LECTURES ON THE INFECTIVE PROCESSES OF DISEASE.

Delivered in the Theatre of the University of London.

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LECTURE I.

Introduction: Infective Processes in General.

IN the present course, I propose to continue the consideration of the subject on which I entered in the second part of my course of 1873. I have been induced to select this subject, by the consideration that it is one of present interest in its bearing on practical questions, and that one or two of the many points which have been hitherto in doubt or obscurity relating to the origin and causation of infective diseases seem, by the gradual progress of clinical and experimental investigation, to be passing into such a position as to admit of satisfactory settlement.

The adjective *infective* is an adaptation of a word which has been long in use in Germany, but is new to English literature. I adopted it, in 1872,* because the word "infectious", which I might otherwise have chosen, had come into common use and acquired a secondary meaning which rendered it unfit for my purpose. That purpose was to bring under one designation two sets of morbid processes—those by which (if I may use the expression without being understood to mean something special by the word seed) the seeds of disease are introduced from outside; and those by which disease, having already taken root in the organ or tissue, affects (or as we say, infects) the whole organism.

As regards the former cases, we attribute the results observed to the action of a morbid poison or contagium; but, in the latter, all that we are able to observe is either that the primary process spreads from the part first affected to other parts which are connected with it by continuity of structure or by channels of absorption, or that it repeats itself in distant organs. To these processes of extension and metastasis, no less than to the others, the term infective is applicable; for, although the channels and modes of infection may be more difficult to trace, there can be no question that the phenomena are more easily understood by referring them to the diffusion or dissemination of morbid material from a centre than in any other way.

In the English edition of the *Encyclopædia* of Ziemssen, the translators have rendered the German word *infectiv* by the English infectious.† Consequently, the English reader is surprised to find many affections included under that term which, in the common acceptation of the word, are not infectious. But they are all infective. Thus, for example, erysipelas is, without doubt, an infective disease; for, quite apart from any conclusion which may be arrived at as to its being determined by the introduction of some morbid material from outside, there is no doubt that, from the moment that the disease has established itself in the organism, the affected parts act towards neighbouring parts as sources of infection.

I have no doubt that we are most of us agreed in the opinion that no contagious disease can originate *de novo* any more than that an organism can originate *de novo*. There may be some, however, who think otherwise on both questions. Fortunately, agreement of opinion on this subject is not absolutely necessary to our uniting in the study of the process of infection. About some infections we are *practically* certain that the true plant never comes up unless the seed has been sown—as, for example, syphilis, small-pox, or hydrophobia. About others, we may allow ourselves more latitude; as, for example, the traumatic affections. Thus, good *à priori* grounds may be advanced for the belief that a wound cannot suppurate unless it is infected from outside, and that nature herself, if not interfered with by external influences, proceeds at once with the process of reparation. Some of us may believe this, others doubt it; but the side we take on this question is comparatively of little consequence, provided that we understand, first, that in a wound infective products are extremely apt to originate, and that, by carefully studying

and observing the conditions on which they depend—whether extrinsic or intrinsic—we are able to prevent or control them.

There can, I think, be no doubt that, during the past eight or ten years, the notion of infectiveness as an important element in diseased processes has become more familiar than formerly. The progress of pathological knowledge has compelled or induced us to recognise infective action in a much larger number of instances than formerly. No one, for example, hesitates to include among infective diseases some in respect of which we have no knowledge of the infective agent. We may not know why it is that a pulmonary catarrh, which in one person subsides without leaving any pathological vestiges, develops in another into pulmonary tuberculosis; but there are few who doubt that a tuberculous process, once begun, spreads infectively. Similarly, as regards common traumatic inflammation and its various results and complications: in a certain sense, it has been long familiar that an inflamed part is a focus from which irritating material is distributed to healthy parts by radiating lines of absorption; but it is only of late years that it has been distinctly seen and recognised clinically that every exudation-liquid of an inflamed part is more or less phlogogenic, *i.e.*, carries with it more or less the properties of an inflammation-producing virus.

It was this fact—that of the property of exudation-liquids to excite inflammation in healthy textures—that I endeavoured to express in my paper on the Infective Products of Inflammation, communicated to the Royal Medical and Chirurgical Society in 1873; and I am going to ask your permission to refer to it, in order that I may bring what I then said into relation with what has been said and written by others since. Starting from the accepted notion that by inflammation is meant simply the sum of the local pathological effects of an injury, I distinguished inflammations into two classes, infective and non-infective. In the non-infective (or normal) inflammations, the pathological effects are co-extensive with the injury, so that no tissue is destroyed excepting so much as has been spoilt by the *noxa* itself; in the infective, the surrounding tissues are damaged and to a greater or less degree destroyed by the extension of the pathological process. It is this destruction of tissue which seems to me to constitute the source of the common infectivity of wounds. When no tissue is disintegrated beyond the limits of the injury, there reparative processes at once begin. Wherever the solution of continuity is surrounded by a zone of damaged tissue, there there is potentially an infective focus; and the question whether it will extend its influence beyond the limits of the part primarily affected depends on conditions which, however they may originate, have their seat of operation exclusively in the wound itself.

The admission and realisation of this fact cannot fail to have an important bearing on every inquiry relating to the mode of origin of traumatic affections, for it leaves us quite free to appreciate the influence of any agent, whether atmospheric or constitutional, whether extrinsic or intrinsic, which can be shown to act on the wound itself.

The proposition that, in order to the right understanding of a pathological process, it is necessary to investigate what happens at the seat of disease, and not to draw conclusions as to its nature from what may have been observed to occur elsewhere, is so obviously true that it might seem waste of time to insist on it; but in relation to the question which is now so much discussed—the question whether living atmospheric particles determine infective changes in wounds—this principle is the only one which will guide us right.

In 1872, I thought myself justified in inferring, on the basis of observation on the traumatic affections of animals, that an essential condition of the development of infective action in a wound or an inflamed serous membrane was the presence in it of the organisms which I had two years before characterised as microzymes. I still think so. I still believe that to the chemical processes which give rise to the production of infective products these minute organisms are not only accessory, but necessary. But the conviction of this fact appears to me to afford no ground for accepting the theory that, under ordinary circumstances, contagium is brought ready-made into a wound by organised germs from the atmosphere. I am led to emphasise this statement by the circumstance that it has lately been suggested that I have changed my views on this subject.* I think I may venture to say that there is no foundation for the suggestion. I have always maintained that the facts we already possess relating to the development of organisms belonging to the same group as bacteria, in association with pathological processes, are such as to make it impossible to doubt that such organisms exercise an important part in those processes; but, as regards the theory that the common air we breathe is constantly charged with infective organised particles, *apart from and*

* "The Infective Product of Inflammation." *Medico-Chirurgical Transactions*, vol. lvi, p. 345.
† "Acute Infectious Diseases." Title-page of vol. i.

* See Letter by Dr. D. Cunningham and F. R. Lewis, entitled "Dr. Sanderson's Changed View of the Ultimate Nature of Contagion," *Lancet*, September 22nd, 1877.

without reference to its having previously come into contact with any specific source of contagion; and that, when a wound goes wrong, it does so because these ordinary air-particles find their way into it—I have never neglected any opportunity of saying that, to the best of my judgment, such a theory is based rather on what I venture to think the misapplication of an analogy than on the direct observation of what actually goes on in the wound itself. But, as I said before, whatever opinion any of us may entertain as to this interference of organisms in traumatic processes, that opinion need not necessarily interfere with the view we take of the ultimate relation which, as I have endeavoured to show, subsists between infection and the inflammatory process itself. I cannot better illustrate this than by referring to the important work on traumatic diseases published by Professor Billroth in 1873. This work has, I believe, attracted very little attention indeed among English surgeons, notwithstanding the great reputation of its author: a fact which may be well attributed to its title. The position held by Billroth may be said to be intermediate between that of the extreme germ-theorists and those who hold to the old ways. Billroth, be it observed, although he calls his book after bacteria,* is by no means, as they call it in Germany, "*bacterien-froh*"† himself. In Germany, indeed, he is one of the most formidable opponents that the germ-theory has had to encounter, for the reason that, being a great surgical pathologist and a successful operator, his name has great influence. But, notwithstanding his anti-bacterial convictions—notwithstanding that he may be theoretically wrong in his opinion that the "*Zersetzung des Gewebes*", which he associates with every traumatic affection, may go on independently of organised ferments, and is due merely to the action of a "phlogistic zymoid"—that mistake, if mistake it be, does not in the slightest degree detract either from the value of his observations or from the clearness of his statement as to the conditions which lead to the development of the infective agent, or as to the mode of morbid action.

With these considerations in view, it will be my aim in these lectures to discuss infective actions from a purely pathological point of view. From the way in which many intelligent persons talk, you might be led to suppose that they imagined that the question of infection might be removed from the field of pathology to that of meteorology; that by merely studying the physical properties of the atmosphere, it might be possible to arrive at some valuable knowledge of the way in which diseases originate in the human organism! To those who have had to meet the difficulties which beset the path of the pathological investigator at every step, it seems surprising that any one should attempt such a task; for to us it appears so plain that the only way to gain knowledge about disease is the direct observation, experimental or clinical, of diseased processes, that we find it difficult to conceive that those questions which so many laborious and capable men have for years been working out step by step to their issues by these methods, can be expected to be at once and without difficulty solved by applying to them general laws derived from the observation of external nature.

Let us, however, guard against the opposite mistake of supposing that progress is possible without a knowledge of these laws. What we object to is not, I need scarcely say, the application of exact natural knowledge to medicine; for medicine is itself merely a department of natural knowledge; nor is it that theories derived from the exact sciences should be applied to in explanation of pathological facts, for the only way in which we can comprehend vital phenomena is by comparing them with non-living standards. What we object to is the assumption that relations of phenomena which are known to be constant outside of the living organism must also hold good when the conditions are modified by the presence of life; not because life has in it anything essentially different from other chemical processes, but because its conditions are extremely complicated and often beyond the reach of direct observation. The hidden processes of disease are known to us by their concomitant phenomena, and by the effects they leave behind them after death; and we recognise as plainly as the natural philosopher does, that they have resemblances with processes which go on in external nature, but we find ourselves compelled to extreme caution in using them, the history of medicine being pregnant with instances which prove that analogies of this kind have much more frequently guided men wrong than right. Consequently, the pathologist is led to limit the scope of generalisation with the utmost strictness. The boundaries of pathological knowledge are as nearly as possible coincident with those of exact observation of pathological

fact. Pathology claims to be a science only in so far as it is a putting together or classification of morbid phenomena.

What has been said as to the possibility of acquiring a satisfactory knowledge of the order of phenomena in the process of traumatic infection by careful observation, notwithstanding that our notions of its intimate nature may be open to correction, applies with equal truth to the investigations relating to the specific contagious diseases. Thus, although in view of certain *à priori* considerations as to the nature of contagium, I find it, for my own part, as impossible to believe that small-pox can originate *de novo* as I find it impossible to realise that any plant or animal can come into existence spontaneously, I am well aware that there are others who experience no such difficulties; and what I desire to make clear is that, if we can only persuade ourselves that it is not necessary to mutually excommunicate each other, this difference of opinion, fundamental as it may seem and no doubt is, as to the why of contagion, need not prevent us coming to perfect agreement as to the how. In order that this agreement should be brought about it is, however, necessary that we should all make up our minds to look at the subject, not as philosophers seeking to harmonise our observations with what we conceive to be the divine order of nature, but as, for the time, only pathologists whose business it is to acquire by accurate observation a complete knowledge of the facts relating to each particular case; as, for example, of the conditions, whether local or personal, which lead to a man's getting typhoid fever. For, however important it may be that we should think correctly, it is still more important that we should observe correctly, and, consequently, if by shelving the more remote questions of ultimate origin of contagia, we could induce people to fix their attention more earnestly and with freer minds on the facts relating to particular cases, it would be well worth the sacrifice. Let me endeavour to illustrate this by an example.

There is no epidemic disease about which there is so much discussion as about cholera, and it is generally imagined that the main question at issue is whether the material cause of cholera is an organism or not. Now, I venture to think that at present the more completely the question of organised germs or no germs is eliminated from the area of discussion about cholera the better; for, although I am, for my own part, entirely unable to explain to myself the dispersion of the material cause of cholera without calling in the aid of organisms, it is not the less apparent to me that, if I could carry that conviction into the minds of all who are at present engaged in controversy on the subject, it would aid in the solution of the ætiological questions at issue, and for this reason. With reference to cholera, as with reference to some other epidemic diseases, there is conclusive and irrefragable evidence to show that at least two distinct influences are at work in producing the disease; viz., a local influence or miasma, of which all that can be said is that, springing from the soil, though acting through the air, it depends on conditions of the soil; and an epidemic influence capable of being conveyed through the air over vast distances, and probably more or less dependent on human intercourse.

Both of these causes must, of course, be material, and must be capable of acting together, i.e., of combining in a single act of infection. The question now at issue, and which has been at issue for some years, relates chiefly to the proportion in which the two factors contribute to the result. In India, the influence of soil-born miasma is known to be so paramount to every other, that it is not surprising that Indian pathologists say unhesitatingly the cause of cholera is local. In Europe, where we know cholera only as an invader, though we attribute great importance to miasma, we are naturally more alive to the existence of those disseminated causes which exist only at epidemic periods, and are conveyed to us from the home of cholera in the far East. The question is, How do the two factors unite their influences so as to produce the result? There are certain European towns which in every epidemic have hitherto been cholera free, others which have as invariably suffered severely. What is the explanation of this? Is it that, in cholera towns, such as Munich, the miasmatic soil is excited by the advent of epidemic influence to morbid fruitfulness, or is it that the inhabitants of such places are prepared by miasma—have their constitutions so modified by local influences—as to acquire a proclivity to the disease? There are, I think, good reasons for preferring the latter alternative; for facts seem to show that persons who have lived in cholera districts carry with them their liability to cholera when they migrate; but that is not the point to which I ask your attention. My point is that, to whichever of these explanations we are led by experience, whether we think that the conjugation of local with epidemic influences takes place in the individual or in the soil, either conclusion can with equal facility be formulated in terms of *contagium vivum*. For, in the imagination, it is quite as easy and as reasonable to embody a *miasma* in a germ as to embody a *contagium*.

* *Untersuchungen über der Vegetationsformen von Coccobacteria septica*, u.s.w. Versuch einer wissenschaftlichen Kritik der verschiedenen Methoden der antiseptischer Wundbehandlung. By Professor Billroth. Vienna: 1874.

† Germ-crazy.

Considering this, and considering that as yet we have no evidence, *pro* or *contra*, of organisms in relation to cholera, it would be certainly better, for the scientific discussion of the ætiological question relating to this disease, to dismiss germs for the present from consideration than to allow them to obscure the facts, or to confuse or bias our judgment as to their meaning. In point of fact, both of the opposing views have actually been so embodied. In most people's minds, the "germ-theory", as it is loosely called, of cholera is identified with contagionist views; but, in the very philosophical work of Professor Nägeli of Munich, just published,* you will find a much more maturely worked out theory of *contagium vivum*, which is in accordance with Indian as well as with European experience.

[To be continued.]

REMARKS ON BATTEY'S OPERATION.

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II.—DR. SIMS'S CASES (*continued*).

CASE IV.—Miss D., aged 20, had never had a menstrual period without pain; but her sufferings were not confined to the period. She was never clear of pain in the pelvic region. She had had the best medical advice in the country without any benefit. She was admitted to the Woman's Hospital in October 1873. She had antelexion, small os tinæ, and contracted cervical canal, with hypertrophy of the anterior wall of the uterus. I promised to cure her by incising the cervix and enlarging the canal, so as to permit a free discharge of the menstrual flow. The operation was done, and she left the hospital not much improved. She returned a year afterwards (October 1874), suffering quite as much as, if not more than, she had ever done. On examination, I found that the cervical canal was nearly as much contracted as it was before the operation a year ago. And I also found, what I had previously overlooked, that the left ovary was the seat of disease, and was probably the source of all her suffering. We now spoke of Battey's operation, but concluded to repeat the operation on the cervix, not with the expectation of making a radical cure, but with the hope of ameliorating in some degree her dreadful suffering. The operation and its effects over, my patient went home; and returned to New York a year afterwards (1875), suffering as much, if not more, than she had ever done before. Then it was that we began to discuss in earnest the propriety of performing Battey's operation. Miss D. was willing to have it done a year ago; but now she was anxious for it. The operation was performed in November 1875. The left ovary, being the principal seat of pain, was extirpated by the vaginal incision. It was somewhat enlarged, and had undergone cystic degeneration. The pedicle was severed with the electro-cautery. This was followed by hæmorrhage, and it was necessary to apply a ligature. The operation produced a severe attack of pelvic peritonitis and pelvic cellulitis, which fortunately terminated by resolution. She was confined to her bed for several months, and barely escaped with her life. She left New York emaciated to a skeleton, and confirmed in the "opium habit". She returned again in October 1876, a most wretched sufferer, but plucky to the last; for she came expressly to have the other ovary extirpated. I did not have the courage to do the operation, though she begged for it. Yet I am sure she can never be restored to health but by Battey's operation by the abdominal section. Here again I made the mistake of removing but one ovary (instead of both), simply because it alone was the seat of the pain.

CASE V.—Mrs. C., aged 38, regular, but had suffered great and constant ovarian pain for more than eight years, which was aggravated during menstruation. The uterus was retroverted. The posterior wall was hypertrophied and tender on pressure. The left ovary was somewhat enlarged, and very painful on pressure. It seemed to be the centre from which radiated all her suffering. She had been under the care of Dr. W. T. Walker for a long time two or three years previously. He operated on the cervix uteri, hoping to relieve the pain of menstruation; and he adjusted a Hodge's pessary for the retroversion. The instrument could be worn with comfort for a little while, but soon the ovary would become prolapsed, and the instrument pressing on it would produce so much pain that it could not be worn. After several weeks of well directed effort by Dr. Walker to relieve her, she went home not

improved. Mrs. C. intended to return to New York for a repetition of the incision of the cervix uteri. But her visit was delayed till after Dr. Walker's death. She then came with her physician to see me.

I found the left ovary to be the source of all her suffering. I gave it as my opinion that there was no remedy for her but in Battey's operation. I advised her to submit to the operation, or to return home and stop all medication. The patient, her husband, and her physician went to their hotel, and held a consultation on the subject, and decided to have the operation done. I then advised extirpation of the ovary by the abdominal section, as in Cases I and II, securing the pedicle in the lower angle of the incision, with the view of curing the retroversion at the same time that the diseased ovary was removed. The operation was performed in February 1876. She was stout and fat; the abdominal walls were thick, and the operation was by no means an easy one. Both ovaries had undergone cystic degeneration, and both burst and discharged their contents during the effort of drawing them up to the surface of the abdomen. Both pedicles were brought out at the lower angle of the wound, and clamped. This was evidently a mistake; for the ligaments were very short, and the walls of the abdomen very thick; and thus the traction on the pedicles produced such intense suffering as to necessitate large and repeated doses of morphia hypodermically. She died of peritonitis on the seventh day after the operation.

CASE VI.—Miss H., aged 20, menstruated at 14, the flow soon becoming regular and normal in quantity and quality. At 15, she had a severe fall, and was badly injured in the spine. She was never afterwards clear of pain in the lumbo-sacral region and through the pelvis. But the most remarkable feature of this case was coccygodynia of such intensity as to require large and repeated doses of morphia by the skin. The morphia never entirely controlled the pain, even when given in the largest doses. She groaned and moaned all day and all night. She appeared to be in constant pain, even when asleep. Indeed, it seemed that she had almost lost the power of sleeping; but, of course, she slept, although she declared she did not. Her mother thought she got short naps of sleep, but certainly never an hour or two continuously.

Her physician, Dr. Mitchell of Mount Sterling, Kentucky, after exhausting all his resources, sent her to Dr. Wright of Cincinnati, who performed Dr. Nott's operation of extirpating the coccyx; an operation which, based on the history of the case and the symptoms immediately following upon the fall, was certainly justifiable. But no benefit whatever resulted from it; and, after a few weeks, she returned home, suffering as much as she had before the operation.

A year after this, Dr. Mitchell sent Miss H. to me. It was truly pitiable to see one so young so worn down with physical suffering. The pain at the end of the spine was continuous and most agonising. It never ceased for a moment day or night. It was never modified in the least, except by enormous doses of morphia hypodermically, and often repeated. She could stand, and walk with unsteady gait, but she could not sit at all, because she could bear no weight on the coccygeal region or on the right nates. She lay in bed most of the time in a semi-erect position, always on the left side, the head bent on the chest, the knees flexed and drawn up towards the chin, with lumps of ice applied to the seat of pain. She usually kept her head covered to exclude the light. Photophobia was a marked symptom of the complete derangement of her whole nervous system. When her head was uncovered, her eyes, so sensitive to light, were kept half-closed, and she was frowning and blinking all the time. She had a strong intellect and a kind heart, but her long suffering had soured her temper and depraved her whole nature. She was at times dictatorial, self-willed, and obstinate. She would often cover up her head and refuse to see anyone, or to speak to her physician (Dr. Harry Sims), even when he spoke in the kindest and most persuasive manner. Her natural gentleness was submerged by her physical suffering. She fretted and whined like a sick infant; or she was dogmatic like a spoiled child.

The idea of examining the seat of pain produced in her great nervous agitation. She shuddered and shrank from even the thought of it.

I found a longitudinal cicatrix, an inch and a half long, over the coccygeal region, marking the seat of the operation performed a year ago by Dr. Wright for the extirpation of the os coccygis. This cicatrix was elevated a little above the level of the adjacent skin. It felt hard and gristly, and was exceedingly tender to the touch. Indeed, the gentlest touch produced intense suffering. Even titillation with a feather was agonising. The sensitiveness was not confined to the cicatrix alone. It extended for an inch or more above, and as much below it. It also extended laterally on the right side of the nates for about two inches, but not at all on the left. The skin on the right of the cicatrix was thicker than natural. Its follicles were hypertrophied. The skin of the whole sensitive region was thicker and coarser than that on the

* Die niederen Pilze in ihren Beziehungen zu den Infektionskrankheiten. By Professor C. V. Nägeli. Munich: 1877.

† Continued from page 842 of last number.