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SPECIFICITY AND EVOLUTION

IN

DISEASE

A Paper read before the Abernethian Society, St. Bartholomew's Hospital, February 28th, 1884.

BY

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TO

HERBERT SPENCER

THIS ESSAY

IS MOST RESPECTFULLY

DEDICATED

[Copy of Letter from Mr. Herbert Spencer].

38 Queen's Gardens, Bayswater, W.
April 3rd, 1884.

Dear Sir,

Being obliged to economize my small power of reading, I have been able only to glance at the earlier part of your essay; but what I see of its drift convinces me that its conception is thoroughly philosophical, and promises to open the way to a considerable reform in pathology.

I have therefore much pleasure in accepting the dedication you are good enough to propose.

I am,
Faithfully yours,
HERBERT SPENCER.

Dr. Collins.

SPECIFICITY & EVOLUTION IN DISEASE.

GENTLEMEN, I ask your attention to-night to a subject, than which in matters concerning both medical theory and practice, I know of none of greater importance at the present time; and in order to justify such apparently unwarrantable presumption, both in claiming such pre-eminence for my subject, and still more in daring myself to treat of it, I may quote what Trousseau has laid down concerning specificity in disease. He said of it:—"It rules all pathology, all therapeutics, in a word, all medicine;"* and again, "Specificity is the key of medicine, without it it is impossible to proceed successfully in the practice of our art."† Now if I can succeed in showing, as I hope to be able to do to-night, that just as evolution goes behind specificity, so does the theory of evolution applied to disease swallow up the lesser and older theory of specificity, then it will follow, that just as Trousseau maintained, specificity ruled medicine, so does evolution rule and direct specificity; and if that be true my presumption may appear a little less unwarrantable. It would be as desirable as difficult, to start by sharply defining the terms:—specificity, specific, species, as applied to disease; but when the learned editor of Quain's *Dictionary*, under the word *specific* writes as a definition "the word signifies that such disease is produced by a special cause and has special characters," he commits a logical blunder which may well serve as warning to more humble and less gifted individuals who might attempt to follow his example.

Few terms indeed are more commonly, and perhaps more loosely employed in speaking of the classification of disease than the word *specific*. That small-pox, scarlet

* *Clinique Médicale de l'hotel Dieu de Paris.* Tom. 1, p. 574.

† *Ibid*, p. 587.

fever, and syphilis are specific diseases probably few would be prepared to deny; but to state precisely the grounds upon which one would differentiate the same from the conceived idea of common (*i.e.* non-specific) diseases would be a difficult, and delicate, if not impossible task. For suppose we were to set to work to divide all diseases upon the single basis of specificity, into which category the common or the specific should we place Erysipelas, Tubercle, Catarrhal and Purulent Ophthalmia, Gonorrhœa, Puerperal Fever, Typhoid, Diphtheria and other diseases *ejusdem generis?* There can be no doubt that the tendency of pathology during the present century, thanks chiefly to the enormously increased means at our disposal to facilitate diagnosis, and fostered by the teaching of Bretonneau and Laennec, has been to specificise disease; and while the recognition of the doctrine of specificity has done much to push on our knowledge by insistance on detail, and the estimation of minute differences, it is quite possible that this tendency may have served its end and surpassed its utility. That such is really the case, we are frequently and forcibly reminded by disorders which come constantly before us, the so-called anomalous cases, where disease does not breed true, or where hybrid symptoms are developed, cases which do violence to our theories and defy our classification. It is easy enough to call such cases exceptional, to pass them by as abnormal and think we have so dismissed them, because they do not approximate to what we have been pleased to select as our types; but nature is uniform and not vagarious and will have none of these hard and fast definitions we seek to impose upon her. Such procedure indeed is unscientific, for it does not embrace the whole of the phenomena, but only such as suit its purpose; it points to faultiness in our system, and our views must be expanded to meet the difficulty, not nature forced into our narrow ways of thinking.

I cannot help believing that the solution of this difficulty

lies in a more thorough recognition of the doctrine of evolution as applied to disease, as ruling and directing specificity.

Dr. Brunton, in his opening address here in 1882, spoke of Darwin's *Origin of Species* as having revolutionised our modes of thought in every department of mental activity. I maintain, that in that department of which Dr. Brunton is so able a representative, its influence until quite recently amounted to almost *nil.* I therefore propose, after referring to and criticizing the current creed, to bring forward an array of cases and facts in favour of a modification of our present notions of specificity, which have accumulated from a three years' study of the question. First permit me to read a letter of mine which appeared in the *Lancet* on May 14th, 1881, which, as it contains the pith of what will follow, may serve to introduce the subject.

"SIR,—Your editorial comments on 'Blood Diseases and the Germ Theory,' in a recent number of THE LANCET, induce me to send you the following reflections on the collateral question, as to how far our commonly accepted notions of specificity of diseases should be modified by the doctrine of evolution.

This doctrine, which has so rapidly and entirely altered the face of the organic world, as we conceive it, has as yet made but a feeble impression, if any, on medical science and pathology.

The importance of the principle of specificity in disease has been clearly and generally recognised since the time of Laennec and Bretonneau; Trousseau, in one of his clinics devoted to the subject, remarks of specificity, 'Elle domine toute la pathologie, toute la thérapeutique; en un mot, toute la médecine;' and the same belief, tacit or avowed, would seem to pervade all modern medical literature.

Now, the argument upon which medical writers base their theory of specificity consists in an appeal to the ana-

logy which exists between the natural history of diseases and the natural history of animals and plants. In the words of Sydenham, 'Unaquæque morborum non minus quam animalium aut vegetabilium species affectiones sibi proprias perpetuas ac pariter univocas ab essentia sua promanantes sortita est;' and until the last few years no better argument was possible.

Since, however, as regards the organic world, we have come to speak of an origin of species, and learnt to trace their differentiation under the influence of evolution to the eventful survival of the fittest, I would ask, should not our views concerning specificity of diseases undergo a similar modification? Is not the theory of specificity swallowed up in the larger theory of evolution? As with animals, so with diseases, may we not henceforth regard the several species and genera as descendents from a common ancestor? Starting from a non-specific inflammation or fever, may there not arise therefrom under favourable conditions an inflammation or a fever different from the parent stock, and capable of reproducing itself? I think so. Look at tubercle, a disease characterised by such well-marked features as to have been long since classed as a specific disorder, but whose natural history, revealed by the experiments of Cohnheim, Burdon-Sanderson, and others, shows that it may be derived by cultivation from the simplest common inflammation as that resulting from a chemical irritant; the non-specific has developed specific characters, the homogeneous become the heterogeneous. So it is with other diseases; even the ordinary stages of a common inflammation, pimple, vesicle, pustule, scab, foreshadow and typify diseases which we call specific, wherein one or other of the stages predominates, while the rest are subordinate. The common ancestry of specific diseases once recognised would do much to remove the hard and fast line so often drawn between disease and disease in text-books and dissertations, but of which nature knows nothing."

I now pass on to briefly consider the orthodox creed concerning specificity in disease, and in this we shall be struck by the wide diversity which obtains between the theories laid down in text-books and the facts of every day experience, so much so that judging by the latter alone you may be inclined to think that what I am arguing for to-night has been known and recognised and believed by every medical man who has had any experience at all, and were it not for the careful contradiction of common experience by the text-books to maintain a theory they hold so dear, I should be of the same opinion. The orthodox view about specificity now, is much the same as was propounded by Sydenham in 1666 and quite uninfluenced by the quickening energy of evolution, viz. "That each species of malady, even as each species of animal, and each species of vegetable, hath taken as its portion its own proper affections, permanent, unequivocal, derivative from its essence." Trousseau two hundred years later writes 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;" and the introduction of the germ hypothesis has, in the minds of many, served only to emphasize and establish this theory of disease.

Taking the class of so called acute specific fevers, and endeavouring by a consideration of them as put forth in text books, to ascertain the properties which give rise to the notion of their specific characters, I think we may enunciate the following as canons of specificity. That they never originate *de novo;* that they are contagious, infectious or inoculable; that one disease never passes into another, but that each runs a course *sui generis* and gives rise to characteristic symptoms; that they are auto-protective, one attack securing the patient from subsequent; that they often prevail epidemically, endemically, or even

pandemically; and that pathologically they are due to specific organisms peculiar to each.

If it be attempted to apply these canons, it is evident at once that many diseases accounted specific fail to comply with the tests. Thus relapsing fever whose specificity is supposed to be sanctified by the discovery of the spirillum germ does not protect against itself. Erysipelas fails in the same respect. Ague classed as a specific febrile disease by Dr. Bristowe is of course not contagious. Putting these aside we have left as the more essential attributes of specificity. 1. The fact that these diseases do not originate *de novo*. 2. That they run a definite, typical and unequivocal course and do not pass into one another, and 3. That each is due to the operation of its own specific microzyme, and although the discovery of such in the case of many diseases lies in the future, it presents to the specifically minded a consummation devoutly to be wished for.

Putting aside the last, and supposing the two former and remaining propositions to be true, let us see what, if that be the case, should follow, and then by comparing what should be, with what is, ascertain whether experience confirms them or reduces them to absurdity.

Well then if these be 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 occuring 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, even tubercle is bred in the laboratory upon a series of animals, 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, either with or without, what I for one think it necessarily implies and carries with it, viz., archebiosis, *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.

On the one hand then we have the Specificity theory which indeed is but a reiteration in the language of pathology of the principles laid down in the first chapter of Genesis, that in the beginning God created the germs of specific diseases, "each after his kind," that each has propagated itself in strict genealogy from that time to this, and if we are to follow Dr. W. B. Carpenter we must also believe that there were at the same time created specific susceptibilities to each and every one of these said diseases in our earliest progenitors, and ample provision made for the perennial perpetuation of the same.

On the other hand, we have Evolution, and whether we accept or not Archebiosis or spontaneous generation along with it, and sure I accept it, and using it as it has been used in the natural history of plants and animals, we find

it equal to explain pathological puzzles, and harmonize theory with practice.

It would be well before going on to bring forward examples of what may be called damaging admissions made by orthodox writers which invalidate and undermine the theory they contend for, but time will permit only a few.

Thus Dr. Bristowe whose faith in specificity is so strong, that he writes of epidemic diseases, they* "*never* pass into one another or lose their specific identity," and defines Scarlatina as "a contagious malady, characterised mainly by *a general punctiform scarlet eruption,* appearing usually on the second day, *and by inflammation of the fauces and tonsils,*" yet goes on to say "the patient may suffer from slight febrile symptoms only, with which may or *may not* be associated evanescent traces of a rash or some degree of roughness of throat," and tells of other cases in which "the patient seems to be struck down by the severity of his attack and dies, often before the rash had time to develop or before the affection of the throat had become a special cause of complaint." What I would ask of specific identity is left after the symptoms enumerated in the definition are taken away?

Mr. Erichsen affirms Gonorrhœa to be a specific disease, yet asserts it† "is constantly developed *de novo,*" and admits "it is impossible to say where urethritis or vaginitis ends and infective gonorrhœa begins."

Trousseau again, who insists that specificity is not a question of more or less, but absolute and essential, yet admits that‡ "the mechanism by which the eruptive fevers accomplish their manifestations on the skin and mucous membranes, has the greatest possible similarity to that which is in operation in the sweat exanthemata. In both cases there is a morbific matter in contact with the blood presenting itself to the different emunctories, and producing

* *A Treatise on the Theory and Practice of Medicine,* 1882.

† *Science and Art of Surgery,* 7th edit., vol. ii, p. 872.

‡ Vol. ii, Lecture XIV. *Clinical Lectures,* New Sydenham Society,

an irritation in them, the result being an eruption. The pathological lesion is equally produced by morbific principles traversing the emunctories, whether the agent be medicinal such as opium, belladonna, copaiba and mercury, or pathological, such as the elements of pus, the putrid elements of typhoid, the virus of small-pox, measles or scarlatina."

Dr. Andrew, writing on Febriculæ in Quain's *Dictionary of Medicine*, says they are of great *practical* importance, though he doubts whether theoretically such a class should be allowed to exist. It comprises he says:—Abortive or incomplete forms of some one or other of the specific continued fevers, instances of some of the exanthemata in which the usual rash is absent or so slight or brief as to pass unnoticed. Intermittent fevers in which paroxysms do not recur, or only at uncertain and distant intervals; and cases in which local symptoms normally attending certain forms of fever are very slight or very obscure and therefore difficult or perhaps impossible to detect; and he admits that diagnosis is nothing more nor less than the exclusion of all recognisable forms of fever, which he says are all of them at their outset occasionally mistaken for febricula. Indeed this febricula group is but a lame and impotent confession of the folly and inability of our rigid classifications, and the examples quoted will serve to show that practical experience wipes out the hard lines with which theory has circumscribed diseases, which it is pleased to regard as unalterably and eternally specific, and that when cut and dried classification has been pushed as far as it will go, there lies beyond its pale a large and important jumble of diseases of divers and often *de novo* origin, which approximate to, but yet differ from, our types, and which at present have no place in orthodox pathology.

I now propose to discuss the subject after the following manner:—

Origin of species. Under which I shall attempt to show, that specific diseases may develope independently of con-

tagion, that is to say *de novo;* that diverse diseases may own a common 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. I shall further endeavour to show under the head of Nondescripts or anomalous diseases, that we are here dealing with natural variations of species, under cultivation. That mongrels and hybrids are results of intermixture and crossing of species, and that all suchlike, instead of standing apart from the rest as incongruous and inexplicable, will now fall in, in perfect harmony and order.

First then, in endeavouring to show a reasonable presumption in favour of the *de novo* origin of specific diseases I will begin with those lying as it were on the out-skirts of specificity, and then proceed to those whose specific characters are considered more sacred and more fixed.

The origin of *Typhoid* independently of contagion has been so ably argued by Dr. Murchison, as opposed to Dr. Budd, and his testimony is so well known that I need not quote it to-night.* The same view has been supported by many observers well qualified to judge, but as I prefer to quote *facts* rather than *opinions,* I will refer to a paper by Dr. Alfred Carpenter of Croydon,† written in the belief

* *Vide especially,* pp. 470-497, *A Treatise on Continued Fevers,* 1873.
† *St. Thomas's Hospital Reports,* vol. i, p. 427.

that all zymotic diseases owe their origin in the first instance to insanitary conditions, in which he relates how that an outbreak of diarrhœa, in a large school, in an isolated district, attacked 54 out of 140 children in 3 days, and that in 3 cases it ran on to typical typhoid fever with rose spots, and lasting 21 days. He could discover no possibility of infection, but just before the outbreak the supply of water had been changed and taken from a well one yard from a sewer, he adds "the writer has met with many similar cases in houses quite isolated from neighbours, where it is evident the typhoid had a local and spontaneous origin, some of the houses being at considerable elevation on the Surrey Hills."

Dr. Thorne Thorne, whose wide experience at the Local Government Board, clothes his words with authority, says in his remarks on the origin of infection,* "no one having access to the reports of Medical officers of Health, can fail to be struck with the number of instances in which trained observers carrying on their investigations in isolated districts, far from the ordinary sources of error attaching to life in large towns, and making full allowance for all known sources of error, entirely fail to connect outbreaks of some of these diseases, especially diphtheria and enteric fever, with any antecedent case, whether near or remote, and how these observers feel themselves compelled to fall back on the view that such diseases, do at times have an independent origin."

The etiology of *Diphtheria* was so well threshed out in 1879 at the Medico-Chirurgical Society, and the opinions there expressed gave such ample corroboration of belief in the non-specific character of the poison, that I could cite opinions enough and to spare.

Sir Wm. Gull,† observed that "twenty years ago he had drawn a distinction between diphtheria and diphtheritic poison; regarding the disease as beginning with a poison,

* *Transactions of the Epidemiological Society*, vol. iv, part ii.

† *British Medical Journal*, May 3, 1879, page 666.

but *sometimes as commencing locally and forming a poison which propagated itself.*"

Mr. J. Hutchinson* "believed that the adoption of the view that diphtheria was not a specific fever, but rather a specialised form of local inflammation would remove much difficulty."

Dr. Thorne Thorne has given valuable evidence pointing in the same direction, which I shall allude to more appropriately in discussing the progressive development of contagiousness.

Dr. Steavenson in this room on Feb. 1st, 1883, told you how his experience at the Children's Hospital, led him to the belief that in its origin and mode of propagation, diphtheria was akin to pyæmia, and that the only cause of a serious outbreak of that disease at the Children's Hospital was "the demolition of four or five old houses on the opposite side of the street."†

Touching *Typhus* I know of no better example of its *de novo* origin than that cited by Dr. Creighton in his suggestive introductory address in Pathology at the British Medical Association at Liverpool last year.‡ He said, "we need not go farther back than twenty years ago, or farther away than the city of Liverpool, for proofs of the *de novo* origin of a specific fever; no more striking proof was ever given of the genesis of typhus out of dysenteric and other filth than in the case of the Egyptian frigate which came from Alexandria to the Mersey in 1862 to be refitted. There were four hundred Arabs on board, who had suffered a good deal from diarrhœa and dysentery; they had met with rough weather all the way from Alexandria, and the hatches had been battened down for two or three weeks continuously. When the ship arrived in the Mersey, the 'tween decks was in a disgusting state

* *Ibid.* p. 665.

† *Medical Times and Gazette,* Feb. 24, 1883.

‡ "On the Autonomous Life of Specific Infections," *British Medical Journal,* August 4, 1883, p. 223.

of filth, and the pilot who took the vessel up the river remarked to his wife when he went home that that ship would be heard of again. He was himself the first victim, for he was seized with a fatal form of hæmorrhagic typhus about a week afterwards. More than one hundred of the crew were on the sick-list, chiefly from dysentery, but it was carefully ascertained that none of the cases were typhus. Most of the Arabs went to a public bath in their filthy state, and in a few days typhus broke out among the bath attendants. Some of the crew were admitted into the Southern Hospital for various non-contagious disorders, and there also typhus broke out."

Coming now to diseases the absolute specificity of which is held by most to be proved beyond question; I will quote a passage from an instructive and far-seeing speech made by Prof. Hüter of Greifswald at the International Medical Congress in 1881.* He asked,—"Is it necessary that a person who falls ill with scarlatina, measles or variola, must have received the infective material from another patient affected with scarlatina, measles, or variola? I, for my part, answer no, while the defender of the theory of absolute specificity must answer in the affirmative. I consider it possible that at any spot the putrefactive processes may take on such a course that micro-organisms arise from them which produce scarlatina."

Mr. Millican who has been working carefully at this question for some time and whose book on *The Evolution of Morbid Germs*† is a valuable contribution to the subject, in September 1883 sent out a circular to medical men asking pertinent questions relating to the etiology of acute specific fevers. One of these questions was,—Have you ever seen, and how often, cases where careful investigations lead you to the conclusion of a *de novo* origin for each of the several acute specific fevers? And in the 58 replies received, 97 such cases were recorded, in 31 of which however some room for doubt was left, among the established

* *Transactions*, vol. i, p. 329-330.

† London: H. K. Lewis, 1883.

cases, there were 24 of the *de novo* origin of typhoid, 24 of diphtheria, 13 of typhus, 13 of scarlatina, 9 of whooping cough, 7 of measles, 4 of rötheln, and 1 each of smallpox, chickenpox, and cholera.

The reason for this great diversity in respect of these different diseases, I will attempt to indicate later on; all I want to show at present is that there is a reasonable presumption in favour of a belief that specific diseases may nowadays, under our very eyes, arise *de novo*.

If I may be allowed to quote a somewhat racy and unconventional, though I believe thoroughly sound and rational, passage from Florence Nightingale's *Notes on Nursing*, I am sure it will be received with respect if not with approbation. She says.* "Is it not living in a continual mistake to look upon diseases as we do now, as separate things that must exist, like cats and dogs? Instead of looking upon them as conditions, like a dirty and a clean condition, and just as much under our control: or rather as the reactions of a kindly nature, against the conditions in which we have placed ourselves. I was brought up, both by scientific men and ignorant women, distinctly to believe that small-pox, for instance, was a thing of which there was once a first specimen in the world, which went on propagating itself, in a perpetual chain of descent, and that small-pox would not begin itself any more than a new dog would begin without there having been a parent dog. Since then I have seen with my eyes and smelt with my nose small-pox grow up in first specimens, either in close rooms or in over-crowded wards, where it could not by any possibility have been caught but must have begun. Nay more, I have seen diseases begin, grow up and pass into one another. I have seen for instance, with a little overcrowding, continued fever grow up; and with a little more typhoid fever, and with a little more typhus, and all in the same ward or hut."

Cholera has been shown by Surgeon General Gordon,†

* Page 29, *Notes on Nursing*, 1861.

† *Medical Press and Circular*, August 1st. 1883, p. 83.

to arise spontaneously in some instances; to be but an exaggerated diarrhœa, he says "when cholera prevails it is difficult to decide correctly when diarrhœa as such ends, and when distinctive cholera begins. The symptoms of the two affections differ in degree rather than in kind."

Dr. W. B. Carpenter, in the *British and Foreign Medico-Chirurgical Review* thirty years ago, related how an outbreak of cholera at Taunton was due to a cargo of putrid oysters, the sale of which was prohibited, being distributed among the children, and all the children who ate the oysters were attacked with cholera.

Scarlatina. Surgical scarlatina, so called, and puerperal scarlatina have long been considered puzzling and anomalous, for my own part 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

* *Clinical Lectures*, p. 349. Scarlet fever after operations.

† Vol. xii, *Obstetrical Transactions*, pp. 47—59.

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 period, it may be fairly urged that in all of fifteen recorded cases the disease appeared on the second or third day after operation, this constancy in appearance rendering such theory improbable, and suggesting a close analogy to traumatic fever, and pointing to a similar causation.

Small-pox is so commonly seen to result from contagion that belief in its spontaneous origin is entirely rejected by many, yet cases incapable of other explanation are recorded. Thus, Dr. Hubert Boëns* relates an interesting case occurring in the prison of Chaleroi, no other of the kind made its appearance previously or subsequently, and there had been no small-pox in the town for more than a year. Mr. Millican† has also recorded a case which, however, as with spontaneous specimens generally, presented deviations from the normal type. Dr. Bridges in his report to the Local Government Board concerning the small-pox epidemic in London of 1876, showed after the almost complete extinction of the disease in 1875 in Greenwich, the first case from which the disease spread, occurred in a particularly empoverished and overcrowded part, the neighbourhood of Deptford Creek.‡

It is however not surprising, but indeed quite natural, that instances of *de novo* origin of the more specificised

* *Bullet. de l'acad. r. de Médicine de Belgique*, 3e ser., t. xv, no. 7.

† *Lancet*, March 18th, 1882, p. 433.

‡ Smallpox (Metropolis) *Parliamentary Returns*, no. 75, 1880.

diseases, should be few and far between. Indeed the greater the specificity acquired, and the further the departure from the common type, the less is *de novo* origin possible; simply because cultivation has conferred a fixity of form which nascent species do not possess.

Local specific infections too, are constantly seen to arise *de novo*. Gonorrhœa for example. The same may be said of catarrhal and purulent ophthalmia which are continually arising *de novo* under favourable conditions of filth.

The specificity of tubercle, of which Laennec was full, has fallen to the ground by the manifestation of its origin in a common catarrhal pneumonia, so brilliantly elucidated by Niemeyer.

I ought not to proceed without alluding to the usual replies that are given to those who believe in *de novo* origin of disease. They are mainly two, and they are neither complimentary; the first disputes the diagnosis, the second insinuates insufficient investigation; the one says you call things alike which are not alike, but which only appear alike to you, the other says the history of contagion could in every case be found if you only knew where to look for it. They are plausible enough, but eminently unsatisfactory, for they leave the matter in blissful ignorance, and simply give it up. But there is an insuperable objection to these panspermists which has been often repeated, and is this, *how do you account for your first specimens?* Either you must believe the special creation theory to hold good for pathology as for orthodox theology or you must admit that "once upon a time" diseases did develope *de novo* and if you admit the latter the burden lies with you to show cause why, what occurred in times past, should not happen again and again.

I next come to consider a group of phenomena under the head of *diversity from unity*, and discuss the influence and importance of *predisposition* to disease and the effect of sowing the same seeds on different *soils*. I will at once

quote examples to illustrate what I mean. Sir James Paget relates the following :—

"One of the crew of H.M.S. Rattlesnake, after slightly wounding his hand with a beef-bone, had suppuration of the axillary lymphatic glands with which typhoid symptoms and delirium were associated, and proved fatal. His illness began the day after the crew left Sydney, where all the crew had been remarkably healthy. A few days after his death, a sailor who washed his clothes had similar symptoms of disease in the axilla, and for four or five months he suffered with sloughings of portions of the areolar tissue of the axilla, arm and trunk on the same side. Near the same time, a third sailor had diffuse inflammation and sloughing in the axilla; and after this the disease ran *in various forms,* through the ship's company, between 30 and 40 of whom were sometimes on the sick list at once. Some had diffuse cellular inflammation, some had inflammation of the lymphatic glands of the head, axilla, or lower extremities; one had severe idiopathic *erysipelas* of the head and neck; another had phlegmonous erysipelas of hand and arm after an accidental wound; others had *low fever,* with or without enlargement of glands. Finally the disease took the form of *mumps,* which affected almost everybody on board."* Here we have apparently a simple case of pyæmia running into erysipelas, low fever in various forms, and even mumps.

An excellent instance of influence of soil in specificising effects of a common poison is the following from the researches of Piringer quoted by Soelberg Wells,† he showed that "the discharge of a severe purulent ophthalmia, if applied to a healthy conjunctiva, may reproduce the disease in 6-12 hours, that from a mild case in 60-70, and that from chronic ophthalmia in 72-96 hours," and he adds "it is of the greatest importance to remember that the

* Paget's *Surgical Pathology,* 3rd. Edition, page 373.

† *Diseases of the Eye,* 3rd, Edition, p. 29,

discharge from a purulent ophthalmia does not always reproduce the purulent form, but may give rise to catarrhal, granular, or even diphtheritic conjunctivitis, just as the discharge from catarrhal, diphtheritic, and acute granular ophthalmia may produce purulent ophthalmia (an instance of reversion of type). The special form of conjunctivitis which may arise will depend upon atmospheric, local, and constitutional causes, and also upon the age of the patient. Thus Von Graefe states that at Berlin, the matter from ophthalmia neonatorum, when applied to the eyes of children two or three years of age, generally produces diphtheritic conjunctivitis, whereas when applied to adults it mostly gives rise to purulent or sometimes to granular ophthalmia." Experiments, I think, as instructive as they are unwarrantable.

Dr. Holland in an article entitled the "Propagation of Diphtheria by mild cases and disguised combinations,"* relates how two children suffering apparently only from nasal catarrh and slight scarlatina introduced the disease into a school in St. John's Wood, and twenty-four failed with "every conceivable variety of diphtheria and scarlatina with and without rash; and the varieties presented such a progressive fusion in their protean combinations that he regarded the diseased states as modifications of one process, and the evolution of a similar germ in dissimilar pabulum."

Prof. Virchow at the International Congress, 1881,† spoke of the same thing going on outside the body in regard to the *bacillus anthracis*, and told how "by giving the organism a special kind of soil and a more vegetable diet it might be made innocent; by a more nitrogenous diet, and under other circumstances, it might be made wild or malignant again," he remarked how this confirmed the ideas developed by Lister, who had found that

* *British Medical Journal*, Feb. 1st, 1879, p. 148.

† *Transactions*, Vol. I., p. 322.

the power of resistance of unhealthy parts was small, but of healthy parts much greater. "*We should thus come back*" he said, "*to the old doctrine of weakness and strength as explaining liability to disease, of which he had always thought highly.*" Sir Thomas Watson meant the same thing when he said, "that by obviating all causes of debility and fortifying the system, we walk with comparative security amid surrounding pestilence."*

Dr. Dickinson speaks of "diarrhœa, purulent ophthalmia, erysipelas, pharyngeal diphtheria and croup or laryngeal diphtheria, all arising from the same cause."†

In puerperal fever, in its multifarious forms, we see the process of pathological evolution whereby several different diseases, often resembling (or as I should say identical with) the acute specific diseases, are differentiated from a common ancestral condition. Dr. de Gorrequer Griffith has written more forcibly on this subject than any one else, and various publications of his, with which he has favoured me, dating from 1875 up to the present time, are devoted to expounding the doctrine of "unity of poison in scarlet fever, typhoid and puerperal fevers, etc., etc.; diphtheria, erysipelas and sore throats; certain forms of diarrhœa and allied affections, usually considered separate and distinct," and he explains how that "by unity of poison, he means *not* that the poison is always the same, but that one poison, the one *origo mali*, whatever it may be, will originate several so-called different affections."

Dr. Sansom‡ considers "the differences of soil even of more importance than those of organisms in determining the nature of the specific disease."

And Sir James Paget in his eloquent Bradshawe Lecture said,§ "just as in agriculture, soils must be studied as

* *Lectures on the Principles and Practice of Physic*, vol. i, 1848, p. 77.

† *British Medical Journal*, May 24th, 1879, p. 779.

‡ *Lancet*, March 11th, 1882, p. 397.

§ *Lancet*, December 16th, 1882, p. 1020.

well as seeds; seeds will not germinate in an unfit soil. And even among those in whom they do germinate the product varies according to the soil."

To sum up this side of the argument then, I contend that starting from *common ancestral conditions* we arrive at *specific diversities,* and that the chief, if not the only element in determining specificity is the *nature of the soil,* in which the poison (whatever be its nature) grows, that is to say on the *predisposition of the individual.* That the predisposition varies with the departure from a state of health, local or constitutional, but that just as in some cases, so great is the virulence of the poison *as the result of cultivation on suitable soil,* that it may even overstep the usually sufficient barriers of health and attack the unpredisposed, so on the other hand predisposing and exciting causes being as it were complimentary, predisposition may be so strong that, apart from contagion, it may develope disease *de novo.* Such disease, however, as with nascent species generally, being more plastic, less fixed, than the more cultured variety and often appearing in aberrant form.

I next pass on to speak of the origin of contagion, the progressive development of infection, contagion or inoculability, as three degrees of the same thing are severally termed. The subject is corollary to what has gone before, its importance is self-evident, it has occupied the thought of several competent observers and thinkers, and has been enunciated in words far more eloquent than I can command by Mr. J. Simon. I quote from a Blue Book, and take this opportunity of offering a tribute of praise to the Privy Council for the enterprising nature of the researches which their late Medical Officer inspired them to ordain. He says,*—"I would mention it as among the most hopeful advances of modern preventive medicine, that some diseases, which, in the sense of being

* *Public Health Reports,* New Series, No. 2, pp. 15—16.

able to continue their species from man to man, are apparently as "specific" as those which I have above-named, seem now beginning to confess in detail a birth-place exterior to man, a birth-place amid controllable conditions in the physical nature which is around us, a birth-place amid the "common," putrefactive changes of dead organic matter. Referring again now to what I have not pretended to be able to analyse in detail — the excess of miscellaneous, and in great part nominally "common," disease in filthy neighbourhoods, I would particularly wish to connect with that subject a reference to our growing scientific knowledge in the matter of the "common" septic ferment. The pathological studies of late years, including eminently certain very instructive researches which Professor Sanderson has conducted under my Lords of the Council, have clearly shewn that in the "common" septic ferment, or in some ferment or ferments not hitherto to be separated from it, there resides powers of disease-production *as positive, though not hitherto as exactly defined, as those which reside in the variolous and syphilitic contagia.* Experimentally we know of this ferment, that, when it is enabled *by artificial inoculations* to act in its most effective way on the animal body, and even more when it has received *a curious increment of strength which its first propagation within the living body seems to bestow on it,* it shows itself *one of the most tremendous of zymotic poisons.* It rapidly in the one animal body developes disease *which then is communicable to another*: febrile disease, with inflammations numerous and intense, and including in marked degree one of the acutest known forms of intestinal inflammation and flux: disease exactly corresponding to certain very fatal and unfortunately not infrequent infections to which lying-in women, and persons with accidental wounds and the wounds of surgical operations, are most subject, but which also sometimes occur independently of such exceptional states; infections, chiefly known under the names of *erysipelas, pyæmia, septicæmia,*

and puerperal fever; infections,' which *we sometimes see locally arising anew in unquestionable dependence on Filth, but of some of which, when arisen, it is perfectly well known that they are among the most communicable of diseases.* And a further, perhaps still more instructive, teaching of the artificial infections is this; that the "common" ferment, which in its stronger actions quickly destroys life by septicæmia, can in slighter actions start in the infected body chronic processes which will eventuate in general tubercular disease."

Dr. Burdon Sanderson* in the experiments to which Mr. Simon alludes, showed how starting with a simple common inflammation, that produced by a chemical irritant (boiled *liquor ammoniæ*) and inoculating the product obtained from the original animal upon a living series he could construct as it were a virulent and increasingly virulent poison, killing by acute septicæmia or in milder action originating tubercular disease.

A striking instance of the same thing came under my notice when Ophthalmic House Surgeon. A woman came to the surgery complaining that two days previously she noticed sudden pain in her left eye as if something had got into it; the eye watered a good deal that evening and discharged a little; next day the other eye began to suffer in somewhat the same way, and on the evening of the second day the right eye of her child, aged three years, became inflamed and discharged pus. I found and removed from beneath the left upper eyelid of the mother, a small foreign body and the ophthalmia was rapidly cured. I looked upon this (as I carefully sought for and failed to discover any history of contagion) as an example of contagious ophthalmia starting from a simple irritant and gathering in the going.

The evolution of diphtheria from simple sore throat has been ably argued by Dr. Thorne Thorne in a paper on

* *Public Health Reports,* New Series, No. 6, 1875, p. 70.

"The Origin of Infection,"* he says "In isolated districts and in houses situated at times miles away from other habitations, and in some instances lying in lonely spots among the mountain ranges, I have met with instances of what appeared to me nothing more than a simple inflammation of the throat, at times so trivial that it has passed all but unnoticed, and yet it has led by transmission through other persons to cases of well marked and severe diphtheria." An instance he urges of "progressive development of infection." Dr. Airey in the same report has an article pointing in the same direction entitled "On Infection from a Darwinian point of view."

This view of infection indeed did not escape the vigilance of Sir Thomas Watson, in his immortal lectures on the "Principles and Practice of Physic," he pithily ventures the remark,† "there is nothing absurd nor unlikely in the supposition that diseases may first arise from some other source than contagion, and then become capable of spreading by contagion; and that in all cases, even when the contagious principle is most manifest, there seems to be something else required besides the presence of contagious matter; there must be a readiness to receive it, a susceptibility of its influence on the part of the person exposed to it; a predisposition which is less common in regard to some diseases than to others; but without which there is scarcely any complaint that can be so propagated."

It would appear then to be a just conclusion from these data, that infectiveness is no longer to be considered as a mark of specificity, but that it is a property resident in every inflammation and every fever, even in those arising from the commonest, and most unspecific, causes.

If what I have urged above holds good, if different diseases arise *de novo* from similar or identical external con-

* *Epidemiological Society's Transactions*, vol. iv., part ii.

† *Lecture* XVII, vol. i, p. 296.

ditions, or poisons operating on diverse internal conditions, we should expect to find in studying the natural history of disease, instances of imperfect evolution, also cases which do not breed true, a vicariousness of disease; cases again where two or more diseases are inextricably blended, examples of hybridism; cases of reversions to simpler types; and lastly aberrant species, freaks or nondescripts, presenting resemblances to, and differences from diseases on either side of them; their anomaly being due to the loss of connecting species, or peculiarity of cultivation, or some forced condition of growth.

As instances of transmutation of disease in transmission, I may refer to a paper by Dr. Meredith,* entitled "Scarlatina evolved from Diphtheria," in which he relates how that at Wellington, in Somersetshire, there had been no scarlatina for months, but undoubted diphtheria, which was introduced into a household by a servant; the cook, who occupied the same bed, caught diphtheria from her; a few days after, three children in the house consecutively failed with scarlatina, and lastly the mother succumbed to diphtheria."

Dr. Thorne Thorne related to this Society on Feb. 20th, 1868, an instance, "in which undoubted varicella was transmitted through four individuals, in each new case assuming more and more the distinctive characters of small-pox, and finally producing over two-hundred well marked vesicles, and causing death."†

I must again quote a sapient passage from Sir Thomas Watson, who says, "There is *no* line of genuine distinction between continued fevers that can be relied on. They run insensibly into each other, even the most dissimilar of them; and are traceable often to the same contagion."‡

* *Lancet*, May 26th, 1883, p. 901.

† *Minutes of the Abernethian Society*, 1868.

‡ *Practice of Physic*, vol. ii., p. 723.

Lastly, I come to the nondescript class, and it will not need many quotations to prove that such a class, and a very large one too, of irreconcilables to classification exists. Neither need I say much under the head of hybridism in disease. The explanation of the latter is the blending of two or more diseases in one individual; of the former, the isolation of certain intermediary forms by the extinction of neighbouring links, or the exaggeration or sporting of what was originally but an aberrant variety of a recognised type.

The case of Mr. Fawcett is fresh in our memories, where diphtheria, erysipelas and typhoid conspired to occasion an alarming complication of symptoms.

Sir James Paget has lately insisted on so-called "bad specimens" of disease, "hybrids and mongrels" he says, "must be even more common among diseases than among species and varieties." He cited Charcot's arthropathies, osteitis deformans, and gouty phlebitis, as recently evolved diseases.

He even suggested the joint lesion was the mongrel offspring of osteo-arthritis and syphilis; and osteitis deformans, the combination of transmitted dispositions to gout, rickets and cancer.*

Prof. Virchow has asserted his belief that diphtheria might combine with scarlatina, variola, or with many other specific infective diseases.†

I have myself witnessed a variolous rash in the course of scarlet fever; several hybrids between scarlatina and diphtheria, typhoid with Rötheln rash, and puerperal fever closely simulating typhoid; I have also many notes of nondescript cases; one case in Radcliffe, in 1880, I particularly would cite, viz., Ellen Tipping, aged 10, who was admitted with symptoms apparently indicating the onset of an acute fever, rigors and so forth; on the second day

* *Lancet*, Dec. 16th, 1882, p. 1019.

† *Transactions, International Medical Congress*, 1881, vol. i, p. 322.

a scarlet rash appeared on the legs, on the third day a papulo-petechial rash appeared on the face, and later on large vibices were developed on all portions of the body. There was diarrhœa with pale slimy stools, and later melæna, there was marked œdema of the ankles, and she complained of epigastric and articular pains. The temperature, however, was never over 100·5, that was on the fifth day. She rapidly improved, although the case was at first deemed almost hopeless, and during convalescence a curious urticaria-like rash appeared on the extensor surface of the arms.

Mr. Millican, in the work to which I have already referred,* details a case in which typhoid, typhous, variolous, and diphtheritic symptoms were intimately blended.

These instances are, I trust, sufficient to establish the existence of hybrid and nondescript diseases, and furnish an important link in the chain of evidence I have attempted to sketch. Had time permitted, I desired to apply the same principle to local inflammations, and endeavour to trace the evolution of specific local lesions from different stages of a common or simple inflammation, as for instance, the various classes of skin diseases, erythemata, papulæ, vesiculæ, pustulæ, and squamæ from the stages of redness, pimple, blister, pock, and scab of ordinary inflammation; and likewise the ætiology of carcinoma, sarcoma, and the specific lesions of syphilis, etc., from ancestral common types.

I have made so great a demand upon your patience, and the nature of the subject has compelled me to repeat myself so frequently that anything in the shape of recapitulation would be wearisome as well as unnecessary. But, I would ask in conclusion what is the practical bearing of the question I have raised, in what way does it affect the germ theory of disease, and what influence should it throw on our method of treatment? And I would assert

* *Evolution of Morbid Germs*, p. 6.

that whether you hold the germ theory of disease or the chemico-physical theory or any other, this doctrine need up set none of it. For panspermists to reconcile their views with these, it is only necessary for them to believe that organisms of an indifferent nature are capable, by cultivation on suitable soil, of taking on noxious and specifically noxious properties. For those who hold the chemico-physical theory it is only necessary to regard the process of evolution of disease after the fashion of the construction of a complex organic compound, perhaps of an alkaloidal nature. While others again who may be inclined to consider the *materies morbi* of specific disease in the light of a secretion from an infected body, and any micro-organisms found therein, as being of and from the body, and once separated therefrom, as capable of autonomous life and further development, will probably see in this theory of disease strong confirmation of their views.

And now a last word concerning the term specificity; rightly understood and studied in the light of evolution it has yet a useful and important meaning; *absolute* specificity indeed is destroyed, but *relative* specificity is as true as ever and indeed truer than before and is simply an expression of the amount of cultivation of any virus, and of the extent of departure from a common ancestral state.

Lastly, as to treatment. To look on disease in this light is indeed the final blow to superstition in therapeutics, prophylactic or curative; it is withal a comfortable view, some might be disposed to regard it as a religious or providential view; it teaches that disease is not inevitably the fate and birth-right of every born child, but is in fact of our own making, it is not outside our knowledge and our power, but within our own control. That filthy conditions and the imperfect removal of effete material, without and within the body, are the factors of zymotic pestilence, which aforetime walked in darkness, but is now made plain by the revealing light of science. And just as the evolu-

tion theory of disease asserts the *origin* of disease so does it foresee the possibility of its *abolition*, at any rate so far as zymotics are concerned; while it was an integral part of the old specific theory to affirm that disease was from everlasting to everlasting, as it was in the beginning it is now, and ever shall be, and that salvation could only be purchased by the propitiating influence of a previous attack.

GENTLEMEN,—In conclusion I beg to apologise for the length of this paper, I apologise for the number of quotations it necessarily contains, I apologise for the not very logical order in which it has been thrown together. I thank you heartily for the indulgent attention you have extended to me, and I am sure you will not be slow to give me your very candid criticism.