℞. Liq. morph. acet, ʒi.
Acid hydrocyan (Scheele), m. iv.
Acid nit. dil., m. xx
Spt. chloroformi, m. xx.
Syr. tolutani, ʒiv.
Aquæ ad., ʒi.
M. Sumat., ʒi., p. r. n.

It is the "Linctus morphiæ" of the Pharmacopœia of this Infirmary.

It now becomes my pleasing duty to express my thanks to Drs. Arlidge and Orton, and to Mr. Ashwell, for the full investigation of the cases under their care, which they have freely permitted me to make.

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ART. IX.—*On the Views of Niemeyer regarding Phthisis.*[a] By HENRY KENNEDY, A.B., M.B.; one of the Physicians of the Cork-street Hospital, and attached to Sir Patrick Dun's.

THE following remarks have been put together, I must admit, somewhat hurriedly, but I was anxious to elicit a discussion on a point which appears to me one of very great practical importance; and I knew no place more suited for my purpose than the meeting I have now the honour of addressing. It will be known to all present that recently a German work on medicine, by Niemeyer, has appeared; and has already been published in an English garb; so that it has obtained a considerable amount of notice from the profession, and this has been increased by the separate publication of one important portion of the work; viz., *The Clinical Lectures on Pulmonary Consumption.*[b] It is to these lectures I would ask attention; for they appear to me to enunciate views, which, to say the least of them, are open to discussion. It may be observed here that the main point of the author's views consists in the idea that inflammatory action is the basis of all phthisical disease. He does not, however, claim the idea as original; but mentions specially the names of Virchow and Buhl, as holding and teaching similar doctrines; whilst we know that in England several physicians, and more particularly the late Dr. Addison, promulgated like views, and

[a] Read before the Medical Association of the College of Physicians, January 18th, 1871.

[b] Clinical Lectures on Pulmonary Consumption; translated by C. Bæumler, M.D.

published them several years back.[a] It may not be out of place here to quote a few of the expressions found in these lectures, and which embody the views of our author. To many, I suspect, they will seem as strange as they do to myself. The following are examples of what I mean:—"In the present stage of science there is but one kind of tubercle, miliary tubercle; and but one form of tuberculosis, and all those changes, which, since Laënnec, have been designated 'infiltrated pulmonary tubercle,' are the product of chronic, especially of catarrhal pneumonia." Again, "In very many cases there is not a single tubercle found in phthisical lungs." Again, "The tubercles, in the majority of cases, are clearly of recent origin, and have complicated the pulmonary phthisis, when it was already in an advanced stage." And, once more, "He speaks of patients who, after having suffered from phthisis for years, had, at last, become affected with tubercles. He also uses the expression, that the greatest danger to most phthisical patients is the development of tubercles."

From these passages it will be observed that the author, assuming that inflammatory action is the basis of phthisical disease, and declares itself in the form of pneumonia, goes on to state that the deposition of tubercle is quite secondary, and that the views held and taught by Laënnec, must be abandoned. Before going further I must here notice that the title of this, the first lecture, is "The Pathology of Phthisis." Now to these words, and the way they are here applied, I must strongly object; for the author all through argues as if the morbid anatomy were the real pathology of phthisical disease; and nowhere does he, as far as I am aware, allude to that general state of the constitution, which precedes, and, to my mind, must precede the development of the disease. This arises from confounding morbid anatomy with pathology; than which there cannot, to my mind, be a greater mistake; and it seems to pervade the entire views of the author. That a something precedes the pneumonia, to which the author gives such a prominence, in the formation of tubercles, is, I think, absolutely certain. Yet, I repeat, he nowhere expresses himself to this effect; but expressly

[a] Since writing what is stated above, I have read again Dr. Addison's paper; and would now observe that there appears to me a very marked distinction between his views and those of our author; the latter makes inflammatory action, in the shape of pneumonia, the basis of all tuberculous disease; that is as preceding it; while the former sums up by saying, "Inflammation constitutes the great instrument of destruction in every form of phthisis;" a statement bearing a totally different meaning, and from which, I rather think, very few would dissent.

states that pneumonia is, in the vast majority of instances, the starting point of phthisical disease; and, at page 13, he asserts that no one form of pneumonia gives rise to the affection; but that all do so, though not equally. And this leads on to the main question; are these views correct? Is inflammation the starting point of phthisical disease, or of phthisis? Or is pneumonia? My answer to these questions must be in the negative; and I cannot help considering the views advanced by the author to be too confined, and not taking that wide grasp of the subject which it appears to me to require. In truth it is but a theory, as it were, which, I believe, does not square with well ascertained facts. In this respect, and with my present knowledge, I would still hold to the views so long since advanced by Laënnec; and I only wish that either the subject had fallen into abler hands to treat, or that more time could have been devoted to the question by myself.

Here it must be observed that the line of argument I would pursue leads me to state some facts which are patent to all. Thus it will not be denied that the locality where tubercles are, in by far the majority of instances, found, is the upper lobe of the lung; or I might say the upper part of the lobe. And nothing is more common than to find a few scattered tubercles here, of which no sign whatever had existed during life. I call them scattered, for there is no kind of union amongst them, and certainly nothing of induration around them. I repeat that such a state of parts is, in my experience, very common; and often exists without any trace of scrofulous disease in any other part of the frame. In fact the patients die of other and totally different diseases. In keeping too with this statement I appeal to the experience of all present as to the earliest physical signs of tubercular phthisis. It would be out of place here, even were there time, to enter into these. But I would ask do they afford any evidence of an existing pneumonia. Is there any crepitus at this stage, or expectoration? On the contrary, is it not known that the cough is dry, and that the presence of crepitus only makes itself known after months, it may be years, during which the presence of other physical signs existed. Even when it occurs, and I admit it may occur, that expectoration is an early symptom of phthisis, its character is not that of pneumonia, as I think all will agree with me. Neither is the crepitus of early phthisis at all like the crepitus of pneumonia; the one is a single glug, and of large size; the other very fine, and the sounds heard numerous.

But if the progress of tubercle be traced further than this mere deposit in the lung, do we find any more confirmation of the views of our author? I believe not. We do, indeed, find that in certain parts of the lobe they become more numerous, in fact, close to each other, with a kind of gelatinous deposit joining the one to the other, but nothing at all of what could be called pneumonic solidification, and any softening process and formation of cavities occurs subsequently to all this again, and, in the first instance, takes place where the tubercles have been congregated and massed together.[a] It is in this state, as I believe, that the tendency to attacks of intermittent pneumonia are so apt to occur, and, of course, add to the serious character of the disease. The readers of Andral may recollect the cases he has detailed, which bear so directly on this point; and I am sure there are none present who have not met similar cases. I have said that it is when tubercles are grouped closely together they begin to soften, and so form cavities; for I believe it to be very exceptional when single tubercles suppurate. Still this does occur, and I have seen it, and have even known instances where a very few tubercles existed near the surface of the lung, and where one of these had given rise to pneumo-thorax. Speaking of isolated tubercles reminds me too of the state they so often, I might say constantly, present in children. Here, as you all know, they will pervade the lungs, liver, spleen, kidneys, and brain, and yet nothing but tubercles in their most simple and uncomplicated form will be found.[b] This part of my subject might be pursued much farther, but my limits forbid.

The attention of the meeting must now be called, for a few moments, to the pneumonia which our author makes the foundation of all phthisical disease. And here I must ask, Is there anything in its natural history to justify such an idea? My hearers know perfectly well that, as the upper lobe of the lung is the common seat of tubercle, so it is in the lower that pneumonia manifests itself. Here, then, as it appears to me, is a most material point in the question, and one which it seems to me impossible to reconcile with his theory. But further, is the progress of this disease—now so well ascertained—capable of, in

[a] It is, I believe, too generally admitted that the softening process takes place from the centre of the mass, not around it.

[b] In children that die of hydrocephalus, the state of parts described above is very common. The glands too, particularly of the mesentry, will be often found enlarged, but rarely suppurated.

any way, supporting the author's views? I think not. For is it not known that pneumonia is, in the great majority of instances, a disease amenable to treatment, and that it is very exceptional when it becomes chronic, and still more so when it leads, or seems to lead, to the deposit of tubercles? And here I cannot but observe that the author appears to me to make a grave mistake in classing together all the kinds of pneumonia as giving rise to tubercles. He says, indeed, there may be some difference in degree; but this is all he allows on the point. Surely I am not wrong in saying that the experience of all the physicians of this city is diametrically opposed to such statements. For myself, I believe that it is only one form of pneumonia—that known as the strumous—which leads on to the deposit of tubercles; and, on the contrary, that the vast majority of cases lead to no such end.

But it will be asked here, do we not meet with the state of morbid parts so much dwelt on by the author? And how are they to be accounted for? In answer to these questions, I would, in the first place, observe that the author seems to me to take quite too narrow a view of the subject. Thus he speaks of tubercles as if they were a something totally different from the other morbid changes with which they are often found to co-exist. I take it that this is a very erroneous way to look at the question. For to my mind it matters not whether the tubercles be grey, or yellow, or accompanied with cheesy deposits, or indurations. They are all the result of a state of the constitution known as the strumous; nor does the presence of a crop of fresh tubercles—on which the author places so much weight—alter this view. I see no grounds for thinking why strumous disease should be different from other diseases; and as they present certain differences in themselves, and yet are the same diseases, so is it with strumous disease. I think too it is an accepted fact, as taught long since by Laënnec, that strumous disease, whether in the form of tubercles or otherwise, developes itself only at certain periods. Hence, a simple explanation of what is so often found—I mean a crop of recent tubercles supervening on much older standing disease. And this shows itself at times in a very striking way. For it is not by any means uncommon, after strumous disease has declared itself in the chest, for the symptoms to subside somewhat suddenly, but only to be followed by more serious disease—if such were possible—in the abdomen, or more particularly in the brain. A very recent example of this latter occurrence I saw very lately with my

friend, Dr. R. Kirkpatrick; and had I not seen similar instances before, I would surely have been puzzled as to its nature.

I have already stated my conviction that the views of the author are too limited; and, as I read the lectures, it seems to me more and more of this is to be discussed. For what will any one say to the following, which I quote from page 27 of these lectures? —"I consider the almost universal opinion, that pulmonary consumption arises independently of accidental or immediate exciting causes, in consequence alone of a diathesis, to be as unproved as it is dangerous." In other words, there is no such thing as a "diathesis" or pre-disposing cause for the occurrence of phthisis. If this be true, others with myself have been grievously mistaken; but, as I can only speak for myself, I hold that pre-disposition is all in all in the production of phthisis; and further still, that the marks of such a constitution are perfectly well known, and, in these countries at least, long recognized. This decided opinion too, I would just observe, does not in any way clash with the possibility of phthisis declaring itself in an individual otherwise healthy, but who has been long exposed to the causes which are known to lead to the disease.

In keeping with what has been just stated, about there being no strumous diathesis, are the remarks of the author on the occurrence of hæmoptysis, on which he dwells at considerable length, and states that proper views have not been held on this point. For he holds that when it happens the blood acts at once as a foreign body, which causes inflammation, and this again leads on to the deposition of tubercle. The author must have some local cause to support his theory, and so the blood is turned to account. Need I say to this meeting how he has overlooked the state which has preceded the bleeding; for a healthy man will not so bleed, and, as was said before, there must be the pre-disposition.[a]

In the last place, my hearers will not be surprised to learn that the author denies the hereditary nature of phthisis—as I take it one of the best established facts which statistics have ever given us. But this would not suit our author or his theory; and so he has ignored it. It is not easy, however, to understand any one carrying his views farther than this. It reminds me of the Belfast

[a] Bleeding into the lung, as all know, is not, by any means, necessarily followed by tubercular deposit; and when it does occur, in connexion with phthisis, it is much more frequently an advanced than an incipient symptom; at least this has been my own experience. In other words, it supervenes on disease, long previously existing.

physician who, to support his theory, has denied all value to cod-liver oil as an agent in the treatment of phthisis.

Other points than those noticed might easily be found in these lectures which would call for discussion. Thus at page 6 is the following passage:—"The conditions under which the product of a common pneumonia undergoes cheesy degeneration are not known to us; but we must particularly mention the fact that this termination is not only observed in those persons who have tubercles, old cheesy deposits, or cavities in the lungs, but at least as frequently in previously healthy individuals, and especially in those affected with long-standing emphysema of the lungs." There is more than one point which might be noticed in this passage; but I have merely given it because the author speaks of the union of "long-standing emphysema" with strumous disease of the lungs. From my own experience, I must directly question this statement; for I believe the occurrence of emphysema to be quite antagonistic to the presence of tuberculous disease of any form, and *vice versa;* tuberculous disease is, as far as I have seen, as rarely seen joined with emphysema.

A second point which may be noticed here as bearing on the general question is the fact that tubercles cannot be injected. Now, on the theory started by the author, it seems to me very difficult, if not impossible, to account for this. For if inflammation were the starting point, and tubercles the direct result of this inflammation, why should not the injection pass into the latter? Yet it does not do so, as any one may satisfy themselves by looking at some of the specimens of diseased tuberculous lungs to be seen in the Museum of the College of Surgeons. Some of these specimens are very remarkable; for while the whole lung is studded with tubercles presenting their ordinary whitish appearance, all the rest of the organ has freely received and been deeply coloured by the injection.

Though I have no intention here of saying one word about the treatment of phthisis, I cannot help observing that if the author's views were correct, it would truly be a very simple matter. A modification of the antiphlogistic plan, suited to each individual case, would as surely cure it as it does common pneumonia. Nay, it should yield much easier; for the amount of lung involved would, speaking generally, be very much less than when common pneumonia existed. Yet is this the fact, or has the experience of any one present confirmed it? On the contrary, is it not certain that

while pneumonia is, in a very large per centage, amenable to treatment, tubercular disease is the very opposite, and it may even be doubted whether we have any direct control over it at all. Certain it is that it is obstinate to a degree, and I fearlessly assert that no stronger contrast could be afforded between any two diseases, than between common pneumonia and ordinary tuberculous disease. But I cannot pursue this point further here. Before concluding, I would wish to observe that I do not, for one moment, question the several statements advanced by the author in these lectures; and more particularly what he has advanced about the morbid anatomy of scrofulous disease of the lungs (not, let it be observed again, the pathology). I have seen all the states of the lung he describes; but I do directly question the line of argument he takes, and the mode of reasoning he applies to his facts, for I believe the interpretation he puts on them to be erroneous, and impossible to reconcile with well ascertained facts ; and here I cannot help observing, and I think those who have read these lectures will agree with me, that they strongly convey the idea that his cases are few and far between, selected from this and that source, while not more than four or five are given altogether. Now I may be mistaken, but I cannot help thinking that this is not the way a great question of the kind should be treated. It is surely not the exceptional cases which are to determine the point, and above all, in such a common disease as tubercular phthisis is known to be. We are to be guided by the hundreds not by the units. Yet it seems to me our author has used exceptional cases only for his argument; and when I recall the facts advanced by myself this evening; when I think of the absence of physical signs, which ought to be present when they are not; when I recollect the different localities which the two diseases he has brought into such close connexion—union I may say—occupy; when I contrast the difference of duration and treatment in the two diseases; when I find that he denies there is such a state as the strumous diathesis, and ignores hereditary transmission as tending to lead to phthisical disease; when, I say, I consider these several points, I can, on my part, arrive at no other conclusion than that the views advanced by Niemeyer cannot be sustained, and that these lectures are not worthy the name of the author.