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AN ADDENDUM TO A PREVIOUS PAPER ENTITLED "PATHOLOGY," FROM AN "EVOLUTION POINT OF VIEW."

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"The elephantiasis of India is found in Ceylon. There, again, it only attacks natives, Creoles, and individuals of mixed blood. Hindoos and Europeans are exempt from it. *Only one case* of this disease had been observed in a European white.

"But this individual had inhabited the island for thirty years." Acclimatisation, he adds, had been carried so far in his case as to cause him to lose his ethnological immunity. In other words, this acclimatisation in some way or other must have modified the internal mechanism of this European sufficiently to create a susceptibility to disease for him which before did not exist. Only on this assumption can this fact be explained.

It would be interesting to pursue this part of my subject further. I have already, however, exceeded the time I have a right to expect you to give me. I shall, therefore, content myself with the hope that I have said enough to demonstrate how important it is for us to recognise in pathology, as in other sciences and departments of thought, the great fundamental principle indicated by the "Law of Evolution," and how, on the assumption of its applicability to questions of disease, what we now regard as abnormal processes may be brought into normality and unity with everything around them.

AN ADDENDUM TO A PREVIOUS PAPER ENTITLED "PATHOLOGY," FROM AN "EVOLUTION POINT OF VIEW."

(*Read at the May Meeting of the South African Philosophical Society,
1885.*)

BY J. H. MEIRING BECK, M.B., C.M., M.R.C.P., ED.

It was not my intention this evening again to bring forward the subject which at the last meeting of this Society I had the honour to introduce. One or two of the members present then, who were interested in the remarks I made, however came to me after the meeting, and requested me to write an addendum to my paper, inasmuch as it was thought that by so doing I should sustain, or at any rate revive whatever little interest was roused then. I hope therefore that you will bear with me, and that if I demand from you what may be considered perhaps too much indulgence, you will kindly lay the blame not so much at my door as at that of the gentlemen responsible for my communication of this evening. I shall try to be as brief as possible.

* * * * *

It will be remembered that I divided my subject into two parts. In the first I dealt with the "Exciting factors"—the germs of disease, and endeavoured to explain how I thought the law of evolution influenced them, as I believe it influences other living organisms, and how as clinical students we were fortunate in having phenomena to observe, associated with living forms so low in the scale of life, and possessing so remarkable and rapid a power of multiplication.

In the second portion I endeavoured to shew briefly how the same laws acting upon the living human organism, and its component cells, might also be inferred to influence *them* so as to establish as it were a receptivity for, or resistance to, this exciting factor.

To make clear my position I quoted cases which had occurred in my own practice which seemed to me to present features bearing upon my ideas. I have since taken the trouble to look into published records from others, and I have been both surprised and pleased at the mass of evidence which has accumulated in support of the views expressed by me. Some of this evidence I propose to bring forward, restricting myself as far as possible within bounds, which in a mixed Society like this it is necessary to observe. I propose again to follow the plan before laid down, and to take up briefly the consideration of my subject from the side taken in my last paper.

Firstly, then. The consideration of cases which seem to indicate the fact of *germ* transmutation. About a year ago was published a very remarkable book by Dr. Kenneth Millican, which to a great degree expresses what I have often thought.

I should in passing observe that prior to the publication of the book I had written and read to various colleagues of mine views so similar to Dr. Millican's that I was in many cases quite startled at the similarity of our ideas. I mention this to define my position with regard to my theories, and at the same time that I may be enabled to express my appreciation of the completeness and originality with which the author has worked out the subject. In the book in question a case is reported which occurred in Dr. Millican's practice, and which I give in his own words as far as I can conveniently do so. He writes:—

"I first saw my patient, a strong healthy farm labourer, twenty years of age, on the third day of a severe feverish attack, when I found the evening temperature 102° F. He had been 'out of sorts' some week or ten days previously. * * * He lay prostrate, at times delirious, and had two or three '*Diphtheritic*'-looking patches on the tonsils. I succeeded with difficulty in detaching a small portion of the membrane from the tonsils, but found on my next visit that the patch had been renewed. On the twelfth day of the fever a crop of '*rose spots*' were visible on the body. These both in their character and appearance resembled the rash of typhus. The temperature had risen to 105° F. * * On the fifteenth day, three days after this extreme temperature, it fell again to 100° F., and the patient seemed easier. The mother called my attention to some shotty *papules* on the body.

"The seventeenth day, two days later, saw the first crop of rose spots almost entirely replaced by new ones.

"By the twenty-first day of the fever, or six days after the appearance of the papules, he looked, as the mother remarked, for all the world as if he had been vaccinated. The papules ran the course of an ordinary small-pox eruption, and after the thirtieth day of the fever he began to mend rapidly." Dr. Millican adds:—

"The combination of symptoms here was remarkable. But for the astounding nature of the assertion one would have felt inclined to say that the patient was suffering from a combination of typhus fever, with diphtheria and small-pox. It was found quite impossible, after the most careful search, to trace any source of contagion for any one of the diseases which were here presented or counterfeited. There was no possible chance of ordinary infection, for the patient lived in an isolated village, from which he had not been so far as four miles for many months.

"There had been no contact with any person labouring under any kind of infectious disease. There were *no cases of infectious disease* of any kind existent at the time in the village or neighbourhood."
 * * * * * It will be readily admitted that the above case offers ample food for reflection, and suggests strongly that by some subtle influence existing at the time in the body there was a struggle for development in different directions of the infecting factor. Either this must be inferred, or the conclusion arrived at of infection simultaneously and separately by the specific germs associated with the diseases named—none of which diseases in the most clear manner possible Dr. Millican asserts were anywhere to be found at the time. But other anomalies are also recorded, which strengthen me in my convictions. For instance, in the *British Medical Journal* for February 1, 1879, Dr. Holland records a case where a pupil at a school contracted "decided scarlatina" from two new pupils, who "though doing ordinary school duties had a nasal discharge and excoriation of the lips, attributed to cold."

During the second week the pupils fell ill in rapid succession to the number of 24 or 25, and the school was broken up. The invalid presented—and this is the remarkable point—every conceivable variety of *diphtheria and scarlatina*. In other words, from an anomalous indeterminate infecting source common to both, seemed to arise two diseases hitherto regarded as specifically distinct from each other. The explanation suggests itself that an indeterminate germ originally finding dissimilar conditions in the various pupils developed in some in the direction of diphtheria, in others of scarlet fever; and curiously coincident with this explanation of these cases are some remarks made by Professor Hueter, of Grufswold, at the International Medical Congress at London, 1881. He spoke as follows:—

"Practically, the question of the specific functional activity of micro-organisms and their relative unity may be stated something in this way:—'Is it necessary that a person who falls ill with scarlatina, measles, or small-pox must have received the infection from another individual affected by scarlatina, measles, or small-pox? I for my part answer the question with No! while the defender of the theory of absolute specificity must answer it 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. The disease might then originate at this spot without there having been any absolute continuity of infection from another case of scarlatina.'" (*Vide Transactions Int. Med. Cong.*, vol. 1, p. 329). Coming from an authority and observer like Hueter, these words are entitled to respect, and if the cases I have quoted above are worth anything they exemplify in a marked manner the truth of his assertions.

Writing in 1872 Professor Stokes remarks (*Vide British Medical Journal*, April 13th, 1872—A Discourse on State Medicine):—

"No one who has not had a lifelong experience of epidemics can estimate the difficulties which exist as to their origin. * * * * * The appearance of epidemics at irregular periods, while their supposed exciting causes remain constant; their disappearance though the causes continue in full operation; their outbreaks in all latitudes, climes, and seasons; their different modes of spreading; the want of constancy in their symptoms and history (for every great epidemic

has its own character); the varieties as to the extent, nature, and effect of the secondary affections which arise in their course; the varieties of the mode of subsidence and behaviour under treatment; their degree of mortality and contagiousness,—all these things constitute the difficulties which surround us in our investigations as to zymotic diseases. They bear on the supposed specific or constant origin of disease, on the error of drawing hard and fast lines between essential affections, *and are with difficulty* reconcileable with the Germ Theory." * * * * * Reconcilable with difficulty indeed, I would add, if we regard with Professor Stokes our disease germs as independent and fixed entities. Reconcilable with the greatest ease if we once allow our doubts and perplexities to be illuminated by the light of "*Evolution*," in as far as it exercises a modifying influence on our disease germs. Illuminated by this light Professor Stokes' difficulties disappear not only, but so perfect a resolution of all perplexity is suggested that one is irresistibly compelled to fly to it in order soundly, practically, and philosophically to clear up doubt, and as irresistibly compelled to accept its application in association with the Germ Theory, as one of the grand *facts* of Pathology. It would be utterly impossible to quote here all the evidence at hand which seems to me to bear upon my subject matter.

I have been content with quoting more or less typical illustrations, and I hope that I have supplied in this connection additions which will not be regarded as unimportant. * * *

It remains for me only to add a word regarding the second portion of my subject. That portion which deals with the internal mechanism of the body, and with its adaptation to outward circumstances.

What I said in my last paper will be remembered, and need not be repeated.

In the *Lancet* of December 16th, 1882, appeared a remarkable lecture by Sir James Paget "*on some New and Rare Diseases*," in which a passage occurs so admirably emphasising what I would like to express that I shall quote it *verbatim*.

This is the passage:—"Perhaps the brilliant success which has been achieved by the recent studies of disease-producing organisms or other materials acting on us from without—a success not equalled in any other field of medical enquiry—has made some think too little of those changes within ourselves which occur in such ordinary conditions of life that they may be called spontaneous.

"Yet these are not less important in the production of diseases, and these must be studied—just as in agriculture *soils* must be studied as well as *seeds*. This is true even in respect of those diseases whose essential causes are most evidently external, even of those which are due to specific contagion, their germs or seeds, if I may so speak, will not germinate in an unfit soil. I suppose there is not a day in which most of us do not inhale or come into contact with the germs of some frequent or contagious disease; but they do not germinate in us any more than do the seeds of tropical flowers in our streets, or in the fields to which the wind scatters them; we do not offer the fitting soil. And even among those in whom they do germinate the product varies according to the soil. And the study of this soil, *this living soil*, is yet more necessary in respect of diseases which come in part or wholly by inheritance; for it is in each as personal and distinct as any

other constituent of personal character, and the study of it must be ultimately personal."

This question of soil, then, is an element not to be disregarded, and associated with its adaptations in various directions are the questions of "*Predispositions in Disease*," the establishment of "*Diathesis in Disease*," "*Immunities from Disease*," &c.

Each one of these developments I touched upon in my last paper, and I endeavoured to shew by concrete examples, how marked adaptations could demonstrably be shewn to have taken place in various directions, of living cell elements *in the body to germs outside*.

It would have afforded me much pleasure to go more fully into this aspect of the matter to-night. Want of leisure, and shortness of time however force me to content myself with what I have said. In conclusion, let me hope that I have succeeded in making clear my views, and that I have supplied a few thoughts which are worthy the consideration of the members present.

Of necessity it is not possible to avoid technicalities in a communication of this sort, but I have tried to be as careful as possible to express in general terms my meaning. Whether I have in consequence appealed as forcibly to you as I should have liked is a subject of doubt to me. At any rate I shall, I hope, be credited with good intentions, and if I have excited even the smallest amount of interest I shall consider myself amply repaid for my slight trouble in preparing these notes.

ON FOSSIL PLANTS FROM INDWE AND CYPHERGAT COAL BEDS.

BY DR. JOHN SHAW, F.L.S., &c.

Mr. R. W. Murray, of the *Cape Times*, handed over for my inspection specimens of shale from the coal beds of Indwe and Cyphergat, having impressions of plants. Unfortunately they had been subjected to barbarous treatment during transit, and I have only been able to make out three. These are ferns, and one cycadaceous plant.

Pecopteris odontopteroides. Morris.

Cycadites pectinoides.

Sphenopteris Murrayana. Nor. Spec.

Diagnosis of the last : Frond dichotomously divided ; each division irregularly pinnate ; pinnae bi-pinnate, segments linear recurved.

This plant is very different from *Sphenopteris elongata* of Carruthers in being altogether more robust and rigid in habit.

Remarks on the probable horizon of the coal beds :—The coal of these fields is, according to Professor Green, sub-aqueous and not a land or sub-aerial formation like that of the carboniferous age in England. It probably belongs to the Jurassic age. Carruthers has referred one coal field with similar fossils, of Queensland, to that age ; and the other coal field to the carboniferous age as having fossil remains similar to those of the true carboniferous horizon. Professor Owen has come to the same conclusion from examination of the reptilian remains of these Upper Karroo coal-bearing beds.

The old idea that coal was confined to the carboniferous age has long been exploded : it has been found in many ages. As has been