do.
,, 2.	19·82	1071	846	225·6	451·2	179·47	4·77	0·879	9·06	0·24	0·044	53·9	1·43	0·264	Do.
,, 3.	,,	1241	846	225·6	366·6	147·10	3·30	1·381	..	..	..	..	..	..	Do.
,, 4.	,,	1241	846	225·6	141·0	145·00	2·68	1·19	..	..	..	..	..	..	Do.
,, 5.	,,	1241	338·4	54·6	239·7	145·0	2·68	1·19	..	..	..	..	..	..	$\frac{1}{6}$ grain in the evening, $\frac{1}{3}$ grain in the morning.
,, 6.	19·59	1241	846	225·6	239·7	157·2	2·11	0·48	8·02	0·107	0·024	56·19	0·75	0·171	Do.
,, 7.		1241	846	225·6	423	153·0	3·67	1·346	..	..	..	..	..	..	$\frac{1}{3}$ grain twice in the day.
,, 8.		1241	308·9	183·3	56·4	147·51	4·115	1·44	..	..	..	..	..	..	Do.
Mean .	19·7	1019	754·4	207·8	420·4	150·19	3·374	1·135	7·62	0·171	0·057	49·2	1·105	0·372	
July 9.		1241	126·9	42·3	None	74·20	2·04	0·570	..	..	..	..	..	..	Do.
,, 10.	16·10	Has ta	ken a lit	tle milk.	42·3	17·50	0·133	0·05	1·086	0·008	0·003	..	..	..	$\frac{1}{3}$ grain.
															Total quantity, 6 grains.

NOTE.—On July 2nd the dry food consumed amounted to 332·76 grammes, or 16·70 grammes per kilogramme of Dog.
,, 6th ,, ,, 279·8 ,, 14·2 ,, ,,
,, 10th salivation and ulceration of gums appeared.

TABLE XIV.

OBSERVATIONS ON DOG 8. DAILY AMOUNT OF BILE SECRETED BEFORE AND AFTER MERCURY WAS GIVEN.

1	2	3				4			5			6			7
Date.	Weight of Dog.	Amount of Food in Grammes.				Quantity of Bile secreted in 24 hours.			For each Kilogramme of Dog there were secreted			For each 100 Grms. of dry food there were secreted			Observations.
	Kilos.	Water.	Milk.	Bread.	Liver.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	
1868.						Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	
July 22.	16·7	566	566	226·4	906·8	412	16·06	3·21	24·67	0·961	0·192	102·7	4·00	0·800	Dog in excellent health.
„ 23.	16·6	„	„	„	„	331·8	13·20	2·91	..	..	..	..	..	..	„ „
„ 24.	16·4	367·9	226·4	„	„	375·4	9·20	3·26	..	..	..	..	..	..	„ „
„ 25.	16·2	566	566	„	„	379·1	14·78	3·07	..	..	..	..	..	..	„ „
„ 26.	16·0	„	„	„	665·9	282·7	12·80	2·68	17·66	0·80	0·167	81·8	3·74	0·775	Decided diarrhœa.
„ 27.	..	..	..	..	..	Lost.									
„ 28.	16·3	„	„	212·2	906·8	363·7	12·62	3·60	..	..	..	..	..	..	Diarrhœa has ceased.
Mean .	16·36	532·9	509·4	224·0	866·6	357·4	13·11	3·12	21·8	0·801	0·190	92·9	3·49	0·811	No mercury given during the above period.
July 29.	15·9	566	495·2	198·1	807·6	390·2	9·59	2·77	23·9	0·603	0·174	110·2	2·7	0·782	10 grains blue pills given 24 hours previous to the collection of bile to-day; slight purgation.
„ 30.	15·7	„	523·5	183·9	906·8	249·7	7·71	1·97	..	..	..	..	..	..	10 grains calomel given as above; slight purgation.
„ 31.	..	„	254·7	84·9	424·5	229·0	6·89	1·74	..	..	..	..	..	..	10 grains calomel given as above; decided purgation.
Aug. 1.	15·4	„	566	226·4	566	221·8	6·94	1·77	14·4	0·450	0·114	68·9	2·15	0·54	10 grains calomel given as above; decided purgation.
Mean of the above 4 days.	15·66	566	459·8	173·3	676·2	272·67	7·78	2·06	17·4	0·496	0·131	89·2	2·54	0·674	
Aug. 2.	..	566	396·7	141·7	906·8	293·9	8·17	Lost.	..	..	..	..	..	..	No mercury given.
„ 3.	15·2	„	253·8	„	„	297·5	7·59	2·43	19·4	0·499	0·15	93·3	2·38	0·76	„

NOTE.—From July 22 to July 28 the mean amount of dry food consumed daily was 384·6 grms. or 23·4 grms. per kilo. of Dog.
„ „ 29 to Aug. 1 „ „ „ „ „ „ 305·3 „ 19·23 „ „ „

TABLE XV.

THIRD SERIES OF OBSERVATIONS ON DOG 6. DAILY AMOUNT OF BILE SECRETED BEFORE, DURING, AND AFTER PARTIAL STARVATION.

1	2	3				4			5			6			7
Date.	Weight of Dog.	Amount of Food in Grammes.				Quantity of Bile secreted in 24 Hours.			For each Kilogramme of Dog there were secreted			For each 100 Grms. of dry food there were secreted			Observations.
	Kilos.	Water.	Milk.	Bread.	Tripe.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	Fluid Bile.	Bile Solids.	Bile Salts.	
1868.						Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	Grms.	
July 26.	29·5	311·7	226·7	141·7	1813	140·8	5·04	0·88	4·7	0·170	0·029	25·1	0·89	0·15	Dog in excellent health.
,, 27.	,,	850·2	,,	,,	1728·7	139·9	5·14	1·00	..	..	..	..	..	..	,, ,,
,, 28.	,,	None.	None.	None.	Liver. 453·4	104·8	7·73	1·03	..	..	..	100·5	7·4	0·98	,, ,,
,, 29.	28·6	453·4	,,	,,	None.	41·6	2·77	0·40	1·45	0·096	0·013	..	..	..	,, ,,
,, 30.	..	850·2	226·7	141·7	None.	85·7	5·11	0·87	..	..	..	79·57	4·74	0·808	,, ,,
,, 31.	28·6	425·0	,,	,,	Tripe. 1813·7	173·9	9·35	1·68	6·08	0·326	0·058	31·0	1·66	0·29	,, ,,

NOTE.—On July 26th the dry food amounted to 560·9 grammes, or 19·01 grammes per kilogramme of Dog.
,, 28th ,, ,, 104·2 ,, 3·64 ,, ,, ,,
,, 29th no dry food was given.
,, 30th the dry food amounted to 107·7 ,, 3·7 ,, ,, ,,
,, 31st ,, ,, 560·9 ,, 19·9 , ,, ,,

TABLE XVI.—Effects of Corrosive Sublimate on Six Dogs, without and with Biliary Fistulæ.

DOGS WITHOUT BILIARY FISTULÆ.

Dog 1—Retriever, 12 Months Old, Weight 30½ lbs.

Days.	Amount of Corrosive Sublimate given.	Effects.
		Animal in excellent health—fæces of a light brown colour, semi-solid.
1st day.	$\frac{1}{20}$th gr.	No change.
2d ,,	$\frac{1}{10}$th ,,	,, ,,
3d ,,	$\frac{1}{10}$th ,,	,, ,,
4th ,,	$\frac{1}{10}$th ,,	,, ,,
5th ,,	$\frac{2}{10}$ths ,,	,, ,,
6th ,,	$\frac{3}{10}$ths ,,	,, ,,
7th ,,	$\frac{4}{10}$ths ,,	,, ,,
8th ,,	$\frac{4}{10}$ths ,,	Colour of fæces changed to a very dark brown. Animal in good health.
9th ,,	$\frac{5}{10}$ths ,,	,, ,, ,,
10th ,,	$\frac{11}{20}$ths ,,	,, ,, ,,
11th ,,	$\frac{7}{10}$ths ,,	,, ,, ,,
12th ,,	$\frac{8}{10}$ths ,,	,, ,, ,,
13th ,,	$\frac{8}{10}$ths ,,	,, ,, ,,
14th ,,	$1\frac{6}{10}$ths ,,	,, ,, ,,
15th ,,	$1\frac{6}{10}$ths ,,	Nasal discharge of mucus. No apparent salivation. Fæces as on the 8th day. Appetite unimpaired.
16th ,,	$1\frac{6}{10}$ths ,, 20 mins. tinct. opii.	Nasal discharge unaltered. Slight diarrhœa. Fæces brownish-yellow.
17th ,,	$1\frac{6}{10}$ths ,, 30 mins. tinct. opii.	Nasal discharge less marked. Diarrhœa more decided. Fæces greenish-brown, contain a little blood. Appetite impaired.
18th ,,	$\frac{8}{10}$ths ,,	Salivation — not profuse, however. Fœtid breath. Slight ulceration under margin of tongue.

Dog 2—Collie, 5 Months Old, Weight 18¼ lbs.

Days.	Amount of Corrosive Sublimate given.	Effects.
		Animal in excellent health—fæces solid, brown.
1st day.	$\frac{1}{10}$th grn.	No change.
2d ,,	$\frac{4}{10}$ths ,,	,, ,,
3d ,,	$\frac{4}{10}$ths ,,	,, ,,
4th ,,	$\frac{5}{10}$ths ,,	,, ,,
5th ,,	$\frac{11}{20}$ths ,,	,, ,,
6th ,,	$\frac{7}{10}$ths ,,	,, ,,
7th ,,	$\frac{8}{10}$ths ,,	,, ,,
8th ,,	$\frac{8}{10}$ths ,,	Colour of fæces changed from brown to greenish-brown, appetite impaired.
9th ,,	No mercury. ¼th grain morph. mur. given	Diarrhœa — fæces greenish-yellow.
10th ,,	No merc. 30 mins. tinct. opii given.	Diarrhœa profuse — fæces contain blood. Slight nasal discharge.
11th ,,	No merc. 30 mins. tinct. opii given.	Diarrhœa profuse — fæces of a slate-brown colour. Nasal discharge more abundant. Decided salivation. Gums unchanged.
12th ,,	No medicine given.	Nasal discharge — muco-purulent, very profuse. Salivation less marked than on previous day. Breath fœtid. Diarrhœa has nearly ceased. Animal is constantly trembling — takes almost no food.

Dog 3—Skye Terrier, 15 Months Old, Weight 24½ lbs.

Days.	Amount of Corrosive Sublimate given.	Effects.
		Animal in excellent health—fæces solid, light brown.
1st day.	$\frac{3}{10}$ths grn.	No change.
2d ,,	$\frac{7}{10}$ths ,,	,, ,,
3d ,,	$\frac{8}{10}$ths ,,	,, ,,
4th ,,	$\frac{8}{10}$ths ,,	,, ,,
5th ,,	$\frac{8}{10}$ths ,,	Appetite impaired.
6th ,,	$1\frac{6}{10}$ths ,,	No change.
7th ,,	$1\frac{6}{10}$ths ,, 30 mins. tinct. opii given.	Slight diarrhœa. Fæces brown.
8th ,,	No medicine given.	Profuse salivation. Breath very fœtid. Slight nasal discharge. Slight diarrhœa—fæces brown.
9th ,,	$\frac{8}{10}$ths grn.	No change.
10th ,,	..	Dog found lying in the morning with a stream of saliva flowing from its mouth. Breath very fœtid. Ulcers on the gums, on the side of the tongue, and on the inside of the lips. The ulcers have a dark-grey surface. Slight diarrhœa—fæces contain blood. Dog died in the afternoon with slight convulsions. Weight 21¼ lbs.

19th day.			matous. No sponginess of gums. Profuse lachrymation. Nasal discharge, muco-purulent, profuse. Fæces fluid, bloody. Animal refuses all food. Dog found lying dead, with a stream of colourless fluid on the floor of the cage, which had evidently flowed from its mouth. Animal much emaciated. Weight 22 lbs.	13th day.	. .	Found dead. Weight 14¼ lbs.			
Total Amount of Corrosive Sublimate given.	$12\frac{1}{5}$ grains, given during a period of 18 days.			$4\frac{1}{4}$ grains, given during a period of 8 days.			$7\frac{1}{5}$ grains, given during a period of 9 days.		

APPEARANCES FOUND ON DISSECTION.

Mouth and Salivary Glands.	Tongue pale, slight ulceration under its right margin. Vascularity of salivary glands not increased.	Tongue pale—œdematous. No ulceration. Vascularity of salivary glands not increased.	Tongue covered with a white fur. Ulcers inside lips on gums and below margin of tongue. Vascularity of salivary glands not increased.
Stomach.	Distended by clear fluid tinged with bile. Mucous membrane healthy.	Contained a quantity of partially-digested food. Mucous membrane healthy.	Empty. Mucous membrane healthy.
Intestine.	Marked redness of mucous membrane of duodenum, jejunum, ileum, and large intestine. Small intestine contained fluid similar to that in the stomach.	The mucous membrane of the small intestine was marked with bright red lines and patches from the pyloric orifice of the stomach to the ileo-colic valve. There are patches of lymph on the mucous membrane of the ileum. Large intestine is quite pallid. Duodenum contained some chyme of an orange-yellow colour.	Marked redness of mucous membrane of duodenum. Slight redness of jejenum and ileum. Mucous membrane of large intestine marked with bright red striæ running longitudinally. Duodenum contained a little bile; the large intestine contained some bloody fæcal matter.
Liver and Gall-Bladder.	Engorgement of hepatic vein. Gall-bladder filled with greenish-yellow bile. Hepatic cells apparently normal.	Engorgement of hepatic vein. Gall-bladder filled with orange-yellow bile. Hepatic cells apparently normal.	Same as the preceding.
Other Organs.	Pancreas very vascular. Hypodermic tissue œdematous where the injections had been made. Other organs normal.	Pancreas very vascular. Congestion of lower lobe of right lung. A layer of lymph on the visceral pleura of this lobe. Other organs normal.	Pancreas very vascular. Hypodermic tissue œdematous where the injections had been made. Other organs normal.

TABLE XVI.—(*Continued.*)

Effects of Corrosive Sublimate on Six Dogs, without and with Biliary Fistulæ.

DOGS WITH BILIARY FISTULÆ.

Dog 4—Young Retriever, 6 Months Old, Weight $11\frac{1}{4}$ lbs.

Days.	Amount of Corrosive Sublimate given.	Effects.
		Animal is rather weak. Fæces clay-coloured.
1st day.	$1\frac{3}{5}$ths gr. given in two doses, with 20 hours' interval between them.	Twenty hours after the first dose of 4-5ths of a grain were given, a nasal discharge of mucus was observed. Two hours after the second dose it was observed to be exceedingly weak; it was in a state of constant tremor, and staggered in attempting to walk. Liquid fæces of a clay-colour mixed with blood were passed. Nasal discharge became more decided. No apparent salivation. No fœtor of breath.
2d "	..	Dog found dead thirty-six hours after the first dose was given. Weight 11 lbs. (See Dog 5, Table IX.)
$1\frac{3}{5}$ grain, given during 1 day.		

Dog 5—Bull-Dog, 2 Years Old, Weight $38\frac{3}{4}$ lbs.

Days.	Amount of Corrosive Sublimate given.	Effects.
		Animal is in excellent health. Fæces clay-coloured.
1st day.	$\frac{2}{5}$ths grn.	No change.
2d "	$\frac{4}{5}$ths "	" "
3d "	$\frac{4}{5}$ths "	" "
4th "	$\frac{4}{5}$ths "	" "
5th "	$\frac{4}{5}$ths "	" "
6th "	$\frac{4}{5}$ths "	" "
7th "	$\frac{4}{5}$ths "	" "
8th "	$\frac{6}{5}$ths "	" "
9th "	$\frac{8}{5}$ths "	" "
10th "	2 grains.	" "
11th "	3 "	" "
12th "	2 "	" "
13th "	$4\frac{1}{2}$ "	Appetite impaired. Breath fœtid.
14th "	No mercury given.	Slight nasal discharge. Profuse salivation. Stream of saliva flowing from the mouth. No purging. No blood in fæces, which are of a clay-colour.
15th "	..	Died during the night. Weight $31\frac{1}{4}$ lbs.
$19\frac{1}{2}$ grains, given during a period of 13 days.		

Dog 6—Collie, 18 Months Old, Weight 37 lbs.

Days.	Amount of Corrosive Sublimate given.	Effects.
		Animal is in excellent health. Fæces semi-solid, clay coloured.
1st day.	$\frac{2}{5}$ths grn.	No change.
2d "	1 [illegible]/5ths "	" "
3d "	1 [illegible]/5ths "	" "
4th "	3 grains.	" "
5th "	2 "	" "
6th "	$4\frac{1}{2}$ "	Appetite much impaired.
7th "	6 "	Fæces slate-coloured—semi-solid.
8th "	No mercury given.	Slight nasal discharge. Fœtid breath. Salivation. Saliva dropping from mouth. Ulcers have formed in the skin, where the injections were made: the ulcers are circular, and about the size of half-a-crown. Fæces slate-coloured.
9th "	..	Nasal discharge and salivation more decided. Dog vomits everything it takes. Blood in fæces, which are semi-solid and of a slate-colour.
11th "	..	Died during the night. Weight 30 lbs.
$19\frac{1}{10}$ grains, given during a period of 7 days.		

APPEARANCES FOUND ON DISSECTION.

Nothing abnormal in the appearance of the mouth or salivary glands.	Tongue pale. Gums ulcerated.	Tongue pale and œdematous. No ulceration.
Contained partially-digested food. Mucous membrane healthy.	Contained a large quantity of colourless watery fluid. Mucous membrane healthy.	Contained a large quantity of colourless watery fluid. Mucous membrane healthy.
Marked redness of mucous membrane in the jejunum. Slight redness of that of the large intestine. The latter contained fæces mixed with blood.	Bright red patches on the duodenal and jejunal mucous membrane. Most marked in the former. Mucous membrane of large intestine marked with bright red striæ.	Mucous membrane of small intestine covered with bright red patches from the pyloric orifice of the stomach to the ileo-colic valve; they were most marked in the jejunum. Large intestine marked with longitudinal bright red striæ.
Engorgement of hepatic venous system. Hepatic cells apparently normal.	Same as the preceding.	Same as the preceding.
Other organs normal.	Pancreas very vascular. Small abscesses under the skin in some of the sites of injection. Other organs normal.	Pancreas very vascular. Suppuration under the skin of the whole back and sides, where the injections had been made. Ulcers in the skin. Other organs normal.

42