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PUTREFACTION AND INFECTION

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ESSAYS ON THE

FLOATING-MATTER OF THE AIR

IN RELATION TO

PUTREFACTION AND INFECTION



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INTRODUCTORY NOTE.

THE VIRTUAL TRIUMPH of the Antiseptic system of surgery, based as that system is on the recognition of *living contagia* as the agents of putrefaction, is of good augury as regards the receptivity of the public mind to new views respecting the nature of contagia generally.

To the credit of English surgeons it stands recorded that, guided by their practical sagacity, they had adopted in their hospitals measures of amelioration which reduced, almost to a minimum, the rate of mortality arising from the ‘mortification’ of wounds. They had discovered the evils incident to ‘dirt;’ and, by keeping dirt far away from them, they had saved innumerable lives, which would undoubtedly have succumbed under conditions prevalent in many of the hospitals of continental Europe.

In thus acting, English surgeons were, for the most part, ‘wiser than they knew.’ Their knowledge, however momentous in its practical applications, was still empirical knowledge. That dirt was fatal they had discovered; but why it was fatal few of them knew.

At this point Lister came forward with a scientific principle which rendered all plain. Dirt was fatal, not as dirt, but because it contained living germs which, as Schwann was the first to prove, are the cause of putrefaction. Lister extended the generalization of Schwann from dead matter to living matter, and by this apparently simple step revolutionized the art of surgery. He changed it, in fact, from an art into a science.

‘Listerism’ is sometimes spoken of as if it merely consisted in the application of carbolic acid spray; but no man of any breadth of vision will regard the subject thus. The antiseptic system had been enunciated, expounded, and illustrated, prior to the introduction of the spray. The spray is a mere offshoot of the system—elegant and effective it is true, but still a matter of detail. In company with my excellent friend Mr. John Simon, I once visited St. Bartholomew’s Hospital, and became acquainted, in its wards, with the practice of the late Mr. Callender. The antiseptic system was there as stringently applied as at King’s College. Immediately before his departure to America I spoke to Mr. Callender on this subject; and he then told me expressly, that his aim and hope had been, not to introduce a new principle, but to simplify the methods of Lister. And yet Mr. Callender’s practice is sometimes spoken of as if it were, in principle, different from that of his eminent contemporary.

It is interesting, and indeed pathetic, to observe how long a discovery of priceless value to humanity

may be hidden away, or rather lie openly revealed, before the final and apparently obvious step is taken towards its practical application. In 1837, Schwann clearly established the connexion between putrefaction and microscopic life; but thirty years had to elapse before Lister extended to wounds the researches of Schwann on dead flesh and animal infusions. Prior to Lister the possibility of some such extension had occurred to other minds. Penetrative men had seen that the germs which produce the putrefaction of meat might also act with fatal effect in the wards of a hospital.

Thus, for example, in a paper read before the British Medical Association at Cambridge in 1864, Mr. Spencer Wells pointed out that the experiments of Pasteur, then recent, had ‘all a very important bearing upon the development of purulent infection, and the whole class of diseases most fatal in hospitals and other overcrowded places.’ Mr. Wells did not, as far as I know, introduce any systematic mode of combating the organisms whose power he so early recognised. But, I believe, in hardly any other department of surgery has the success of the antiseptic system been more conspicuous and complete than in that particular sphere of practice in which Mr. Wells has won so great a name.

A remark in the paper just referred to would seem to indicate that, in regard to the further possible influence of germs, the thoughts of Mr. Spencer Wells had passed beyond the bounds of pure surgical practice. ‘Their influence,’ he says, ‘on the propagation of

epidemic and contagious diseases has yet to be made out.'

This shows that at the time here referred to the Germ Theory, in its wider medical sense, had begun to ferment in England. Two years, indeed, prior to the above occasion, and for the use of the same Association as that addressed by Mr. Wells, the late Dr. William Budd had drawn up a series of 'Suggestions towards a Scheme for the Investigation of Epidemic and Epizootic Diseases,' which strikingly illustrate the insight of a man of genius, withdrawn from the stimulus of the metropolis, and working alone, at a time when the whole medical profession in England entertained views opposed to his. Budd states in succession, and with perfect clearness, the points which he considers most worthy of the attention of the Association. He recommends inquiry as to the nature of the evidence alleged to prove the disease under investigation to be contagious or communicable. Whether such disease admits of being artificially propagated by inoculation or otherwise. Through what surface or surfaces the virus may be shown to enter the body, and to leave it, when the disease is taken in the natural way. Whether the disease is distinguished by eruptions external or internal. Whether it has a period of true incubation; and if so, what are the length and limits of that period. Whether one attack, as in smallpox and many other contagious diseases, preserves against future attacks. Whether in the case of human disease animals as well as man are susceptible, and if so, what animals. What is the evidence, if any, as to the particular country or region in which the disease first appeared. What are

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its present geographical limits. Whether there is any evidence of its modern or recent introduction into countries previously exempt. How far any such disease may have been prevented from invading new countries, or from spreading from any particular centre, by measures directed against contagion. Above all, to determine what is the nature, and what the true value, of the evidence supposed to show that the specific poison of a contagious disease may originate spontaneously, or be generated *de novo*. 'What we most want to know,' adds Budd, 'in regard to this whole group of diseases is, *where, and how, the specific poisons which cause them, breed and multiply.*'

Budd's own relation to the question here raised was distinct and, under the circumstances, impressive. 'After giving many years of time and thought to an examination of the evidence bearing on this question,' he comes to the conclusion 'that there is no proof whatever' that the poisons of specific contagious diseases ever originate spontaneously. 'That the evidence on which the contrary conclusion is founded is negative only; that evidence of precisely the same order, only to all appearance still more cogent, would prove animals and plants, even of large species, to originate spontaneously; that this evidence is therefore of no weight; and, lastly, that all the really important facts point the other way, and tend to prove that these poisons (to use a term which is probably provisional only), like animals and plants, however they may have once originated, are only propagated now by the law of continuous succession.'

The word 'poisons,' here provisionally employed, was a concession on Budd's part to his weaker brethren; for he, without a shade of doubt, considered the poison to be a real living *seed*. There was, I believe, but one physician of eminence in England who, at the time here referred to, shared this conviction, and who imparted to Budd the incalculable force derived from the approbation and encouragement of a wise and celebrated man. It gives me singular pleasure to write down here the name of the venerable Sir Thomas Watson, who lent to William Budd unfailing countenance and support, and who has lived to see that the views which commended themselves to his philosophic judgment are at the present moment advancing with resistless momentum among the members of the medical profession. It was far otherwise at the time to which we here refer. 'Opinions like these,' said Budd, 'are no doubt, at present, those of a small minority. A very large, and by far the most influential school in this country—a school which probably embraces the great majority of medical practitioners, and the whole of the "sanitary public"—holds the exact contrary; and teaches that sundry of these poisons are constantly being generated *de novo* by the material conditions which surround us.'

Budd's remark regarding the spontaneous generation of 'animals and plants, even of large species,' is both pregnant and pertinent. In reference to special and solitary outbreaks of contagious fever, I have frequently heard physicians of distinction affirm, without apparent misgiving, the 'impossibility' of importation from

without. On such occasions a reply, in the strict sense affirmed by William Budd, was always at hand ; for I was able to adduce cases of solitary mushrooms, found upon out of the way Alpine slopes, to which the evidence would apply with greater force than to the cases on which the physicians referred to based their conclusions. With the atmosphere as a vehicle of universal intercommunication, it is hard to see any just warrant for the reliance of medical men upon the negative evidence stigmatized by Budd as valueless. It is, however, evidence by which many physicians are still influenced, and the effects of which it will probably require a generation of doctors, brought up under other conditions of culture and of practice, to wholly sweep away.

These conditions are growing up around us, and their influence will be all-pervading before long. Never before was medicine manned and officered as it is now. To name here the workers at present engaged in the investigation of communicable diseases would be to extend beyond all reasonable limits this Introductory Note. On the old Baconian lines of observation and experiment the work is carried on. The intercommunication of scientific thought plays here a most important part. It will probably have been noticed, that while physiologists and physicians in England and elsewhere were drawing copiously from the store of facts furnished by the researches of Pasteur, that admirable investigator long kept himself clear of physiology and medicine. There is, indeed, reason to believe that he was spurred on to his most recent achievements by the papers of

Burdon Sanderson, Koch, and others. The union of scientific minds is, or ought to be, organic. They are parts of the same body, in which every member, under penalty of atrophy and decay, must discharge its due share of the duty imposed upon the whole. Of this 'body,' a short time since, England provided one of the healthiest limbs; but round that limb legislation has lately thrown a ligature, which threatens to damage its circulation and to divert its energies into foreign channels. In observational medicine one fine piece of work may be here referred to—the masterly inquiry of Dr. Thorne Thorne into the outbreak of typhoid fever at Caterham and Redhill. Hundreds were smitten by this epidemic, and many died. The qualities of mind illustrated in Dr. Thorne's inquiry match those displayed by William Budd in his memorable investigation of a similar outbreak in Devonshire. Dr. Budd's process was centrifugal—tracing from a single case in the village of North Tawton, the ravages of the fever far and wide. Dr. Thorne's process was centripetal—tracing the epidemic backwards from the multitude of cases first presented, to the single individual whose infected excreta, poured into the well at Caterham, were the cause of all.

The Essays here presented to the reader belong to the A B C of the great subject touched upon in the foregoing Note. The two principal ones, namely, Essays II. and III., were prepared for the Royal Society, and are published in the 'Philosophical Transactions' for 1876 and 1877. But, though written for

that learned body, I sought to render their style and logic so clear as to render them accessible to any fairly cultivated mind. The Essays on 'Fermentation' and 'Spontaneous Generation' have already appeared elsewhere; while the first Essay, on 'Dust and Disease,' has been for some years before the public. It may be regarded as a kind of popular introduction to the more strenuous and original labours which follow it.

The Essay most likely to try the reader's patience is No. III. On the whole, however, and particularly in its bearings on the Germ Theory of disease, it is probably the most important of all. The difficulties which sometimes beset the experimenter in these investigations are best illustrated by this Essay. It shows, to my mind in a very impressive manner, the analogy of the spread of infection among organic infusions with its mode of propagation among human beings. The vital resistance of certain germs to heat is strikingly illustrated in the third Essay, one infusion being there proved to maintain its potentiality of life intact after eight hours' continuous exposure to the temperature of boiling water. Under the plain guidance of the Germ Theory, it is however shown that an infusion of this stubborn character may be infallibly sterilized by discontinuous heating, in one hundredth part of the time requisite when the boiling is continuous. Another question, to my mind of fundamental importance, is also disposed of in Essay III., where it is shown that the germs which exhibited the foregoing resistance are neither contained in the air, nor attached to the surface of the vessel, above the liquid, but that

they manifest their extraordinary vitality in the body of the liquid itself.

On public sympathy the sanitary physician has mainly to rely for support, in a country where sanitary matters are left so much in the hands of the public itself as they are in England. But sympathy without cause—that is to say, without some basis of knowledge—is hardly to be expected. It is as a contribution to such knowledge that these Essays have been collected, and thrown into their present handy form.

J. TYNDALL.

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