particularly for such operations as thyrotomy for the removal of a laryngeal growth. In one case, that of a powerfully built man who had had half his larynx removed on a previous occasion and who had then given me some trouble owing to the difficulty of keeping up deep anæsthesia by means of a Junker's apparatus, I administered a small quantity of morphine before the chloroform and obtained a very marked improvement in my results. Obese subjects, if only obese, are not as a rule difficult to anæsthetise, but if the obesity be associated with such conditions as an inadequate nasal air-way, perfect teeth, or a receding lower jaw, if it be grafted on, so to speak, to a previously well-developed muscular system, or if the patient be alcoholic or a great smoker, a good deal of difficulty may be experienced. As a general rule, the mixture of chloroform and ether administered slowly produces better results than ether or chloroform.

The commencement of an operation before perfect anæsthesia has been produced is in most cases unattended by anything more than inconvenient reflex movement. There is no conclusive evidence, to my mind, that the circulation is specially liable to be depressed by operating in moderately deep anæsthesia. But in certain subjects, and particularly in muscular and obese patients, the commencement of an operation during moderate anæsthesia is strongly to be deprecated owing to the reflex suspension in breathing which may result. This particularly applies to rectal surgery. I have notes of one case in which this reflex suspension of breathing seemed to be the starting point of an asphyxial condition which was with the greatest difficulty overcome. It is highly probable that this state has often been erroneously regarded as primarily cardiac. In heavily built and obese men, with flabby tissues and thick necks, any interference with free breathing may quickly lead to such swelling of parts within and about the upper air passages as to completely occlude them. Further clinical observation may perhaps enable us to state what traumatic stimuli are most likely to interfere with respiration during moderate anæsthesia. The point is important, not so much in relation to fairly healthy persons, such as we are now considering, but as regards patients who are dangerously ill and who cannot with safety be deeply anæsthetised. Fortunately there seems to be far less tendency for such patients to respond either inconveniently or dangerously to the stimulus of an operation begun during partially established anæsthesia.

An Address

ON

THE PROBLEM OF INFECTION AND IMMUNITY IN TUBERCULOSIS AND THE ISSUES INVOLVED.

Delivered before the West Somerset Branch of the British Medical Association at Taunton on Oct. 31st, 1902,

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GENTLEMEN,—I thank you for the honour of the invitation to meet you and I crave your indulgence for my selection of so commonplace a subject for my address, but as this subject is equally interesting and important to us all I shall, I hope, gain your ready sympathy. The revolutions in diagnosis, prognosis, and treatment of tuberculosis have been rapid, especially when attacking the lung, and it is to the lung that I shall devote my attention. However, it is not these aspects of tubercle upon which I would dwell, but rather upon problems which, while of long-standing interest to our profession, have recently taken their place in the arena of the town-hall and the county council chamber as subjects for public discussion. I refer to the problems of infection and prevention. The public will be appealing to us for guidance and it is well that we should take stock of our knowledge and form a reasoned opinion. The cry is "Educate the public," and we are the educators.

Let us presume that as their first lesson the public are learning the resolutions passed by the British Congress on Tuberculosis of 1901—viz., (1) that spitting in public should be suppressed; (2) that it is necessary to provide sanatoriums for consumptives; and (3) that they should do away with tuberculous meat and milk—a lesson in which we all heartily concur. The first is a matter at least of simple cleanliness, the second is a judgment based upon ample experience worked out first in other lands, and as to the third the meat and milk are simply tested for their purity like any other food. But we all recognise that it was not on such common general grounds that a congress was anxious to pass good resolutions: it was upon one main ground—the fact that tuberculosis is now recognised as ranking among diseases which are infective diseases. The educated public appreciate this and are asking, Is it not very infectious? This is what we have to answer and desire knowledge of; this degree of infectivity it is that we must state; the conditions of risk we must lay down, for it is our duty to consider our patients at least as much as their neighbours, and our patients will suffer, and suffer severely, if credited with an evil eye that is not theirs.

Professor G. Sims Woodhead has recently written, "Tubercle bacilli may become so modified that they can flourish saprophytically." Dr. Kanthack stated that the organism of tuberculosis was capable of thriving outside the animal body, while Dr. Sidney Martin stated that it had no independent existence. The variation of medical opinion, nay, in many cases the irreconcilability, upon the point of infectiveness, may be gathered from the words of speakers at the recent autumn conference at Berlin. From abstracts of their communications, representing medical opinions, I select the following quotations as evidence. Professor Sangman protested that "the utility of spreading among the public the fear of contagion by prescribing preventive measures such as partitions between each two persons sitting in offices and workshops as advised by Flügge is doubtful." Dr. C. Savoie, speaking of workshops, schools, and offices, said of curable and incurable tuberculous persons "they would, in all cases, be separated from other people." Dr. S. von Unterberger wrote that "the contagion of tuberculosis in hospitals is not to be feared," while Director Gebhard stated that "the homes for disabled consumptives will have to be built so that their neighbourhood is not a danger of contagion and that every danger is as much as possible removed from the persons in good health who frequent it or are employed in it." Dr. C. Denison said: "To my mind the difference in importance is as 10 to 1 between the subject of susceptibility and immunity on the one hand and infection and virulence on the other"; and to that point Dr. Denison desired to direct the discussions.

You will see that I am not tilting at windmills when I mention instances of the disabilities that are already afflicting some patients abroad. "In some parts of France they have had to abandon the idea of sending into the villages single patients of the working class, however strictly selected and although free from bacilli, for the peasants greatly fear tuberculosis, and for their cattle's sake, not for their own health." The Bethel (Sullivan County) board of health has adopted an ordinance imposing a penalty of 10 dollars for the first offence and 50 dollars each week, for entertaining a stranger or guest affected by tuberculosis other than immediate relatives or those dependent on the family. Let us mark how the sufferers from this sub-infectious malady differ from those suffering from most infective diseases: they are adults, not children, and the term of their illness may be counted by years, not weeks; moreover, as often as not, their powers of work are efficient. If we had sanatoriums in plenty and funds to replace the earnings of bread-winners it would not even then be necessarily right to follow the lead of the peasants of France or the board of health of Bethel. As we are not yet, however, sufficiently endowed with these aids, let us at least recognise that any exaggerated statement of the infectivity of tubercle will induce similar hardships in this country, together with other hardships consequent upon social taboo, whilst the ultimate result may well be an increase and not a diminution of tuberculosis.

The present state of our knowledge may be, broadly, gathered from two regions: (1) the region of biology, pathology, and comparative pathology; and (2) the region of experience in the present and the past, the incidence of the disease in man. Now the evidence collected in either region

cannot be said to conflict nor can it be said to coincide; it is conspicuously incomplete and links of association are evidently missing.

In the field of biology one may briefly note tuberculosis of man, of bovine origin, of birds, and of fish. Between bovine and human tuberculosis I shall not attempt to draw distinctions, however real may prove those differences which Professor Koch in 1901 impressed upon us and recently reiterated. Small though be the amount of transmissibility from cattle to man which Professor Koch admits, even that is sufficient to give inherent probability to those experiences in America related by Professor Ravenel in 1901, and recently at the Berlin meeting by de Schweinitz and E. C. Schroeder, besides the recent results of Dr. G. Dean and Dr. C. Todd at the Jenner Institute, and it is sufficient to make us believe that bovine and human tuberculosis are convertible if not identical.

In birds avian tuberculosis, we learn, is with difficulty convertible into the mammalian form in guinea-pigs (Nocard).

From fish and slow-worm cultures have been shown at the 1901 congress of great interest, as both had their origin in association with tuberculous sputa. In the fish the cultures were obtained from a carp found in a pond contaminated with tuberculous sputa; the slow-worm had been previously infected with tuberculous sputa by Dr. Moeller. Now in either case a bacillus is found growing best at the low temperature of 22° C. and not pathogenic to warm-blooded animals. One would not think of associating it with tubercle but for the history. If it be a transmuted tubercle bacillus its vagaries transgress the lines that usually mark specific difference, so that the discoverers would place it among the acid-fast bacilli only and not name it bacillus tuberculosis. These acid-fast bacilli need not detain us, except one—the timothy-grass bacillus. This will grow at the same temperature and produce the same lesions as tubercle, but Moeller, its discoverer, finds by virulence and culture character that it is not tubercle nor is it convertible; the less initiated might well hesitate to class it. Other acid-fast bacilli are of interest as pitfalls for the unwary who rely upon one or two bacilli as sufficient ground for a diagnosis—they come from butter, grass, manure, and other sources. Thus we see that the tubercle bacillus no longer stands alone—it is one of a class widely spread in nature. A classification of tubercle bacillus has been made. Many students of morphology separate it from the bacteria, the class that provides most of our discovered infective agents, and place it with actinomycosis among the bacteriomycetes—a step nearer the moulds. Our knowledge of the bacillus (as we will still term it) of tubercle outside the body has not advanced. Coates has confirmed and extended Cornet's researches in dust; he did not find it in 10 dirty houses that had been for some years free from tubercle. I have not come across other searches for it in a habitation supposed to be free from contamination.

In pathology recent work has been mainly directed to determining the site of inception and to see how far a necropsy sustains the opinion that milk is the source of infection in children. Judging from gross lesions the more advanced lesions of the mediastinal glands have been taken to suggest primary pulmonary infection in the vast majority of tuberculous children. I think, however, that I see a desire to propound a theory where evidence cannot be forthcoming, a theory that bacilli may pass through a mucous membrane without breach of surface. Seeing that the tonsil though not ulcerated yet may be supposed often to pass the bacillus on to the glands of the neck, I am inclined to support these theorists and to suggest that, after all, the early affection of the mediastinal glands may pass through the squamous epithelium of the oesophagus. The inert bacillus requires a conveyance, the salivary corpuscles would suffice to carry the bacillus through the wall, stopping at the lymphatic gland; the gland most frequently affected in children lies on the oesophagus in the fork of the bronchi and trachea. I commend this view only for future testing, but I am glad to find a particle of supporting evidence. Mr. S. G. Shattock last June showed primary tuberculosis of the oesophagus in a python. He remarked that in man it was only known by direct extension and was not primary, but there was no lymphatic structure in the human oesophagus as in that of the python, but most remarkable of all was his statement that infection had occurred by invasion between the cells of the epithelium which remained intact without any ulcer.

The work of Professor Benda has made plain some of the puzzles that exercise our minds in the contrasts of acuteness

and chronicity in tuberculosis. The acute general tuberculosis and the tuberculous meningitis of general infection in children come, as it were, by anatomical accident. Their acute symptoms are not apparently referable to any specially virulent strain of micro-organisms, but to the suddenness of their access to the blood stream. This access is from the frequently advanced disease of the mediastinal gland, which while being thoroughly permeated by advancing bacilli, is the final barrier—their next outlet is into the thoracic duct and blood stream. Like the power of a drug intravenously injected, these toxins owe the acuteness of their symptoms to the fact that they travel directly in the circulation. Further, the bacilli make their home now on the walls of the blood-vessels and so can continue to administer poison as rapidly as ever unless checked by thrombus formation. Then in our conceptions of variety in virulence we must remember the varied path of extension. Another instance is the unfortunate experience, common to many of us, of a patient with phthisis who is rapidly improving but who is suddenly seized with a pleurisy that spreads over all the pleura, hampering respiration and pouring poison of tubercle into the system; this owes its source often to a mediastinal gland from which disease has had outlet to the pleura.

Comparative pathology I may only touch upon. Nearly every other animal that dies in the Zoological Gardens gives, I believe, proof of its proclivity to tubercle and *per contra* I am not aware that any animal whose life was spent in a state of nature has ever been found to die from tubercle. Cattle, like children, have a special proclivity to advanced infection of the mediastinal glands—90 per cent. was the proportion of this lesion in a recent inquiry by Professor McSweeney. The most interesting and striking evidence of contagion in cattle may be read in Professor Bang's successful prevention of the disease in calves by their segregation. The comparative virulence of bovine tuberculosis may be a factor in its spread, but the more simple explanation is the way in which cattle use their tongues upon their coats, their mangers, and their neighbours. What a dose they must take. If we should feel alarm, taking the infection seen in European cattle to apply to ourselves, let us take solace in the immunity of cattle in Japan which may as logically be applicable. In any case we should avoid pressing analogy between animal and man too closely. In cattle the veterinary surgeon applies tuberculin and he sees the cattle slaughtered. So thorough and complete a view of the disease exposed to the eye of the pathologist cannot be exceeded. He can thus calculate with accuracy its prevalence, with the astounding result that roughly a million of the kine of Great Britain and Ireland are found affected. Besides milk containing bacilli we now know that the urine is often bacilliferous though there be no renal disease, and an enteritis may allow of vast numbers of bacilli being cast upon the ground. Many countries are trying to get free from the disease by the slaughter and isolation of cattle; some have desisted owing to the magnitude of the task. Victoria has had healthy herds to deal with and has been at work many years, but the extinction of the disease does not seem to be yet in sight.

Turning to the other region of our knowledge—our present and past experience of the disease in man—we find educated (that is to say, medical) opinion holds this view of infection, "Healthy persons run a distinct risk if the contact between themselves and the phthisical sufferers is close and prolonged." Instances of the risk occur to us; a few have been published from time to time. However, in 20 years so little evidence has come forth of the kind to be expected after the discovery of the bacillus that I think it probable that professional experience will, as a rule, agree with the experience of the observers in British sanatoriums who thought that they traced infection in 2½ per cent. of 700 cases. No doubt the over-estimate of the influence of heredity prevented any such view at all occurring to any but one or two medical men before our attention was aroused by Villemin and Koch.

I attempted to study past experience of a generation of phthisis in England a few years ago; it was a simple voyage of discovery. The returns showed that the amount of phthisis deaths varied with sex, with age, with general mortality, with place and time, food and air; the relation to all of these could apparently be traced but one could not trace infection. Rural phthisis had been stated to be due to foci of infection, village infection; but if inquiry were pushed and one probed into parish records it remained untraceable. If we take the extra phthisis death-rate of a county asylum as representing for the sake of

argument a small epidemic, that will infallibly mark the phthisis rate of that rural union (not parish) as much heavier than any of its neighbours. One could not find in ten-year periods epidemics which would at all equal the extra rate due to the presence of an asylum. The amount of variation in the factors which I have mentioned was almost enormous. For instance, taking sex—between the age of 10 and 15 years 200 girls die to 100 boys; taking time—then 40 years back those 200 girls would have been 500; and taking place either now or then—500 in Somerset would mean about 1000 in Northumberland. The place factor is really the overcrowded house, and the time factor is the food-supply. While not finding evidence of infection like that of other infectious diseases I allow that the conditions of the search would not detect so small a percentage as $2\frac{1}{2}$ or even 5, but I believe that I am justified in saying that there is not the accidental rise or fall that would indicate chance infection as in other infectious diseases to an extent of 10 per cent.—it might well be *nil*. Is there any explanation why such evidence should not be forthcoming? It has been suggested by some writers that the latency of tubercle affords a solution of the problem. There is a kind of latency (visible to the anatomist when he finds old cicatrices) which is common but very unequally distributed, being much more frequent in the old than it is in the young. Now the frequency in incidence of phthisis is greater in the young; further, latency if it took the line of origin of other infective diseases would show the same rise and fall and therefore this theory affords no solution. The only kind of explanation to my mind is an inference which is not infrequently uttered, "We, all of us, repeatedly are exposed to infection."

Returning to our more definite experiences, while evidence of infection is in the main negative, evidence of immunity is of a positive character. The prevention of phthisis taught us how to measure our ventilation and by that means afforded to the artisan a large extent of immunity. That was 40 years ago; just at present we are realising from the sanatorium and by the expositions of Dr. A. Ransome that by using a higher standard of ventilation a higher degree of immunity will be afforded. From 1865 to 1897 the mortality from phthisis in boys from 10 to 15 years of age fell from 100 to 32. During these 32 years no action was taken owing to any idea of infection; indeed, aggregation was increased by density of population and by school attendance to a three-fold extent and risks of infection must have increased accordingly. Their diminished death-rate was due to avoidance of malnutrition, of the infectious fevers, and to a less degree to avoidance of bad air and faulty sanitation. Recently we have been improving our workhouse dietary, and still we have the "out-of-works" as our most frequent patients. Having on a former occasion¹ detailed much of this evidence I will not delay you by repetition. It is evident that the defensive powers of a healthy man can cope with the attacks of the bacillus; and these powers are infinitely greater in him with regard to tubercle than when they are placed in opposition to the attacks of the bacilli of other infectious diseases. In tubercle, therefore, the first aim should be to reinforce such defence by attention to nutrition, ventilation, and sanitation.

Let us consider next those persons whose contact with sufferers from phthisis is close and prolonged ("close" one considers an attribute of the room too small to ventilate well); are they to be considered unresponsive to the reinforcing of their defensive powers? Some unfortunately are so, but they are much less responsive to any other line of policy. In speaking of these I am referring to the "submerged tenth." Others are very responsive, such as inmates of asylums, and a policy of segregating these for tuberculosis which has been begun should be copied at all those asylums where there may be this close and prolonged contact of the unaffected with sufferers from phthisis; at the same time the standard of air-supply, food, and sanitation should be maintained. In workhouses similar tactics may well be pursued.

We see how our attention has to touch on very many points necessary to achieve success in preventive measures. Accordingly our education of the public should be so broadly based that they may learn what are the conditions which make or mar healthiness of life. To effect this I would strongly recommend that the elements of hygiene be forthwith taught in our primary schools.

There is a hiatus between the cultural experimental and dead-house evidence of the working of the bacillus on the one hand, and our present and past experience of the incidence of the disease among our patients on the other. In one case the life-history can be followed throughout and with precision the bacillus can be traced from point to point. In the other there is a fatal precision of death that marks so many attacks, but the bacillus is not traced.

We wish to deal with evidence, not theory, and there is usually no evidence upon the source of the infection according to our experience. It is open to one to argue that it is all of human origin, of bovine origin, or even derived from acid-fast bacilli of unknown powers of transformation.

"There are more things in Heaven and earth, Horatio,
Than are dreamt of in your philosophy."

We do know this, however, that as the disease bears no relation to density of population outside the home itself, nor any relation to the frequency with which the healthy meet the sick (of which latter medical attendants and patients form a notable instance), therefore the disease is not infectious in the sense employed in speaking of the infective fevers. Then, I say, we shall be wise to utter no more than belongs to our own experience; "healthy persons run a distinct risk if the contact between themselves and phthisis sufferers is close and prolonged." Those utterances abroad one is glad to find without parallel at home; yet here and there we see signs of our leaders in public health taking theory to work upon rather than experience.

I read a suggestion of a most admirable provision for dispensary, sanatorium, and hospital accommodation for consumptives at Liverpool. I would desire to see this scheme realised, for it is to a sanatorium especially that one looks as the great technical educator in the prevention of phthisis. In this suggested scheme Dr. Nathan Raw promises "that by isolating the incurable cases the municipality can largely prevent the disease." From what I have already put before you I believe that such is not the present expectation—i.e., balance of probability. And how greatly will a statement like that tend to procure the fulfilment of Dr. Raw's own prophecy, "In a few years it may be a matter of extreme difficulty, if not an impossibility, for a consumptive to find ordinary means of employment." For his scheme Dr. Raw finds that a halfpenny rate is necessary. Many a ratepayer would gladly add to that halfpenny a sufficiency to replace the bread-winner's earnings. Such an addition to the scheme I deem essential to its success. Let us hope that Liverpool will never seek such avoidance of the sick as first prevents employment and then allows a consequent depletion of the sufferer's pockets, a depression of his spirits, and in the end a crop of tubercle in his impoverished family.

And what are we to do in the future? For the cattle, plenty of fresh air, sunshine, and slaughter of visibly affected beasts—it is in this order that the chief inspector of Victoria proclaims his method. If we essay complete prevention why not attempt it first in some limited area like the Isle of Man?

Among our patients we cannot fail to recognise how enormous is the proportion of the phthisis of to-day which exists in the lowest wage-earner, the most ignorant of the poor. Are we going to pillory the educated clerk for no risk or for an infinitesimal one, while these uneducated folk remain, whose potentialities for danger, by whatever standard we may measure them, are infinitely greater and cannot be dealt with? If we do allow this we diminish the food or house room of a bread-winner and create the predisposition to tubercle in his family.

The term "infection" is too loose a term to apply to both measles and tuberculosis. When its use does damage to the workman's prospects it is perpetrating an injustice upon him; "sub-infectious" is a more proper term to employ in public health. By difference in the kind and mode of infection, and still more by the greater difference in resistance to the disease, our patients differ from cattle and the argument of isolation does not apply. When we see the poor results in the isolation of infective fevers by hospitals for fever, those who rank tubercle as most highly infectious must seriously doubt if it is a right policy to apply such a system to tubercle. No, we must proceed in the interests of all by education and attention to the wants of the people in food and in air, and we shall see the successes of the past excelled by the higher successes of the future.

I earnestly hope that the medical profession at large will not encourage the public to avoid their tuberculous fellow

¹ THE LANCET, April 15th, 1899, p. 1005; Brit. Med. Jour., August, 1901; Catalogue of the Museum of the British Congress on Tuberculosis, 1901.

creatures. Such a course will not, in my opinion, diminish the amount of tubercle. It will, however, infallibly swell the ranks of the unemployed, it will depress the phthical wage-earner's spirits, it will empty his pockets, and ultimately, by want and distress, reduce his family to that condition of low vitality which the tubercle bacillus requires for a successful invasion.

In conclusion, gentlemen, I would urge you, as the educators of the public in these matters, distinctly to let it be known that tubercle is not highly infectious. State that it is not a disease that requires isolation and that only under certain quite exceptional conditions does it appear to be infectious at all. Insist that healthy people enjoy extraordinary immunity, that fresh air and open windows are the great armour against its attacks, and that the instruction of the young in the general principles of hygiene will prove more valuable eventually than the isolation of the unwilling artisan or the excommunication of the clerk.

AN IMPROVED METHOD FOR THE MICROSCOPICAL DIAGNOSIS OF INTERMITTENT FEVER.

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In the Harben lecture, delivered at King's College, London, on Nov. 10th, 1902, I described in detail the method referred to in this note¹; but as the matter may be of interest to a large number of practitioners I think that it would be advisable to publish a brief description of it here.

There are at present two methods in universal use for the detection of the parasites of malaria. Both consist in spreading out over a large area a small quantity of blood. In one method the blood is examined in a liquid state and in the other it is dried. In both cases about one cubic centimetre of blood is spread out over about four square centimetres of area, so that the film of blood is a very thin one. This attenuation is necessitated by the fact that a thick film of blood is too opaque to permit of the easy detection of the parasites through the mass of hæmoglobin. Unfortunately, the attenuation, though required for the reason mentioned, has one great disadvantage—namely, that if the parasites are scarce we are compelled to search in many fields of the preparation in order to find them, and it is obvious that the number of fields which we must examine varies directly with the scarcity of the organisms.

It has long occurred to me that we should be able to detect the parasites in a thick film if only we could succeed in treating the blood in such a manner as to do away with the obscuring effect of the massed corpuscles. The method which I adopt for this purpose depends upon the fact that the parasites, even the smallest of them, adhere to the stromata of the corpuscles which contain them; so that, as long as we retain these stromata we retain the parasites. Now the opacity of a thick mass of blood is due, not to the stromata of the corpuscles, but to their hæmoglobin and it is easy to wash out this hæmoglobin without washing out the stromata, either in a dried film of blood or in a dilution of blood.

I have tried numerous variants based upon these considerations, but I select the following as the simplest and easiest for general adoption. The finger is pricked by a lancet-shaped needle or by the point of a lancet. A large drop of blood, amounting to, say, 20 cubic millimetres, is taken up on the glass slide or cover glass and is *slightly* spread out by means of the needle or lancet—that is, it is spread over an area which can be covered by an ordinary cover glass. It is then allowed to dry, either naturally or over a flame (without heating so much as to fix the hæmoglobin). The result is that we have a thick dried film of blood in which about 20 cubic millimetres of blood (instead of the one cubic millimetre generally used) are disposed over an area which is not larger than the area of blood usually examined by the ordinary processes. As soon as the

film is perfectly dry I lay upon it by means of a glass rod a quantity of aqueous eosin solution sufficient to cover the film of blood. The solution is the one which is usually employed in order to obtain the Romanowsky effect. It is allowed to remain upon the film of blood for a period up to about a quarter of an hour. This period should be inversely proportionate to the strength of the solution. As the film of blood has not been fixed the solution of eosin will take out the hæmoglobin of the dried corpuscles and, at the same time, will stain the residual mass consisting of the stromata of the corpuscles, the leucocytes, blood plates, and parasites. After the solution has remained upon the film for the period required it is washed off by a very gentle stream of water. As the film has not yet been fixed to the glass it is most essential here to use no strong current of water, or else the whole of the residual mass will become detached from the surface of the glass. As soon as the eosin solution has been washed out a weak solution of the methylene blue which is employed for the Romanowsky stain is allowed to run over the film, just as was done for the eosin solution. The blue is permitted to remain only for a few seconds, the time varying inversely with the strength of the solution. Especial care must be taken not to stain the preparation too dark a colour. Next the blue is washed off very gently in its turn and the preparation is complete. We can now dry the stained film and mount in Canada balsam, or examine it directly by placing a drop of cedar oil upon it for the immersion lens; or we can mount it in water, place a cover glass over it, and examine as usual.

The preparation will differ from those in ordinary use in two particulars; first, it will contain no hæmoglobin and will consist only of the transparent stromata of the corpuscles, the leucocytes, blood plates, and parasites; and, secondly, 20 times the amount of blood used in an ordinary preparation will now be found disposed over the area covered by the cover glass. From this it follows that we shall find 20 times as many parasites in each field of the microscope as would be found in an ordinary preparation of the same blood; in other words, we may say that these preparations will have 20 times the diagnostic value of an ordinary preparation. If the Romanowsky stains are efficient the smallest parasites will be seen scattered over the field as small blue rings with a deep crimson dot within or upon the ring, this dot being, of course, the nucleus. If the preparation has not been over-stained the pigment of pigmented parasites will also be visible. I make two preparations—one stained by the method described above and one in which the hæmoglobin is simply washed out with water. In the latter the larger pigmented parasites are very easily visible, owing to their pigment, without any staining at all.

Dr. J. W. W. Stephens, formerly of the Malaria Commission of the Royal Society and now of the Liverpool School of Tropical Medicine, kindly tested these methods for me by making comparative numerical counts of the parasites found respectively in a preparation made in the old way and one made in the new way, both from the same patient at the same time. In an ordinary liquid film of blood he found 15 parasites during five minutes' search. In a de hæmoglobinised thick-film preparation of the same blood stained by the above method he found no less than 76 parasites in one minute, showing that this preparation was 25 times better for diagnostic purposes than the former preparation. In a single field of it he counted no less than 70 parasites. In another case in which only one crescent was found in several ordinary specimens after long search I discovered five in five minutes in a de hæmoglobinised thick-film preparation (unstained).

It is necessary to add that the student should familiarise himself with the appearance of the parasites in this kind of preparation. He had better practise at first with cases containing considerable numbers of organisms.

I have experimented on de hæmoglobinised dilutions of blood, "centrifuged" so as to drive down the residual stromata to the distal end of a capillary tube, but cannot say that the result is better than that given by the very simple method described above.

Skilled students have long been accustomed to search for the larger parasites in the thick edges of liquid films. I have used de hæmoglobinised thick-film preparations for finding crescents since 1895, and Manson employs them for staining flagellate bodies; but I do not think that the facility with which we can detect the smallest parasites in such specimens when properly stained is generally recognised.

Liverpool.

¹ Journal of State Medicine, December, 1902.