SPECIFICITY AND EVOLUTION IN DISEASE.

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WITH THE DISCUSSION THEREON.

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SPECIFICITY AND EVOLUTION IN DISEASE.

IN a work on Darwinism, published in March this year by the one living man best qualified to treat of it, the first chapter is most appropriately headed "What are Species?" The question of their origin before Darwin did his work was, in the words of Sir John Herschell, "the mystery of mysteries." De Candolle defined species, "a collection of all the individuals which resemble each other more than they resemble anything else, which can by mutual fecundation produce fertile individuals, and which reproduce themselves by generation in such a manner that we may by analogy suppose them all to have sprung from one single individual." Propagation "after its kind" was a phrase as familiar then in the mouths of scientific as of theological expositors. Sir Charles Lyell, who was regarded as a thinker who had advanced to a perilous extreme, and who attributed considerable importance to variation, yet declined to allow his confidence to be shaken in the "reality of species," and set his face resolutely against the theory of progressive development, so convinced was he of the truth of the doctrine of special creation. It has been the work of Charles Darwin and of Alfred Russell Wallace to alter the face of organic nature as we conceive it, and to give us truer and finer conceptions alike in ontology and biology. It is the object of this paper to inquire now, as I inquired eight years ago, has the science of medicine, as we are pleased to term it, reaped the full benefit of these new conceptions? A small and increasing number of philosophic pathologists has doubtless essayed to bring the quickening influence of evolution to bear upon the dry facts which for centuries have been accumulated with such an infinity of labour and prolixity, and such a modicum of insight and synthetic thought. We have but to take up any ordinary text book of medicine to find that such underlying scientific conceptions as it may happen to include are but a dim reflex of the fossilized biological notions which did duty before Darwin's day. The old absolute, unalterable, and eternal specificity theory is there in all its crude inconceivability and its theological attire. The dictum which Sydenham propounded in 1666 is the inspiring principle of average modern pathology, viz.: "Unaquæque morborum non minus quam animalium aut vegetabilium species affectiones sibi proprias perpetuas ac pariter univocas ab essentia sua promanantes sortita est." Trousseau, 200 years later, wrote in the same strain: "In diseases which seem to bear the strongest resemblance to one another there are specific characters, as distinctive as those which distinguish the different species of the same family of plants, or the different species of the same class of animals." Unlike Sydenham, or even Trousseau, our modern scribes are without excuse, they seem to fail to observe that inasmuch as the major premiss upon which these great authors reposed their pathological doctrine has been undermined and destroyed, their conclusion is left with as little reliable support as the elephant and tortoise of Hindoo mythology. Indeed, this would appear to be the view of the learned editor of "Quain's Dictionary," who apparently discards subtle biological analogies, and in expounding the meaning of "specific," takes refuge in a *circulus in definiendo*, for he says, "The word signifies that such disease is produced by a special cause, and has special characters."

I therefore propose briefly to discuss in the light of most recent research the views entertained respecting specific diseases, whether in general or local manifestation. Such a question, I submit, is of fundamental importance in all that concerns the public health, it is of even greater interest to the practical hygienist than to the clinical physician, and it meets us at every turn in medical legislation.

The old theory of specificity requires little introduction at my hands. Laennec, Bretonneau, Trousseau, and the French school generally have insisted that "elle domine toute la pathologie, toute la thérapeutique, en un mot, toute la médecine." This theory is but a reiteration in the language of pathology of the principles enunciated in the Mosaic cosmogony; that "in the beginning," or at some time since that eventful epoch, were created the germs of specific diseases, "each after its kind," and ample provision was made in the shape of specific susceptibilities for their perpetuation in strict genealogical sequence. This theory denies *de novo* origin of specific disease, takes no account of aberrant cases hybrids and nondescripts, shuts its eyes to variations, and dismisses the atypical as unworthy of serious consideration. Such diseases as diphtheria, puerperal fever, erysipelas, gonorrhœa, purulent ophthalmia, *et hoc genus omne*, furnish an inconvenient objection to the supporters of a hard and fast specificity. Indeed, if this theory were true we should expect to find that a disease, originating in the first place confessedly and admittedly *de novo*, could never develope characters as typical as those possessed by so-called specific diseases, we should expect to find no hybrids or mules in disease, no nondescripts or unclassifiable class, no variation of disease under domestication and cultivation; no new diseases; no abnormal cases; all would be plain sailing, diagnosis should present no difficulty, it would be reduced to mathematical precision, the merest child's play, as easy as sorting a pack of cards into suits.

Now I ask, does common experience confirm or deny that view? and the question is answered in the asking. The notion lands us in preposterous absurdity, the very opposite we know to be true; we know that diagnosis is difficult, often impossible, that new diseases develope, that under cultivation and domestication, as explained by Pasteur, diseases may be intensified or attenuated, developed or destroyed; that there are daily occurring crowds of cases we cannot classify, nondescripts such as the Febriculæ so called for want of a better name, hybrids representing two or more diseases inextricably interwoven, the same cause originating different diseases in different persons; everywhere we see a tendency of the common to become the specific, and finally if there be diseases which do not appear to originate *de novo* now, we only ignore, we do not explain their origin, by calling them specific.

And while the specificity theory can never explain the development of specific diseases, though it cradles their birth in remotest antiquity, the evolution theory can—and at the same time asserts, that what took place once in times past, can, if the circumstances repeat themselves, take place again, and that the *de novo* origin of disease may be as true to-day as it was before the flood.

In the year 1884 I attempted, with the approval of Mr. Herbert Spencer, to apply the principle of evolution to the elucidation of the genesis of specific diseases, and I brought forward a collection of observations gleaned from reports of medical officers of health and other sources, which led me to conclude that diseases usually accounted specific have under certain conditions arisen independently of contagion, that is to say *de novo;* that diverse diseases may own a common external ancestry; that the same poison may produce different diseases in different individuals; that specificity depends on the soil on which the seed is sown and on which it is subsequently cultivated; that the property of contagiousness or infectiveness dormant in every inflammation and fever increases in proportion to specificity, in proportion to successful cultivation on suitable soil; that there is in disease as in all nature a tendency of the common to become the specific, the homogeneous to give birth to the heterogeneous; that the distinctive characters of the acute infectious diseases have been, and are being progressively built up from an ancestral, amœboid, common fever. And on the other hand that the relations of the specific fevers to common fever have an exact parallel and counterpart in the relation of specific local inflammation to common inflammation; and, further, that under the head of Nondescripts or anomalous diseases, we are dealing with natural variations of species under cultivation. That mongrels and hybrids are results of intermixture and crossing of species, and all suchlike, instead of standing apart from the rest as incongruous and inexplicable, will now fall in, in perfect harmony and order.

While this view may appear to some to be so rational and natural as to need no argument to enforce the conviction of its truth, it is nevertheless at the present time far from being generally adopted, as the criticisms it occasioned have proved, and as every medical text book can bear witness. Doubtless an obstacle in the way of its acceptance has been the predominant position which the germ hypothesis has occupied during the last ten or fifteen years. Micro-organisms have come to be regarded too much in the light of their relation to diseases of man and too little in that of their own life history; the old notion of a specific organism or entity as the *vera causa* of disease commends itself to acceptance if only on the score of its simplicity and easy comprehensibility. The *raison d'etre* of fission fungi in the minds of some panspermists would appear to be the infliction on mortals of exanthematic disease, for such purpose they hold they are and were created. Whereas I apprehend the truer view to be that it is just in so far as living tissues under certain conditions afford suitable soil for their growth and multiplication that there is any relationship between the two; and it is only by the results of their presence and growth, facilitated by the means of absorption and distribution which the mechanism of the living body provides that they come to be really pathogenic. One of the moulds, aspergillus fumigatus, inoculated upon rabbits has produced metastatic deposits in kidneys, lungs, liver, muscle, and even in the bones and nervous system, showing that mould fungi as well as fission fungi are capable of dissemination and reproduction within the body. Buist, too, has shown that the Torula or yeast fungus inoculated on monkeys produces a constitutional febrile disease which is to some extent prophylactic against future infection. Messrs. Roux and Chamberland have found that the growth of the septic vibrio produces a chemical poison antidotal to itself, and whose inoculation on guinea-pigs confers immunity against fatal septicæmia.

The paramount importance of soil has been absurdly underrated as a determining factor in specificity, and indeed in pathogenic property altogether. The bacillus of mouse septicæmia, which is uniformly fatal to house mice within three days, is harmless to field mice. Again, while goats, hedgehogs, sparrows, horses, cows, and most breeds of sheep are susceptible to inoculated anthrax, dogs, cats, white rats, and Algerian sheep are not so. But that disturbing conditions may bring about a susceptibility in the habitually unpredisposed is shown by the fact that fish and frogs which are normally unaffected by anthrax are infected if their temperature be artificially raised.

Bacteriology has doubtless done much for pathology, but it has done much less than scientific persons both in and out of the profession are apt to imagine. It has not yet helped us to understand the nature of any one of the ordinary acute specific diseases of man, in the sense that it has unquestionably identified a specific morphological unit as the *vera causa* of any one of them. Relapsing fever, with its spirillum Obermeieri, is only a doubtful exception to the statement.

There are many considerations which have tended of late to direct the minds of thoughtful pathologists to a chemico-physical rather than a biological explanation of zymotic poisons; and in cases where a micro-organism is habitually present it may be questioned whether its pathogenic *modus operandi* may not be merely to facilitate a chemical change which may be effected without it, as we know may occur in normal digestive processes. The researches of Gautier and others have sufficed to show how numerous and potent are the ptomaines or animal alkaloids which are recoverable from dying tissue, the result of decomposition of the complex albumin molecule, and there is no reason to suppose that their pathogenic effects would afford less satisfactory or less rational explanation of the process of specific febrile disease than a morphological noxa whose presence is so assiduously sought for. The ophthalmia produced by the insertion of an infusion of Jequirity (abrus precatorius) into the conjunctival sac was thought to be due to the bacilli it contained; but it was found that boiling the infusion destroyed its irritant property without destroying the spores of the bacilli, and cultivations therefrom, though rich in bacilli, were harmless in effect. The conclusion being that a chemical poison *abrin* is the pathogenetic agent.

The foregoing facts, I submit, do not warrant our insisting that the acceptance, within limits, of the bacterial hypothesis of certain diseases implies a strict acquiescence in the truth of a rigid specificity. Since mould fungi are as capable of remote metastatic diffusion as bacteria, and the inoculation of yeast fungus gives pathogenic effects, it is evident that the dissemination of fission fungi in the case of disease may be rather epiphenomenal than of the nature of a predestinated cause. Again, since what may stand as a specific series of morbid phenomena may be determined by a chemical poison in which the bacilli which produced it are conspicuous by their absence, how far is their presence to be held a proximate cause in disease production? Lastly, inasmuch as soil may operate to the extent of nullification of the action of a bacillus which is uniformly fatal to another variety of the same species of animal, how great is the importance to be attributed to it as a factor in the propagation and modification of disease!

The elucidation of the problem of the genesis of specific fevers will probably be best assisted by the study of such a disease as syphilis, which, while analogous to the exanthemata, is slower in its process, and in consequence more amenable to study. The date and circumstances of its first recognition, together with clinical evidence of every degree of infecting sore, followed by greatly varying secondary manifestations, are conclusive to my mind that its specificity has been and can be acquired by cultivation. Cowpox again, which presents many points of resemblance to syphilis, as Dr. Creighton has so ably insisted, has hitherto baffled both cattle pathologists and bacteriologists in search of its specific contagion. It is held to originate "spontaneously," upon highest authority, and all our modern methods of research, coupled with the inducement of the Grocers' prize of £1,000, have failed to establish any micro-organism as its essential cause.

Surgical and puerperal scarlatina to my mind afford striking examples of a disease arising *de novo*, yet possessed of infectivity. I should feel disposed to regard them as examples of *de novo* origin of scarlatina, due to absorption from the surface of wound or uterus of putrefactive products, akin to traumatic or to septic fever, and producing the efflorescence which characterises them, after the manner of the sudoral exanthemata, as pointed out by Trousseau, that is, by the influence of poisoned sweat. In support of this view I would point out that Sir James Paget has remarked that the scarlatina which so often follows lithotomy undergoes certain modifications, and that Dr. Hicks, speaking of puerperal scarlatina, has also observed that in many instances the disorder deviates widely from the normal type. Instances, I would suggest, illustrating in a marvellous manner, the shrewd observation of Darwin that species when nascent are more plastic. In these forms of scarlatina but little removed from the amœboid or ancestral form of simple traumatic or ephemeral fever, the specific characters have not acquired that fixity of form which comes only by cultivation and long domestication. The two theories hitherto propounded, certainly seem to me to be unequal to the explanation of the facts, they are:—1. That the operation predisposes to scarlatina. 2. That it hurries on the incubation of scarlatina already caught. In reply to the first I venture to think the sequence of scarlatina after lithotomy too frequent to lead to the belief that it is not more proximately related to it, than as merely a predisposing cause; in 16 per cent. of Mr. T. Smith's lithotomies under ten, scarlatina has followed. Besides, the theory presumes the attendance of the scarlatinal germ upon the knife of the lithotomist in manner truly wonderful. Against the second theory, that the disease is scarlatina with a hurried incubation it may be fairly urged that in all of fifteen recorded cases the disease appeared on the second or third day after operation, at a time when traumatic fever and its congeners are apt to supervene.

The two great facts of self-multiplication and of contagion naturally suggest that the materies morbi of infective diseases should be biological rather than chemical; but it is impossible at present to go further than to allow that the influence which makes for disease is in some instances associated with the growth of fission fungi in the body. How vast and incomprehensible may be the potentiality vested in a minute morphological unit we know in the phenomena which follow the inter-action of ovum and spermatozoon; of the intimate nature of contagion we know but little more. Yet if it be true that in the life history of the lowest of organic things lie the momentous influences which may determine plagues and pestilences, it is reasonable to believe that in organisms whose cycle may be less than an hour and whose rate of propagation is incalculable, that evolution must be powerfully at work eventuating in the survival of those most fitted to their environment; and that in this as in other directions man's influence may modify natural selection, and by acting in accordance with law learn to conquer nature by submitting to her.

Mr. Blyth said they must be struck with the extraordinary ability of this paper. The subject would more profitably be discussed in some 20 years hence than at the present time. It was too early to discuss the subject until they had more facts about it. Dr. Collins had thrown into his *de novo* theory many strong facts, but he had omitted many pointing in an opposite direction. Some diseases had a very definite connection with the soil. It did not follow that a disease originated *de novo* because they could not trace the human chain of causation. He thought the references to the Grocers' prize were misleading. The founders of the prize did not care what theory was connected with vaccination; what they wanted was to obtain something that would produce the effects of vaccination cultivated outside the body. He thought this class of paper was very valuable, because it was out of the usual run. Although his opinions were quite diametrically opposed to the *de novo* origin of disease, he was glad to recognize the ability with which the paper had been put before them.

Dr. Bond did not think the paper was so much a claim to the *de novo* origin of disease as a plea against rigid specificity, a point which he thought it was extremely important to keep before their minds; that was what might be called the plasticity of the germ. Types of disease which underwent such transitions in characteristics as to show they had originated from germs of an extremely plastic character, which had taken on an amount of modification which helped to destroy, so far as they were concerned, anything like an idea of absolute specificity. Whilst agreeing with the last speaker as to the desirability of looking upon such papers as hypothetical and tentative in their nature, he looked upon them and the hypothesis as extremely valuable. The general result of his observations had been to lead him so strongly in the direction of what Dr. Collins had shadowed forth, that he felt he had travelled altogether from the original ideas as to specificity which he entertained when he first undertook the work of a medical officer. He had such a number of hybrid cases that he could only explain them by accepting in all their fulness the laws of the modification of germ which had been taught them so rightly by Darwin and confirmed by the researches of Pasteur and others.

Dr. Sykes felt that Mr. Wynter Blyth had a trifle misunderstood Dr. Collins. There could not literally have been a *de novo* origin except the original creation, if ever such a thing took place. (Laughter.) The term *de novo* was rather confusing. He remarked upon the tendency of the extremes to meet, with the result that they got a medium product, and said he believed there was some law at the back not yet declared. If Dr. Collins had done nothing more than to teach them they must not lay down any hard-and-fast rules, he had done some good.

The President said he would be glad if Dr. Collins would kindly say exactly what meaning he attached to the phrase *de novo*. It would also be desirable if they had a distinct idea what they meant when they spoke of the *de novo* origin of diseases.

Dr. Collins said he did not believe anything ever was spontaneous in this world, and he preferred to use the expression *de novo* to spontaneous. In the sense of spontaneous origin he entirely rejected the words *de novo*. It was because he felt that this highly theoretical subject could only be advanced by such practical men as the medical officers of health who throughout the country were conversant with every form of disease and variation that he had ventured to lay the paper before them.

The President said he thought they might congratulate themselves that the discussion had been of an eminently practical nature, of the highest importance to medical officers of health.

Dr. Bond proposed a vote of thanks to Professor Corfield, and the conference terminated.