

is not followed by œdema. Even hydræmic plethora produced by over-filling of the vessels with diluted blood, though it leads to increased transudation, does not do so till the dilution has been carried to an extreme degree. Even then the œdema does not make its appearance at the parts which are the usual seat of hydræmic œdema in man. We must therefore look for another explanation of the œdema of cachexia and of nephritis, in which disease the function of the kidneys is disturbed. According to Cohnheim they owe their origin to a change in the vessel wall. This change is due to the watery character of the blood, or to some deleterious substance circulating in it. Hydræmic œdema is akin to inflammatory œdema, but it is not identical with it. This appears from the fact already alluded to, that the liquid effused in the former is much poorer in albumen than that in the latter, and that it contains considerably fewer of the corpuscular elements.<sup>6</sup>

As to the diffusion of diseases by the lymphatics. The granulative formations are all distinguished by similar characters. Their development usually stops short at the fibro-blast stage, and having reached it, or even before that, the constructive process gives place to retrogressive changes. Cicatricial development being arrested, the granulation tissue persists for a time unmodified, and often develops to a considerable amount. For this reason Virchow described the formations as granulative growths or granulomata. All of these growths have, furthermore, the clinical character of infectiveness. Hence they have been termed "infective growths" by Klebs and Cohnheim, and "specific inflammations" by Rindfleisch. Their infective character may be recognised by various signs. Thus they are all locally invasive—that is, the granulation tissue spreads centrifugally from a centre into the surrounding structures. At the same time the central or oldest part of the new formation usually dies and disintegrates. In many cases the lymphatic system becomes affected, so that secondary granulative foci are formed in it. From the lymphatics the process is at times transferred to the blood, or it may invade the bloodvessels directly. The final result is the spread of the disorder to various organs, or throughout the system.

In most of the granulomatous disorders, we may have not merely a diffusion of the disease throughout the individual organism, but also a transference of it from one individual to another. The affection is inoculable. If one person be inoculated with the inflammatory products derived from another, he acquires a disease whose course is exactly similar to that of the original one, and which yields identical inflammatory products. This latter character of infectiveness is that by which it is most readily recognised. To this group of infective granulomata belong the neoplastic formations found in tuberculosis, syphilis, leprosy, lupus, glanders, and actinomycosis. All these affections are due to the invasion of the body by a virus or poison derived from the outer world or from the body of another individual. This virus may probably be produced by vegetable parasites. In leprosy (Hansen, Neisser), tuberculosis (Koch), and syphilis (Klebs), bacteria have been found, and in actinomycosis a special fungus. These are declared to be the originating causes of the respective diseases. Our ideas as to the nature and character of these affections are as yet mainly based upon their clinical course, but we have also derived something from inoculation experiments. Tuberculosis and syphilis are thus known to be communicable from one person to another; tuberculosis is also communicable from man to the lower animals.<sup>7</sup> Inoculation with sputa containing bacilli of tubercle gives rise to tuberculosis, which is developed more rapidly when the bacilli are abundant. The inoculated bacillus multiplies in the organism, and invades the lymphatic system, liver, spleen, and serous membranes; by its presence it sets up a chronic inflammatory process, with the formation of products which become caseified, and subsequently soften, producing gradual destruction of the organs. The bacillus is found in the centre of the tubercles in their earliest stage of formation, and hence is the cause and not the effect of the morbid process. The lymphatic vessels, amœboid cells, and the blood current are the means by which these micro-organisms are transported from one organ to another and diffused throughout the body.<sup>8</sup>

(To be concluded.)

## REMARKS ON THE COMMUNICABILITY OF PHTHISIS.<sup>1</sup>

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[DR. BARRS having reviewed in detail the experiments and observations of Buhl, Klencke, Villemin, Marcet, Wilson Fox, Koch, Watson Cheyne, Spina, Cohnheim, Tappeiner, Schottelius, Aufrecht, Weichelsbaum, Poulet, and others, proceeded as follows]:—

According to Koch and Watson Cheyne, then, our definition of tubercle in the future is to be by the presence of the tubercle bacillus; and I am not sure that we, who are not mycologists, are in a position to oppose this change. For some years past there has been a general falling away from old morphological or histological definitions in this matter; caseation is gone as a test; the giant-cell has been found in many admittedly non-tubercular substances, such as the syphilitic granuloma; and the old intercellular network has been removed by Cornil and Ranvier. Almost all pathologists are prepared to define tubercle, apart from the bacillus, rather by its distribution and tendencies—its life history in short—than by any peculiar structural element. That tubercular processes, as we most commonly meet with them in phthisis, constantly show the bacillus in greater or less quantity, I have no doubt; I have myself found it in all the cases of phthisis (thirty in number) which have come under my observation during the last year, and the number of cases on record in which others have found it now far exceeds five hundred. Of negative observers upon this point, I shall refer to one only, who, if he still adheres to his original position in regard to the bacillus, stands almost alone. I need scarcely say I refer to Spina, of Vienna, who, with great energy and some feeling, took up every point in Koch's paper, and produced arguments and experiments to refute them. He denied the characteristic reactions of the bacillus, and that they were found in tubercular matter only, and adduced many other points which have needed confirmation from the time they were published (1883) to the present. In short, the constant presence of the bacillus in tubercular tissue has been affirmed by all who have taken the trouble to look for it, and in no other substance has it been found.

Time does not permit me to enter into any discussion of the relations of tubercle to phthisis. Were I to propound a view which I held almost up to the time of collecting the materials for this paper, I should, I doubt not, be accounted one of a bygone age. The simple broncho-pneumonic theory, as propounded by Niemeyer, now nearly twenty years ago, and twenty years before that by Addison, had always seemed to me one which could be embraced without doing violence to one's reason or to clinical and pathological facts. Indeed, when tubercle was looked upon as clinically malignant, one saw no other way of explaining the cases of recovery which one was now and again obliged to admit, as well as the large number of cases which used to be designated, and, as it seems to me, rightly so, mechanical phthisis; but the discovery of the bacillus has changed all this. When in every case of phthisis an organism is found which is common to it and all other manifestations of tubercle in the human subject, and also in all animals subject to the disease, spontaneously or artificially produced, one cannot but abandon, though with much regret, the dualistic theory; in fact, I believe the unity of pulmonary phthisis is now almost universally admitted.

As to the positive evidence of contagion in the human subject, there is little forthcoming, and that little is of a very doubtful nature. In this country the only extensive investigation made in this direction was made by the Collective Investigation Committee last year. Prior to this the only cases on record are those previously referred to, published by Dr. Hermann Weber and Villemin. The Collective Investigation Committee, as will be remembered, issued to the members of the British Medical Association, on two occasions inquiries as to the communicability of phthisis. Of the 10,000 or more members, only 1028 replied; of these, 673 had seen no examples, 261 reported cases they

<sup>6</sup> Ibid., part i., p. 48.

<sup>7</sup> Ibid., part i., p. 183.

<sup>8</sup> Sormani London Medical Record, 1884, p. 53.

<sup>1</sup> Being the concluding portions of a paper read at the opening of a debate on the Communicability of Phthisis, at the meeting of the Leeds and West Riding Medico-Chirurgical Society, March 7th, 1884.

believed to support the infective theory, and the remainder were doubtful or negative.

I propose to criticise with some minuteness Dr. Hermann Weber's cases of supposed transmission from husband to wife. I do this because they are the most fully reported cases with which I am acquainted, and because they are not unfrequently quoted by the complete believers in communicability as providing almost all that is to be desired in the way of evidence in favour of their views. The cases are nine in number, and are reported in the Transactions of the Clinical Society for 1874, though they seem to have occurred some years previous to that time. The husband is in every instance described as of consumptive family; they all (the husbands) suffered from hæmoptysis between their eighteenth and twenty-first years; and they all survived, save one, a second marriage at least; and one survived a fourth, and another a third. The marriages, too, were not repeated with absolutely indecent haste, consequently the husbands survived for some considerable period in each case the first marriage, in which they are supposed to have transmitted phthisis to their partners. They are all described as healthy, or considered so, at the time of marriage; and they all, save one, died of phthisis, as stated by their medical attendants or as ascertained by post-mortem examination. Amongst the many remarkable points in these cases, the most noteworthy to me is the period of time which elapsed between the supposed infection of the first wife, when of course the husband was regarded as suffering from phthisis, and his death. In Case 1 the period I have calculated from the facts given was at least twenty-five years, in Case 3 nine years, in Case 4 twelve, in Case 6 eight, in Case 7 eight, and in Case 8 ten; in the remaining cases no definite figures are given, but calculating from the number of pregnancies of the wives and other statements it may be roughly stated at from five to nine years. The man in Case 9 married his first wife in 1867, his second in 1868, and was living and well in 1872. To return to Case 1. A man said to be of marked phthisical family, to have had hæmoptysis at twenty-one, and to have infected four successive wives with consumption, lived through twenty-five years to tell the tale. Can this seriously be taken as an example of infection from husband to wife? Suppose him to have been perfectly well, as stated, at twenty-seven, when he married his first wife, who showed symptoms of phthisis after the third pregnancy—that is, four years after marriage at the earliest,—and take this as the commencement of his own disease; after an interval of ten years he marries his second wife, whom he infects; the intervals between the second and third and third and fourth are not given, but say the time allowed to elapse between the death of one wife and the marriage of another was twelve months; this, added to the time which each wife lived, will give us a period of not less than seven years, during which period he must have been exhaling the contagium of phthisis, or, in other words, have been suffering from active lung disease, and then survived the last of his supposed victims for eleven years, when he died and presented the following changes in the lungs: “a cicatrised condition of the former seat of disease, and also the recent affection.” I confess I do not see my way to accepting in any sense this case as proof of direct infection from husband to wife; rather it is to me a more than gratifying example of the curability of phthisis, and a proof that a man may cohabit with four individuals dying of consumption, who is himself of consumptive extraction, who has exhibited lung symptoms from his twenty-first to his twenty-fifth year, and lives to the not unripe age of fifty-seven, having expressed some doubts as to the propriety of even a fifth marriage.

This line of criticism, which I deem not unfair, may be applied with the same result to all Dr. Weber's cases. They prove too much, not only of contagion, but of recovery from what we regard as a commonly lethal condition. The cases published in abstract in the Collective Investigation Record scarcely admit of analysis, the statements being in so many cases simply the conclusions of the reporters, and not the facts upon which the conclusions are based. In Dr. Weber's cases, and also in the collective investigation cases, I beg you to notice the entire absence of cases of acute general tuberculosis, of tubercular meningitis, strumous arthritis, and of urino-genital tuberculosis, all of which diseases are to be accounted manifestations of tubercular affection in the human subject, and cases of which we have a right to expect if the theory be true as applied to man in these so-called examples of infection.

To proceed further with the negative side of the question. I need scarcely refer to the very striking statistics of

the Brompton Hospital in regard to the affection of attendants resident in the hospital. During the last twenty-five years not a single case of phthisis fairly traceable to the surroundings could be found amongst more than 250 officials connected with the hospital during that time. The mortality, in fact, amongst residents was lower than in some general hospitals. It has been said that this proves only that under such conditions as exist at Brompton phthisis is not contagious, and that it cannot be taken as in any degree settling the whole question. But, I would ask, do the conditions at Brompton differ in kind or even in degree from the condition under which life will go on amongst us? The element for contagion has been shown to exist in the atmosphere of the building generally, and Dr. Charnley Smith has shown also that the bacillus is continually escaping from the air-passages of phthisical patients.

My own conclusions upon this extremely wide and difficult question are, I need scarcely say, subject to revision. In the main they are as follows:—1. The tubercle bacillus is constantly found in all lesions of a tubercular nature, and in all cases of pulmonary phthisis in the human subject. 2. So far as the communicability of tuberculosis in animals is concerned, this is clearly proved by inoculation experiments. Whether it is so without antecedent traumatism there is very little evidence to show. 3. The great prevalence of phthisis amongst those of certain occupations, and the comparative rarity with which those presenting caseous changes in the lungs die by means of a general infection, tend to show that certain predisposing causes are necessary for the production of phthisis—some more definite condition than a mere obliquity of construction, such as we mean by constitutional tendency. The bacillus is not in this case the real and primary cause of the disease, but by its constant presence may determine a wide distribution of the lesion and a fatal result. 4. Although the discovery and isolation of Koch's bacillus and the inoculation experiments upon animals make the direct infection of individuals with tuberculous matter possible under certain conditions, these conditions are not established by our present mode of life. 5. The clinical evidence of direct infection from man to man is at present of a quite untrustworthy nature. I need scarcely add that the want of proof of direct contagion in no way militates against the view that pulmonary phthisis is an infectious disorder—that is, due to the introduction of an infective particle from without.

#### SOME STATISTICS AND OBSERVATIONS REGARD- ING LITHOTOMY IN THE HYDERABAD CIVIL HOSPITAL, SINDH.

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TABLE I. is a summary taken from the monthly returns of the above hospital of the operations for lithotomy performed by myself from the 1st January, 1883, to the 31st August, 1883, a period of eight months. It gives the number of cases, with the ages, results of the operation, &c. The patients were all in good health, with the exception of the symptoms arising from stone in the bladder. Of the 102 cases operated upon, 81 were Mussulmans and 21 were Hindoos, of whom 98 were males and 4 were females. To avoid taking up too much space, I have been obliged to arrange their ages into quinquennial periods. The table will show that Sindhees are infinitely better subjects for operation than Europeans, being so much less liable to inflammation setting in after the operation, owing probably to the simple manner in which they live, and perhaps to the very dry climate of Hyderabad. The operations at the other hospitals in Sindh, though fewer, have reached to pretty nearly about the same favourable results; consequently lithotomy here in Sindh is not looked upon as a formidable procedure, as is the case in Europe, owing to the mortality being comparatively so very low. The table will also show the average number of days spent in hospital, as well as the average duration of the formation of calculi, arranged in quinquennial periods, to correspond with the ages of persons