racter is marked by fickleness, impulsiveness, irresolution, want of composure, unrest, and other unmistakable signs of a radical feebleness of will—by wilfulness, it may be, for wilfulness in nine cases out of ten means the reverse of fulness of will—will overridden by some feeling or fancy—waywardness—an involuntary rather than a voluntary state of mind.

Another marked mental peculiarity in neuriasis, more marked, perhaps, than any of those which have been already noticed, is fancifulness. Due care is not taken in forming firm foundations in fact. Conclusions are jumped to hastily. The dominant faculty is, not reason, but imagination; and the fancies formed are too often undistinguishable from true delusions. A pain or uneasy feeling, for example, is supposed to point to some disease; and soon the reason refuses to be convinced that the disease is not present. This is the characteristic misdirection of the imagination in the so-called hypochondriac. What is at first a mere fancy, in the end becomes a delusion, by the removal or burial of the landmarks which mark the line where soundness of mind becomes continuous with unsoundness. As a rule, however, the change does not go so far as this in neuriasis. Fancies of all kinds are continually crowding in upon the mind; but they can be ejected by a combined effort of will and reason, though not always without very great effort. What is wanted is not so much the capacity to reason soundly, as the ability to do so without help. What is wanting is intellectual independency, rather than intellectual clearness; and for the most part the fancy will walk steadily enough, if she can but lean upon the arm of a good counsellor.

Along with this fancifulness, and in a sense a part of it, may be noticed also a marked disposition to imitativeness. There is a curious readiness to copy the thoughts, words, and acts of others—a readiness which would seem to imply a certain want of originality in character. There is the gift which, if sufficiently under control, makes the actor. And herein may often arise much confusion; for what at first may be simply acting, may afterwards be more than this, the actor being carried away by his part, and unable to become himself again, in consequence of not being sufficiently on his guard against the danger of a fancy becoming a fixed delusion if it be given way to too much. The attempt to deceive, persisted in too long, may easily lead to the deceiver himself being deceived, as is shown in a hundred different ways in the history of the condition of hysteria so called.

Unbalanced spirits may be also mentioned as another of the mental peculiarities of neuriasis. The mental state in this respect is the reverse to that which is called composure. The spirits, judging them by any common standard, are always higher than they ought to be, or lower—the latter more frequently than the former. In neuriasis, indeed, as in mental unsoundness proper, a disposition to melancholy would seem to be more common than a disposition to the contrary state of levity—so common as to have led many to speak of hypochondriasis as a variety of melancholia. Moreover, it is in cases like these that the patient can so well understand what Jeremy Taylor means when he speaks of men being “desperate by too quick a sense of constant infelicity.”

Last of all, there is often to be noticed, among the mental peculiarities under consideration, a comparatively feeble sense of moral obligation. In not a few cases, no doubt, exception may very justly be taken to a statement like this. In not a few cases, without question, there is rather a morbid regard for truth and a tenderness of conscience by which the most trifling exaggeration or wrongdoing is made to become a positive torment. But more frequently by far there is a state of things in every way opposed to this—a state marked, if not by a habit of actual lying, by the most extraordinary disposition to exaggeration, and a moral deafness which makes it difficult to hear even the loudest calls of conscience. It is notoriously difficult to get the truth out of a so-called hysterical patient, and it really seems as if she could not help exaggerating and misrepresenting; and, though not so notorious a fact, a fact it is nevertheless, that she is more apt to excuse than to condemn herself when in the wrong. And what is true of this case is, so far as my experience goes, not less true in the great majority of cases of neuriasis—is so true, indeed, as to leave no doubt as to the propriety of placing this feeble sense of moral obligation among the other mental peculiarities of neuriasis.

And thus—to recapitulate briefly the heads of what has been said respecting the physical and mental peculiarities of neuriasis—among the former will have to be placed,—prone to pass large quantities of pale, limpid, neutral urine, under any emotion or excitement, or after it, with a frequent irresistible *besoin d'uriner*; prone to abdominal flatulence, *pneumatose intestinale*; prone to fits of laughing and crying, without sufficient reason for them; prone to globus; prone to overflow of saliva; prone to constipation; together with tenderness on pressure, with more or less uneasi-

ness or actual pain apart from pressure, in one or other part of the abdomen, or under the left nipple, or somewhere in the course of the spine; with a marked disposition to spasm; and with as marked a disposition to periodicity in one form or another:—and among the latter, or mental peculiarities,—undue self-sensitiveness; overpowering craving for sympathy; quick and perverse likes and dislikes; feebleness of will, showing itself in impulsiveness and in many other different ways; fancifulness; imitativeness; unbalanced spirits, the inclination being commonly towards despondency; and, lastly, a comparatively feeble sense of moral obligation.

Few in the present day will be prepared to contend that the womb has any special connexion with hysteria, or the liver or any other abdominal viscus with hypochondriasis; or to doubt the correctness of the view which regards both hysteria and hypochondriasis as disorders, closely allied, of the nervous system. Still it may be doubted whether, practically at least, this latter view is as clearly realised as it ought to be—whether the words hysteria and hypochondriasis do not still retain a good deal of their old meaning, and not unfrequently mislead effectually in practice. At all events, hysteria and hypochondriasis can be no fitting names for that general derangement of mind and body of which I have been speaking. Another name is evidently desirable, which will point away from the womb, or liver, or other abdominal viscus, to the nervous system; and hence my excuse for proposing the term *neuriasis*. I hope, however, that a time will come before long—the time is not yet ripe, I allow—when it will be possible to get a step further than this, and choose a name which will include the *mental* as well as the *nervous* half of the disorder in question; for I cannot but think that the *nervous* trouble is the effect rather than the cause of the *mental* trouble. Nay, I am even sanguine enough to hope that eventually, by the institution of a sound discipline, in which the mind is taught—chiefly through the influence of wise mothers acting upon it in the morning of life—to know and exercise its high powers of subjecting the feelings and thoughts, as well as the actions, to sound reason and will, that mankind may be delivered, not only from the evils of neuriasis, but even from the still graver evils of actual insanity.

THE ANATOMICAL RELATIONS OF PULMONARY PHTHISIS TO TUBERCLE IN THE LUNG.

By H. CHARLTON BASTIAN, M.D., F.R.S.,

Professor of Pathological Anatomy in University College, London; Physician to University College Hospital; and Assistant-Physician to the National Hospital for the Paralysed and Epileptic.

THE question which has been ably discussed by Dr. Wilson Fox is one of so important a nature, and at the same time one concerning which so much difference of opinion has always existed, that it would seem most useful for the different speakers in this debate to set forth as concisely as possible the views which they themselves entertain upon the general question, without entering too much into matters of mere detail. I shall therefore follow the example of Dr. Wilson Fox, and endeavour, as briefly as the complexity of the subject will allow, to set forth the nature of my views concerning the relation of “Tubercle” to Pulmonary Phthisis, with no more reference to the opinions of others than is necessary for my own exposition. And if these views are found to differ, in some respects, very much from those which Dr. Fox has expressed, I feel sure that he will, nevertheless, be one of the first to recognise the desirability of looking at the question from all sides.

I may, however, state at the commencement, that any discordances which may appear between my own opinions and those of others, will not be so much differences with regard to facts, as differences concerning the interpretation and mutual relation of facts, about the reality of which we are all more or less agreed.

It seems to me, in the first place, quite impossible adequately to consider the question of the nature of tubercle, and its connection with pulmonary phthisis, wholly without reference to the relations of such a product to chronic inflammatory changes, to scrofulous changes, to the new growths known as lymphomata, and to those met with in leucocythæmia and other morbid conditions. It appears to me also highly inexpedient to consider the lung-affection alone, and wholly apart from the light which may be thrown upon the question before us by a brief consideration of parallel morbid changes in other organs. It is the treating of the question in this isolated manner which, as it seems to me, alone makes it possible to consider as legitimate, or expedient, views which may tend to introduce much needless confusion into pathological science.

Taking up the involved question as to the nature of tubercle at that stage in its history to which it had been brought by the labours of

Addison, Reinhardt, Virchow, and others who had preceded them, we find that it presents itself somewhat in this fashion. The notion that caseation, or cheesy degeneration, constituted the essence of tubercle, was thrown aside as one which a larger and more minute experience could in no way sanction. The characteristics relied upon by Laennec and others had been shown to be almost wholly valueless as characteristics of any special and peculiar something which it had become the fashion to name tubercle. The masses previously so named were shown to be in the main mere products of chronic inflammation in different tissues and organs. And inasmuch as such products, both in their incipient and in their advanced stages, were specially abundant in the lungs of persons dying from pulmonary phthisis, this affection became one of which tubercle could no longer be considered as the pathological essence. It was now known that epithelial impactions from overgrowths in the bronchial tubes, and fibroid indurating infiltrations of a chronic inflammatory nature, were in the main the special tissue changes which subsequently gave rise to the various lesions of chronic phthisis. The word "tubercle" having been thus restricted in its meaning, what remained to which the name could be applied? Only one product, which Laennec had been in the habit of regarding as an early condition of a deposit destined subsequently to undergo the caseation supposed to be characteristic of tubercle. It is true that Laennec's central idea had been shown to be erroneous, it may be true also that the body upon which the name of tubercle was now to fall had originally been described by Bayle as a non-tubercular substance.* This product was the "grey granulation," a body now known to have a constant and invariable structure, in the main resembling that of lymphatic gland-tissue. It was, and is, the anatomical mark of an obscure general affection, having no necessary connection with phthisis in the strict sense of the term—that is, as an ulcerative lung-disease; it was a product prone to occur in more or less abundance within the cranium, within the chest, and within the abdomen of persons suffering from what we now term Acute Tuberculosis. But if this obscure and protean febrile affection, in which grey granulations are found disseminated through the organs, has no necessary relation to phthisis, on what pretence can it be called by its present name, and what possible reason could be assigned for giving to the grey granulation the mere name of tubercle, when the essential meaning of the term—that of a caseating product productive of phthisis—had previously been so different? To which questions it seems only possible to return the following replies. In the first place, we must assume a deeply-rooted reluctance wholly to cast aside the word "tubercle," a word which, up to this period, had borne such an important connection in men's minds with phthisis—although phthisis had now been fully shown to be a non-tubercular affection in the past sense of that term. And, secondly, because there was a very flimsy justification for such a course. Although this obscure febrile affection, Acute Tuberculosis, is one which may occur quite independently of any traces of phthisis, still it has a special aptitude also to occur as an intercurrent affection in the course of chronic phthisis. Grey granulations are therefore to be met with at times in the lungs of those suffering from phthisis. For this reason, and because they were bodies possessing the knotty or granular characters which were originally implied by the term tubercle—because, in addition, they everywhere possessed a constant structure, Virchow and others were content to let the name "tubercle" rest with them, although it must have been fully recognised at the time that such bodies had no necessary connection whatsoever with pulmonary phthisis as a chronic destructive lung-disease.

A few years ago this point of view was very widely accepted by pathologists in many parts of Europe. In the time of Laennec, pulmonary phthisis, in its common forms, meant tubercular disease of the lung, and tubercle was regarded as a "specific" product. Forty years later, the common varieties of pulmonary phthisis were regarded as due almost solely to various forms of chronic inflammatory changes in the lung; tubercle was regarded as a mere occasional and quasi-accidental complication; both phthisis and tubercle were robbed of their so-called "specific" attributes. This was the kind of view into which I finally drifted about the year 1866, recognising, however, fully, that the word "tubercle" had attained a thoroughly artificial meaning, wholly different from its original signification, and that the preservation of the name was one of questionable expediency.

Now, however, we see a powerful reaction setting in, and a marked tendency to restore to some of the old "infiltrations" the name of tubercle, so as at the same time to make phthisis again an essentially tubercular affection. What wranglings and never-ending disputations the very prospect opens up! What, it may be asked, is the meaning of the new

point of view? We must endeavour to answer this question first, before attempting to come to an opinion upon the desirability of reverting to what one might almost venture to call an old and worn out doctrine.

The reasons which have been most influential are, I think, not difficult to find. Most valuable investigations have of late years been made, both here and abroad, upon the so-called "Artificial Production of Tubercle"; and many of those who had carried them on had adopted the views of Virchow concerning the limitations to be attached to the word tubercle. Now the Rodent animals, and guinea-pigs especially, are in more ways than one very peculiar creatures; so peculiar, as it seems to me, that it is not altogether safe to pass judgment offhand concerning the similarity of certain processes which may be set up in them, to those which are known to occur in the human subject. We now know that this artificial so-called tubercular affection may be initiated in guinea-pigs by the mere introduction of an ordinary irritant into and beneath the skin of the animal. Whether tubercular or not, therefore, the affection established is one which, in the old sense of the term, can have nothing very "specific" about it. The growths set up by local irritation gradually spread to lymphatic glands, and subsequently internal organs, such as lungs, liver, spleen, etc., also become affected. Such growths were at first believed by Villemain to be tubercular, simply because they had followed the inoculation of "tubercle" beneath the skin of the animal, and because such a result was deemed harmonious with the supposed specific nature of the inoculating substance. But when it was clearly shown that ordinary chronic inflammatory products—even that the introduction of a mere seton beneath the skin—might give rise to similar morbid conditions and products in guinea-pigs, this reason became no longer of any avail. Something was, however, to be said, in favour of the lesions being tubercular, owing to the nature of the growths themselves. They had, in the main, the characteristic lymphoid or adenoid structure, which the grey granulation of acute tuberculosis possesses. But was this similarity sufficient to make it absolutely necessary to consider that the condition frequently established in the Rodentia was tuberculosis, and that the lesions were tubercular? This is a most important question for pathological science, and one which, as it seems to me, was never very adequately considered by those who were themselves engaged in these most interesting and important investigations.

I cannot too strongly draw attention to this stage of the argument. Here was a generalised affection set up in guinea-pigs, marked by new growths in the lungs and other organs, which presented the microscopical characters of lymphatic tissue. Was the histological constitution of the growths alone sufficient to justify us in calling these products tubercular? This question, thus nakedly put, would, I believe, have been unequivocally answered in the negative by many pathologists. None of the modern pathologists who had accepted the views of Virchow have ever pretended that the grey granulation had any characteristics specific and peculiar to itself. It was always recognised to be of the lymphoid type, and it was always admitted that growths in no way distinguishable from it histologically were to be met with in the organs of persons suffering from leucocythæmia and other allied affections. Growths of the lymphatic gland type—but to which no one thought of attaching the name tubercle—were in these affections more or less disseminated through different organs and parts of the body. Obviously, therefore, mere histological structure alone could give us no right to look upon the new growths in guinea-pigs as tubercular. As I have before endeavoured to point out, it was the grey granulation of Acute Tuberculosis which pathologists had arbitrarily agreed, in default of other products, to regard as the only true tubercle. If, therefore, this body were admitted to have no specific structure, if other products histologically similar were not named tubercle, then it becomes clear that the notion of tubercle was arbitrarily centered, not so much in the histological characters of any individual product, as in its structure combined with its mode of occurrence in individual organs and throughout the body—it had become centered, in fact, in the grey granulation as the sole specific and invariable mark of that general constitutional affection known as Acute Tuberculosis, rather than in any mere form and histological characters of the grey granulation itself.

Thus, the real question to be considered came to be, whether the general affection set up in the Rodent animals was or was not identical with the febrile affection in man which, in the strict acceptance of the term, goes by the name of Acute Tuberculosis.

I know not whether the question was ever argued out in this manner by others. It was obvious, however, that the majority of pathologists were quite willing to call these growths in the Rodent animals tubercle, and the general condition itself tuberculosis. To this view, I was never able to give in my adhesion. My difficulty arose from the fact that I was unable to see a real identity, such as appears to some, be-

* On re-reading Bayle's four cases of uncomplicated "Granular Phthisis," I have become convinced that M. Thacon is right, and that these could not have been cases of what we now understand as Acute Tuberculosis.

tween Acute Tuberculosis as it occurs in man and the affection which could be easily established in the Rodentia. The mode of origin; the comparatively slow and gradual spread of the guinea-pig affection from organ to organ; the almost invariable absence of growths in the brain or meninges; the mode in which lungs, liver, and spleen were affected—the wide spread infiltrations and the greatly increased bulk of the latter organs—were all in striking contrast with acute tuberculosis as it occurs in the human subject. For in this latter affection we find an obscure origin, and the more or less sudden outbreak of an acute febrile malady which almost invariably leaves some marks upon the meninges, and in which the strictly essential anatomical lesions were minute granulations, either separate or aggregated, scattered through organs of almost normal size. With such differences staring us in the face, and with the patent, though by no means insignificant fact, that the means which sufficed for inciting the affection in question in the Rodentia, were wholly incapable of setting up Acute Tuberculosis in man, it has always seemed to me to be very difficult to come legitimately to the conclusion that the two affections were similar, and therefore equally difficult to come to the conclusion that the products found in the Rodent animals have any real right to be considered as tubercle. My interest in these investigations and notions concerning their importance are, however, not in the least diminished merely because I do not feel justified in applying a particular name to the morbid conditions or products themselves.

These doubts, however, which I still very strongly entertain, concerning the propriety and legitimacy of the present generally received views as to the name which should be attached to the affection in the Rodent animals, have not been shared by others. Foreign investigators, as well as Dr. Wilson Fox, Dr. Burdon Sanderson, and others in this country, have not hesitated to look upon the condition as tuberculosis, and upon the products as tubercular. Nay, more, Dr. Sanderson stated nearly four years ago, in a communication which appeared in the *Edinburgh Medical Journal* for 1869, that the occurrence of "infiltration", in which large patches of lymphoid new growth appeared in the liver or other organs of the guinea-pig, should suffice to open our eyes with regard to the different forms in which tubercle might manifest itself in the human subject. This, if I might venture to say so, was the insertion of the thin edge of the wedge which Dr. Fox has now, with so much vigour, endeavoured to drive home. Already there was the dawning notion that infiltrations and chronic inflammations were again to have their day as tubercular products. I mentally shuddered at the chaos into which—I say it with all respect—it seemed to me we should again be introduced, and I was by no means comforted by the fact that new doctrines of "infection" were to be introduced with the view of explaining multiple morbid processes in general. In Dr. Sanderson's opinion, tubercles are adenoid bodies (or lymphoid patches) enlarged; and, for the establishment of phthisis three things are necessary: (1), a constitutional predisposition; (2), a local irritation leading to an increased growth of pre-existing lymphoid structures; and (3), a process of infection, by means of which the morbid growths extend to adjacent or related parts. Concerning this process of infection Dr. Sanderson says: "The word designates the fact that, wherever a chronic induration, due to overcrowded corpusculum, exists in any organ, it is apt to give rise to similar processes elsewhere." Dr. Sanderson would apply these views even to the mode of extension of "the so-called infiltrated forms of induration" met with in ordinary cases of phthisis. Here, then, the tendency was strongly manifested to consider that all infiltrating fibroid indurations which were marked by an "overcrowded corpusculum," might spread by a process of infection, and the logical outcome of the views stated was, that such infiltrating indurations were tubercular in nature. The transition to such a belief is all the more easy because, as I have before insisted (*Path. Trans.*, 1868, p. 54)—in anticipation of what has followed—rapidly advancing fibroid overgrowths in their early stages are as notable for their number of corpuscles as the most typical lymphoid tissue. The two differ in fact merely in the degree of perfection of their intercorpuscular stroma, although almost all transitions are to be met between the two. That difficulties may sometimes exist in distinguishing between such products seems also to be the opinion of Dr. Wilson Fox, since he says, "The reticulum under a microscope with high powers is to be found in almost all forms of tuberculosis except in the most recent granulations, and there nuclei and small cells crowd upon one another, forming a dense mass, and no reticulum can be seen."

The views which Dr. Wilson Fox has now so ably expounded will thus be found essentially similar to those which were more or less explicitly stated by Dr. Sanderson, and I cannot doubt that he also has been largely influenced by considerations similar to those which have found favour with Dr. Sanderson. It is true Dr. Fox claims to have an additional warrant for his views, by reason of the fact that, in the lungs

of children dying from Acute Tuberculosis, infiltrating corpuscular lesions are to be met with in addition to grey granulations in different stages. Such a fact, however, as it seems to me, can have no real title to induce us to modify our views, when we recollect that the restricted signification of the term tubercle, adopted by Virchow and others, was confessedly arbitrary, and necessitated by the complete overthrow of the old doctrines as to the nature of tubercle. Almost all forms of phthisis had previously been regarded as tubercular, under the belief that a "specific" and peculiar product was almost invariably present. Afterwards it had been shown that the substance previously supposed to be specific had nothing peculiar about it, and was the result of common inflammation in various forms, and that none of such products were worthy of the name tubercle—although, as a mere concession, pathologists were willing to allow this name to be retained by a product (the grey granulation) which was an occasional accompaniment of phthisis. It is literally true, therefore, that in the minds of those who had accepted this position, "tubercle had come to be a mere accidental complication of phthisis, and in no respect its chief anatomical distinction." And as it seems to me the fact that an arbitrary signification of the term had been consented to as a mere compromise, and simply with the view of retaining the old name for something—even when its original meaning had been entirely taken from it—is lost sight of by Dr. Wilson Fox, when he now comes forward and urges us to accept a new signification of the term merely because, in Acute Tuberculosis as it occurs in children, pathological products other than the grey granulation are apt to be found, although such extra products are confessedly not necessary elements of the disease. It seems, moreover, to me, almost wholly beside the question to say, as an additional reason for the adoption of the new views, that "proof is also wanting that tubercle in the lung can appear in no other form than the isolated grey granulation." No proof was even attempted to be given: we must again urge the fact, that the restriction in the use of the term was from the first confessedly though advisedly arbitrary.

The products which Dr. Wilson Fox and Dr. Burdon Sanderson would now have us include under the name tubercle were, therefore, amongst those which, amidst the shipwreck of the old term, were deliberately cast aside. They were known and recognised as products of an irritative or chronic inflammatory overgrowth, under the names of interstitial pneumonia and fibroid indurations. Such products, not only in the lung, but also in other organs, are, as is well known, very largely, and often almost exclusively, made up of minute corpuscular elements similar to those occurring in lymphoid tissues. And if in some cases these overgrowths, or portions of them, as they occur in the lungs or other organs in man, have a still more exact resemblance to lymphoid tissue, the reason of this has now been made perfectly clear and simple to us, owing to the important discovery by Dr. Burdon Sanderson that very minute patches of lymphoid tissue are normally present in the peribronchial tissue of the lung, and upon the walls of the vessels in other organs. This being the case, what could we expect but that under the influence of such disturbing or irritative influences as lead to the overgrowth of the connective tissue-elements in any region of the lung, these intimately intermixed patches of lymphoid tissue should also undergo an irritative increase or hyperplasia. But, in the face of all that has been said, is this a justification for calling such bodies tubercles? Certainly not; no more than the possession of a similar histological structure was a sufficient warrant for immediately calling the morbid products met with in the guinea-pigs tubercle. If we are now to suppose that tubercle can be described broadly as "an adenoid body enlarged," or as a "lymphatic overgrowth produced by irritation," what are we to say of the lymphatic overgrowths which occur in the liver in leucocythemia? Are they also tubercles? Again, what are we to say of a mere irritated and enlarged lymphatic gland? Is this a tubercle? Or is the body no longer tubercle when it attains a certain size? Everywhere, as it seems to me, such views as have now been advocated land us in nothing but confusion. If the word tubercle is to be retained at all, we cannot start beyond the narrow circle of the limited and confessedly arbitrary signification given to it by Virchow and others, without plunging ourselves into a mere whirl of inconsistencies and contradictions.

Let it not be supposed, however, that I am in favour of retaining the word—even with the limited signification given to it by Virchow. No; it seems to me, for many reasons, by far more expedient to renounce its use altogether. To this opinion I have come slowly and deliberately, after an experience of several years in teaching the present doctrines. Year by year I have been more and more impressed with the altogether gratuitous and unnecessary difficulties besetting the path of any teacher who endeavours to explain to students what is and what is not tubercle, and why any given product is or is not honoured by such a name.

The principal difficulties seem to me to be of this nature. First, there is the fact that in ordinary chronic phthisis some of the lesions are most prone to appear in the form of granulations either simple or aggregated, although such granulations have nothing whatever to do with Acute Tuberculosis in the strict sense of that term, and therefore have no right to the name tubercle. Such granulations may be softer than the ordinary grey granulation, owing to their containing a larger proportion of epithelial elements or of their derivatives; others may be harder and more pigmented; whilst others still may, both in naked eye characters and in microscopical appearance, be almost indistinguishable from the grey granulations of acute tuberculosis. And yet there is oftentimes not the slightest suspicion that Acute Tuberculosis has existed—far from it, growths of this kind are occasionally to be met with in such a mere local, though chronic, disease as Cirrhosis of the lung. In one of the most typical cases of this affection—where the opposite lung was healthy and where no granulations were to be found in other organs—the part in which the fibroid consolidation was still advancing was thickly studded with minute granulations, having microscopical characters closely resembling those found in Acute Tuberculosis. Again, take the affection commonly known as “tubercular peritonitis,” but which I prefer to call Granular Peritonitis. Here the parietal and visceral peritoneum is more or less densely overgrown with granulations, presenting a truly lymphoid structure. Yet such an affection has, as I believe, no necessary connection whatsoever with Acute Tuberculosis. If we would be consistent, therefore, we must say that the products are not tubercular. Occasionally, such growths in the peritoneum and omentum are more lawless still: the reticulum is not developed; we have nothing but a prodigious overgrowth of minute corpuscular elements, so exuberant, however, that the growth may win for itself the more dignified appellation of “cancer.” Again, the correlation between lymphoid overgrowths and mere ordinary irritative overgrowths of connective tissue elements is most close. In the five or six cases in which I have made an examination of persons dying from well marked granular peritonitis, I have found the liver similarly and very characteristically altered. The organ has been more or less enlarged, pale in colour, and minutely mottled both on its external surface and on sections. What appeared to be minute, pale, and yellowish granulations, were thickly sown through a basis-substance, much of which had a more or less pellucid appearance. On microscopical examination, the pale and yellowish areas were found to correspond to islets of liver-cells more or less distended with fat, or simply granular and bile-stained, these areas being imbedded in an enormous new growth of tissue which had replaced the proper liver-substance. The new growth consisted, in part, of distinct fibre-tissue; in part, of a mere nuclear overgrowth, amongst which there was no definite reticulum; and in part, though in much smaller quantity, of more distinct patches of characteristic lymphoid tissue. On the other hand, kidneys, spleen, lungs, brain, and meninges have been entirely free from any characteristic or constant changes. There has been no evidence, therefore, of the existence of Acute Tuberculosis.

What I have already said may suffice to indicate a few of the difficulties besetting the path of the teacher, who, seeing the destructive snares which lie in wait for him if he venture beyond the narrow circle of a confessedly arbitrary definition of tubercle, tries what he can do to keep within these narrow and monotonous limits.

Attempt to step beyond these bounds, and see what follows. In answer to the question, what is tubercle? let one who is disposed to be adventurous, reply in the terms of Dr. Fox and Dr. Sanderson—that it is a new growth, lymphoid in nature, and resulting from a hyperplasia of a pre-existing nidus. Before he could maintain such a position, he should at least be fully prepared to answer various queries, some of the principal of which would be of this nature. What is scrofula, what is the nature of a scrofulous growth, and how is it distinguished from tubercle? What is the nature of a leucocythæmic growth in the liver, and how would you distinguish it from tubercle in the same situation? What is a lymphoma, and how would you distinguish an infiltrating growth of this type in the walls of the intestine or in the kidney from tubercle? Are there any transitions between growths which you are in the habit of calling tubercle, and those which may result from a mere irritative overgrowth of connective tissue? If there be, do you think you could always distinguish between such products; and if so, upon what differential characteristics would you rely? Do you consider that such a discrimination is easy, and likely to be made successfully by ordinary observers? If not, would not much confusion inevitably result between so-called tubercular and mere chronic inflammatory affections of organs? In the face of a questioner who had the power to exact a stern consistency, I strongly suspect that the champion seeking to establish wider limits for tubercle, would have to retreat to his narrower fold.

On the other hand, let him attempt to remain there in peace, and see what follows. Some ingenuous student, seeking for light and information, asks—What is tubercle? and he receives for his answer the statement that the term is now limited to a grey granulation, having a lymphoid structure, which occurs more or less disseminated throughout the head, chest, and abdomen in the affection known as Acute Tuberculosis. You show him such granulations, and make him familiar with the appearance of sections of them under the microscope. Subsequently, at an necropsy on a case of chronic phthisis, he sees bodies which have a close resemblance to grey granulations in the lung, and he asks you whether they are not tubercle. Whilst you may state that there are bodies to be found in the lungs in these cases which, though not really tubercle, present the naked-eye appearances of such a growth, and that there are others exhibiting even a still further correspondence of microscopical characters, which also have no title to such a name—still you could not positively tell such a student that the bodies in question were not tubercle. Why not? Because, as you would have to admit, chronic phthisis is a condition of things in which, as before stated, there is a tendency for Acute Tuberculosis to supervene. How could you say, therefore, that the bodies about which you were asked were not tubercle until you had disproved the possibility of the coexistence of acute tuberculosis? You would have at least to examine the brain and meninges, and also to examine the liver, spleen, and kidneys before you could even return a provisional reply. And if mere naked-eye inspection failed to reveal any trace of grey granulations in these various situations, you would, if pushed, still have to admit the possibility of the earlier evolution of grey granulations in the lung, though others in an incipient condition (not easily detectable without the aid of the microscope) might be present around the vessels of the meninges, or in the liver, spleen, kidneys, etc. Now this is a very roundabout process to have to go through when you are asked whether one of the granulations of chronic phthisis is or is not tubercle. The difficulties, again, are almost similar with regard to Granular Peritonitis. Here is an affection which has been invariably known as “tubercular peritonitis”; it is characterised by what appear to be the most typical grey granulations, and in this view you are confirmed by a microscopical examination. The puzzled student asks you why the growth is not tubercle; and if you would show him that you are consistent, and not merely perverse, long and involved explanations are needed. The old views concerning tubercle, their overthrow, and the new position taken up, must be explained. Then you have to tell him that, although such typical granulations are present, yet this granular peritonitis has no necessary connection with the febrile affection known as Acute Tuberculosis. And if you yourself might be somewhat disposed to relent in this case, still reflection upon all the dangers which beset your path outside the narrow confines of your present tubercle area, have convinced you that you cannot make one concession without being called upon to make others—without, in fact, taking up the very untenable position which requires you to answer all the more or less insoluble problems to which I have before alluded.

On all sides, therefore, the preservation of the word “tubercle” is a course beset with difficulties. At present, we are confused, hedged in, or hampered in a manner which is altogether ludicrous. Nobody knows what another means when he makes use of the word tubercle. We cannot explain our views on the subject without entering into tedious statements concerning conflicting views. No position that we have as yet taken up is natural or strictly defensible; and if we attempt to make it less objectionable, we shall probably only succeed in delivering ourselves over to still more fatal inconsistencies. All this worry and trouble is taken about a word which can never be of any use as a mental symbol for many generations—about a word, too, which even now we can perfectly well do without.

The facts themselves lose none of their significance because we cease to make use of a word which, amidst all the contrariety of opinion concerning it, has practically ceased to have any meaning. On the contrary, cease to make use of such a term, and these facts come out into the light of day, freed from the dense cloud of controversy and discussion by which their real significance had previously been obscured. A few words will suffice to indicate the views which, as it seems to me, would be more or less generally accepted if we consented to sink the use of the term “tubercle” altogether, and with it all the blinding notions concerning the “specificity” of phthisis, which are almost inseparable from its use.

The principal affections of which we have been speaking would be thus characterised and related to one another.

1. The disease now known as Acute Tuberculosis might the more completely, to eradicate the old idea, be spoken of as Granulæ—that is, we might adopt the name which has already been given to it by M. Empis, one of the writers who has contributed most largely to our

knowledge of this protean malady.* All the facts really known concerning the affection would continue as before. We might still speak of its typical growths as grey granulations, merely ceasing to use their synonym "tubercle." The meningitis, which occurs in this affection, we may again speak of as Granular Meningitis—a name under which it was described in 1827 by Guersant, and whose use was continued by many subsequent writers, such as Rilliet and Barthez, Bouchut, and others.

2. The more localised affection hitherto known as Tubercular Peritonitis, and also characterised anatomically by the presence of grey granulations, we may henceforth speak of as Granular Peritonitis. This condition has no necessary connection with the acute general affection Granulæ. All known facts concerning its natural history would remain unaltered by its change of name—certain misleading suggestions, indeed, implied by the old name would be got rid of, and we should be free to seek in an independent manner for the causes of this somewhat obscure malady. Its almost invariable association with an acute cirrhosis of the liver is a most noteworthy fact. The liver-affection is probably a sequence of the peritoneal condition; but the absence of anything like a general infection of the system is both instructive and remarkable, when considered in relation to the affection next to be mentioned.

3. Occupying a position, as it were, intermediate between these two affections, though substantially agreeing with no malady which is known in the human subject, we should place, as it appears to me, the lymphatic affection in the Rodent animals in which the so-called "Artificial Tubercle" is produced. Like Granular Peritonitis, it is at first a purely local malady, though in the guinea-pig affection there is a tendency to rapid "generalisation" of the new growths, which is almost wholly wanting in the allied disease in man. This difference is all the more instructive, because the malady in Rodents is as capable of being initiated by the introduction of irritants into the peritoneum as by their introduction beneath the skin. The direct influence of irritants upon parts in which lymphoid tissues abound, almost always suffices to induce a rapid hyperplasia of these tissues in guinea-pigs; and whether the part first implicated be subcutaneous tissue or peritoneum, this local overgrowth is succeeded by a gradual and more or less complete generalisation of similar overgrowths. In man, however, we meet with the most striking differences. There are the best of reasons for believing that no such extraordinary excitability of the subcutaneous tissues exists in him, and that lymphoid overgrowths are by no means so easily determined beneath his serous membranes. Moreover, when they are determined to a very marked extent (as in Granular Peritonitis), we see an almost complete absence of that tendency to generalisation, the exhibition of which makes the Rodent animals so remarkable and peculiar. In the one organ through which the blood immediately returns from the enormous mass of diseased tissues existing in the Granular Peritonitis of man, we do find what appear to be secondary effects induced. Even here, however, we have not a special overgrowth of lymphatic tissues in the liver similar to what occurs in the Rodentia, but a much more ordinary result of excessive irritation—we have an acute cirrhosis, in fact, in which the lymphatic tissues are altogether thrown into the shade by the enormous overgrowth of the connective-tissue elements amidst which they are interspersed. These facts, therefore, tend strongly to confirm the view which I have previously mentioned, viz., that no affection exists in man answering to that of the Rodents, because there is an almost complete absence in him of that extraordinary excitability of the lymphoid tissues which they display—an excitability showing itself in them by an extreme proneness of these tissues to overgrow under direct irritation, and by the manifestation of a like aptitude on the part of similar tissues in remote organs under the influence of obscure secondary irritant agencies, the precise nature of which is wholly unknown.

4. Next we have two allied affections as distinct from Pulmonary Phthisis as those to which I have already alluded, although they also are characterised by the occurrence of disseminated new growths having a distinctly lymphoid character. These are Leucocythæmia, and the affection named Adénie by Trousseau, in which multiple lymphomatous infiltrations and tumours are met with in various parts and organs of the body.

5. Lastly, taking the great majority of cases of Pulmonary Phthisis, we find in them the ever varying representatives of an anatomically protean malady—but of a malady which has nothing more "specific" about it than belongs to Chronic Bright's Disease in the kidney, or to any ordinary disease of another organ. The anatomical peculiarities of the lung are such as eminently favour the more or less simultaneous occurrence of different kinds of lesions under the influence of inflam-

matory or irritative influences. It is the various proportions in which these different kinds of lesions exist in different cases, combined with their different stages of evolution or decay, which accounts for the varying appearances presented by one case of phthisis as compared with another. In the main (and omitting the condition of blood-vessels) these tissue changes, as almost every one admits, belong to three different categories. They are as follows.

a. *Epithelial or more or less Purulent Impactions* within the bronchi and air-cells, occupying areas of very different sizes, and presenting different colours of grey or yellow, according to the amount of fatty degeneration or caseation which they have undergone. Changes of this kind are more or less intimately intermixed with those which follow.

b. *Fibroid Overgrowths* resulting from a hyperplasia of connective tissue elements in different parts of the portion of lungs affected; these being characterised by tissues presenting every grade of structure or variety between mere nuclear overgrowths of an embryonal character and the densest fibro-cartilage.

c. *Lymphoid Overgrowths*, more or less inextricably interblended with tissue-changes of the kind last mentioned, due to an irritative hyperplasia of the normal lymphoid patches which are to be met with around the bronchial tubes and their minute ramifications.

The changes described under b and c together constitute the well known *indurating infiltrations*. The two kinds of changes occur in intimate union; and, moreover, the more characteristic lymphoid hyperplasias shade away insensibly into the ordinary embryonal or nuclear overgrowth of the connective tissue elements. It is no more surprising or peculiar that the one should occur than the other; they are both mere irritative overgrowths of pre-existing tissues. Causes which stimulate the growth of one, should suffice to stimulate the growth of the other kind of tissue. The amount of actually developed fibrous tissue in this intermixture of b and c increases with age or in proportion to the slowness of its evolution. As Dr. Fox has forcibly pointed out, the tendency to undergo caseation or cheesy metamorphosis is by no means confined to the epithelial impactions; it extends also to these indurating infiltrations, and is generally well marked in direct proportion to the rapidity of their evolution, and the abundance of their corpuscular elements.

Such indurating infiltrations commence, for the most part, by the production of aggregations of minute granulations, into which epithelial overgrowths enter more or less largely; then comes a subsequent fusion of such granulations, together with a rapid overgrowth of connective tissue and lymphatic elements in and around the patch. Contiguous new centres of morbid growth arise; and larger and larger patches, becoming fused by mutual growth, subsequently undergo the most varied changes. All this, however, has been very accurately described by Dr. Fox.

The more acute the case of phthisis, the more apt are we to meet with a predominance of mere epithelial or purulent impactions; whilst the more chronic the cases, the more abundantly do we find the indurating infiltrations and granulations above referred to. It is, however, as Dr. Fox has remarked, almost impossible not to get some amount of the other change where either of them exists to any well marked extent. Taking into account the ulcerations, pigmentations, and other changes which these morbid tissues and products are apt to undergo, all the anatomical characters of pulmonary phthisis are explicable enough, without the necessity of our ever having recourse to a word the very mention of which suffices to summon to the mind a confusing cloud of unproved assumptions and conflicting theories.*

And if the lesions themselves of Pulmonary Phthisis can be fully explained without having resort to or occasion to use the word "tubercle", this is also certainly true concerning the general constitutional condition associated with the malady, and, concerning anything that we may know or say as to its hereditary nature. Those who have occasion to use the phrase "Tubercular Diathesis," will find themselves none the less wise if, in future, they speak or think of a Phthisical Diathesis. And, surely, nobody in these days, when so much more is known than was patent to our predecessors concerning heredity, will think it necessary to keep up old notions concerning the "specific" nature of a very ordinary disease, simply because there is evidence to show that a tendency to such a disease is frequently transmitted from parent to offspring. A man may inherit from his ancestors a well- or an ill-developed brain, and similarly he may inherit a well- or an ill-developed lung. If he have an ill-developed brain—a brain, I mean, so constituted that its tissues are more than usually prone to pass over into this or that form of morbid change, either spontaneously or under the influence of the very slightest determining causes, we should say that he inherited a

* In his work entitled "De la Granulie ou Maladie Granuleuse connue sous les Noms de Fièvre Cérébrale, de Méningite Granuleuse, d'Hydrocéphalie Aigue, de Phthisie Galopante, de Tuberculisation Aigue," etc. Paris: 1865.

* The so-called "Tubercular" Ulcerations of the Intestine, which are so apt to occur in the course of various chronic lung-diseases, may be equally well understood by us if we simply call them Granular Ulcerations of the Intestine.

predisposition to brain-disease, though the particular form which might appear would be altogether uncertain. Nobody doubts, however, that quasi-pathological accidents may determine in another individual, who inherits no such predisposition, similar forms of disease. The case is precisely the same with regard to lung-diseases. A man may inherit from his ancestors lungs which contain within themselves the elements of weakness—organs the tissues of which are so constituted, with relation to the whole organism, that the very slightest determining causes suffice to initiate a set of changes which terminate in one or other of the forms of pulmonary phthisis. And, similarly, just as brain-disease may be acquired in the life of an individual who inherits no predisposition, so, under the stronger pressure of general and local causes, may any of the forms of phthisis manifest themselves in individuals who inherit no family predisposition to such a disease.

Where is the difficulty? What occasion have we to resort to the use of the word "tubercle"? How does so much depend upon the use we make of it? I must confess myself unable to understand what Dr. Wilson Fox means when he says: "The etiology of phthisis, the therapeutics of phthisis, and the prognosis of phthisis, all hang upon this point—how far tubercle is concerned in the morbid anatomy of phthisis." To me it seems quite the reverse. I regard this question as one of a mere verbal nature, and as capable of being separated entirely from all problems as to the etiology, therapeutics, and prognosis of phthisis.

Were it not for the very important nature of these questions under discussion—were it not for the fact that professional opinion in this country might be led into grooves which would subsequently, as I conceive, introduce a lamentable confusion into the science of medicine and pathology—I should not have ventured to speak at such length. What I have said, however, may be taken as the expression of views which I have arrived at slowly and deliberately after the most earnest consideration of all the facts. I would say then, emphatically, if we accept the new signification which has of late been proposed for the word "tubercle", we involve ourselves in an endless series of disputes and differences of opinion as to the real nature and limits of such a growth as compared with many Chronic Inflammations, with Syphilitic Indurations, Scrofulous products, Leucocythæmic growths, the Lymphomata of Adénie, and other morbid products. Whilst, on the other hand, if we throw away this indefinite and almost meaningless word "tubercle", we shall at the same time get rid of an entangled brushwood of conflicting opinions, and of a series of pitfalls which simply hinder our progress and prevent that almost complete unanimity concerning the mere facts themselves which would otherwise prevail.

ON A PECULIAR KIND OF HARE-LIP; AND ITS TREATMENT.

By KELBURNE KING, M.D., F.R.C.S.,
Surgeon to the Infirmary, Hull.

HAVING had several cases of hare-lip similar to those which are figured in the accompanying photographs, I have thought it worth while to notice briefly the very remarkable nature of this deformity, and the method which I have used for its remedy.

The first of these (No. 1) occurred in the autumn of 1864; No. 2

Fig. 1.

and No. 3 were children of the same parents, who had a third child with the same deformity; and No. 4 represents No. 3 after being operated on.

The deformity includes the bones and the soft parts which enter into the composition of the upper jaw, the hard and soft palates, the upper lip, and the lower part of the nose. A chasm, an inch in width, separates the right from the left segment of the lip, and through this aperture there protrudes a portion of bone, consisting of what ought to have been the central part of each superior maxillary. This is adherent to and projects beyond the tip of the nose, and is covered by a portion of soft parts, consisting of integument, mucous membrane, and prolabium, corresponding to the central deficient part of the upper lip. The projecting bone contains the two incisor teeth on each side, and corresponds to the two premaxillary bones, separated from the maxillary, thrust forward to the tip of the nose, and carrying before it the central

Fig. 2.

portion of the lip. A glance at the photographs will, I think, make clear to any who may not happen to have seen a similar case the nature of the deformity. There is also in Erichsen's *Science and Art of Surgery* a very good print of it.

Like many other deformities, this is no doubt primarily a case of arrested development. The German poet Göthe is said to have been led to the discovery of the presence of a premaxillary bone in man by his belief in ideal archetypes (*Nature*, June 2nd, 1870); and, if so, Professor Tyndall might have adduced this as another instance of the scientific use of the imagination. In cases like the present, and indeed in all cases of hare-lip, the deformity consists in a continuance beyond the proper time of the separation between the premaxillary and maxillary bones. But in most cases of hare-lip this junction, though late, takes place before birth, and the separation of the lip on one or both sides, constituting the ordinary hare-lip, is all that is left to show the original arrest of development. Very frequently the intermaxillary becomes attached to one, usually the right maxillary bone, forming a projection which is separated by a greater or less space from the left, and accompanied by more or less cleavage through the hard and soft palates. This is the class of cases in which the projection may be thrust back, and the

Fig. 3.

injury repaired, without loss of bone or teeth. But in such cases as those of which I am speaking the projection is too great, and the separation between the maxillary bones too wide, for this proceeding to be successful. In the photograph pictured (No. 3) the amount of separation was rather more than an inch.

In considering what ought to be done, it seemed to me necessary, first of all, to remove the projecting bone, and then to unite the right