A Lecture

ON

MODERN METHODS IN THE
DIAGNOSIS OF PHTHISIS.*

BY

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I HAVE selected the subject of modern methods in the diagnosis of phthisis with two very definite ideas in my mind. First, I find that some of them are credited with a value to which they have no claim; or their value is misunderstood or exaggerated, greatly to the detriment of older and more reliable methods of diagnosis. Secondly, I find that others are subjected to a neglect they do not deserve, with the result that valuable evidence in cases of difficulty is left out of consideration. In the diagnosis of phthisis at a really early stage we cannot afford to leave any witness unquestioned. Every piece of evidence has its due importance, but above all is it vital that we should appreciate as nearly as may be what weight to attach to each kind of evidence, and that we should not be deceived by the glamour of novelty, nor, on the other hand, put off by the difficulty of mastering a new technique.

The first of the modern methods I am going to bring to your notice first saw the light in the year 1863! It is true that it had not reached adult proportions till some quarter of a century later—namely, 1889—but my claim to regard it as a modern method depends on my belief, possibly unfounded, that not a dozen of my audience have yet made use of it in any regular manner. I refer to topographical percussion, a method, I may say at once, of great help in the diagnosis of early phthisis, and also of commencing disease at the apex of the second lung where the first is already involved.

Topographical Percussion.

In its best known form topographical percussion is associated with the name of Krönig, and though attempts were made to map out an anterior and posterior apical resonance before his time, the credit of fully describing the boundaries of the apical resonance certainly belongs to him. He was the first to join the anterior and posterior areas of resonance by a narrower "isthmus" over the shoulder. The method of finding Krönig's area consists in mapping out, by the highest possible percussion, the band of resonance passing from the front to the back of the chest over the shoulder on each side. Percussion may proceed either from dullness to resonance, or vice versa; the end of the finger only must be used, the area marked with a skin pencil and carefully verified after it is marked. Having marked out both shoulders, the resulting figures are compared and measurements taken at the isthmus, and perhaps elsewhere. Normally the two sides correspond, save in cases of chest deformity. Lack of correspondence points to a one-sided lesion, which may or may not be phthisis.

Now the changes we find in early phthisis are either a blurring of the outline of Krönig's area on one or other side, leading later to the appearance of a band of relative impairment, or a retraction of the apical resonance as a whole. It will be remembered that Birch-Hirschfeld, Schmorl, and others, found the earliest lesions in phthisis in the medium-sized bronchi running to the apex; disease leads to blocking and collapse of lung, and infiltration of the collapsed area follows. In some cases, no doubt, infiltration keeps ahead of collapse, and in a few apical infiltration is itself the primary lesion. Topographical percussion seems to bear out this view of the common march of events in apical disease. After the size and shape of the two apical areas have been compared, the next point is a careful comparison of their resonance to light percussion, and this is conveniently done in finger-breadths above the clavicle.

Now Krönig's area is easy to map out and the information we get from it is often valuable and decisive, but it is not, as Goldscheider objected, the actual lung apex we are mapping out, but only an artificial area of resonance projected to the surface from points mostly on the outer side of the apex. Goldscheider set himself to percuss the lung apex itself in front and behind, and by this means we have another method of topographical percussion. The apex in front lies between the heads of the sterno-mastoid muscle, but as it rises little more than a finger-breadth above the clavicle—Stuart Dickey made it 2.2 cm., or nearly an inch—and as the inner end of the clavicle presents no good point for taking a measurement, I do not recommend you to do more than note the presence of resonance in this area, and perhaps compare it with that on the opposite side. I say "perhaps" because the ability to do so correctly lies partly in the arrangement of the lower heads of the sterno-mastoid muscle. The space between them, and the prominence of one or other head varies greatly in individuals, and it is only when percussion can be made directly backwards, to avoid tracheal resonance, that the lung apex can here be reached.

Behind, the matter is different; the apex lies close to the spine, but it is thickly covered by muscle and protected by the first rib and lateral process of the vertebra. Nevertheless, it is possible to determine the height of the actual apex at this point, and this should be done, the normal resonance rising here to the spine of the first dorsal vertebra.

So much for topographical percussion—Krönig's method, easy to apply and accurate, giving, indeed, a magnified view of the lung apex; Goldscheider's, difficult in practice, of little value in front, but of distinct worth behind. What do we learn from topographical percussion? Not the presence of phthisis any more than this can be deduced from ordinary percussion used alone; but the presence of:

1. Equality on the two sides both as to size and resonance, evidence of health.
2. Inequality of size: shows retraction of an apex, over-expansion of the opposite apex, or both together.
3. Equality of size, but blurring of outline passing on to production of bands of relative impairment: generally points to infiltration with little or no retraction in early phthisis.

These points give us valuable information for further investigation. We have interviewed a useful witness; we record his evidence and proceed.

Tidal Percussion.

Before leaving topographical percussion, however, I may well add a word about tidal percussion, a method designed to discover the movements of the lungs, whether at apex or base, during deep respiration. At the apex I have found the method of but slight value, for the movement here is slight, and may be none at all in health. At the base useful information as to pleural adhesions is obtainable, and the presence or not of such adhesions may be of value in a diagnosis between apical collapse accompanying nasal obstruction, and somewhat similar signs of tuberculous origin. Tidal percussion is closely comparable to Williams's diaphragmatic and Litten's shadow-phenomena, and the results subject to similar interpretation.

Radiography.

The next point I should like to bring to your notice is the value of the x rays in the diagnosis of phthisis. Not only do they give us help in cases of difficulty, but they have to some extent altered our views with regard to the disease. For a long time the teachings of von Behring, Paul Römer, and others, that phthisis was only the final stage of a childhood infection, and that this smouldering disease and the immunity it caused were the conditions which made a chronic pulmonary tuberculosis possible, were hardly tolerated by most of us. Now the skiagram seems to bear out the correctness of this view. For we find thereby not only that old disease of bronchial and pulmonary glands is common or even the rule, but also that in cases of phthisis such disease appears to spread round the bronchial branches into the lung, especially towards the apex, and that fresh outbreak tends to occur

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in the neighbourhood of these strands. The implication of apical bronchi so commonly observed, and especially of the posterior branch, as stereoscopy has demonstrated, falls into line with the teachings of Birch-Hirschfeld already quoted, and this no doubt is quickly followed by conditions of partial collapse which allow recognition of the disease at an early stage on the surface.

Before giving any opinion on skiagrams in cases of chest disease, and their probable significance, I would warn you to study carefully the picture presented by the normal chest. You must note the "hilus shadow" caused by the large vessels and perhaps the bronchi, and deepened where these cross one another on the ribs or are viewed tangentially. Calcareous or fibrosed glands must be looked for, but their presence need not be viewed askance. Arborization of vessels and tubes may be seen even to the periphery of the lung, and especially visible are branches running to the apex and also those to the base. Shadowing of the apex and outer boundaries of the lungs caused by thick muscle layers and the inner border of the scapula are to be expected, and these may be thicker on the right side. Fat obstructs the rays, as do also the breasts in women, and the diaphragm is normally somewhat higher on the right side. Not only must all these things be allowed for, but also temporary differences between expansion of the apices of the two sides and also inequalities due to differences of technique, such as position of plate, duration of exposure, hardness of tube, and subsequent development of the photograph.

What we generally see in an early case of phthisis may be grouped as follows:

1. Old disease at the hilum of the lung in most cases; Ziegler and Krause, Stürtz and others declare they have rarely or never seen apical disease without hilum disease of the same side.

2. Attention must be directed to an imaginary triangle, bounded by the clavicle above, the mediastinum within, and, below, a line joining the hilum to the shoulder, for in this space the earliest signs of activity are usually observed. As a rule, two shadows of vessels and tubes will be clearly seen running to the apex, and it is around these that changes are most early seen. In some cases these represent peribronchial tuberculosis, but in others it seems to me quite possible that the shadows are only those of dilated vessels directed to an apical inflammation. Where the disease tends to fibrosis, thickened strands form the main feature; where an acuter process is present irregular flecks of bronchopneumonia in the lung tissue, growing and coalescing, may be seen.

It happens in some cases that disease of unexpected amount is found by the x rays where surface signs were not discovered. It must not, therefore, be supposed that radiography can in any way replace ordinary examination. Where the earliest changes consist in an exudative process round a few scattered tubercles, the stethoscope has ample warning, while the skiagram shows nothing. Moreover, disease of ascending bronchi, though it appears to the radiographer to be far from the surface, yet soon causes slight impairment and stethoscopic signs from interference with blood and air supply. You cannot long interfere with the lines of communication without its being apparent at the distant front! In addition, the x rays do not, as a rule, tell you much about the actual apex of the lung, and they only give partial information as to activity of the process visualized. They should be used in conjunction with other methods, and are then of the highest utility. They may confirm the diagnosis in a doubtful case, give great assistance in the determination of the extent of disease, help to show whether the other side is free—for the spread on the second side invariably starts deep in the lung and may be appreciated earlier in a skiagram than by auscultation and percussion.

Tuberculin.

We now come to tuberculin diagnosis, a matter round which no little controversy has raged. The facts about the value of tuberculin are nevertheless pretty clear, and their interpretation should be open to but little doubt. Two points need to be clearly separated in the use of tuberculin at the outset:

1. The detection of tuberculous infection.
2. The diagnosis of active disease.

1. *Tuberculous Infection.*—The tuberculin reaction is truly specific, and there is no other comparable test of tuberculous infection. As you know, its most delicate application (intracutaneous and subcutaneous local tests apart) is in the cutaneous or von Pirquet test, and to this nearly all adults react. Its utility is confined to the first few years of life, when infection generally spells active disease, and undiluted tuberculin should be used, since the stronger the solution the more inclusive the test.

2. *Active Disease.*—For the detection of active disease tuberculin has had, so far, only partial success, and that largely because its limitations have not been clearly understood. It would have seemed clear to most that at any rate the ordinary cutaneous test, which discovers tubercle in about 90 per cent. of healthy 14-year-olds and in nearly all adults, would hardly be claimed as a test of clinical disease. Yet it is only some weeks since this claim was put forward by a writer in one of our best known medical periodicals.

The reaction to tuberculin subcutaneously by fever and other disturbance is, as you know, not infrequently relied upon by practitioners in deciding the diagnosis of phthisis in a doubtful case. And yet, on inquiry, what are the facts in regard to tuberculin given subcutaneously in the usual doses? The well-known experiments of Franz on healthy Bosnian soldiers showed that 61 per cent. reacted to doses up to only 3 mg., whereas, after a year's stay in Vienna, 76 per cent. gave positive reaction. Beck, among 2,137 convalescents, found 54 per cent. react to injections of 1, 5, or 10 mg., and Fränkel obtained similar results. Obviously a test for active tubercle which gives such result among the clinically non-tuberculous is utterly worthless. A few days ago I came across a very interesting communication to a discussion by Dr. Hamman, of the Johns Hopkins University. It is so much to the point that I will quote it. He says:

We had innumerable cases of early pulmonary trouble in those days, many more than you could find on our records now. We were further intrenched in our confidence by the confirmatory evidence lent by tuberculin. To give added assurance we administered tuberculin to these patients subcutaneously. Of about 35 patients so tested at Ludowood, all but one reacted. Slight physical signs and the tuberculin reaction played so smoothly into one another's hands that we at last became suspicious and began to give tuberculin indiscriminately to as many patients as we could control, and, to our dismay, found that about 60 per cent. of all individuals react, whether they are suspected of having tuberculosis or not.

We may take it, then, that a positive febrile reaction to tuberculin gives little evidence either way in the diagnosis of phthisis. True, the amount of sensitiveness is of some importance, but time does not permit me to go into this point. On the other hand, a negative result to subcutaneous injection is of very definite value in excluding active disease, and this alone makes the test of considerable value in properly selected cases. Hammer among sanatorium cases got 7.4 per cent. of such negative results. Hamman on similar material found that he got definite evidence from subcutaneous injection of tuberculin in 26 among 75 suspected cases; 12 were negative, and 14 gave a focal reaction.

This brings us to the other element of the subcutaneous test which is of distinct value in the diagnosis of phthisis. The negative febrile and the positive focal reaction are our two stand-bys in tuberculin diagnosis! The focal reaction shows itself in symptoms—cough and increased expectoration, pain or sense of oppression in the chest, and in the appearance or increase of physical signs, whether to percussion or auscultation or both. Its value is not only that it is truly specific and points clearly to a tuberculous process, but also that it definitely indicates the position of disease in the lung. There is also good evidence to believe that a focal reaction gives evidence of activity, or at any rate of disease which is far from obsolete and may well become active. The frequency of appearance of a focal reaction to injected tuberculin varies enormously with the observer; it needs considerable skill to detect its appearance in some cases; moreover, it is probably not difficult to imagine its presence when attention is concentrated on it.

Otten, working under Professor Romberg of Tübingen, records 324 cases, tested with a view to focal reaction. Of these, 60.8 per cent. gave a focal with a general reaction,

7.4 per cent. a focal reaction alone, and 23.8 per cent. a general reaction alone. Walterhöfer obtained a focal reaction in 54.5 per cent., Hamman in 36.5 per cent. Other observers have found it more seldom, A. Fränkel in only 10.8 per cent., showing how largely the personal factor enters into its discovery. No uncertain signs must be accepted as evidence of its presence.

The question which especially assails us with regard to the focal reaction is that of its possible danger. This can only be insured against by the most scrupulous observation of all the rules and contraindications which experience of the method have indicated. Especially important among these are two: First, never to give subcutaneous tuberculin for diagnosis to a febrile case, and this rule is the one most sinned against, with disastrous results. The writer has several times seen disease lighted up and rendered hopeless by tuberculin needlessly given to cases of obvious febrile phthisis. The second rule is never to increase the dose, but only to repeat it, where a doubtful reaction has been obtained and it is decided to test the matter further. If, for instance, no reaction occurs to doses of $\frac{3}{16}$ mg. and 1 mg., and a doubtful one to 5 mg., instead of going on to 10 mg., the 5 mg. should be repeated, or even a smaller dose given, when the sensitiveness produced will be amply sufficient to secure a definite result in the presence of active disease.

Tuberculin should not be given subcutaneously where the diagnosis can be made without—meaning where the diagnosis can be made without by those with special knowledge and skill, for quite obvious disease may be overlooked by those whose skill is not kept up to date. If this rule were followed out, the test would remain, where it should be, in the hands of those of special training, and specially qualified both to discover and to interpret any reactions which may appear. It would be used in but a few selected cases, and the risks would disappear. Bardelier and Roepke have given ten to twelve thousand injections without any evidence of harm, and it has proved harmless in the hands of other experts of similar experience. Take it out of these hands, and it becomes a fallacious and dangerous instrument.

Quantitative Cutaneous Test.

I have already mentioned that the amount of sensitiveness to tuberculin is of some moment, though, unfortunately, it can only tell us of probabilities more or less high. Of recent years a method of measuring sensitiveness has been applied to the skin reaction, and designated the "quantitative cutaneous test." This is most conveniently applied after the method of Ellerman and Erlandsen, where four simultaneous scarifications on the skin are treated with four dilutions of tuberculin rising in geometric progression. As a result of this papules develop whose diameter is said to rise in arithmetic progression from lowest to highest. From the size of these papules and the difference in size between them, there can be calculated the lowest dilution of tuberculin to which a particular patient will react, and the reciprocal of this represents his sensitiveness. Thus, a patient who reacts to 1 per cent. tuberculin has a sensitiveness = 100, one who reacts to one-thousandth tuberculin has sensitiveness = 1,000.

This method has points to recommend it, but it is unfortunately open to certain fallacies. The technique presents much room for error, since differences in the absorption of tuberculin are apt to occur from differences in depth of scarifications, quantity of tuberculin left on the skin, time it is allowed to dry, and so on. Above all it is easy to mistake the "traumatic" reaction occurring after the use of the von Pirquet scarifier for a slight real reaction to the lower dilutions. To avoid this error, I am accustomed myself to make a fifth control scarification beside the lowest one on the arm, and consider this essential to the proper interpretation of the results.

Given the individual skin sensitiveness, we are provided with information whose value is still very difficult to gauge and apt at times to play us false. Skin sensitiveness, like general sensitiveness to subcutaneous tuberculin, is generally higher in active disease than in the presence of an obsolete or healed focus, but the line is difficult to draw, and there will always be exceptions both above and below it. The figure 100 is usually put as the sensitiveness above which active disease is usually found—figures

below this are generally said to indicate its absence. But Hamman and Wolman found 16 per cent. of the non-tuberculous sensitive to 1 per cent. tuberculin; and 44 per cent. of cases of incipient tubercle negative to 1 per cent. tuberculin—that is, with a sensitiveness below 100. These figures fall into line with my own experience of the quantitative cutaneous method, and I do not think it wise to pin much faith to its findings at present. At the same time there is value in the test at the further ends of the scale—a very high sensitiveness on the one hand as evidence of disease, and a very low one on the other in the absence of other explanation, as evidence of its absence. We are, however, working with probabilities only and must view all such evidence in the light of information obtained from other sources.

Laboratory Methods.

First of all comes the sputum examination and the advances that have been made of late years in its technique. The introduction of the antiformin method by Uhlenhuth was the most notable achievement. By this other less resistant organisms and organic material are brought into solution, and the tubercle bacilli collected by sedimentation. Then comes the discovery of Much, that there exists a granular form of tubercle bacillus which does not retain the stain in Ziehl-Neelsen's method, and requires a modification of Gram's staining to demonstrate its presence. Roepke found 12 per cent. failures to the acid-fast method, with positive results in all these to Much's method, and he thinks both must be used where certainty is required. Beyer had 4 cases among 78 where Ziehl-Neelsen failed and Much succeeded. A further novelty in sputum examination concerns the search for the tubercle bacillus by animal experiment in cases where sputum examination proves negative. Not only can antiformin be used before injection to render the sputum less septic to the guinea-pig, but also it is of value to dissolve the tissue pulp in searching for the bacillus in the inguinal glands. Further, Bloch, by bruising the glands before injection, has succeeded in so lowering their resistance that bacilli can be demonstrated in them as early as fourteen days after injection in a positive case.

Another method, which may become more useful than was expected, is the examination of the sputum for *albumin*. By this means it is determined, not whether the disease is phthisis or some other lung disease, but whether the sputum is only bronchitic or reveals a pulmonary origin. This may give valuable information in a differential diagnosis between phthisis and an apical bronchial catarrh of simple causation. Further, a cytological count of the sputum may be of assistance, since it has been shown by Wolff-Eisner that a large preponderance of mononuclear cells (called by him lymphocytes, but alveolar cells by French observers) occurs in early phthisis, and may focus suspicion on a case before the tubercle bacillus has been discovered. Where this fails is in the fact that in most really early cases no true lung sputum exists, but not all cases requiring diagnosis are at so early a stage as this.

Some Newer Methods.

The examination of the blood for tubercle bacilli has hardly yet reached the stage of practical utility, though it may do so in the near future. Complement deviation or Wassermann's reaction appears to be of little use in diagnosis, as positive results are obtained in cases of old healed as well as active disease. Much more may be said for *tuberculo-opsone* determination if the indices are done by a bacteriologist of known skill.

A series of indices taken under different conditions are needed for the diagnosis of phthisis, or other form of autotoxic tuberculosis, the best plan being to induce a definite autoinoculation and take the blood at certain times before and after. Walking exercise may sufficiently autoinoculate some patients, but I have found it fail to give sufficiently marked results, and more violent exertion may be required. Most certain is the use of a suction mask, for which purpose an ordinary ether mask may serve a temporary need; with this a few minutes of forced respiration will give the required result. The index should be taken before this autoinoculation with the patient at rest, about an hour after the mask, and again six hours, and preferably also twelve hours later. If the

case is tuberculous, a fluctuation of the index follows, and, according to the amount of the autoinoculation, the negative phase comes late or early (or is not found at all), and is, finally, followed by a positive phase. The number of examinations and their spacing are timed to hit off, if possible, both negative and positive phase, so that the double evidence of disease may be obtained. The evidence from opsonic examination must be viewed in just proportion with evidence derived from other sources. Not too much reliance should be placed on fluctuations of slight extent, for experimental error is large, probably 20 per cent., even in the most competent hands.

And now I will end my lecture with a few words of advice on general lines. To begin with, I would remind you that phthisis is a disease of intermittent course, that the real beginning is often passed over as an influenza or a "chill," and that it has generally advanced to quite diagnosable stages by the time the patient has discovered his symptoms. This is true, as a rule, even in cases of haemoptoic onset; even here signs are generally discoverable to skilled examination.

Secondly, I would advise you to devote all the attention you can to ordinary physical signs, especially to light percussion, and after this to auscultation. The modern methods cannot in any way take the place of physical examination, though they are most valuable aids to it. Do not, also, neglect the temperature chart, nor yet the evidence of symptoms.

Thirdly, study carefully the value of the evidence to be derived from modern methods; do not be put off by their apparent difficulty, nor misled by the glamour of their newness. Blind reliance should not be placed on any of them, any more than it should be on any single physical sign. I think we have reached a stage in the diagnosis of phthisis where a warning on this point is greatly needed: we should all like to diagnose 100 per cent. of the cases, but I fear some people are diagnosing more than this at the present time. It gets too like the old age pension in Ireland, which was said to go to fully 120 per cent. of the people over 70 years of age! Most of these modern methods give us valuable information, but this must be weighed against other information from other sources before judgement is given. If this is done, and the other methods, under modern improvements, are not put aside, it may be said that we have made considerable strides in the diagnosis of phthisis in recent years. It is, indeed, only here and there that a case is met where skilled examination is not able to decide the matter, and that fairly rapidly, one way or the other.

THE NOMENCLATURE OF TUBERCULIN DOSES.

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WHATEVER value we may individually attach to tuberculin in the treatment of its specific disease, there can be little doubt that it has come to stay, and however restricted its use may become in future, it is scarcely thinkable that it should again vanish from our armamentarium. Hitherto in its recent renaissance its use has been in the hands of two classes of practitioners. Firstly, a small class of specialists who have studied it exhaustively, and know everything about it that is known. Secondly, a larger class who know very little about its technique, and who use it in a rule-of-thumb manner.

Now, most of the latter are highly intelligent practitioners, but in this matter have been overwhelmed by the minuteness of the doses, and the absurd manner in which these doses have been expressed. It has been customary to take the milligram or the cubic centimetre as the unit, and the early doses have been represented as fractions having a long series of noughts, like a belated railway viaduct!

Now, it is an elementary psychological fact that the mind cannot grasp fractions so readily as multiples. We talk glibly of a million pounds, but we should all be rather staggered at the thought of a millionth of a pound. We can all conceive of a million cubic centimetres, and the

conception becomes more realistic when we think of it as one thousand litres, and when we look at one pond containing a kilolitre the realism becomes complete. But "0.000001 c.cm.," or " $\frac{1}{1,000,000}$ c.cm.," or even the much simpler form " 10^{-6} c.cm.," are too unwieldy to be constantly intelligible. I wonder how many practitioners who have not been working at tuberculin or at recent mathematical problems could sit down and write off without thinking it out the decimal expression or the minus index for one-millionth. I think in many cases there would be a zero too little or too many. And it is very difficult to carry these highly fractional figures in mind. The obvious remedy is to use smaller units. All scales of weights and measures are founded on this principle, and when the conventional scale does not go far enough in any branch of work, the worker invents new measures. The millimetre used to be the limit of length, but when the microscopist found this too large for his unit, he invented a smaller one, the "micron" or μ . Now the first unit that occurs to us is the cubic millimetre, $\frac{1}{1,000}$ of a cubic centimetre, and this has already been suggested by Dr. Clive Riviere, but this is not small enough, as the millionth of a cubic centimetre has been given, and it is well to select as final unit the smallest dose likely to be given. For this unit a name must be found, and it should be an "unappropriated" Greek letter, with an associated word with meaning suitable to its minute dimensions, and so I suggest the letter ψ and the word *psilon*, which may be conveniently translated "shred," "residue," or "atom," though it occupies a big space in Liddell and Scott. Thus:

1,000 psilons (ψ) = 1 cubic millimetre (c.mm.).
1,000 cubic millimetres = 1 cubic centimetre.

Thus fractions may be obviated, the dose being given in psilons until it reaches the cubic millimetre, and then be given in cubic millimetres until it reaches the cubic centimetre.

Another suggestion is the abolition of the absurd English practice of speaking of the dose of TR in terms of milligrams of the original bacillary substance, when three-fifths of that substance is no longer present. It is just like speaking of a dose of liq. strychninae, not as 5 minims, or as $\frac{1}{20}$ grain, but as the original weight of nux vomica bean from which this quantity of liquor had been made. If, however, clinicians still wish to prescribe TR in terms of the weight of the original bacillary substance, it would be still practicable to use the symbol ψ for 100,000 milligrams of original bacillary substance of TR, and its quantity would be the same as if the dose had been expressed in the Continental fashion—that is, in cubic centimetres of tuberculin fluid.

In the method of prescribing tuberculin which we have adopted at the General Hospital, Wolverhampton, we use the psilon unit, and the cubic millimetre unit. And the cubic centimetre unit is only used when the dose has attained unity or beyond. If this method were generally adopted it would render the use of tuberculin more popular and more intelligible to busy practitioners, who do not give their whole time to unravelling these ponderously expressed fractions.

THE Third International Conference on Cancer recently held at Brussels decided that its next meeting should be held at Copenhagen in 1916.

A Yellow Fever Bureau *Bulletin* states that a report by Seidelin on the subject of vomiting sickness in Jamaica will soon be published. Scott has recently shown conclusively that at least a certain number of cases of so-called vomiting sickness are really cases of cerebro-spinal meningitis, as had been previously suspected but never satisfactorily demonstrated; secondly, it is stated that Seidelin's investigations have not resulted in establishing the identity of vomiting sickness with yellow fever. For practical purposes it is suggested that the islands, meaning essentially the seaports and the commercial highways, are free from yellow fever, though it is admitted that endemic foci may continue to exist in remote parts of the interior, the prevailing forms of the disease probably being mild. The possibility of spontaneous cases occurring occasionally cannot be excluded. It can, however, be definitely stated that yellow fever is not prevalent in Jamaica to any considerable extent.