

ON THE

SPECIFIC ORIGIN OF GENERAL DISEASE.¹

By J. WALLACE ANDERSON, M.D.,

LECTURER ON MEDICINE, GLASGOW ROYAL INFIRMARY MEDICAL SCHOOL,
ASSISTANT-PHYSICIAN TO THE ROYAL INFIRMARY.

I NEED offer no apology for the subject of the present paper. At the present day it is suggestive enough. At once a whole host of microbes, spores, and other horrid things rise up before us, for in this age there are germs, germs everywhere. They try to win our toleration, if not our sympathy, under the thin garb of a new name: bacilli possibly, little walking-sticks, they call themselves, with quite a wont-you-have-me kind of air. But I am not going to take up the matter lightly. It is a burning question, and one of the greatest interest, the relation between these organisms and disease; and apart from the practical results that may yet follow the subject is not without its attractions as a mere speculative inquiry. It has been left too much in the hands of the pathologists. Surgery has within well-defined limits studied the question, and with an advantage to suffering humanity that few, if any, will gainsay; but when from the wide domain of medicine individual diseases are brought in, and we are told that this and that germ is the only true *fons et origo mali* of each, we may with reason both wonder and doubt. I would still leave to the pathologists the task—a most agreeable one doubtless—of further germ discoveries, believing that there are other views of the question that should readily invite the attention of the medical practitioner. What I have proposed to myself is an abstract inquiry into the essential cause of individual diseases, and what I wish to present to you is the reasonableness of the opinion that many—we can never say how many—diseases are caused by distinct specific germs. Holding this view implies the existence of an opposing force. Now, without going into the old scholastic inquiry as to the nature of health and disease, I suppose it will be admitted that their relation is one of antagonism. Certainly we do act as if we were engaged in a conflict. We endeavour to strengthen the vital forces. We do this by such means as improved sanitary conditions, better food, tonic medicines, or change of air; by keeping up the supplies, so to speak. Again, we do the opposite to the opposing forces. We cut off their supplies by improved hygienic measures: pure air frequently, if possible continually, renewed; attention to cleanliness in all respects. That cuts off the supplies of the invaders. If universally carried out it would lead to the complete extinction of typhus fever, for example. We even go further, and take a lesson from actual warfare by laying waste the fields over which we believe the enemy will march. Do we not do this? do we not willingly submit to the lesser evil, in the expectation of avoiding the greater, when by vaccination we substitute cow-pox for the dangerous and disfiguring small-pox? Practically, then, we act as if we were face to face with foes. But I have often thought that it is on the most general grounds reasonable to suppose that the condition of health is constantly assailed by influences around us. I think we may very properly hold the view that every disease is but a response to an impression of some kind. This law of response seems to be universal in nature. Every act, circumstance, or condition has its response, physical, and, in the case of sentient beings, intellectual as well. It was while reflecting on the nature of fever and its relation to these organisms of contagion, the existence of which I am assuming, that my thoughts went first in this direction. It seemed to me that fever might be viewed as a form of resistance or defence (as a response to an evil necessarily is) against these pernicious influences around us—just as truly as that, in the world of feeling, the flushed countenance, the passion, and it may be the knock-down blow, are the response to the imputation of falsehood. So far as we can judge, this principle is universal, or, as Emerson says, “The law at the foundation of all things is retribution.” It will thus be seen from what standpoint I wish to view the origin of disease in this paper. Jonathan Hutchinson, in his lectures on “The Pedigree of Disease,” approaches the question from the other side. He dwells on tempera-

ment, idiosyncrasy, and diathesis as causes of disease. He does not of course exclude external influences, but holds, or at least inclines to, the opinion that intrinsic conditions rather than extrinsic influences are the determining causes of the endless variety of diseases. But there are two sides to every question, and it is the other I would venture to take and present on this occasion. I am on the side of the germs. My creed is: “The law at the foundation of all things is retribution.”

In proceeding further to the consideration of the precise question before us—viz., a specific organism the cause of disease,—I am confronted with the proverbial difficulty of knowing where to draw the line. One has little difficulty with the common infectious fevers, yet many will raise doubts regarding the specific origin of erysipelas and diphtheria; and what shall we say of such diseases as pneumonia, still more of ordinary catarrh? That is where the doubts and the discussion come in. Let us begin with what is pretty well assured—the dependence of each of the common fevers on a specific organised germ. That they are caused by an organism, and not mere inorganic particles, is inferred from the general character of the fevers themselves. They have an incubation, a growth, a maturity, a power of reproduction, a struggle for existence doubtless, a decline and death—the attributes of all living beings. Then it is a matter of common remark that a particular fever reproduces itself and no other. The distinctive characters of each are always preserved. The typhus germ strikes suddenly and vigorously, living on the system as a whole and on no individual part; its death is sudden and on a fairly definite day, and then the sufferer is safe. How different is the germ of enteric fever! Unlike that of typhus, in its immature or larval condition, it is an amphibious creature, most likely taking to the air occasionally, but preferring water, or perhaps rather milk, when it can get it. I say larval state, because from analogy we may infer that it reaches the mature condition in the human body. Once there, it lives longer than the typhus germ, having had a distinctly longer incubation and slower adolescence than its altogether shorter-lived typhus brother. It has its peculiar well-defined local manifestation, and often exhibits that indication of a relapse which is perfected in some of the other fevers. In all these respects it is specifically distinguished from typhus fever. But however interesting the study of the distinctive characters of the common fevers may be, and I think nothing to the physician can be of greater interest, I need not here dwell upon them merely as a proof of what is generally accepted, even were it only accepted truisms I had to offer. I would only ask to be allowed to make an exception in favour of intermittent fever and erysipelas. The ague germ presents some interesting peculiarities. It is essentially an aquatic bird, for though a warm atmosphere invites it, it will not fly far from its home in the marsh. In its fondness for the water it resembles enteric fever, but, unlike the latter, it is in no sense cosmopolitan. But it would seem as if it had another distinctive peculiarity—viz., its size and weight. A belt of trees will catch the germs in its meshes and save the troops beyond, and they are substantial enough to be carried a considerable though still quite limited distance by the wind. Their greater weight may be inferred from the fact that a pretty high temperature is needed to raise their ponderous bodies from their bed, and in no case are they raised to any great height. They have a curious but perhaps not absolutely peculiar affinity for water, as a running stream between is known to be something of a protection to a body of troops on the further side. In all respects the germ presents the most marked contrast to such a one as that of epidemic influenza, which asserts its powers at long intervals, is often pandemic when it does appear, and would seem to be transmissible unimpaired for many miles at least, and solely through the air. Such are some of the best examples I can think of in evidence of specificity.

Now as to erysipelas. I select it as a distinct departure from a class of diseases whose specificity is pretty generally accepted. Perhaps regarding no disease has the question of specificity been more disputed than that of erysipelas, but I do not refer to it to enter upon an enumeration of its special characters, though the subject might be made as interesting as it is ample enough, but rather to call attention to the arguments usually adduced in favour of its non-specificity, because, so far from being conclusive, they rather disclose further the question under consideration. “The arguments in favour of its being a non-specific and local disease,” says Bristowe, who summarises them con-

¹ Read before the Glasgow Medico-Chirurgical Society.

veniently for us, "are chiefly the following:—First, the fact that the disease appears in many cases to arise from exposure to cold and various other non-specific causes; second, that a previous attack so far from precluding subsequent attacks, as is generally the case with the infectious fevers, encourages them, as is the common rule with the non-specific inflammations; third, that contagiousness is not an attribute of the specific fevers only, for many varieties of simple inflammation (catarrh, ophthalmia, and the like) are apt to spread by contagion."² Now the first argument, while qualified by the word "appears," yet to that extent begs the question, for we hold that exposure to cold in the case of erysipelas, as in many other diseases, simply permits the common but still specific germ to gain for the time the mastery over our system. Dr. Francis Henderson is the only one, so far as I know, who boldly advocates that view on abstract grounds. In an able paper on the Rationale of Cold as a Cause of Disease,³ he maintains the following proposition: "Cold is not of itself adequate to cause these various forms of disease; it is only one of two factors which in ordinary circumstances require to act conjointly. The catarrh, the inflammation, and the fever which are produced by exposure to cold have a real *materies morbi*; but this *materies* could not be the cause of disease if the vital powers of the part or of the system were not abstracted or lowered by the agency of cold." That is the view we have always held. Now, the germ of erysipelas seems to resemble that of ordinary catarrh in respect of its being a very common one, yet acting in its own special ways—amongst others, in taking effect on our system, as some suppose, only through an abraded surface, however minute. As to the second argument, the fact of a disease being taken possibly, or even readily, a second time, is little evidence of its non-specificity. Relapsing fever is a typically specific affection, and one attack is no protection against another. So with regard to intermittent fever; and MacLagan would instance acute rheumatism as another common exception. Then as to contagiousness not being an attribute of the specific fevers alone, we simply say it is an attribute of specific diseases alone, for it need not be a fever, and catarrh is one of the commonest illustrations of this. It may be thought I am hurrying past many difficulties, but these will come up again under the heading of objections.

But now we come to the difficulty of knowing where to draw the line. What can we say of such diseases as pneumonia, asthma, dysentery, or any other malady taken at random?—although these I have really selected, as we shall see immediately. It is with such diseases that the real difficulty begins in our present state of knowledge, and all that we can hope to do is possibly to suggest some points for further reflection. Well, I think we sometimes create a difficulty by forgetting that symptoms which present a general similarity may indicate very different diseases. We can imagine an ignorant person including an eczema, an erysipelas, and a slight burn under the term inflammation of the skin, or grouping together as one disease enteric fever, dysentery, and simple diarrhoea; and we can suppose a much less ignorant person considering a bronchial spasm due to bronchitis to be the same thing as pure idiopathic asthma; and so on. If it be not admitted that either bronchitis or bronchial spasm is due to a specific cause, and I do not say so myself, it will be allowed that the very same symptom, the asthma, may in some people be the result of a specific factor—e.g., the pollen of hay. Those who believe that there is such a thing as hay asthma will admit that it is a specific complaint. It seems to me, indeed, that the term asthma is as comprehensive and as vague as that of dropsy. And may it not be as vain to ask if pneumonia is a specific affection? May there not be really different diseases included under that term, some specific and others non-specific? Some think a fractured rib can cause a pneumonia. Can that possibly be the same disease as ordinary idiopathic pneumonia, or still more the exceptional epidemic forms? Just as one would expect, the report on pneumonia recently issued by the Investigation Committee of the British Medical Association shows that some kind of division of this term is necessary if we are to make anything of it at all. Without passing any opinion on the classification given by that committee, it seems clear enough from their report that, besides certain

forms of pneumonia which are apparently set up by various non-specific influences, there are others of a strictly epidemic character, falling on a district and spreading thence so widely and so inexplicably as to imply a generally prevailing yet special cause. Examples, too, are given which appear to prove conclusively the infectious nature of certain forms. And so with regard to dysentery. Many cases presenting symptoms which lead us to diagnose that disease may no doubt be the result of non-specific causes; but I think few can doubt that the epidemic form handed down through ages with a constant history, and of such exceptional severity, must be the result of a fixed specific cause.

Further than this I shall not attempt to go. We have, indeed, touched but the border-land of disease as a whole in regard to specificity; but beyond we could only wander in vain imaginings. But, possibly, not the least interesting part of the question lies in the consideration of the objections that may be urged against even the position I have now taken. And this will form the second and shorter part of our subject. In the first place, it may be said that the infecting particles of even a specific fever are not organisms at all; it may be held that they are inorganic. I need not dwell upon this objection; for I find that MacLagan many years ago discussed this point very elaborately in his work on "The Germ Theory of Disease." After all, the one essential distinction is that a number of living germs merely sufficient for successful development is all that is needed to secure the maximum effect; whereas the effect of inorganic or lifeless particles is always commensurate with their numbers or amount. The power of infection, too, as distinguished from contagion, is, I should imagine, the prerogative of a living germ; but that may be disputed; the former cannot be, and its bearing on our subject is obvious. Inorganic particles cannot, properly speaking, have an incubation; they do not mature, nor can they reproduce, their kind.

To return to the question of specificity. Though we do not venture to affirm that this and that disease are of specific origin, it may occur to some of us that very many diseases are so. We find that not only is ordinary catarrh epidemic and infectious, but in particular seasons we have special varieties of it—say in the form chiefly of a myalgia. Now, one may ask himself if such special forms may not depend on a special form of the catarrhal germ? The supposition is fantastic enough, but I make it for that very reason. It forces us to ask if we are to suppose a germ for everything—for a kick on the shins, perhaps. Simply letting the last remark pass, I would say, first of all, that the specificity of practically an endless number of diseases is not to be considered unlikely on the ground of the limited resources of nature. The variety that nature presents in the visible world is endless, and everything tends to show that the same variety obtains in what is, to our unaided eye, the unseen world. No doubt one instinctively refuses to believe that there is a germ for diphtheria, and for erysipelas, and for pneumonia, and at first may think it simply ludicrous to imagine that common catarrh can depend on a specific germ. It is too quixotic to suppose we are fighting daily with germs and microbes—not indeed by the day, but at every turn we take and every breath we breathe. But I say we are not to consider the position untenable from any limit to variety in nature. Is it not practically infinite? The variety in the vegetable kingdom strikes the dullest, and the learned can divide and classify into groups innumerable the vast array of form and kind; but the work is never done. In moss, and heath, and fern, and such-like meaner things—for so we reckon them who know little of them—the earnest student finds a continual feast; and amongst the lichens, that almost look like a part of the rock from which they seem to draw their scanty store of life, there are curious secrets hidden, of which even a Leighton or a Stirton has never dreamed. Well, then, one may admit the possibility of a more substantive disease having a specific origin, but refuse to believe that such a vulgar thing as common cold can lay claim to any kind of descent. But is the contrast greater or nearly so great as many which we find in the visible world? Is the contrast between common catarrh and cerebro-spinal fever greater than between the grass that clothes our fields or the weeds that infest our path, and that rare fern that is only found in one shady glen, or the lichen that still lingers, the last of its race, on but one mountain side? It is not half so great. Catarrh is the common chickweed; cerebro-spinal fever the rarer plant that we only meet with

² Theory and Practice of Medicine, 5th ed., pp. 296, 297.

³ Glasg. Med. Jour., Oct. 1881.

now and then. Common plants will grow anywhere—that is why they are common; but rarer plants are of special soils. Anyone will take a common cold, but special cultivation, so to speak, is needed to make anyone take an erysipelas, though in the few it will grow apace. This reference to special soils reminds us of the undoubted fact that some are much more prone to the contagious fevers than others. Occasionally we meet with a person who has had scarlet fever repeatedly, and this exceptional liability we sometimes find is not an individual peculiarity merely, but one that may affect a whole family. I am as firmly convinced, on the other hand, that other individuals and other families present a specially unfavourable nidus for the development of a particular germ. And here too the question of heredity might be discussed, were it not quite understood now that by that expression one only means, where an external morbid influence can be assumed at all, an inherited peculiarity of constitution that is specially favourable to a particular spore. And, lastly, I can suppose this objection still urged: In the natural world there are doubtless many rare plants and animals too, as well as those which are very common; but they all exist somewhere. They do not disappear and return every other year or half century perhaps, as some epidemic diseases do. Or, without looking for analogies at all, one may say, "I will not believe that a malady like true epidemic influenza, which we only see a few times in a century, arises under the same conditions as those diseases which are always with us; it is absurd to suppose that these varied diseases we have been considering originate under one common law." Well, I am not going to prove that the germ of true epidemic influenza or of relapsing fever is always about us, but in an immature state. I do not think we shall get much further in that direction than did Sydenham, when by "epidemic constitution" he meant or supposed a peculiar atmospheric state by which such exacerbations of disease were brought about. But I think we have analogous excesses in the visible world. Those of us who have lived in the country have observed how special years have been marked by special flowering seasons; when the apple-blossom decked every garden, or the primrose covered every grove and meadow. One year the poor man gets his potato cheaper; another will make the rich man pay willingly for the finer wine. In the animal world we have special years of migratory visitants which defy the naturalist to explain. One may see in all this the law of variation which is universal in nature, but we cannot explain the phenomena themselves. I do not think such reflections are idle. They have made me cease to wonder at such an occurrence as a sporadic case of enteric fever. I am no believer in its spontaneous origin. Its germs are likely ever present, but either themselves immature or waiting for a convenient pabulum. And once a centre formed, the extension is a matter of no difficulty. But I must not be tempted to go beyond our present subject. The origin of disease has many sides, and I have only presented one in this paper. It has long been a hobby of mine, and took its origin certainly quite apart from the germ discoveries in medical disease of the last year or two. Of these bodies which the pathologist shows us I know nothing. One may be a whole cityful of germs, or no germ at all. Other views of the question seem to me to have been too much neglected, and one of these I have done my best to offer in the foregoing remarks.

FURTHER OBSERVATIONS ON THE CURE OF WRITER'S CRAMP.

By A. DE WATTEVILLE, M.A., M.D., B.Sc.,

PHYSICIAN IN CHARGE OF THE ELECTRO-THERAPEUTICAL DEPARTMENT,
ST. MARY'S HOSPITAL.

THERE are two phases in the early history of every scientific discovery or new application of practical methods. Its truth or value is at first denied; then, jealousy springing up, the originality of the discovery or invention is placed to the credit of others. When a principle only is involved, the claim is sometimes made for the dead; at other times, however, especially when practical issues are bound up with the question, the attempt is made for the benefit of the living. I had the opportunity a short time ago¹ of de-

scribing the remarkable results obtained by Mr. Wolff, of 19, Upper Berkeley-street, London, in the treatment of writer's cramp by a combined application of massage, gymnastics, and caligraphic exercises. The same occurrence which had signalled his first appearance in Germany some years ago saluted his introduction to the profession in this country. It was asserted that his method contained nothing new, that massage was known to the Greeks, and that Swedish gymnastics are as old as the century. Now I did not claim for Mr. Wolff anything but that he alone, among those who devoted themselves to the treatment of writer's cramp, had been generally, nay, almost regularly, successful in his endeavours. I have in vain looked through literature for similar results; and I am glad to find that in the last works which the medical press has given us on *mechanico-therapeutics*² the authors refer to the subject in terms which fully confirm these conclusions. In order to illustrate the value of Mr. Wolff's method I publish two cases, which will speak for themselves. No commentary is needed in presence of facts so clear and so telling.

CASE 1.—Mr. G. H.—, a merchant of Liverpool, with no noteworthy family history or personal antecedents, noticed seventeen years ago a weakness in the thumb, index and middle fingers, and a tendency of the arm to turn inwards, when he was writing. These symptoms steadily grew worse up to 1874, when pains began to be experienced, extending from the wrist to the elbow along the ulnar side of the arm. He persisted in his attempts to write, however, and these brought on spasmodic movements and pains in the upper arm, whilst, strange to say, the motor and sensory troubles in the arm and wrist rather showed a tendency to mitigation, being then replaced by tremors. For the last four years the use of the pen has been practically impossible, and the patient exclusively used the type-writer for his correspondence. The use of this instrument rather increased the tremors when attempts were made to write, whilst it developed sensations of pain in the fingers. I first saw Mr. G. H.— in 1883, and again on the 16th of January, 1885, when he came to London at my instance, to place himself in the hands of Mr. Wolff. Several specimens of his handwriting were taken. (See facsimiles.) It took him seven minutes to trace four or five very short lines of very shaky characters. The facsimile bearing the date Jan. 16th was taken just before the first manipulation; the others will tell their own tale. The improvement was steady, and on the 30th of the same month (January) he left London to return to his business in Liverpool. He was then able to write without difficulty for several hours a day. I need hardly say that the ordinary resources of therapeutics, including a rest of several months on board ship, had been in vain resorted to before recourse had been had to Mr. Wolff's method. In fact, so despondent was the patient as to the possibility of a cure, that I had no small difficulty in persuading him to make another attempt to obtain relief from his apparently hopeless malady.

CASE 2.—Mrs. C. K.— came to Mr. Wolff for treatment. The case being a well marked one, I obtained permission to watch it for my own instruction. The patient's power to write was tested at intervals of three or four days, from January 14th to February 18th, 1885, on which latter date she considered herself as cured. (See series of facsimiles.) I append a few notes concerning this patient. Her age was forty-three, and the first symptoms of her complaint date from 1871. She then experienced a mere feeling of fatigue and discomfort in the arm on writing. Till 1875 no change was noticed except an increasing powerlessness; though she tried to write a little, this became so accentuated that she had to give up almost every attempt. She rested for some years, during which the feelings, which had in the meanwhile assumed the character of pain, persisted, and even extended upwards to the shoulder. Tremors in both arms made their appearance, occurring first on performing other actions than writing (combing the hair, &c.), and finally persisting for the whole day. No treatment seemed to give her any relief. When I saw her the act of writing was almost impossible; it took her five minutes of effort and pain to trace eight or ten very shaky letters. As the treatment proceeded the symptoms gradually subsided. On the 6th the persistent feeling of pain in the arm had disappeared. She could then write for a few minutes. The motor symptoms gave way also, as is well shown by the series of facsimiles. The specimen taken on the 9th February coincided

¹ British Medical Journal, Feb. 14th, 1885.

² Reibmayr: *Die Massage, and Die Technik der Massage* (Vienna, 1884).
Graham: *On Massage* (New York, 1884).