by old women and quacks; and we are apt to forget, in our zeal for medical reform, and amidst our earnest appeals to the legislature for a legal separation between the qualified and unqualified practitioner, that quackery thrives as easily on our own neglects as on the ignorance of the public; and that there are public men who know how to throw this in our teeth. So long as there are certain classes of disease which are thought unworthy of the attention of the hospital teacher and college examiner, so long will the profession itself prepare a fruitful soil in which quackery will grow and thrive. And my object on this occasion has been, not to discuss the comparative merits of different kinds of ointment, plasters, dressings, and other minor details, but to exhibit in a disencumbered form the principle on which all practical men agree—that of giving firm, and constant, and equable support to the whole surface of the limb, from the toes to the knee. If this principle were universally understood and carefully acted out, we should hear of no more incurable bad legs healed by Holloway's ointment—no more amputations for mere ulceration—no more ankylosed joints from mere inaction of the joint induced by the tenderness of the skin or the presence of an old and irritable ulcer.

Lectures

ON CERTAIN VIEWS ON THE NATURE AND TREATMENT OF PHTHISIS BY PHYSICIANS.

DELIVERED AT THE

BROMPTON HOSPITAL FOR CONSUMPTION AND DISEASES OF THE CHEST, IN THE SUMMER OF 1856.

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LECTURE I.

BASES OF MEDICAL KNOWLEDGE. TUBERCLE IN THE LUNGS IS NOT ESSENTIALLY DUE TO THE SCROFULOUS DIATHESIS, BLOOD-DISEASE, OR HEREDITARY INFLUENCES. RESEARCHES AS TO THE QUANTITY OF THE INGESTA AND EGESTA, SHOWING THAT PHTHISIS IS NOT ESSENTIALLY DUE TO DEFICIENCY OF FOOD, DEFECT IN DIGESTION, OR WASTE OF FOOD.

ALL the knowledge which the medical profession possesses must be based upon demonstration, personal experience, or reasoning. The second, conjoined more or less with the third, constitutes what is known by the term authority, and acquires its value in our judgment from some supposed or real superiority in the author, as to the extent of his field of observation, or his habits and power of observation and reflection. Of these two bases, viz., demonstration and authority, this age has exhibited a decided preference for the former; and it is evident that a demonstration must be more conclusive than an assertion. The only objection that can be raised, is the liability to error which must ever exist in all the inquiries which are required to constitute demonstration; and hence even a demonstration may be held to be truth in one age and falsehood in another. But in how much higher a degree must authority be exposed to error, when men are so liable to be deceived by the imperfection of their senses and the appliances employed to aid the senses, by the prevalent opinions of their day, by the imperfection of their judgment, and by the uncontrollable influences of prejudice. And such sources of fallacy are apt to be more open at a late period of life; that is, at the period when authority is naturally claimed and allowed. Hence authority in general knowledge during the middle ages, and in medical knowledge in the persons of Hippocrates and Galen, at first gave stability to knowledge, and then fixedness to error: and so it must ever be. With the increase of medical knowledge during the last two centuries, and particularly in our own time, authorities have vastly multiplied, and each has held the sceptre for a limited period, with the result of losing it in all, and of regaining it in a few instances. Thus our profession is continually changing its so called truths; and in a few instances, as that of the doctrine of the humoralists, this

continued movement has completed its circle, and has returned to the point whence it started centuries ago.

In a profession which is so much based upon personal knowledge, there must always be a deference paid to authority; and those who have few opportunities and small powers of obtaining truth must be content to sit at the feet of others; but it would be only in accord with the spirit of the times if they ever looked with some degree of dissatisfaction upon authority, and demanded demonstration. Whilst, therefore, I hold the opinions of the many eminent men of this day in the greatest esteem, I shall not be thought presumptuous if I ask for their facts and weigh their arguments; and if, by the aid of demonstrative inquiry, I should vary the opinions which now prevail. As it is the inherent right of a man to use well his mental powers, and to cherish freedom of thought, so it is the right of our profession to multiply its points of inquiry after truth, and to avoid rather than seek for the shackles of mere authority. Our profession, and especially that department of it with which we are here more particularly engaged, is likely to add to its influence for the good of mankind, as it permits and gives due weight to both demonstration and authority, looking, however, to the latter as a fertile source of false theories, and the former of false facts.

I have for some time been closely engaged in inquiries of a demonstrative character in reference to phthisis, and have obtained facts and opinions which to my own mind have called for a modification in the received view of the nature of that disease. To authority I do not appeal; but I venture to hope that the facts which the inquiries have elicited may be received as a sufficient ground for the opinions which I shall now venture to bring forward.

On inquiring into the nature of phthisis, we can seize upon two circumstances, viz.:—1st. The morbid condition which it leaves behind it; and 2nd. The changes in the working of the system which attend it. The former has of late years been more particularly brought under notice, probably on account of the constancy with which tubercle is found associated with it in the lung, and the readiness with which its appearance, at least so far as the naked eye may guide us, is recognised. Thus tubercle has been elevated to the dignity of the essential part of phthisis, without our having agreed as to what it is, whence it is derived, or how it is deposited in the lung as we find it. Now, it must be evident that this tubercle is the effect of anterior conditions, and consequently an effect instead of, or in addition to its being, a cause; and if that may be admitted, it is equally clear that it may be an accident, and not an essence; and that the condition of the system or the organ in which it is produced, is that to which attention is primarily due, and which may prove phthisis to be one manifestation of a state which, with equal readiness, may exhibit, under other circumstances, other evidences and other so called separate diseases. To regard the tubercle as the essence is to limit greatly the range of our observation; but to look upon it as an effect or an accident, opens a wide field for study and investigation.

If we start, then, from this point, we may search for its cause either in the immediate spot in which it is found, and thus regard it as a local disease, and a general disease only so far as every part must be influenced by the general system; or we may consider it as proceeding essentially from a general cause, and found locally only so far as every entity must have a location. We will now enter on this part of the discussion.

It is abundantly evident that the disease has a local habitation in the lung, and also that, as it progresses, the system in general exhibits many conditions opposed to those of health; but it would not thence follow that the tubercle resulted from any state of the general health, but rather that the increasingly diseased condition of the general system resulted from the deposit of the tubercle and the interference which it must effect in the action of an organ so essential to health; or, perhaps more likely still, from a condition which may have been anterior to and causative of both.

Now, there is much to support these latter views of the disease; for so little is the general system affected in the earliest moments of the deposition, that of common agreement we affirm that the exact period of first deposit cannot in any case be determined with certainty. Nay, so insidious is its march, even locally, and so gently are its effects there and upon the general system developed, that we often find the deposit most abundant, and the destruction of the organ considerable, when the patient's own attention is first called to it. Surely this is far opposed to any theory which would attribute the deposition

of the tubercle to a state of system which, by any ordinary test of health, may be distinguished from other conditions of ordinary health. Then, if we add to this that, in the experience of all of us, the first evidences of the disease have in not a few cases been observed to follow some other local condition of the lung, as that of inflammation, and may therefore possibly, if not probably, be fairly attributable to that preceding local disease, we cannot summarily reject the argument under consideration.

The chief objection which has been raised against this view of the case is, that in many instances the same matter is deposited in other organs, both distant from and in structure unlike to the lungs. Now this fact is indisputable, except on the ground that, as we cannot agree as to the intimate nature of tubercle in the lung, we cannot fairly contrast that deposit with other deposits elsewhere. There is so close a resemblance between the so called tubercle-cell, as it changes through its various stages, and various corpuscular elements found naturally in the body, and even the still disputable epithelium of the air-cells of the lung, that, in the present state of our knowledge, we dare not affirm this or that to be tubercle, except when fortified by the circumstance of locality, quantity, or other adventitious aid. It is quite certain that the naked eye is unable to help us in this difficulty, or to be a guide which would satisfy a microscopist, or be a firm basis on which to establish an hypothesis; and it is equally clear that chemistry, even when the observer is capable of fairly employing it, leaves the matter much where it found it. Then the argument stands thus:—There is in all cases of advanced phthisis a large deposition in one organ, which we agree to call tubercle; and, in a limited number of these cases, there are lesser deposits in other distant structures, which, to the naked eye, and also to some microscopists, bear some resemblance to the deposit in the first mentioned organ; and hence the inference is, that, in this limited number of cases, there was tubercle in various places; and that, whenever tubercle is found in one organ, there is a probability that it will be found in some or many others. Now this is not induction, but assumption, until the identity of the two deposits has been so proved as to satisfy the mass of scientific inquirers. Let us, therefore, regard it as an assumption which has some show of truthfulness; but let us not forget that the fact of the diversity of structure in which the tubercle is said to be deposited is a *prima facie* argument against the identity of the deposit; for, with the exception of another unknown disease which we call cancer, with its subdivisions, diseased actions of one kind appearing in different parts of the body at the same time commonly select the same structures. But, passing on, if we admit for a moment that the deposits are one and the same, we now know perfectly well that, in many of the localities in which they appear, the structures are in fact identical. I have no doubt that there is an epithelial lining to the air-cells of the lung, any more than I have that there is an epithelial covering to the serous membranes of the abdomen and the head; and by common consent we now believe that the deposit of tubercle usually, if not exclusively, takes place upon the free surface of the membranes. Now the free surface is the layer of epithelium; and therefore, whether the earliest point of deposit be in the air-cells of the lung, or on the peritoneum, or on the arachnoid, it is in one and the same structure, viz., the epithelial cell. The argument, it must be remembered, has reference only to the first point of the deposit on any particular spot, and not necessarily to subsequent deposits; for by subsequent deposition the relations of parts become changed by pressure and by altered structure. If, then, we admit that true tubercle may be deposited, not only in the lungs, but on the serous and mucous membranes, we have nearly embraced all the localities in which it is said to be found when so generally deposited, except in the structures of the glands; and, although the anatomy of these latter organs is very ill understood, we know that they chiefly consist of corpuscular elements. Thus the argument opposed to the theory before mentioned is reduced almost entirely to this, that, in similar organs apart from the lungs, a matter is simultaneously deposited which resembles or is tubercle. But this does not prove it to be a general disease of the system; it only affirms that, at the same time, in the same structures, there is the same or a similar local change proceeding. If from any cause the epithelium of the air-cells may become or may produce tubercle, there is a high probability that the same local power or sufferance of change may exist in the epithelium of other parts, either to a greater or a less extent. Hence, for aught this argument can furnish, the deposition of tubercle may be a purely local dis-

ease. This being granted, there is no insuperable difficulty in accounting for the more abundant and frequent deposition in the lungs, when we remember how infinitely multiplied are the epithelial cells in the lung, in comparison with those of any other part of the body; and more particularly if we add to this the incessant action of the lungs, and consequently the necessity for the constant renewal and removal of the epithelial cells. It is, however, right to notice that, if an obstruction could exist in any part of the body to the free exit of effete epithelium, thereby causing an accumulation of effete matter, it might occur in the air-cells of the lung; for, at the termination of the minutest bronchus, the canal dilates and forms its cavities or air-cells, which are arranged in a spiral manner around the central line of the tube, and hence the area occupied by the cells is vastly greater than the area of the termination of the bronchus. Thus, at this point of constriction of the return current, the matter discharged might accumulate, and offer a peculiarity in the lung which does not exist elsewhere. And further, the epithelium of the air-cells is tessellated, whilst that of the minutest bronchus is ciliated; and thus any substance once placed within the area of the bronchus is almost sure to be passed on by the vibration of the cilia. This is not so in the air-cells; and as, in the so called tubercular mass found within the lobular membrane in acute phthisis, there are not any (or at the most only few) ciliated epithelia, we may prove the limitation of the disease to the air-cells, as well as the accumulation of epithelium in those cavities. There is no necessity, as it appears to me, to assume any especial liability in the lungs to this deposit, except upon these palpable grounds; and consequently, so far, none to modify the argument of a local origin for the deposit in phthisis.

Now, having carried the inquiry to this point, and shown that there are probabilities in favour of a local origin, I do not consider that it by any means excludes the idea of a change in the general system which may facilitate that deposition either in the lungs only, or in those organs as a part of the general system. Whatever may vary the action of the lungs may surely vary the degree in which this deposition may be facilitated.

But another objection has been raised to the idea of a local origin, and an explanation given of the supposed general origin of tubercle, by promulgating what is known as the blood theory. It is assumed that, at the moment of earliest deposition of tubercle, the blood is diseased, and that at the points of tubercular deposit it gets rid of its morbid material. This of course implies a morbid condition of the blood both anterior to and causative of tubercle, and not resulting from or simply coincident with the deposit of tubercle; and it is presumed that the elimination of the tubercle from the blood is not either the result of morbid action in the elementary organs, or of secondary changes in the matter which the blood may directly or indirectly deposit, *as due simply to local causes*. Now, if we grant, as we may readily do, that the blood must vary in each individual, and at every hour of the day and period of existence, from a variety of causes which influence the quantity and quality of food and its elaboration, as also the excretion of effete material; and further, if we admit that a deposit of a like nature, appearing in the course of a disease in various parts of the body, is presumptive evidence of the deposition of this material from a common and general source, we are far from having proved that tubercle results from any specific condition of the blood. To presume that tubercle is deposited directly from the blood implies that it had previously existed in that fluid. Yet Lehmann has affirmed that he could not detect any change in the blood which could be ascribed to phthisis, much less that is causative of phthisis; and that any change, as, for example, an excess of fibrine at one period or of water at another, is due to such accidental circumstances as inflammation and profuse sweats. Hence the theory which ascribes tubercle to a fibrinous crasis is a theory built upon an effect or an accident, seeing that, when tubercle is first deposited, no such crasis exists; and if it did exist, it would be an assumption, and not an induction, which should state that the fibrine being effused into the air-cells is converted into tubercle. There is no proof that fibrine is effused into the air-cells as an essential condition precedent to tubercle.

Rokitansky, the high priest of the idol-worship of the present day, appears to me to treat this subject in an unphilosophical manner—a circumstance which, with such a man, must imply a defective subject and a preoccupied mind. He ranges tubercle under the head of fibrinous crasis, and

begins by asserting that it will not account for tubercle universally; and then goes on to say, with great precision, that the fibrine may be either in excess or defect, inasmuch as both conditions are found in the course of the disease. So far, then, he is clearly illogical; for it is not presumed that tubercle can arise from the existence of fibrine absolutely; and if it may exist without any change in the fibrine, and also with the great diversity of both excess and defect, is it likely that the fibrine can be causative source of the tubercle? He meets this difficulty by assuming that the change is in quality, and not in quantity. But how does he attempt to prove this? Simply by taking the two conditions of "surpassing proneness to deposition" and "high grade of coagulability" which mark tubercle, and affirming that such are evidences that the fibrine is changed in quality. It is evident that this is mere assertion on two different subjects. If tubercle were due essentially to a condition of the fibrine, that condition must have existed at the moment of earliest deposition; but, on the showing of this great authority, it may exist without any change, and that change may be either an excess or deficiency of quantity; and, as to quality, no proof of any essential change at that period has been discovered; and finally, to sum up the whole, he gives the remarkable opinion contained in the following words: "*Still these said causes by no means serve to throw much light upon the nature of tuberculosis.*" (*Pathological Anatomy*, Sydenham Society, vol. i, p. 377, et seq.)

But, to return, it is true that our having failed to detect tubercle, or indeed any essential change in the blood, except on some questionable occasions, is no proof that it does not exist there; but it is presumptive evidence in the highest degree that it does not exist, and it is for those who maintain this theory to prove its existence. Then, again, as we have already considered, it is certain that generally, if not always, tubercular matter is found with secreting organs; and if it be a secretion or excretion at all, it is probably due to the eliminating function of those secreting organs. Thus, in this point of view, the *fons et origo mali* may be at least as probably in the secreting organs as in the blood whence they obtain their material. One statement is as probable as the other; but if there be a preference, it ought rather to be taken from the blood, and ascribed to the secreting organs as a cause, seeing that, in the earliest moment of deposition of tubercle, the general system does not necessarily appear implicated; and that, even in the latest period, the tubercle cannot be shown to exist in the blood. But it has not even been proved to be a secretion or an excretion at all in its earliest moments. Schröder Van der Kolk, Dr. Addison, and others, have advanced the theory that it is essentially a mass of epithelial cells (that is, of natural organs) which have undergone certain changes; and the great similarity of the structures with their almost invariable association, added to other arguments for a local origin, seem to lend great support to this doctrine. Hence, so far from tubercle being necessarily due to a specific change in the blood, there is no evidence that it is even a secretion or an excretion, much less that it exists in its proper form in the blood.

At this point, then, we might arrest the discussion. But if it might be admitted, for the sake of argument, that it is due to the blood (without admitting that it exists in the blood), it is evident that we gain no available information by calling it a blood-disease. There is a probability against any disease (not accident) being a purely local disease, and there is a certainty that all local parts are supported by the blood; and hence it may with much truth be affirmed that every disease is a blood-disease, however varied its manifestations may be: but what have we gained if we stop short at this point of the inquiry? When we have said this, we seem to have got to the *ultima Thule* of our knowledge; whereas, if we could trace the disease to that source, we should then be only on the threshold of the inquiry.

But, failing any knowledge in this direction, it is assumed by the advocates of the blood theory that the argument is sustained by referring to the organisation of the individual, and showing that, in the same family, more than one member has been afflicted with it, or that even the parents or the grandparents of the patients were its victims; and hence has arisen the notion of its being an hereditary blood-disease; and further, that, in one member of the family, phthisis appears, whilst others present evidences of scrofula; and hence the further notion that there is a diathesis or system which affects others than consumptive patients, and may be described by a generic term as that of the tubercular diathesis.

Now, it may be readily admitted that there is a sort of com-

munity of organisation in families, and that the parents communicate a part of themselves to the child; and hence, that there may be a parity in the parents and the children, *inter se*, as to the degree and kind of health which they may have, and consequently as to the predisposition to disease which may attach to them. But surely it is a speculative refinement which assumes that there is a standard type of health with which individual systems may be compared, but which is yet incapable either of exhibition or definition, and which centres changes from the type of health in the blood, and gives to them the broad term of diseases. There is no blood communicated from the parent to the child; but there is something which carries with it and subsequently obtains the vital principle or the combination of actions which together form what is denominated the living organism. It is manifest that, in the development of the embryo, parts of the organisation are formed anterior to anything which can be correctly called blood, and that the blood and its nature must be due to the vital principle acting through the more early formed organisation; and hence the blood of the child is due directly to the child itself, and only indirectly to its parent. If this be admitted, it would be more logical to refer any general state of the system to the whole organism, rather than to a specific part of it which may seem to play a more prominent, but yet, in fact, a very subordinate part, in it. The expression "blood-disease", from its indefiniteness, may be a convenient one for common parlance, so long as our knowledge is so limited; but it adds nothing to our knowledge, and must not be literally construed to mean a state of the blood which necessarily implies a deposition of a specific morbid matter.

But, in truth, it is more than probable that this notion of hereditary taint, as evidence of parity of disease in parent and children, is much exaggerated in its special application to phthisis. That, on a large scale, any number of individuals should be afflicted with the same or similar diseases, is surely nothing marvellous, when we remember how like men are the one to the other, and how identical they are in the means by which their bodies must be nourished. If it were not for the different circumstances in which men are placed as respects poverty, vice, and their attendant evils, it would rather be a marvel that all had not the same diseases. We do not single out men who may have indigestion, derangement of the liver or other organ constantly in action in the vital processes of the system, or who even may have what is a more general condition of the system—some inflammatory disease. We know that it is highly probable that their fathers and their brethren had similar complaints; and we are satisfied, without ascribing any peculiar hereditary state of the blood to them. But in phthisis—a disease afflicting so many persons, that one-tenth of those living die from it alone—cancer, and a few other unknown diseases, we are not content with seeking for their causes in the systems in which the diseases are found, but must attach an essential (not accidental) importance to the blood of their progenitors. I am convinced, from an inquiry which, however, is not yet complete, and also from the investigations of our distinguished colleague, Dr. Walshe (*Report on Phthisis*, pp. 16 et seq.), that the notion of hereditary taint, as being especially applicable to phthisis, has been far too highly magnified, and that but in a proportion of cases has there been phthisis or any other of the so called tuberculous or scrofulous diseases in their families. No doubt, in many instances, the parents or the brethren had the disease referred to; and such a fact is apt to impress itself upon the mind, and, in the absence of any clue to unravel the origin of the disease, we may be too apt to attach undue importance to this. Hence I submit that the doctrine of blood-disease has done very little to prove phthisis to be due essentially and chiefly to general causes, as opposed to local conditions; and that, at the most, the term may be permitted to express a certain peculiarity of the general organisation, and not a direct morbid matter obtained from the parent, or held in common by members of the same family. We cannot but attach considerable importance to this question, for a belief in this theory of blood-disease has done much to induce the profession to distrust any instance of the origin of phthisis from temporary and local causes—a distrust which I believe to be unjustifiable, and inimical to the right study of the disease.

So far, then, we may sum up the argument by affirming that the weight of evidence is in favour of a local origin of phthisis, but one probably associated with a condition of the general system, which may either be original or acquired.

[To be continued.]