antiseptics, has lessened the number of instances of sympathetic ophthalmitis. By clearing a corneal wound even when encroaching on the ciliary region, of all entanglements of iris, an effort can be made to save, without undue risk, an eye which formerly would have been condemned and excised. I know many instances in which this conservative treatment has been carried out successfully. At the same time an eye which has been irretrievably lost had better be enucleated with as little delay as possible. There are cases on record, fortunately rare, in which, in spite of enucleation, sympathetic ophthalmitis has appeared in the other eye. I have met with at least 4 such cases in my practice, 2 were in children, one boy and one girl, and 2 were in men, each aged 26. In 3 the eyes were enucleated 16, 23, and 32 days after the accident, whilst the ophthalmitis showed itself 32, 40, and 106 days after the injury. Unfortunately only one of my cases was of the mild character described as usually present under these circumstances, for one progressed to absolute blindness, another almost so, and the third to very greatly impaired vision. The other

Fig. 6.—Workman (timekeeper) removing "mote" from fellow-workman's eye.

fortunately made a complete recovery and has been able to pursue his, or any, employment uninterruptedly for many years. In these cases it would seem as if the enucleation was too late, and with regard to some, at all events, that the disease, if latent, was present prior to the enucleation.

The freeing of a wound from iritic adhesions should be employed, even when a considerable time has elapsed since the accident. An interesting case illustrates this: Many years ago a man was under my care for a punctured wound of the right eye beyond the upper corneal edge, with prolapsed iris, caused by a packing needle. He did not come under my care for some time after the accident, and shortly after sympathetic ophthalmitis showed itself in the other eye. Fortunately it was mild in character. An operation was performed for the prolapse in the right eye, and the old wound was reopened and cleared as far as possible of all iris entanglements. Subsidence of the ophthalmitis in the sympathizing eye quickly took place. Enucleation in this case was out of the question, as vision in the injured eye was not far from normal. The result was recovery of normal vision in each eye.

It should be borne in mind, when considering the question of enucleation, that the injured eye may ultimately be the better one, and also that, when the sympathetic disease

has obtained a hold, the enucleation of the offending eye may not stay or alter the progress of the disease in the fellow organ.

Recently, for the purpose of another article, I worked out carefully my records of the children who had been inmates since its foundation of the Sheffield School for the Blind.* The number was 335, and of these no fewer than 22, or nearly 7 per cent., owed their blindness to sympathetic ophthalmitis. It was interesting, but sad, to note how many of the original injuries were really of a preventable kind. The use of a fork to undo a bootlace is a common practice among children and a frequent cause of eye accidents. Three of my cases were injured in this manner.

An Address

ON

THE SCIENTIFIC USE OF EVIDENCE IN SURGICAL PRACTICE.

DELIVERED BEFORE THE READING PATHOLOGICAL SOCIETY.

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GENTLEMEN,—I am informed that it has been your custom for the last ten years to have an annual oration, but that your Committee impose no conditions, and thus it is that he whom you honour with the position I now occupy is left to his own devices. In these circumstances and after careful consideration I have selected a subject which has often forced itself on my attention. I allude to what I may term the scientific use of evidence in the practice of medicine. But, in order to lay this question before you in any adequate manner I must submit some introductory remarks.

The present is a time in which science has broken through its accustomed boundaries and is spreading like a flood over the whole field of human activity. And this, not because the phenomena which surround us have suddenly become scientific, but because having witnessed many marvellous discoveries and seeing that in every direction there is more beyond, that "in everything there is inexhaustible meaning," men are diligently working to find things out and see what they really are, and how they can be made to serve our own needs and aspirations.

It is strange to see how the term "science" had until quite recently lost its true meaning, and come to be limited to such subjects as astronomy, geology, and chemistry, and to have no application to brewing or the dyeing of fabrics, or to the multitudinous affairs of everyday life. Is it not often cast in our teeth that medicine itself is not a science, but merely an art? But what a chaos of thought is here disclosed! The term "science" is the English form of the Latin *scientia*, and "knowledge" is its Anglo-Saxon equivalent, and the adjective *scient* means knowing, perceiving, discerning. Thus, whenever we use the term "scientific knowledge" we lapse into tautology, as if we should talk of speedy rapidity or of ascending upwards. A moment's thought will show that "science" applies not to any object, but to the method of investigation. The pursuit of science means the effort to obtain knowledge, the search after the facts which appertain to whatever subject is under examination. There is thus nothing—from the planet Mars or the sun itself to a wart on a ploughboy's knuckle or dust on the window-sill—which is outside the field of science. Even a laugh so foolish or a remark so inane (in contempt of science) that it provoked physiological investigation, might conduce to a more scientific classification of the feeble-minded. Those who tell us either that medicine is or that it is not a science are, I take leave to say, talking mere nonsense. Was medicine not a science when it was studied by Harvey and Hunter,

* Vide BRITISH MEDICAL JOURNAL, November 2nd, 1907, p. 1215.

Lister, Paget, and Hughlings Jackson? can it ever be a science when practised by a man who never thinks?

A good illustration of the fact that science lies not in the object but the method, is that Principal Donald MacAlister, when he was fresh from his triumphs as Senior Wrangler and first Smith's Prizeman, gave a lecture at the London Institution on the structure of a marrow bone. Now a marrow bone is used to support a cow when she eats turnips; in the eyes of a savage it is a thing to be smashed in order that the marrow it contains may be got at, or it is a weapon to be used on the skull of an adversary, while the cook boils it to obtain "stock." But the man of science sees in it an epitome of mechanics, displaying buttresses, arches, leverage, one of the most perfect ball-and-socket joints, and the distribution of material to the greatest advantage in regard to function—an object, in short, from which an architect might learn the first principles of his craft. This address of Principal MacAlister, I have reason to know, was suggested by the method by which my predecessor at Cambridge—Sir George Humphry—taught anatomy. While many anatomists were content to state mere facts, in regard, for example, to the femur that it possessed a shaft, a head, two trochanters, two condyles, and ridges for the attachment of muscles, Humphry's attitude was more scientific; making the study of structure subservient to the study of function, he taught the mechanism of the bone, and explained how its various parts were adapted to the purposes which they served. Thus treated anatomy becomes a subject of very real interest, in the pursuit of which an intelligent student is constantly meeting with adaptations of means to ends which command his admiration, stimulate his curiosity, and imprint themselves indelibly on his memory. In this manner he finds that he can easily learn and not easily forget "his anatomy."

If we turn to the application of the scientific method to practical medicine, we may find food for reflection when we call to mind the way in which we deal with evidence. Evidence has been defined as that which makes evident and enables the mind to see truth; proof arising from our own perceptions by the senses, or from the testimony of others or from inductions of reason. Now have we all trained ourselves always to bear in mind the supreme importance, in the practice of medicine, of treating evidence in a scientific manner?

I think we must be struck by the sharp contrast which exists between the practice of medicine and of law in respect to evidence. In the eyes of the law evidence is a subject of the first importance, and the principles on which it rests have been scientifically examined; and naturally so, for a lawyer makes it his first business to ascertain all the facts and circumstances of any case in which he is concerned. Having a problem to solve, he begins by an exhaustive investigation, and thus the value of the different forms of evidence commands his closest attention. Moreover, a chief duty of a presiding judge is to see that rules, methods, and restrictions in the arrangement and presentment of evidence are observed. Lawyers are careful to divide evidence into that which is positive or direct and that which is circumstantial or indirect. Thus in one murder case the prisoner is confronted by positive evidence—he was seen to overtake his victim and to fire the fatal shot, the revolver was wrenched from his hand, and the death of the murdered man was witnessed by those who were present at the time; while in another instance no one was present except the murderer and his victim, and no positive evidence can be obtained. In such a case those who are conducting the investigation have to depend on a study of the circumstances which they are able to ascertain—that is, on circumstantial or indirect evidence. For example, a man is found murdered; prints in the snow correspond with a boot worn by a man in the village who has a club foot; it is found that this man was absent from home on the night of the murder, and that he cannot give an account of his movements; but he was seen going towards the spot where the body was found, a knife which he bought in a town in the neighbourhood two days before is found near the body, and when arrested he has the victim's watch in his pocket.

Lawyers, as I have said, have not failed to see that these two forms of evidence are entirely distinct from each other and must be kept apart, so that the value of each,

in different instances, may be correctly gauged; and the necessity for the exercise of critical care as to the construction which may safely be put on the evidence in any case is fully recognized by lawyers. They know that when they have a sufficient amount of positive evidence they are on safe ground, while, as to circumstantial evidence, they see that though by its cumulative weight it often amounts to absolute proof, it may sometimes be misleading in a manner and to a degree which is hardly conceivable. Thus the jurists endeavour to ascertain the principles on which evidence rests, to detect its different forms and their relations to each other and the methods by which they can severally be most correctly utilized. It has seemed to me that the importance of this judicial and, as I think it may be justly termed, scientific treatment of evidence has not been fully recognized in our profession, although the essential likeness between law and medicine, so far as the use of evidence is concerned, must be readily apparent to all. This likeness consists in the circumstance that, unlike the theologian whose bed-rock consists of faith and dogma, or the metaphysician or the artist who, if he has the gift, may take an entirely new departure and found a new school, or the poet who may

Pursue things unattempted yet in prose or rhyme—

lawyers and medical practitioners have to do with facts. In both cases the first step must be to ascertain what the facts really are. The lawyer and the medical practitioner no doubt use facts, when they have ascertained them, differently. The object of the lawyer in defending a prisoner is to make the facts subserve the acquittal of his client; he therefore selects, arranges, and magnifies or belittles them to this end, so that they become the basis of an ingenious or even a sophisticated presentment of the case, which he considers is that which the jury are most likely to accept. With the medical practitioner it is different. He cannot adapt the facts to his view; he cannot set up an artificial case by dwelling on some facts and ignoring others; he must take the facts as governing the whole situation. The lawyer can, in many instances, make facts his servants—the medical practitioner will always have them for his masters. If he overlooks, mistakes, misconstrues, or ignores them, mischief will inevitably follow. Now, in dealing with facts, our danger lies in the tendency there is to be superficial, instead of following the inquiry to its ultimate terms. This defect is easily explained. It is always more easy to be superficial rather than thorough. It is an almost universal defect. And we must remember that we are superficial by inheritance and tradition. In early times it was impossible to be anything but superficial—to see more than appeared on the surface. Men knew rocks by their general outline, hardness, and weight for centuries before they knew anything of their origin, structure, varieties, or distribution. And it was the same with diseases. Conceptions of them were formed entirely from their superficial aspect. It was the deformity which caught the eye in tuberculous disease of the spine, and so the patient was said to have "angular curvature." When a patient had carcinoma involving the common bile duct and turned yellow, he was said to have jaundice. Thus it was that results which caught the eye were spoken of as if they were causes; appearances produced by disease were spoken of as if they were diseases in themselves. But, crude and archaic as such an attitude appears at the present day, must we not own that it is still to be met with? We still allow ourselves to be superficial and to be dominated by first impressions and thus to fall away from the scientific attitude. In a recent address Mr. Haldane remarked that we do not think enough. He might, of course, have said (but the phrase would not have been either so forcible or so picturesque) that we are too superficial. John Hunter, you may remember, said "Don't think, go and see." Yet he was not contradicting Mr. Haldane, for he meant, Do not be content to theorize about things at first sight, but go more deeply and ascertain what lies beneath the surface; dig out the facts as explorers dig out the foundations of a buried temple.

We see how superficial we can be in the way in which we use the word "symptoms," meaning thereby a list of features by which a disease may be recognized. Ought we not to remember that, as no two cases are alike, and as individual cases present very different features in their

various stages, diagnosis by formula is fraught with chances of grave error? The symptoms of hip disease are given as a list: Lameness, pain, flexion, stiffness, swelling, and muscular wasting. But a very little experience will show that this uniformity is not met with in practice. In early cases, in which a correct conclusion is of paramount importance, there may be no pain, no swelling, and no flexion; while, if the child has been in bed for two or three days because he has limped, there may be no stiffness, because muscular spasm will have subsided.

Now, an observer who came to the conclusion that, because the list of symptoms so far was incomplete, no disease was present might fall into serious error. While, were he to discard the mechanical process of diagnosis by the list and look critically for every fragment of evidence, he might, perhaps, detect very slight apparent lengthening, and very slight yet quite appreciable muscular wasting. While in the presence of these two fragments of evidence the report of lameness would claim recognition. Arrived at this stage, the investigator would either feel justified in coming to the conclusion that disease was present or be constrained to keep his mind open until the examination had been repeated at an early date. In other cases of early disease pain, flexion, stiffness, and swelling may be all present.

But, further, as we all know, a group of symptoms which, in our unguarded moments, we speak of as typical of some particular disorder may be present in all their distinctness in some entirely different affection. If, following Mr. Haldane, we thought more, we should see that for diagnosis we must search closely for features which are distinct and exclusive—which, in other words, afford the means of distinguishing one thing from another. But have we not all seen cases of early infantile paralysis and of disease of the lumbar spine because of superficial resemblances mistaken for hip disease? I lately met with a case of infantile scurvy which was regarded as one of acute disease of the hip. A great protection against errors of this kind would, I think, be secured if we could shake off the traditional and cramped method of diagnosis by "the list" (of symptoms) and proceed by the more scientific process of collecting every available shred of evidence, both positive and circumstantial, and refrain from any conclusion until it has been all fully sifted and weighed and after we have endeavoured to bear in mind the alternative possibilities. Do we not sometimes remember afterwards that we have overlooked some point that lay outside the traditional list of symptoms, and that we have therein failed to be scientific?

A good instance of the influence of tradition in keeping us from thinking enough is seen in the fact that many still regard "fluctuation" as the all-sufficient evidence of suppuration. If asked how they would settle whether pus is present, many would say they should feel for fluctuation. This wave-like displacement of fluid, however, from one part of the cavity which contains it to another part is a physical phenomenon which can only occur under the following physical conditions: The fluid must be in sufficient quantity, say 1 ounce at least; there must be a firm background, or, on pressure, the whole mass will recede, as, for instance, in the axilla or the abdominal wall; a pliant front—in an empyema fluctuation cannot be obtained; pus must be moderately superficial—fluctuation around the kidney with brawny swelling of the loin cannot be detected. And there are two grounds, in certain cases, on which fluctuation must not be tried for. It may be very dangerous, for the pressure used in searching for it may rupture an abscess (say around an appendix, when pus would escape into the abdominal cavity) and a search for it may cause severe suffering—for example, in an inflamed mammary gland or a testis. Obviously fluctuation cannot be reasonably regarded as the evidence of matter in the general run of cases. Such a view is so crude and superficial that it is entirely unscientific. Indeed, fluctuation should be looked for in only one group of instances, namely, in large cold or chronic abscesses which lie not far below the surface and are painless, of which a good instance is a psoas abscess. For in all small acute abscesses—that is to say, in about 80 per cent. of all cases—fluctuation cannot be obtained, and its very name ought to be suppressed in this connexion.

The presence of small quantities of pus may be detected by (a) elasticity, (b) pitting on pressure, (c) the tender spot, or (d) the soft spot. But, at the present day, those who have discarded tradition, and the bondage exercised by the old ideas about suppuration, and who grasp the true bearings of a case in which acute inflammation, although there is as yet no suppuration, is producing general illness as the result of absorption, together with tension and severe pain, will not wait for any evidence that pus has formed; they will interfere in order to arrest the inflammatory process at an earlier stage, so that suppuration, which has perils of its own, will be prevented.¹ No one nowadays waits for suppuration in appendicitis, septic arthritis, or inflammation of the sheaths of tendons.

A further clear instance in which we must acknowledge the truth of Mr. Haldane's *obiter dictum* that we do not think enough—that we, so to say, leave off thinking while we are still on the surface and looking merely at results, instead of going more deeply and unearthing causes—is found when we use such terms as "synovitis," "arthritis," "peritonitis," or "adenitis." Here we do no better than our predecessors, who spoke of jaundice and dropsy. Take "synovitis." This means that a joint is inflamed; that is, that we are witnessing a result, of which some half-dozen possible causes could easily be named. Can we, if we have imbibed even the rudiments of the scientific method, stop here? Does not our real work in diagnosis still lie before us? Have we not to seek out the cause on which the synovitis depends and drag it into the light in order to remove it? Only then does the old saying apply, *Felix qui potuit rerum cognoscere causas*. Yet how carefully must the search be pursued; how easily are we put off the scent! We all know that synovitis is often septic. But in a given instance we must proceed to a critical investigation as to the particular infection which it presents and we must be sure of our facts. I have repeatedly seen cases of synovitis of several months' duration in which gonorrhoeal infection was flatly denied, in which micturition was reported to be natural, and in which no appreciable gleet was present, but in which when a laboratory examination was made gonococci were found, when a catheter was passed stricture was detected, and where the synovitis cleared up when adequate urethral treatment was carried out. But if no gonococci are found there must be a further search for the cause, which may be discovered in a septic throat or even a carious tooth, some old sinus, or a cutaneous "boil." In short the scientific method will not allow us to stop short until the most complete knowledge that is attainable of every fact in the case has been secured—until we have made it as sure as we can that everything has been weighed and that nothing has been overlooked. May I not ask, again, have we not all had a feeling of chagrin, almost of terror, when, half an hour after giving an opinion in a case, we have suddenly become scientific and remembered something that was at the time overlooked? I remember a case in which after a great consultant, whose name we all revere, had diagnosed intercostal rheumatism and prescribed for it, the patient said, "By-the-bye, sir, they tell me I have got an aneurysm of my abdominal aorta"—a condition which on examination was obvious enough. How well does this case remind us how easy it is, if we do not think enough, to mistake referred pain for pain originating where it is felt.

It may be interesting to bring out the difference between positive and circumstantial evidence in medical practice by a few examples. But I may first remark that it is somewhat disturbing to find how frequently we have to depend on circumstantial evidence, for we must bear in mind that in medicine, as in the law, while (as I have already said) circumstantial evidence may be conclusive, it is yet often deceptive, even when it is pursued in the spirit of scientific thoroughness. Turning, however, to so common a case as that of fracture, we see that even within the compass of a single bone we must use for diagnosis now the one and now the other form of evidence.

In fracture of the shaft there is positive evidence: the bone is felt to be in two pieces, these two can be moved on each other, there is deformity, and often there is crepitus. But in the diagnosis of impacted fracture of the neck of this same bone this positive evidence is absent. There is no mobility of one part of the neck on the other; there is no deformity of the neck to be felt, for

the bone is here so deeply placed that its outline cannot be ascertained, and there is no crepitus. Circumstantial evidence, on the other hand, may, when it is carefully made out, be conclusive; that is to say, there are circumstances which cannot be explained except on the view that fracture has occurred. Thus, after a heavy fall on his trochanter, it may be observed that the patient, who was previously sound, has an inch or more of shortening of the limb; the trochanter is, for a like amount, above its normal position and less prominent than its fellow, and there is eversion. Or take the skull: In the vault the evidence of fracture may be positive—the fracture is often compound and visible to the eye, and the loose fragments can be moved upon each other. But in fracture of the base all is different. Neither abnormal movement, deformity, nor crepitus can be detected. Yet circumstantial evidence may clearly show that fracture is present. This is so when there is a large hæmorrhage from the ear followed by the prolonged escape of a considerable amount of watery fluid, and paralysis of the facial muscles on that side. The hæmorrhage and watery fluid are larger in amount than they could be unless a fracture allowed their escape from within the skull. Or take the diagnosis of a slipping semilunar cartilage in the knee. Occasionally there may be positive evidence, for during flexion and extension the cartilage may be clearly felt alternately receding and projecting in an abnormal degree, and tenderness on palpation may be detected. But in the majority of cases the evidence is wholly circumstantial. There is the history of repeated attacks of sudden pain, and perhaps locking of the knee, with sudden relief on some casual movement and a varying amount of subsequent synovitis. These attacks are all alike; always involving the same (generally the inner) part of the joint, they usually follow a twist or wrench, and they are separated by longer or shorter intervals in which the joint is apparently normal.

It is not long since we had to depend on circumstantial evidence in the diagnosis of renal calculus. Various phenomena might concur to render the presence of calculus extremely probable—pain in the loin, or extending to the testis, colic, hæmaturia, and urinary crystals; but no positive evidence could be obtained—the stone could be neither seen nor felt—and the case was a strong one to show how deceptive circumstantial evidence can be; for in the *post-mortem* room, or on operation, it was not seldom found that a wrong diagnosis had been made; either no stone was present, or instead of being in the kidney, it was in the ureter. Now the *x* rays afford positive evidence in at least a considerable number of cases, and every one rejoices over the change. It is, perhaps, too much to anticipate that increasing knowledge will ever supply us with positive evidence in all cases in which diagnosis has to be made, but means of avoiding mistakes have been greatly increased by the introduction of the *x*-ray method, since, in many departments of practice, it affords positive evidence and enables us to detect things or prove their absence by ocular demonstration. The ophthalmoscope, the laryngoscope, and the endoscope—and the microscope in diphtheria, tubercle, anthrax, etc.—are, of course, in the same class as the *x* rays; they are means by which diagnosis is rendered positive instead of remaining merely circumstantial, and they all alike add immeasurably to the accuracy with which diagnosis can be made. A further advance in the same direction lies in the use of surgical exploration. In numerous instances in which circumstantial evidence has been carried to its fullest limits a perilous degree of doubt may still remain. In such cases positive evidence in the form of direct inspection can be safely procured by an exploratory operation—for instance, in abdominal cases, the urinary bladder, and the joints.

To be thorough (or scientific), we must bear in mind that often both positive and circumstantial evidence are available in the same case, and we must compare their relative values. For instance, when we meet with an affection of the lip we can see it and examine it by touch, and thus obtain positive evidence as to its aspect and physical characters, and we may be led to suspect epithelioma. But we must not stop here. The scientific method impels us to look for further evidence; and if in our search we detect circumstantial evidence in the form of ulceration of the tonsils, a cutaneous rash, condylomata, or some other of

the many manifestations of early secondary syphilis, the diagnosis, governed by circumstantial evidence, has become clear—it is syphilis and not epithelioma.

Now, such a case as this shows us that, if we are scientifically minded and think enough, we shall not only check circumstantial by positive evidence, but we shall sometimes find ourselves reversing the process and checking the positive form of evidence by the circumstantial. For instance, a boy of 18 has an affection of the lip which is either a chancre or an epithelioma. Now, although the lesion may look and feel like an epithelioma, the circumstance that the patient is at an age at which epithelioma is practically unknown, although not conclusive, would preponderate in a very high degree. It would, in fact, compel us to go still further, and to resort to a positive test which we might otherwise, however wrongly, neglect, namely, a microscopic examination. Or, take a lesion on the upper lip. It may, to look at and handle, be much more like epithelioma than syphilis, yet the mere circumstance that it is on the upper lip, where epithelioma is extremely rare, would indicate that it was almost certainly not epitheliomatous but syphilitic. And then instead of at once excising it, we should examine a piece of it under the microscope, or ask potassium iodide for its verdict.

This form of circumstantial evidence comes under the head of what may be termed numerical probability, and in dealing with it we must remember that when the odds (if such a term may be used) are, say, 500 to 1 against a particular view we must not accept that view until it is established by positive proof—that is, in the case I have taken, by microscopic examination.

Sometimes circumstantial evidence may deceive us by suppressing the positive evidence upon which we are accustomed to rely. A man of 60 was admitted into St. Bartholomew's Hospital with the history that his femur had broken in its upper third as he was walking across his room. A skiagram showed a small growth at the seat of fracture. Now, in a man of 60 a secondary deposit from a carcinoma is much more likely than sarcoma. The House-Surgeon and I, therefore, made a careful search, but no primary carcinoma was anywhere to be found. The patient was very old for his years and very feeble, so he was ordered a fluid diet and left for further observation. He grew more weak and died in ten days. *Post-mortem* examination showed an annular carcinoma of the oesophagus with only moderate narrowing, and no ulceration. The growth in the femur was carcinomatous. The oesophageal disease remained undetected in this case, although all were on the alert, because not one of the usual evidences of carcinoma of the oesophagus was present. No difficulty in swallowing was manifested, for, owing to his general condition, he was given a fluid diet, and he could swallow fluids easily; thus there was no regurgitation, the growth caused little irritation, and he brought up neither mucus nor any blood streaks, and he complained of no pain. This case seems worthy (beyond my present purpose) to be recorded as one in which carcinoma of the oesophagus was, so to say, entirely occult.

And now, gentlemen, I will not detain you longer than to thank you for listening to remarks many of which, I am only too conscious, contain so little that is new that they must have made you impatient, and to others which some of you may have regarded as on the border line of impertinence, as they suggested defects from which you have long since freed yourselves. But, in truth, in what I have written I have been not so much reflecting on the shortcomings of others as confessing the faults which I have observed in myself in the practice of surgery.

REFERENCE.

¹ *Clinical Essays and Lectures*. J. and A. Churchill, 1902, p. 97 et seq.

A MEDICAL department for the higher medical education of women has recently been established at Kieff. There were more than 1,000 applicants, but there is at present room only for 350. The provision for practical teaching is very limited, and a subvention from the State is not to be looked for. The course of instruction is not sufficiently complete to entitle students to more than a certificate, which will not carry with it the right to practise. It is proposed in the meantime to create only such a teaching institute as the means at disposal will allow. The president is Dr. Alexander Muratoff, Professor of Midwifery and Gynaecology at Kieff.