noma, gangrene of the scrotum, and cancrum oris, frequently occur among natives, and the slightest abrasion or ulceration is apt to pass into a state of phagedæna. There is a tendency to the formation of fibrinous coagula in the heart and arteries; death occurs rapidly, with symptoms of apnoea. The tendency to embolism in the arterial circulation is shown in the gangrene of limbs and other parts. And here I would call attention to another morbid susceptibility impressed on those who are the subjects of malarial cachexia, and on others living in a malarious climate—the so-called urethral fever, very apt to follow on catheterism. The mere passage of an instrument will produce a severe attack of rigors, followed by fever and sweating, and it may give rise to symptoms of a pyæmic nature.

I may also notice a form of fever connected with a malarial origin, in which the spermatic cord, epididymis, scrotum, and occasionally the prostate gland, are congested and swollen, attended with great suffering, and sometimes accompanied by gastralgia, nausea, and vomiting. This form of fever not unfrequently attends the periodic hyperæmia which leads to permanent hypertrophy—a condition frequently associated with a disordered and dilated state of the lymphatics, and accompanied by periodic returns, which the natives attribute to lunar influences; and by the presence of filariæ in the blood, as pointed out by Lewis, Manson, Bancroft, and others. These febrile recurrences are sometimes called elephantoid fever, and the hypertrophy elephantiasis, which thus seems to be closely related etiologically to other forms of malarial fever.

(To be continued.)

THE LUMLEIAN LECTURES ON INFLAMMATION.

Delivered before the Royal College of Physicians.

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LECTURE II. PART I.—ÆTIOLOGY OF INFLAMMATION.

In the last Lecture, I endeavoured to show that if we take the word inflammation to mean what it has always meant, namely, an acute local disorder, in which the affected part is hot, red, and swollen, we can account for the concurrence of these objective changes completely on the theory that they are the direct physiological effects of injury; the ground for this view being the observation that, by acting on an organ in such a way as merely to depress or impair its vitality, without destroying its texture or otherwise interfering with its continuity with surrounding parts, we induce in that organ a state of things which, although not inflammation, is so immediately related to it, that out of it, without any intervening process of development, inflammation at once declares itself.

For what I attempted to show in last lecture was, that it is possible, under favourable circumstances, for inflammation to come into existence *d'emblée*. That in general its development is gradual, arises from the gradual action, extension, or dissemination of the cause; but if the cause be of such a nature that it damages a considerable area at once and in the same degree, as when for a time you cut off the circulation from an external organ, or subject it to the action of high temperature, the physiological effect follows directly on the injury. What we saw in the ear of the rabbit was this: immediately after it had been exposed to a temperature just high enough to spoil its tissues, there was no visible effect whatever; its circulation was normal, and no sign existed that it had been injured. But soon afterwards it became hot, cedematous, and congested. The invisible change of which these physiological phenomena were the inevitable consequences may be conveniently labelled damage, it being understood that we mean by this term the state of things which immediately precedes inflammation, whatever may be the nature of the agent that produces it.

I am well aware that so simple a conception of the origin and essence of a pathological process is, by reason of its very simplicity, extremely difficult to seize upon. For inflammation is itself so positive a thing, and we are so accustomed to think of it in connection with ideas of excessive action, that it is very hard indeed to reconcile the mind to regard its phenomena, not as outward signs of an inward process, not

as visible indications of disordered function, but merely as the expressions of loss and failure.

The question with which I have to deal to-day lies behind the one which was discussed in the first lecture. In saying that the immediate antecedent of inflammation is local damage, and that it may consequently be defined as the physiological effect of injury, it might appear as if we had already settled the question of its origin. Thus, for example, when we see the state of inflammation brought into existence as the immediate result of damage due to an obvious cause, we could not be accused of inaccuracy in attributing the pathological result produced to the injury inflicted, and most people would regard it as mere quibbling to suggest that some other agent, to which the obvious cause was merely auxiliary or predisposing, lay concealed behind it. Yet it is suggested, and even dogmatically asserted by some pathologists, that, in the genesis of every inflammation, however definite may be the relation between the antecedent damage and consequent reaction, this relation is, after all, merely secondary. The true cause is the introduction into the damaged tissue from the atmosphere of invisible germs.

This theory of the etiology of inflammation, when so presented, appears exaggerated and even absurd. There is, however, much more to be said in favour of it than at first sight appears; and it will be the purpose of this lecture to endeavour to come to a right conclusion as to the way in which, if not germs, minute organisms, co-operate, as they unquestionably do, with other more easily recognised conditions, in the production of inflammation.

I think I shall best present to you the points which require consideration by again beginning with inflammation of the cornea, as being more within reach of experimental investigation than any other. The particular case to which I will ask your attention—that of the superficial inflammation of the cornea, which occurs after severance of the fifth nerve—is advantageous for our purpose, first, because the antecedent injury is remote from the seat of pathological change, and exercises no direct influence in producing it, so that free play is given for the operation of other concomitant circumstances; and secondly, because the organ primarily affected is so transparent and so superficial, that the slightest change in it can be appreciated by the naked eye; at the same time, that its structure is so simple, that the finer details of such change can be easily investigated by the microscope. Let me first state what these changes are, and in what order they occur.

As regards the eye, the immediate effects of the severance of its afferent nerves are, of course, the abolition of the protective reflex of winking and the arrest of the secretion of tears, whereby the corneal surface is exposed to a variety of accidental injuries, at the same time that it is deprived of the mechanism by which, under natural conditions, foreign bodies are removed from the surface of the cornea and conjunctiva. The subsequent effects are as follows.* About twelve hours after the section of the nerve (in the rabbit), the cornea begins to lose its brightness. By the end of twenty-four hours, the turbidity is distinct; and if, at this period, the animal be killed and the cornea excised, it is found that the epithelium has become partially detached. Up to this time, there is no congestion of the limbus, no exudation of leucocytes from the dilated veins—in other words, no inflammation. These soon afterwards follow, but as yet the change is apparently a very slight one, and it is confined to the surface of the cornea. What is its nature? To find this out, we have to appeal to the microscope. Sections of the corneal substance show that the corneal corpuscles have, in the opalescent parts, simply wasted away. There is no "proliferation", nothing to be seen suggestive of disorder of nutrition in the sense of Goodsir and Bowman. What has happened is, that the surface-layers of corneal tissue have died, and, in consequence, that the epithelium is beginning to separate. If we look at the same eye about thirty-six or forty-eight hours later, the state of things is very different. The cornea is surrounded by a ring of inflammation which has extended to the whole conjunctiva; its surface is uneven and opaque, and, on microscopic investigation, it is found that its whole tissue is pervaded by colourless corpuscles, some of which have escaped from the episcleral vessels, while others have entered from the conjunctival sac. How, at a still later stage, pus finds its way into the anterior chamber, and the destructive process eventually involves the whole eyeball, I need not occupy your time in describing; for to us the whole value and interest of the process I have described relates to its mode of origin, not to its issues.

Let me first state what evidence we have that the damage of the cornea, which here, as in other cases, is the immediate antecedent of inflammation, cannot be attributed to the cutting off from an organ so richly supplied with nerves as the cornea, of whatever trophic or other influence may be exercised on it by the cerebro-spinal centres. Without

* Senfleben, "Ueber die Ursachen und das Wesen der nach der Durchschneidung des Trigemini auftretenden Hornhaut affection," Virchow's Archiv, vol. 65, p. 69.

going so far as to deny the possibility of such influence, it is demonstrably inadequate to produce the result; for it has been conclusively proved that if, after section of the trigeminus on one side, the eye be carefully nursed, *i.e.*, protected from what may be called rough accidental injury during the process, no change whatever ensues; and, further, that if both eyes be subjected simultaneously to irritation of the same kind and degree, the paralysed eye is not one whit more affected than the sound one;* so that there is no ground, so far as the cornea is concerned, for supposing that loss of innervation acts either as an exciting or as a predisposing cause of inflammation.

This being admitted, it is evident that the real cause of damage is traumatic, *i.e.*, it is dependent on the fact that the unprotected and feelingless eye is brought by the animal repeatedly into collision with surrounding objects in such a way as to spoil the surface of the cornea; and we are justified in this conclusion by the observation that a spoiling of exactly the same kind can be produced experimentally by mechanical means, provided these are of such a nature as to leave the epithelium intact. But the question which now interests us lies behind this. What we have to ask is, not how the surface was damaged, but how it happens that this damage leads to inflammation. A dozen years ago, we should without hesitation have said, the disordered nutrition of the non-vascular cornea extends by virtue of continuity of structure to the vascular limbus, causing the vessels to exude liquor sanguinis, and the connective elements in their neighbourhood to proliferate. Such an explanation is no longer possible, for we know that the primary damage has nothing to do with proliferation; its condition is simply (to borrow Mr. Lister's expressive language) "approximated to the condition of ordinary matter." The question is, how does this purely negative change acquire the power of so affecting, or as we may, without assuming too much, call it, infecting, the surrounding vascular tissues? The answer which modern pathologists make to this question is simply—micrococci; and this, indeed, is the only one that can be given; for neuroparalytic keratitis is a type of what may be called a mycotic inflammation, one of which the spread from tissue to tissue is accompanied by the growth and multiplication of those organisms.

Twenty, or even ten, years ago, we should, for want of the means of observation, have failed to recognise them. Now, their existence can be determined (thanks to recent methods of preparation) with as much certainty as pus-corpuscles, and almost as easily. Admitting all this, we have still not got to the root of the matter. In the case of the cornea, it is scarcely necessary to show that, however probable it may be that micrococci are the agents by which the primary lesion extends its influence to the surrounding vascular structures, their appearance on the scene is subsequent to the damage of which they are, in fact, among the pathological consequences. In other words, while their presence is the proximate cause of the secondary iritis and conjunctivitis, it does not follow that they are but originators of the whole process. The question of the mode of origin of acute abscesses is a similar one. Dr. Ogston has, I think, pretty conclusively shown that micrococci invariably take part in this process, and act as if they were the sparks from which every such inflammatory focus is kindled.† All this we may admit, and at the same time demur to the inference that he seems to draw from it, namely, that acute inflammation in general depends on the morbid action of septic organisms. As regards this particular instance, we have sufficient reason for demurring, in the consideration that an acute abscess is no more a primary inflammatory lesion than the conjunctivitis and iritis with which we have compared it. An abscess does not come into existence in a healthy organ *d'emblée*; it must always have been preceded by an inflammatory process. The proof, therefore, that micrococci were concerned in its origin, even if it could be carried so far as to show that around them the first pus-corpuscles grouped themselves, leaves the question of the origin of the preceding inflammation still open.

In a paper published in the Royal Medical and Chirurgical Society's *Transactions* in 1873‡, I endeavoured to show, on the ground of experiment, that the only inflammations to which minute organisms stand in relation are those which, from their proved dependence on previously existing inflammation, may be properly termed secondary or infective; and, consequently, that the organisms in question were, in all probability, not so much mischief-makers as mischief-spreaders. That is to say, although an inflammation may come into existence without their aid, their presence communicates to it, after it has come into existence, the power of reproducing itself in previously healthy tissues, whether by extension or by dissemination. During the ten years which have

elapsed since 1872, the question of the intervention of micrococci, which was then strange even to professed pathologists, has become familiar to every one, and now the tendency exists to believe that germs explain everything; so that, whereas formerly one had to vindicate the very existence of such things as parts of pathological processes, it has now become one's business to protest, with all needful vehemence, against the attribution to them of functions which they do not possess. It will perhaps tend to clearness (which, in relation to so complicated an inquiry, is a matter of moment), as well as to the saving of time, if I submit to you at once the propositions which seem to represent the actual state of our knowledge on the subject. They are (1) that the exudation of a normal inflammation is not infective; (2) that no organisms endowed with inflammation-producing (for which term the word phlogogenic may be conveniently substituted) functions exist in the atmosphere, or in the ordinary aqueous liquids with which our bodies come into contact; (3) that whenever an inflammation becomes infective, it owes that property to chemical change in the exudation liquid, of which the presence of microzymes is a necessary condition; (4) that the introduction into the lymphatics, and eventually into the blood-stream, of microzymes which have been grown in septic exudations, is one of the mechanisms by which the seeds from which secondary or infective inflammations spring, are disseminated. Thus we attribute to our organisms two distinct functions, that of developing what may be called the phlogogenic infection, and that of conveying it to all parts of the body. What we deny is, that there exists at present any evidence that the infection in question actually originates in any other way than by the development of a septic process inside the organism. These several points I will endeavour to establish on the basis of experiment.

[To be continued.]

THE GULSTONIAN LECTURES

ON

PULMONARY CAVITIES: THEIR ORIGIN, GROWTH, AND REPAIR.

By WILLIAM EWART, M.D. Cantab., F.R.C.P.,

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LECTURE III.—PART I.

MR. PRESIDENT, FELLOWS, AND GENTLEMEN,—I turn with a feeling of relief, which you doubtless share, from the consideration of the destructive activities of phthisis to the methods adopted by nature for the healing of the pulmonary lesions. The doctrine of the curability of pulmonary excavation is comparatively young. Although Hippocrates had noticed the beneficial effects of the rupture of pulmonary abscesses into the bronchi, and founded upon this discovery a method of treatment by succussion, the recognition of cavities during life remained matter of inference rather than of observation; and speculations as to the possibility of their healing were controlled by no scientific test, until, with the progress of medicine, it became possible to follow the history of a cavity from its origin to its ultimate stages. To Laënnec belongs the credit of having pointed out the reparative action frequently observed in phthisis (see Cases XVII to XXIII, *Traité de l'Auscultation Médiate*). By him, two degrees are recognised in the healing of cavities: (1) the transformation of an ulcerating excavation into what he terms a fibro-cartilaginous fistula (see Cases XVII to XIX, *loc. cit.*); (2) absolute closure by means of cicatrisation.

With Laënnec, we are justified in considering the dwindling of cavities and the fibrous metamorphosis of their walls to be a step towards cure; and I have no hesitation in stating that a fair proportion of pulmonary cavities not resulting from acute phthisis undergo more or less of this reparative action. Of the complete cicatrisation of cavities, I am unable to speak with the same confidence. Roger and Boudet's observations on the frequency of concretions and scars in the lungs (present, according to them, in four-fifths of the necropsies performed on subjects beyond seventy years of age) give us a high idea both of the prevalence of cavities and of their complete obsolescence. More recently, Heitler ("Ueber die Spontan Heilung der Lungen-Schwindsucht", *Anzeiger der k. k. Gesellschaft der Aerzte in Wien*, 3 Juin, 1880), from a study of the *post mortem* registers of the Vienna a

* Senfleben, "Nachträgliche Bemerkungen", Virchow's *Archiv*, vol. 72.

† "Micro-Organisms in Surgical Diseases", *BRITISH MEDICAL JOURNAL*, March 12th, 1881.

‡ On the Infective Product of Acute Inflammation", *Med.-Chir. Trans.*, vol. vi, p. 345.