were offered to her. She sat in a bent position, with her head tightly fixed between her hands in her lap, night and day, for a period of ten years, as delineated in the accompanying sketch. Her death occurred in 1847. The body was brought to the dissecting-room, and dissected in the same position it had remained in for the last ten years of her unhappy life. The autopsy revealed considerable organic lesions of the brain.

The case is mainly interesting and worthy of record in consequence of the long period during which the patient continued in the position exhibited in the drawing. Her state was not cataleptic, neither was there the slightest symptom of a tetanic condition of the muscular fibres.*

28, Cavendish Square, January 1857.

Lectures

ON CERTAIN VIEWS ON THE NATURE AND TREATMENT OF PHTHISIS PULMONALIS.

DELIVERED AT THE

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

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LECTURE II.

INTERFERENCE WITH RESPIRATION. RESIDUAL AND TIDAL AIR. DIMINISHED VITAL CAPACITY AND LESSENER ACTION OF AIR VESICLES LEADING TO DEFECTIVE CHEMICAL CHANGES, TO COLLAPSE, AND TO MORBID PRODUCTS. ALL GENERAL CAUSES ACT BY LOWERING NERVOUS POWER AND LESSENER VESICULAR ACTION. SEX. AGE. TUBERCLE—WHAT IT IS, AND WHY IT SELECTS THE LUNGS AND THEIR APICES.

In considering the nature of an object, it is essential to fix the mind only upon such circumstances as are universal. It is also of equal importance to distinguish effects from causes, and both from merely coincident conditions. These may appear to be very trite observations, but it is to the disregard of them that must be attributed nearly all untrue theories; and to avoid such an error is always a matter of no small difficulty. In applying these remarks to the subject now under consideration, I would direct your attention only to the period of the earliest occurrence of phthisis; for it is manifest that, although new points of deposit may continue to occur in the course of the disease, it is then less difficult to isolate their causes, since the complicating circumstances are fewer at the moment of origin than at a later stage of the disease; and if there be a universal condition causative of phthisis, it must be equally existent at the former as at the latter period. To build a theory of the nature of a disease upon circumstances which attend, and therefore may arise from it, is manifestly to court error, as I conceive to have been the case, in such a theory as that of the fibrine crisis of tubercle.

In my former lecture, I endeavoured to prove that on the weight of evidence phthisis ought rather to be regarded as a local disease resulting from general causes, than as a general disease having simply a local manifestation; and consequently that it is not essentially due to the scrofulous diathesis, to blood disease, or to hereditary influence. In objecting to the prevalent error of ascribing it to a disease of the blood, I stated that the advocates of that theory have to contend with the following adverse conditions, which are generally admitted to exist;—

1. That it may arise from local causes only.
2. That it may occur without disease of the blood.
3. That the morbid product has not been found in the blood.
4. That no state of the blood is known which is characteristic of the disease.
5. That the changes in the blood which have been seized upon are such as accompany and do not originate the disease.
6. That of those conditions the fibrine (to which the diseased condition has been referred) may be unchanged or too much or too little.

* Should any members of the Association visit the interesting town of Freiburg, they cannot do better than place themselves in the hands of an exceedingly obliging and intelligent *valet de place*, of the name of John Andrew Ritschel, at the *Hähringer Hof*. He will be found a charming companion in a day's ramble to various objects of attraction.

7. That the change in quality which is affirmed to exist is proved, not from the fibrine itself, but from the tubercle; or, in other words, that the fibrine is tubercular, because sometimes tubercle is found in it.
8. That at the origin of the disease the general system offers no change which is opposed to the quality of health of the individual.
9. That the disease is arrested in any stage without any vicarious elimination of morbid matter having being set up, or without any change in the general system having appeared, which is in anywise specific.

It was not denied that a state of the blood may be one of the causes of phthisis; but since we cannot point out some specific characteristic of that state, it avails nothing in a practical point of view to regard phthisis in general terms as a blood disease.

Afterwards, it was proved that phthisis could not be essentially due to insufficient food, to imperfect digestion, or to waste of nutritive material.

We now proceed to consider *the condition of the organs concerned in respiration, and the interference with the function of respiration which undoubtedly exists*. There is no doubt that the nutrition of the body demands not only the introduction of suitable food, and its digestion by the alimentary organs, but the complete elaboration of it as it passes through the capillaries of the lung; and hence any hindrance to the latter is as prejudicial to nutrition and to life as prevention of the former. It is also clear, from the products of respiration, that effete matters pass off by that function, and that any impediment to respiration must be followed by a diseased action of the general system. However readily we may subscribe to the facts now mentioned, I think it must be admitted that, in reference to the nutrition of the body, we too much forget the importance of the respiratory process, and give almost exclusive attention to that fact which is most cognisable to our senses, viz., the introduction of food. That air is absolutely essential to life, no one denies; but that a full supply of it to the system is as essential to health, we seem practically to forget; for we commonly content ourselves by sending patients into the fresh air, forgetting that that does not necessarily imply that they will take more air into the lungs. Hence, although we have proved that the digestive organs are not at fault in the production or in the early stages of phthisis, we have yet left for inquiry a function quite as essential, although it be less cognisable to the senses, and in some sense also of more difficult investigation. I am anxious to impress the importance of the subject, since, in my opinion, it is in that direction that we are primarily to look for the solution of our difficulty.

It is very evident to us that the quantity of air admitted into the lungs must be regarded under two aspects. Thus—1. When the lungs have been once distended with air, they never entirely lose it in the ordinary act of respiration, but a large quantity of it remains after even the most complete expiration. This is known as the residual air; and to this must be added the reserve air, or that quantity which, after an ordinary expiration, can be emitted by forced expiration. 2. By ordinary respiration, a certain volume of air is taken into and emitted from the lungs; and this quantity is capable of the greatest variation in health, according to stature and exertion, and especially according to the voluntary effort to breathe more or less deeply. Thus, I am about six feet in height, and breathe about 30 to 35 cubic inches at each inspiration; but by a full inspiration I can inspire 260 or 280 cubic inches, in addition to at least 100 cubic inches of residual air; that is to say, the capacity of my lungs beyond that used by me in the ordinary act of breathing is about 330 cubic inches, or a quantity ten times as great as that which I commonly inspire. This is the tidal air, and is in the proportion of about 1 to 5 of the residual and reserve air conjoined. Hence it is clear that we must have regard both to the effect upon the blood or the system due to the residual air, and to the smaller portion which is continually changing in the act of respiration. It is quite possible that both of these may vary greatly from states of disease, and that each may vary independently of the other. Thus, as the residual air is found in every part of the lung in health, it is to be inferred that any cause which would expel that air permanently from any part of the lung would lessen the quantity of this residual air, and yet at the same moment the quantity of air entering and passing out by the trachea during respiration shall remain the same.

But, since the functions of the body are maintained in health by this full volume of residual air, and the smaller quantity of tidal air which is changing in ordinary respiration, of what use is it

to regard the total capacity of the lung, which has already been shown to be naturally so much beyond that which is employed under ordinary circumstances? The answer must mainly have reference to the quantity and the action of the residual air, as just mentioned; for it is that air which is more immediately in contact with the capillaries; but doubtless there is a second reason. If we passed our lives in one quiet state of repose, we should require but that little variation in quantity which may depend upon food, fasting, sunlight, darkness, posture of body, and waking in sleep;* but we do not do so: on the contrary, at intervals, we have all to make bodily exertion; and when that is great, as in rapid running, the frequency of respiration is not only increased, but the quantity of air entering with each act is increased; and hence, at these times, we need lungs of a volume larger than is called for in a state of repose. This of course applies most to those persons who are the most exposed to violent exertion; and hence, if, from any cause, the lungs cannot permit this largely increased volume of air, so called for, the evils will fall most heavily upon those classes who most need the increased volume. Hence, at the same stage of any disease, thus limiting the capacity of the lung, the evils of this limitation will be the most felt in the working classes, or in any individual whose requirements in life call for much bodily exertion. Thus, although the lungs are so much more capacious in health than is commonly called for by the usual requirements of life, any considerable diminution must be a permanent evil in lessening the amount of residual air in the lung, and a temporary evil whenever the requirements of the patient compel him to call for a larger temporary supply.

The first of these evils is doubtless the most important, seeing that it is permanent and induces a diminution of the aërication of the blood and the elaboration of the food, which is essential to health and possibly to life. It is, therefore, of moment to be able to measure this evil, and this has been attempted on the largest scale, by my predecessor here, Dr. Hutchinson. He does not profess to measure the quantity of the residual air either in health or disease; but it is manifest that as in disease the capacity of the lung is lessened, the measure of that diminution will be an inferential measure of the lessened quantity of residual air.

But of the two modes of inquiry considered apart, viz., the quantity of air which is being constantly changed, and the quantity which is comparatively fixed in the lungs, I consider the latter is more likely to be due to disease and will progress as the disease progresses, whilst the former may at least be open to inquiry as a cause. The latter, moreover, is important, on the ground that the residual air to be of use must be changed more or less perfectly and frequently, and whilst the diminution of the residual air would be an evil by limiting the local extent of the process of aërication, any lessening of the quantity of air in ordinary respiration would lessen the act universally, and also lessen that upon which, alone or mainly, the value of the residual air depends. But as both acts are necessary to complete respiration, they must be considered together, lest we should find ourselves in a difficulty when we observe that the one may remain normal whilst the other is greatly changed.

Now that the capacity of the lung is greatly lessened in cases of phthisis, is evident from the large amount of that organ which is occupied by solid matter, an amount which often seems after death to have left but little room for air. Dr. Hutchinson's method also proves this during life, and also measures both the existing extent and the loss which has been sustained. Upon this, then, we may rest the fact of a diminished quantity of air in the lung. In reference to the variation of the quantity which is changed by respiration, much is open to investigation and fallacy. I have ascertained, on various occasions, that it is possible for the vital capacity to be reduced one-third, or probably more, and yet the quantity of air in ordinary respiration shall not have lessened greatly from the healthy standard. But in these cases it does not by any means follow that there has been no diminution in the process of aërication, and hence the mere quantity of air ordinarily inspired is no correct measure of the capacity of the lung or of the vital process.

I do not adopt the view of Liebig that the blood in phthisis is unduly oxygenated, because with a great diminution in the number of air cells the same quantity of tidal air, and therefore of oxygen, is received into the few as would have been into the many cells. In many diseases, one part of an organ undertakes a double share of duty when another part has ceased to act, but

this is not so in phthisis except in a very limited degree and at the earliest stage, for we do not find the chest expand in one part as it flattens at another, or any evidences (except with the above limitation) of increased action in the air cells. As the chemico-vital changes proceed in each cell, the loss of one cell must diminish them, and hence when three-fourths of the cells are obliterated, the defect must be very great. The continuance of the due quantity of tidal air is no evidence that it must penetrate further into the few remaining healthy cells, for this air does not enter the cells in currents, but by diffusion of gases, and moreover in most of such cases there are cavities with pervious bronchi which will admit a quantity of the tidal air and yet not permit any chemical changes. We have also yet to learn more in reference to the varying capacity of the bronchi before we can fully account for the continued reception of the usual quantity of tidal air after the destruction of so large a portion of the air cells.

This, then, leads me to the point in which we all take the greatest interest, viz., the information to be obtained by the physical examination of the lungs in reference to the quantity of air in those organs.

There are two stethoscopic signs which directly indicate the entrance of air into the lungs, and which are in some degree a measure of the quantity, viz., the tubular breathing and the vesicular murmur. In a state of health the sound of the latter masks the former, except over the larger tubes; but when the vesicular murmur in any part is lessened in degree, then the tubular sounds begin to be heard more distinctly. This last I consider to be the explanation of the term, harsh breathing; for when the gentle buzzing universal continuous sound of the movement of the air in the air-vesicles is absent, the clear, short, intermitting sound of the air passing through the minuter bronchi is alone heard. It is also probable that when the minuter bronchi in any part become closed, and the larger ones alone remain open, that the breathing is still more harsh. It appears to me to be quite easy to distinguish the vesicular from the tubular breathing, and to measure with some nicety any diminution of the former, both absolutely by the lessened sound, and inferentially by the perception of the tubular breathing.

Now since from the structure of the bronchial tubes it is clear that air within them does not come into contact with the blood, they cannot be supposed to exert any influence over the process of aeration, and hence are not essential to the function of the lungs, except so far as the iron tubes which convey the water through the streets may be said to allay our thirst. They are simply conduits, and so to speak, accidents and not essences of the act and process of respiration. A large amount of air in these tubes would be no evidence of even the smallest amount of aeration of the blood, but the absence of this air would be negative evidence of the absence of aeration in the parts of the lung to which it leads. The absence of the tubular sound is, therefore, of definite importance, but the presence of it is no evidence of the integrity of the lung. Indeed, so far is the latter from the truth, that the existence of much tubular breathing may always be taken as evidence of the want of integrity of the lung.

The air-vesicles which are in close proximity to the minuter bronchial tubes are the essential respiratory organs, and the sound attending upon the movement of the air in the lungs is the only evidence which we have of the aeration of the blood. The amount of this sound is also a measure of the amount of air which is exposed to the action of the blood, although it may not necessarily be a measure of the effect which the air produces upon the blood. This latter is unquestionably the point at which we wish to arrive, but it is of difficult attainment. It may be that the amount of vital or chemical change effected in the air vesicles may vary not only with the quantity of air, but the state of the air-vesicles, the condition of the blood, and the amount of vital energy, and hence no one of these conditions may exactly represent the other, yet it may perhaps be permitted to infer practically, that where the state of health is not far different from that of men in general, the amount of air passing into the air-vesicles is a tolerably fair measure of the amount of vital change which follows. Hence the circumstance of the greatest moment is the ability to estimate with accuracy the amount of air which enters the vesicles. This cannot be done by any instrument which simply measures the quantity of air passing into the lungs on inspiration, for the obvious reason that the greater part of this air may not pass further than the bronchial tubes. It can only be effected by carefully noting the amount of vesicular murmur in various parts and in the whole of the lungs. This requires much careful attention and

* See my articles in "Transactions of Royal Medical and Chirurgical Society," 1856; and "British and Foreign Medico-Chirurgical Review," April 1856.

also practical knowledge of the amount which in various circumstances occurs in health, and hence is of difficult attainment, and must mainly rest upon personal investigation and experience. This change is observed in two ways: first, by the length of the inspiratory vesicular sound; and, secondly, by the degree of intensity and extent of this murmur.

A little reflection will suffice to convince us that the length of the inspiratory act must mainly depend upon the amount of air-vesicles then in use, for they not only constitute the bulk of the lung, but are the points where the greatest resistance is offered to the entrance of the air; and they, therefore, claim both length and force of inspiration. On the other hand, if the entrance of air is confined to the various divisions of the bronchi, and especially if it is denied at the same time to many of the smaller bronchi, the length of the trajet of the current is reduced, and the resistance to its entrance greatly lessened, so that a shorter and a more feeble inspiratory effect would result. As, however, the entrance of the air cannot be confined during life to the bronchi alone, in proportion as the quantity of open air-vesicles is very small, so will nature commonly (apart from phthisis) make greater effort to expand those few, in order that they may perform a double share of duty; and thus, with short inspiratory acts, there will be considerable muscular effort in inspiration. The force of inspiration is, however, different from the force of the muscular attempts to inspire; and it is quite certain that there may be the greatest effort in inspiration, and yet the current of air passing into the lungs shall be both short in its extent and feeble in its motion. If, however, the system is inadequate to this increased effort to inspire, as is so constantly seen in feeble and phthisical persons, there is both short and feeble inspiratory effort, as also little muscular effort, since entrance to a large quantity of the air vesicles is denied.

In estimating the extent of the respiratory process in any case, we must, therefore, give almost sole attention to the vesicular murmur, and ascertain, both generally and locally, its vigour, intensity, and extent. Whenever, generally or locally, it is feeble, coterminous with the act of inspiration, and of very short duration, it will be just to infer that there the respiratory process is deficient. Generally speaking, where there is universal deficiency of respiratory murmur there is also a short inspiratory act; for as either few only of the air vesicles are thus expanded, or if many, then they are only very partially expanded, but little air need to enter or quit the bronchi. It must, however, be borne in mind that as the total capacity of the lung is so much larger than is required for the common acts of life, there may be a certain amount of diminution before the health is really endangered from the want of aeration of the blood, and even that is warded off for the time by increased frequency of respiration. Yet as the residual air occupies the whole lung, any exclusion of it from any part must so far lessen the action of the air upon the blood, and an injury, slight or otherwise, must thenceforward be inflicted upon the system.

Now, what evidence have we that the amount of vesicular murmur does vary in a perceptible degree? It is evident that in phthisis, wherever there is consolidation or destruction of a part of the lung, there the vesicular murmur is lessened or lost, although the tubular breathing may remain more or less extensive. There is frequently a source of fallacy in this circumstance, at least at first sight; for even then, unless the mischief is very extensive, some portion of the vesicular sound is audible; but if attention be carefully given, it will be evident that this sound is more distant or isolated than is usual, and it is, in fact, due to isolated portions of more healthy lung in contiguity with the part which is diseased. It is, therefore, essential to fix the mind only upon the spot where the vesicular sound is deficient until it is impressed with the deficiency, and then it may seek to ascertain to what extent and in what direction there is more healthy lung. The loud continuous vesicular murmur in healthy lung which is heard through the stethoscope is not entirely due to the layer of lung, which is superficial and not far removed from the base of the instrument, but in fact consists of the action of the vesicles which are central and more distantly seated. In like manner, although a part of the lung may cease to admit air into its vesicles, it may be surrounded by portions of healthy lung which emit the vesicular sound. So, in old cases of bronchitis with emphysema, there is found more or less extensively an absence or a diminution of the respiratory murmur, whilst the tubular breathing may be even louder than is usual. But it is needless to multiply evidence that in confirmed disease of the lungs the vesicular murmur may be reduced or lost.

Now, what is the condition of the air-vesicles in the early stage of phthisis—at a stage, in fact, when the system appears to have suffered but little, if any, from the presence of the disease? We find a slight, it may be the slightest, amount of dullness on one or both clavicles, with some flattening of the chest over and under the clavicles, and a marked diminution of the vesicular murmur at this and perhaps other parts. It is at the same time ascertained by Hutchinson's method, that the diminution in the vital capacity may be at this stage upwards of 100 cubic inches; and it is that startling fact which gives great gravity to the case. With, then, only slight dullness and lessened vesicular breathing, there is a very large diminution in the vital capacity of the lung. Now to what is the slight dullness attributable, and to what do we look for the explanation of this serious discrepancy between the extent of the physical signs and the deficient capacity of the lungs. The only fact upon which we have at present seized is the deposit of tubercle; but our experience of *post mortem* examinations, and our belief of the extent of the disease in this instance, from the intensity of the physical signs, are quite inconsistent with the amount of diminution of vital capacity. Surely an amount of tubercular matter deposited in the air vesicles which could displace 100 cubic inches of air, or which could be so disseminated and situate as to prevent the entrance into the lungs of so large a quantity of air, must be infinitely more than is found at the early stage of the disease. The conviction is thrust upon us that there must be something beyond the deposited tubercle which is sufficiently extensive to prevent the entrance of so large a quantity of air into the lungs.

However, at the very early stage of the disease there is lessened vesicular murmur and greatly lessened vital capacity. But what is the state anterior to this condition of the lung— anterior so far as we can ascertain to the deposition of tubercle?

This question cannot be answered by reference to any specific case, because it is not possible to state that the case to be referred to would ultimately have tubercle deposited in the lung; or that any case which appeared likely to become phthisical had not then tubercle actually deposited. We must rather refer to the set of cases which, by common consent, are believed to be prone to phthisis. Such are persons of delicate organisation, contracted chests, great excitability without firmness of system, or of much sluggishness of habit, with pallid complexion. There are no returns of investigation into the vital capacity of persons afflicted with other diseases than phthisis, and hence we are left to our own observation. Amongst the mass of out patients of the Hospital there are always some who appear to be on the verge of phthisis; but in whom there are then no evidences of the existence of that disease. All these agree in this one common condition: shortness of the inspiratory effort, and general diminution of the vesicular murmur in its depth and intensity. In such persons it is oftentimes difficult to count the rate of respiration, on account of the very slight breath motion; and on the application of the stethoscope one is led to ask for a deep inspiration in order to hear the inspiratory sound at all. I am convinced that in those cases which have a marked disposition to pass into phthisis, and those in whom there is strumous or other disease of the system indicating debility, there is, with original or acquired flattening and narrowing of the thoracic cavity, less breath motion, less air admitted into the lungs, and less vesicular murmur, than is found in a state of health.

It is thus proved that in phthisis, even in its earliest stage, there is a great diminution in the vital capacity of the lung; and it is asserted that with this there is less respiratory force and less vesicular murmur; and it is also asserted that in those persons whom experience has shown to be most likely to become phthisical, there is less breath motion and less vesicular murmur; which, together with the flattening and deficient mobility of the chest, the external evidences of deficient expansion of the air-cells, must lead us to conclude that in the earliest stage of phthisis there is a great diminution of the function of the lung in aerating the blood; and we must believe that the like fact is observed in persons anterior to the production of phthisis. Whether, therefore, phthisis be due or not to this lessened aeration of the blood, it is manifestly preceded by it, and is accompanied by it in all its stages. We have hence found, as an anterior and attendant condition of phthisis, one of the vital functions of the body greatly reduced in extent and vigour.

[To be continued.]