AN ADDRESS

ON THE

ETIOLOGY OF ACUTE SPECIFIC DISEASES.

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GENTLEMEN,—Nothing but a most imperative duty could have prevented me from presiding at the last meeting of the Society. Now that I have the pleasure of taking this chair, I desire first to thank you for the honour you did me when you elected me your President. The Presidentship of the Clinical Society is, in my estimation, one of the highest honours that can be conferred on a physician. It was not until after I had accepted the nomination that I realised the fact that former Presidents had inaugurated the Session with an address; and, when I realised this, conscious of my own inability for properly discharging that duty, I felt misgivings as to the wisdom of having accepted the honour. I trust to your kindness to-night to overlook my deficiencies.

The Clinical Society was founded for "the cultivation and promotion of the study of practical medicine and surgery by the collection of the reports of cases, especially of such as bear upon undetermined questions in pathology or therapeutics"—a very wide field of inquiry; for, as far as my knowledge extends, there is no disease, even the most common, respecting which there are not undetermined questions in regard to its pathology and its treatment. There is not a single disease, however common, respecting which it can be said that its etiology is quite determined; and no report of a case can clinically be complete, the etiology of which is not given. There is no disease, respecting which it can be said that the symptoms which mark its very outset and its course, the symptoms which mark its decline, the symptoms which enable us to tell at the outset what will be its course, the effects which follow after the disease has apparently ended, and the influence which pre-existing conditions of health and disease, as well as that which drugs and other therapeutic agents exert on its progress, termination, and sequelæ are perfectly known. Regarding all these points in every disease, even the most common, there are undetermined questions. Many of these questions admit of answer; many of them ought to be answered; and that many of them are not answered, is, I think, discreditable to us as a profession. Medicine has made very great progress; and, if it were my aim to-night to illustrate that progress, I could find ample material for doing so. Medicine has made great progress; few sciences have made more; but, still, in regard of every disease, in reference to every one of the points I have mentioned, we are wanting facts—facts which could and ought to be supplied; and it is in the hope that my words may stimulate the Fellows of the Society to assist in the collection of these facts, that I would to-night illustrate our present deficiencies rather than dwell on our past performances.

Let me illustrate my meaning first by a reference to one or two points in regard of the etiology of a class of the commonest and best studied of acute diseases—I mean the acute specific diseases. In reference to these, one may say that they are all contagious; that is, from some part of the sick something is given off which can, when properly applied, excite the same disease in another, and which can excite no other disease. Little is known of what this *something* is; and yet it is most important for the prevention, and even for the treatment, of these diseases that we should know. Little is known of those conditions of system which render this something sufficient to the production of the disease in one person, while in another it is inert. For example, at a certain time one of these diseases is what is called epidemic. Scarcely a person who has not had it and is exposed to the emanations of the sick escapes taking it. On another occasion, the disease is not epidemic, and then many, with equal degrees of exposure, escape. Measles was not epidemic in a certain town at the time to which I am about to refer. A child came from school, having just recovered from measles.

The family consisted of sixteen persons, eight of whom were children, and one only took the disease. Another family at the same period contained nine children. One contracted measles at school, and returned home; only two of the eight suffered; all of them being freely exposed to the poison.

Among the questions which are asked and ought to be answered are: What are the conditions which favour outbreaks of epidemics? Are they atmospheric? And, if so, are they conditions existing at the time of the outbreak, or are they conditions dependent on pre-existing atmospheric conditions? Or are the conditions that favour the outbreak of an epidemic states of health of the population, the consequence of coexisting or pre-existing atmospheric or other conditions? Although some of these questions, it may be thought, refer to subjects outside the scope of clinical medicine, some certainly are within that scope.

As a good illustration of the influence of pre-existing individual conditions in regard of susceptibility to contagious poisons, I may refer to the well-known fact of the intimate relation between the state of the system left after measles and the susceptibility to the poison of whooping-cough. The individual who has just suffered from one of these diseases is, as is well known, prone in a remarkable degree to take the other. That is to say, after the occurrence in an individual of one or other of these diseases, a very small dose of the poison of the other disease is sufficient to produce the development of that disease. And, as bearing on this subject, I may refer to Dr. Aitken's paper on the influence of the previous residence in Bulgaria on the health of the troops in the Crimea.

One point respecting the etiology of these acute diseases has recently been brought before the profession and the public in rather strong language, the question; namely, whether these diseases can arise spontaneously. A controversy has lately—I was going to say raged—about the mode of origin of typhoid fever, and it well illustrates at the same time our ignorance, the difficulty of ascertaining facts, and the danger of reasoning upon insufficient facts, on an insufficient number of facts, on insufficiently observed facts, and the unwisdom of drawing general conclusions from facts which bear only on one side of a question. It is said by some that typhoid fever is undoubtedly contagious; it is said that, being contagious, it can never originate *de novo*. That is to say, as it can be proved in some cases to have its origin in the entrance of the emanations of the sick into the previously supposed healthy, it can never arise in any other way. On the other side, while admitting that the disease may be spread by the emanations from those suffering from the disease, it is said that the admixture of sewage decomposing animal excreta, with fluids used for drinking purposes, although no typhoid excreta are present, will produce typhoid fever *de novo*; that is to say, in those that drink the fluid; and it is said that the inhalation of sewer-gas is sufficient itself to produce in persons previously healthy typhoid fever *de novo*. Before considering briefly some of the arguments that may be adduced in support of any of these statements, I may say that, to my mind, from the known facts, the question should be held to be still *sub judice*, and that it is only to be solved by the combined efforts of those who can investigate the origin of typhoid fever when it occurs as an epidemic or in the form of severe local outbreak, and of those practitioners who, being engaged in private practice in country districts, can examine into and report on the solitary cases occurring under circumstances favourable for excluding fallacies. It is rarely that solitary cases can be satisfactorily investigated in towns; and it is because this important question can only thus be solved, that I desire to impress on the members of this Society the importance of reporting every solitary case which can throw light on the question. We have already had, and, no doubt, shall have, many more ably described epidemics, severe local outbreaks, and local outbreaks on a smaller scale. We shall have these traced to their origin by men of the highest order of intellect and the largest possible experience. We have had such reported now in numerous cases; and, as the points that still require elucidating in these local outbreaks become better known, these men will be sure to discover the missing links; but no investigation of such local outbreaks can answer completely the question which I say is still *sub judice*; and, therefore it is, that those who see the solitary cases must report their personal experience, if we are to have the question definitely and indisputably answered. I know how difficult it is for men engaged in large private practice to hunt out all the facts bearing on the origin of a single case; but, when they consider of what immense importance it is to the public and to the scientific advancement of our profession, I trust that some of them will be willing to sacrifice a little time for the general good.

One general assumption made by those who advocate the sole origin of typhoid fever by contagion, requires special consideration. This general assumption is this, that no admittedly contagious disease ever originates in any other way than by contact with the emanations from the sick; and therefore that typhoid fever, spreading by

contagion—a mode of spread which I think is now pretty generally admitted by all those who have studied the question—can be spread in no other way. This general assumption is as yet wanting in such proof as conveys conviction to my mind; and I cannot help thinking that it is wanting in such proof as should convey conviction to any unprejudiced mind, accustomed to investigate questions of this nature. My own prejudices are greatly in favour of the specific origin of this in common with all contagious diseases. I have long advocated the specific differences, and held that each had its specific cause; that is, a cause which is unable to produce any other acute specific disease; and if each could, in every case, be referred to emanations from those previously sick of the same disease, it would greatly strengthen the general views I have long held and advocated. I am prejudiced, therefore, in favour of this opinion. I hope it is true; but then I must say that, at this moment, the weight of evidence and of argument is rather on the other side. When an admittedly contagious disease is epidemic, it is evident that facts to elucidate the question of its spontaneous origin cannot be collected. There is so much of the special poison in the locality, that no one can be sure that he has not in some way exposed himself to its influence. But, when the disease is no longer epidemic, when isolated cases occur, then it is that, if a disease never originated *de novo*, we ought—I will not say in every case, but in the majority of cases—if we inquire carefully, to be able to trace back the apparently spontaneous case to its source. Of small-pox, the most contagious of all diseases, I do not hesitate to say, notwithstanding Dr. Budd's assertion to the contrary, that the large majority of solitary cases can be traced with due care each to its probable source; and when a case has been imported into a locality previously free from the disease, new cases almost invariably spring up in its vicinity; so that the contagious origin is proved first, by tracing the case to its source; and, secondly, by the new cases which spring up from the first isolated case. Small-pox is eminently contagious; and not only so, but from the vast proportion of instances in which we can trace isolated cases to their source, from the rarity of the cases in which it is impossible to trace the probable origin to contagion, we may fairly conclude that small-pox never originates *de novo*.

But now let us pass to the other end of the scale of undoubtedly contagious diseases, namely, diphtheria and erysipelas. I mean erysipelas of the head and face, the acute specific disease of which erysipelatous inflammation of the throat and face, beginning usually from the bridge of the nose, is the anatomical character; a disease which is attended with definite symptoms, sometimes before the eruption, and sometimes accompanying the eruption, as are the acute specific diseases. Of the contagious nature of diphtheria and erysipelas, I think there can be no more doubt than there is of the contagious nature of small-pox. They are not so contagious; but they are both contagious. They are not as contagious, for it is more common for the healthy, exposed to the emanations from the sick, to escape from these diseases, than it is for those exposed to the effluvia of small-pox to escape small-pox. Now, while in regard of small-pox, it is the exception not to be able to trace the source of the contagion; in diphtheria, it is comparatively rarely that the first case, even of several, can be traced back to contagion. In regard to erysipelas of the head and face also, undoubtedly, as I say, contagious, it is the exception to be able to trace the case to contagion. Now, it is evident that the more contagious a disease—that is, the less the quantity of poison needed, the shorter the time of exposure to the poison necessary, and the less preparation required in the system of the individual receiving the poison—the less frequently, *a priori*, should we expect that the disease should be traced back to its primary source; while, with the diseases of this class which are the least contagious, which require for their spread a longer exposure or more decided doses of the poison, or the administration of the poison in a particular way by a special channel, we should expect that, in every new case—if every new case were due to exposure to emanations from the sick of the same disease—we should the more easily be able to trace them to their original source, the more easily should the parent case be discoverable. But we know that it is not so; for those which are the most contagious are the most commonly traceable to their origin, and those which are the least contagious can the least frequently be proved to have originated in contagion; and we are driven thus, it seems to me, towards the opinion that erysipelas frequently, and diphtheria not uncommonly, originate *de novo*.

Typhoid fever, like diphtheria and erysipelas, holds a low position in regard to its contagious quality. It rarely spreads, excepting when the excreta from the sick are administered by a particular channel. I have never known a case removed from its place of origin to give the disease to the inmates of a house into which it was removed, unless there were a communication between the source of the drinking fluid and the bowel excreta of the patient. Last autumn, ten young men, students at

Oxford, went to stay in the same house in Cornwall. After a short residence, each returned to his own house, and seven sickened with typhoid fever. These young men evidently contracted the disease in the house in Cornwall. A highly contagious disease, in the ordinary sense of the word, ought to have been the focus from which other cases should have sprung. Had eight young men suffering from small-pox or scarlet fever been distributed into eight houses, I do not hesitate to say that some of the inmates of those houses would have suffered from small-pox or scarlet fever, as the case might be. I have never known a case of small-pox admitted by accident into the hospital to which I have the honour of being physician, without some one or more persons contracting the disease. When formerly I was physician to the Fever Hospital, I took my clinical class to the hospital for the purpose of showing them fever in its various stages on a large scale. Now, while I never knew a student to contract typhoid fever there, it so happened that on one occasion there was a case of small-pox in an outer ward of the hospital—a ward of very large dimensions. While no student took, on any occasion, either typhoid or typhus fever, one on that special occasion took small-pox; and on another occasion, there being several cases of scarlet fever in the ward, one took scarlet fever. But in the case of these young men from Oxford, who were distributed into separate houses, not one conveyed the disease to any inmate of the house to which he was removed. That typhoid fever can spread from one person to another without the swallowing of the excreta, if the person be exposed to the concentrated emanations from the sick frequently and for a considerable time, there can be no doubt. I have twice known students contract the disease who were diligently taking the temperature of the patients sick of typhoid fever, before the registering thermometer was in use, and therefore were obliged on each occasion to put the head many times daily into the bed of the patient. And I doubt not that most of those present could add to the number of these young martyrs to science.

Still typhoid fever has such limited power of propagating itself that the poison must be in large doses, or long breathed, or taken into the stomach, to produce the disease. Therefore I say that the disease ought, in the majority of cases, especially in country places and isolated houses, to be able to be traced to its source, if the source be in every case the excreta or emanations of those suffering from the disease. But, in fact, its origin has not hitherto been able to be traced to pre-existing cases in a very large proportion of the solitary cases observed. But not only do numerous cases occur in which typhoid fever is not traceable to exposure to emanations or excreta from the sick with typhoid fever—that is, to contagion—but there are a sufficient number of cases to render it *probable* that the admixture of sewage not typhoid with drinking water may produce the disease *de novo*; and most of us must have met with cases in which the breathing of greatly diluted sewer-gas continuously for some time, especially at night, has seemed to be able to produce typhoid fever *de novo*. But new cases, sceptically scrutinised and carefully recorded, are required to settle this question. It is only in exceptional instances that cases free from all possible, at least from all probable, sources of error can come under notice; and such cases can only be seen, as I have said, in private practice, especially country practice; and it is for this reason that I especially dwell on the subject to-night, my desire being to urge the careful observation and reporting of these isolated cases.

The following case serves to illustrate the difficulties which beset inquiries such as this. A young lady was under my care with typhoid fever. She had been for some months an invalid, and was so placed that all probable sources of the communication of the disease from the outside could be excluded. She resided in a completely detached villa. She was confined to her own room. There were few inmates of the house, and those were entirely devoted to her service. She had lived in the house about two years, and during that time no one had suffered from typhoid fever. On the floor on which she slept and lived at the time, there was a water-closet well ventilated, used only by two or three persons in perfect health. Now, this young lady was the only person who never left the floor, that is to say, she was day and night breathing air on this floor. A sewer-gas odour was detected, and, on taking up the floor, it was found that there was a crack in the soil-pipe of the water-closet just below the seat, in such a position that the gas escaping from the soil-pipe at this part would find its way between the joints and the boards of the floor, and thus pass into the rooms on this floor. It is true this young lady had received a few visitors in her bedroom, and it is possible, it may be said, that some of them were the subjects of typhoid fever. But the persons who visited her were all well known, and there was no ground for thinking that any one of them suffered before or after from typhoid fever. Again, the drains of the house opened into the drain of the town, on the outskirts of which the villa is situated; but, then, the drains were trapped

and the traps were new and efficient. It is only by the multiplication of solitary cases that proof on the one side or on the other can be obtained.

With reference to these three contagious diseases which are unable to be traced, in many cases, to pre-existing cases of the same disease, that is, typhoid fever, diphtheria, and erysipelas, there are three points worthy of notice. First, they are more liable than the most contagious, as small-pox, measles, and scarlet fever, to recur in the same individual. Secondly, when not referable to contagion, each is pretty constantly referred to a particular and the same cause; for example, diphtheria to exposure to cold; erysipelas to exposure to cold when the exposed person was depressed from fatigue, mental or moral causes, fasting, etc.; typhoid fever to foul gaseous emanations or contaminated water. Thirdly, certain primary constitutions seem more prone than others to suffer from these less contagious diseases. All people seem to be susceptible to scarlet fever, to measles, and small-pox, when exposed to the poisons, but these three diseases occur in certain constitutions especially. We are all acquainted, I am sure, with families that exhibit these proclivities. I have known diphtheria, for example, occur in four members of a family at long intervals of time, and in localities widely separated—one in London, one at Brighton, one at Scarborough, and one at some other place in the country. Now, when we consider the infrequency, comparatively speaking, of diphtheria, such a fact as this shows on the part of the members of the family a great primary constitutional proclivity to the disease. I have known six out of seven males of a family to suffer from typhoid fever, no two of the cases occurring within less than two years' interval, and no two at localities within less than fifty miles of each other, while in the same family one only of the same number of females suffered from typhoid fever. Here, again, there must have been a great proclivity to typhoid fever on the part of the males of the family. I do not say, nor do I think, that the arguments and facts able to be adduced in favour of the origin *de novo* of the contagious diseases are conclusive; but I do say, and I do maintain, that they are strong enough to make us pause before we accept the theory advocated by Dr. Wm. Budd, and to which Professor Tyndall has lent the weight of his great name, a weight which would, however, be greater on the point in question, if he had himself studied the subject on which he has, I am sorry to say, addressed the public in a strain calculated to check unprejudiced individual inquiry.

What I have said on the etiology of the acute specific diseases shows how much has yet to be proved, how hard is the work to be performed ere the opinions entertained on the subject can be converted into knowledge. I will not dwell longer on the etiology of the acute specific diseases, but I must allude briefly to the condition of the individual which precedes the establishment, in many cases, of chronic organic disease. We are apt to pay too little attention, to attribute too little importance to the general—to use a very barbarous expression—"out-of-healthness"—which is so common in previously healthy persons from overwork, from anxiety, from overfeeding, from overstimulation short of so-called intemperance, from want of food, from bad air, from deficiency of exercise, from deficiency of stimulus, to "out of healthness" in which the patient refuses to admit that he is ill, although he is a little, as he says, out of sorts, and only seeks advice because his friends notice that he does not look well and urge him to consult a physician. This condition cannot be studied in hospitals. Those only who are engaged in family practice can trace the origin, can watch the course, can estimate the effects of this condition in all its bearings. For myself, I have been long satisfied that a very large proportion of the subjects of organic diseases, tubercular, malignant, and degenerative, using those words in their widest sense, are mistaken when they attribute the symptoms of imperfect health, from which they have long suffered, to incipient local disease, and for not detecting which they too often blame their first medical attendant. The deteriorated general nutrition to which I have referred as mere "out-of-healthness" was the cause of their sense of want of perfect health; and, ultimately, this imperfect general nutrition of every part of the body culminated in the extreme malnutrition of that part or organ which natural constitution or accidental circumstance had rendered less able to resist organic change.

As to the symptoms which mark the very outset of a disease, in illustration of my position that there is not a disease respecting the symptoms at the outset of which we are not wanting recorded facts, I may refer again to that much studied disease, typhoid fever. In Wunderlich's excellent work, is the following statement. "We may exclude typhoid fever when, between the fourth and sixth day, in a child or adult under middle age, the temperature never reaches 103.1°; and indeed, he says, if it fail to do so two or three times. It is impossible not to see that this conclusion is based, as are other of his statements, on cases admitted into hospital; and every one who has enjoyed much experience

in private practice would, I think, be disposed to temper this law with so many exceptions that, as a law, it would hardly exist; yet, in proof of the existence of these numerous exceptions, recorded facts are wanting.

With reference to our lack of recorded facts as to the course of common diseases, I shall only mention one disease concerning which I thought two facts quite certain; but, on talking to others, I heard doubts expressed as to the truth of those facts, and I soon found that the observations on record were not detailed with sufficient precision to justify me in drawing the conclusion which, if the supposed facts be real facts, flows from them.

Does emaciation make rapid or steady advance when an external organ, unneeded for life or health, as the breast, is the seat of scirrhus cancer, when the appetite is good, and there is neither ulceration nor discharge? If it be that this steady loss of flesh occurs when the breast is the subject of scirrhus cancer, does it take place without any elevation of temperature? Had there been recorded facts to answer these two questions in the affirmative, then it seemed to me that this important conclusion would follow. In the process of nutrition, there are three factors—namely, the cells of the body generally, the nerves that regulate their functions, and the material out of which tissue is to be formed. It is clear that atrophy of the body generally, like atrophy of a part, may be due either to excess of waste, or to deficient supply of new material; but, if there be no elevation of temperature, there can be no rapid waste of the whole body; and, as we know that in cancer there is no disturbance of the nerve-function of the body generally, and as the cells of the body generally are healthy in structure, then we should have to hold that the emaciation must be due to the third factor in nutrition—namely, the blood. I mention all this, not as affording any clue to the pathology of cancer, which would be out of place here, but as illustrating the want of facts, of accurately observed and carefully recorded facts, in relation to the course of one of the commonest of diseases.

Time will not permit me even briefly to illustrate our lack of knowledge in regard to the other headings to which I have referred; but, before concluding, I must direct your attention to two or three points bearing on our knowledge of the treatment of disease. It does seem to me to be again, I may say, discreditable to us as a profession, that so common diseases as rheumatism and whooping-cough—each, so far as we know, uniform in its pathology—should have for their cure so many drugs. For the good service able to be rendered by each of these drugs, men of high position, of great power of observation, of unquestioned honesty of purpose, have vouched in the most emphatic manner; and yet no one of these drugs is found by the profession, speaking generally, to yield unequivocally favourable results; while the precise recorded observations of one of the distinguished men who have filled this chair, and his colleague Dr. Sutton, have thrown so much doubt on the value of some of the remedies which have enjoyed the highest reputation for the cure of rheumatism, that a re-examination of all these so-called curative remedies is imperatively called for. The difficulties in the way of advances in true therapeutic knowledge are very great; but are the difficulties insuperable which stop the way to our ascertaining the real value of the cures for acute rheumatism or for whooping-cough? Could not an unprejudiced examination of the effects of all the remedies hitherto propounded by men of repute, purporting to have tangible evidence in their favour for acute rheumatism and for whooping-cough, be undertaken on a large and decisive scale? There are two kinds of evidence to be adduced in favour of the value of any drug on the course and termination of a disease. One is the evidence afforded by individual cases observed separately, all the special points of which can be weighed at the bedside by the practitioner—weighed, it is true, rapidly, and perhaps roughly, but still weighed: for example, the age, the sex, the habit, the constitution, the presence or absence of complication, of concurrent or pre-existing ailments, of all the surroundings, etc. Every remedy must be, as a final court of appeal, put through this trial; and, if time be given to dispel the illusions of fashion, and to allow the weight of great names to be reduced to their true standard, it is pretty certain that the verdict is correct.

What may be called incomplete diagnosis has often led to erroneous generalisation in regard to treatment, identity of symptoms being confounded with identity of pathological conditions. The late Sir Benjamin Brodie had great confidence in the bichloride of mercury in the treatment of paraplegia; and he, and others following his example, prescribed it indiscriminately in cases of paraplegia. He saw it cure syphilitic paraplegia; and, not knowing that the particular case he was treating was syphilitic in nature, he regarded bichloride of mercury as the remedy for every case of loss of power in the lower extremities. Other members of the profession tried the effect of bichloride of mercury in paraplegia; and, if it happened that they gave it in two or three

cases of syphilitic paraplegia, they too expressed the greatest confidence in bichloride of mercury as a cure for paraplegia. But others happened to give it in succession to two or three cases not syphilitic in nature, and they declared it to be useless, or worse than useless, as a cure for paraplegia.

One mode, then, of estimating the value of therapeutic agents, may be called the individual; and, in testing the value of particular modes of treatment, members of this Society generally may assist. The other mode of estimating the value of therapeutic agents may be called the analytical; that is to say, we test the worth of the drugs by the analysis of a large number of carefully observed and recorded cases. For cases thus collected to be compared, they must be numerous enough to afford a sufficient number more or less identical in regard of age, etc. Researches of this kind can only be conducted in hospitals; and I think, for the purpose of collecting such cases in sufficient number, for the purpose of directing attention to special comparable facts, committees might be formed from members of this Society; and, were this done with due care, every member of the Society might aid in obtaining valuable, because real, therapeutic knowledge. And the testing clinically the value of the several remedies proposed by great authorities for the cure of acute rheumatism and of whooping-cough seem to me to be objects worthy of such a committee, and the questions involved seem to me to be those that such a committee might in time definitely answer.

Excuse me yet a little while, whilst I speak of the treatment of a special class of diseases. When any acute specific disease is epidemic, the educated public call loudly for a cure, and too often, I think, the members of our profession call out as loudly "Eureka!" Now, to me it seems that there are no grounds for expecting that a cure will ever be found for diseases of this class; that is to say, for expecting that a drug or medicinal agent will be discovered capable of arresting the progress of the organic changes which, set in motion by a special cause, following each other in definite and ascertained order, constitute what we call an acute specific disease; for, in place of being diseased actions, these several organic changes, the evidence of which we call symptoms, are, so far as our present knowledge extends, processes, the first of which being called into action by some external cause (for example, the poison of the disease), are essential for the restoration of the intimate organic changes to the order and intensity which constitute health. I may illustrate my meaning thus. Each of these diseases may be likened to a single fit of ague. We administer drugs to prevent the recurrence of the fit, but we do not cure the fit itself. The cold stage having commenced and attained a certain intensity, the hot and sweating stages are essential to the restoration of the balance of health. So with small-pox. By vaccination we prevent its occurrence; but, when the first of the phenomena—which, following each other in certain and definite order, constitute small-pox—occurs, we know that there is no road to health but by the sequence of changes, the symptoms of which mark the several stages of the disease; and it is to my mind very questionable whether, if it were possible by administration of a drug to arrest the essential changes which constitute any one of these stages, health would be the result. What is true of small-pox in this particular, seems to me to be equally true of all acute specific diseases; and when I read the list of cures for cholera which swarm in the columns of the press at the first inroad of an epidemic of that disease, I should smile, did I not know that a column of cures for cholera in a leading newspaper may be the death-warrant of numbers. Although the science of medicine can never hope for a cure for any one of these diseases, it can prevent death from all, and it is time that the public, and not the profession only, had correctly appreciated what medicine can and what it cannot be expected to do, in this class of diseases. Nor would medicine as a practical science fail in public estimation, or the physician be less highly esteemed, were the public instructed in the matter rationally. At one time, the chemist was valued especially, because it was supposed that, by his aid, baser metals could be transmuted into gold—"baser", as they were foolishly called; and day and night he sought for that "power of projection", as it was called, by which the transmutation could be at once effected. Faith in this transmutation, faith in the discovery of this powder of projection is exploded; but is the chemist less highly valued, his aid less earnestly sought? No. The work he can do is of more worth than the work it was fancied he could do. So with the physician; he cannot transmute by a special drug the sequence of processes which we call an attack of cholera into the sequence of processes which we call health, any more than the chemist can transmute iron into gold. The "certain cure for cholera" and the "powder of projection" of the alchemist are alike apocryphal; the dreams of a prescientific age, or the base attempts of charlatans and quacks to prey on those whose minds are yet in the prescientific stage of development; that is to say, on the ignorant and the credulous. The public must learn that the

true, the real in regard of the relation of medicine to these diseases is as different and as superior to the fancied as is chemistry to alchemy, or natural science to the black art. Medicine teaches us how to prevent in many of the acute specific diseases the first of those processes, the sum and sequence of which constitute the disease; and, when the first of that sequence has commenced, the physician, though he must reprobate the folly of those who cry out for a drug which will stay the sequence of processes which are in reality steps to health, does not stand helplessly by and leave the patient to his fate; but, by the judicious application of the rules of art, so moderates the necessary disturbances of formation, that they shall eventuate, when the last of the series of restorative processes is over, in health in place of death. The physician in such cases may be likened to the physicist who, although he cannot prevent the discharge from the electric-laden cloud, can yet save the threatened tower by conducting the lightning-flash harmlessly to the earth.

LECTURE ON COMPARATIVE PHARMACOLOGY AND THERAPEUTICS.

Being part of an Address delivered at the Manchester Club of Aberdeen Medical Graduates.

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THE physician has to do primarily with functions, and, secondarily, with structure; and it may be thought that molecular physics, including chemistry, has made such rapid strides of late years that it will afford, along with physiology, as secure a basis for his treatment as molar physics along with morphology does for that of the surgeon. But this is not the case. The effects produced upon organisms of all kinds by bodies in mass, is much better ascertained than those produced by the molecules of substances circulating in the fluids of the body. And this lack of knowledge of the effects produced by the molecules of substances which gain entrance into the circulation, constitutes the greatest gap in our knowledge of the sciences which form the immediate basis of treatment. But even this gap is being rapidly filled up. I am not an old member of the profession, yet my text-book on the subject which treats of drugs and of their actions, which was a closely printed octavo volume of about 800 pages, and which was full of all sorts of information respecting the drugs themselves, gave only the most scanty information with regard to their action upon the body. The only information which it contained with regard to the action of such a commonly used and useful drug as bicarbonate of potash was, that it was "antacid, antilithic, diuretic, and resolvent", and it gave no fuller information with regard to the action of any other drug. Things are somewhat mended since then. We have such works on *Materia Medica* as that of Nothnagel in Germany, Rabuteau in France, Dr. Wood, jun., in America, Dr. Sydney Ringer, and a first instalment of a work by Dr. Charles Phillips in this country. I have not yet seen Dr. Wood's work, but I am told that it is very accurate, and contains a vast amount of information respecting the action of drugs. I can, however, speak from a personal acquaintance of the other works which I have mentioned, and I must say that each and all of them are immensely superior to the treatises which were in existence during my student days. But, notwithstanding these works, much requires to be done before a scientific basis is given to drug-treatment. In every department of knowledge, the systematic application of the comparative method is that which has given the most valuable results to the scientific workers of the present day. The science of comparative anatomy, or more correctly of comparative morphology, has been constituted within the memories of many living; while the foundations of comparative physiology, comparative psychology, and comparative philology, have been laid by our own contemporaries; and even now comparative theology is pressing itself upon our notice. And, to give a scientific basis to drug treatment, we must found the science of comparative pharmacology. We want to discover a general body of formulæ, which shall be true not simply respecting the action of drugs upon this or upon that organism, but respecting their action upon all organisms whatsoever. And if our facts were fitly arranged for the application of the comparative method, many such formulæ could be readily obtained; indeed, a glimpse may be got of a few of these formulæ even with our present arrangement of the facts. Take, for instance, any one agent, such as ethyl-alcohol, and compare its action upon different organisms. Alcohol