uncertainty was removed, and I had less hesitation in exploring the temporo-sphenoidal lobe and dura mater covering it. The spot selected for this operation was just one inch and a quarter above and one inch and a quarter behind the centre of the cartilaginous meatus of the ear (Fig. 1 c), Reid's "base line" ($\alpha\alpha$), running from the lower border of the orbit through the centre of the auditory meatus being taken as the most convenient basis of measurement. This point lay upon the inferior posterior angle of the parietal bone, and over the posterior part of the middle temporo-sphenoidal convolution, as proved afterwards by photographs taken of an adult skull, and of the brain underlying it *in situ* (vide Fig. 2 c). This, I knew from expe-

Fig. 2.—Photographed from the same skull with the brain exposed *in situ*. The lettering is the same as in Fig. 1. The point at which the opening was made in the cerebral cortex (c) is observed to lie on the posterior part of the middle temporo-sphenoidal convolution.

rience in the dead-house, would bring me down upon the most usual seat of abscess in the temporo-sphenoidal lobe, and also of subdural abscess, should the pus lie along the squamoso-petrous suture. It would also be above the posterior branch of the middle meningeal artery. In this particular case, too, this spot had the advantage of being somewhat removed from the opening (x) previously made in the mastoid antrum, which was still discharging very fetid pus, and would be therefore very likely to contaminate the surface of the brain, if an opening were made at all near to it. That it was possible to prevent this is very encouraging.

A silver drainage-tube was used in this case for the cerebral abscess, as it was found easier to introduce and retain in position than a rubber one.

The size of the abscess was at least that of a sphere one inch and three-eighths in diameter. I find by experiment that a body of that size displaces as nearly as possible six drachms. Now four drachms and a half were actually received in a measure-glass from the cannula, and at least one drachm and a half to two drachms must have been evacuated after the latter was withdrawn. Probably the abscess contained nearly one ounce of pus. The latter was intensely fetid. In thrusting the needle into the brain, I aimed inwards, forwards and downwards from the point of entry. Travelling in this direction, the point of the needle would eventually have nearly struck the roof of the tympanum at a distance in the adult of about one inch and a quarter from the surface of the brain.

As the pus was reached at about half an inch from the surface of the brain, it was not necessary to go farther; but, in an adult, it would always be safe to go as far as an inch and a half from the point and in the direction given above; and, by doing so, it would be almost impossible to escape striking any collection of pus present in the whole area most liable to suppuration, that is, between the squamous petrous suture and the roof of the tympanum.

So far as I can learn after careful search—with the help of Dr. Neale's invaluable "*Digest*," the *Centralblatt f. Chirurgie* from its commencement until now, Schmidt's *Jahrbuch* for the last two years, and Virchow and Hirsch's *Jahrbuch* for the same time—this appears to be the first case in which a cerebral abscess, due to tympanic suppuration, has been correctly diagnosed, localised, and evacuated by operation with complete success. Traumatic abscess of the brain has often been treated successfully by operation, but it belongs to a totally different category to the disease in question. If following fracture, whether simple or compound, it is easily diagnosed and localised; if

due to a blow without fracture, it may not be quite so easy to localise; but, at all events, if an error be made, the processes about it are not septic. But, in the above case, not only was there a difficulty in diagnosis and localisation, but the risks of operation were enormously enhanced by the intense foulness of the whole suppurative process in the temporal region; and the fact that a fetid septic abscess had to be opened and drained through a healthy portion of brain and the unaffected arachnoid space. That this should have been possible is extremely encouraging. The figures I have drawn from photographs kindly made of my own dissections of an adult skull and brain *in situ* by my friend Mr. Marriott. To insure accuracy, all measurements and markings were made on the parts before being photographed.

NOTE.—December 4th. Patient returned from the Convalescent Home to-day. He suffers no pain of any kind, and appears in excellent health, and is fat. Dr. Gowers reports that the optic neuritis has not yet quite disappeared, being practically in the same condition as when the patient left the hospital. Both wounds behind the ear are completely closed, and there is only a trace of moisture in the deeper part of the ear.

EXPERIMENTAL RESEARCHES CONCERNING PASTEUR'S PROPHYLACTIC.¹

By T. WHITESIDE HIME, A.B., M.B.DUB.,
Medical Officer of Health, Bradford.

DR. HIME referred, at the commencement of his paper, to the difficulties which were placed in the way of scientific research in this country in all that concerned the life and well-being of man and the higher animals. Fortunately there was no difficulty in pursuing such researches abroad. For some months he had been, in Paris, engaged in studying Pasteur's method, and availing himself of the extreme kindness and generosity of the Professor in following the practice of his laboratory. The affability with which Pasteur received all comers, and the candour and want of reserve with which he explained and demonstrated his methods to all earnest inquirers, were in his experience unequalled, and worthy of imitation everywhere. It was unavoidable that the subject of rabies should be unfamiliar to the vast majority. It was not given to every man to have a supply of the virus at his disposal. The number of cases and deaths from rabies in this country was comparatively so small, that few medical men had ever seen one, and Pasteur's method of treatment was still unfamiliar to the general public, except through unscientific reports in the daily papers. Hence to some extent the attitude of the profession in this country, which had preserved a cold silence on the subject. Hence also the boldness of the few critics who had ventured to oppose Pasteur. One of these was a lady, who felt her belief in the existence and goodness of a Deity shaken by Pasteur's success. Spitzka, of New York, was another critic. His familiarity with Pasteur's method was strangely illustrated by his request, published in the *BRITISH MEDICAL JOURNAL*, to be informed what were the symptoms of rabies in the rabbit; while his experimental skill in the practice of the method was shown by his statement that it is "a very difficult matter to keep any rabbit inoculated with fresh brain material by trephining alive" (see *JOURNAL*, Oct. 30, 1886). Dr. Hime could assert, from personal knowledge, that there was no more difficulty in keeping a rabbit alive in such circumstances than in keeping an infant alive after vaccination. The interference with physiological function in the former case, if the operation were done by an operator skilled in the method, was nothing like so marked as in the latter, there being no fever, no loss of appetite, no restlessness, and no local symptoms. Another critic in this country had shown himself possessed of more dialectic skill, but he, too, was entirely ignorant of the practice of Pasteur's method; he had never seen the practice in Pasteur's laboratory; he had never seen any other person perform the inoculation; he had never done it himself, and had never seen either a living or dead rabbit which had been so inoculated; yet he could criticise the method, and take Pasteur to task (without a shadow of reason) for not performing control experiments. Having devoted himself to the study of the question for months in Paris, and performed numerous experiments, Dr. Hime related to the Society some which he desired to be regarded as types of many others having the same result. He had selected them because they were, he thought, important in themselves; and, further, they met the objections raised against Pasteur's method in this country. The facts which he would detail were all within his own knowledge.

That the disease in the rabbit from which Pasteur obtained his

¹ Abstract of a paper read at the Pathological Society on December 7th, 1886.

material for human inoculations was true rabies was indubitable. Nerve matter taken from such a rabbit and inoculated on a dog intracranially—the only certain method—invariably produced true rabies. If nerve matter be taken from a human being dead from an attack of canine rabies, and be inoculated on a rabbit, a guinea-pig, and a dog, all three will take rabies and die. The dog will have rabies according to his kind, with symptoms either of the dumb or furious character. The guinea-pig will exhibit very characteristic symptoms, and the rabbit will develop the symptoms peculiar to its kind—such as, for convenience, may be called the “Pasteurian disease.”² But an interesting and important fact must be borne in mind. The virus used for the experiment, being canine virus which had passed once through the human subject, retained certain canine peculiarities as to period of incubation and date of death. If instead of human nerve-tissue from persons who have died of canine rabies nerve-matter direct from a dog which has died of rabies was employed for inoculation, the animal would be similarly affected, and the disease produced in the rabbit could not be distinguished from that caused by inoculation with nerve-matter taken from a human being who had died of rabies after inoculation with canine virus. If, again, nerve matter were used taken from one of the rabbits employed by Pasteur for protective inoculation, the course of the disease in the various animals would be shorter. Pasteur used for his protective inoculation virus which, originating in ordinary canine rabies, had been passed directly from rabbit to rabbit in some hundreds of cases. The result was that the virus had become intensified, and killed more rapidly than the original canine virus.

It was alleged at one time that the deaths following Pasteur's inoculations were due to them. This was triumphantly disproved by the experimental demonstration that the virus in the deceased was canine-virus (derived from the original bite) and not rabbit-virus inoculated by Pasteur. The opposite result was obtained if the virus were passed from monkey to monkey through a long series; the virus became weaker and weaker. But, no doubt, just as the virus of anthrax or fowl-cholera, which had been modified by cultivation, could be brought back to its primitive virulence, so the weakened rabic virus of the ape, and the intensified rabic virus of the rabbit, could be brought back to their original degree of virulence. If Pasteurian virus were passed through the dog once, and then inoculated on a rabbit, it would be found not to have resumed canine characteristics by passing once through the dog. But more frequent passages through the dog would probably restore its primitive characteristics. The symptoms in the rabbit inoculated with Pasteurian virus were special; the period of incubation (five to six days) and the date of death (nine to ten days) were more invariable than in any other disease of man or animal known to Dr. Hime. Further, the symptoms were very characteristic and unchanging, notably the slowness of the general symptoms, even after paralysis had become marked. The temperature³ also was very interesting, and markedly different from that found after intracranial inoculation with other materials than the Pasteurian virus or rabic virus from some other source. The following is a characteristic chart of the temperature of a rabbit which died of the Pasteurian rabies:—

First day ⁴	102.6° Fahr.	Sixth day	105.6° Fahr.
	102.2° „	Seventh day	105.8° „
Second day	103.0° „	Eighth day	106.4° „
Third day	102.8° „	Ninth day	104.1° „
Fourth day	103.0° „	Tenth day	97.0° „
Fifth day	104.2° „	Eleventh day	86.0° ⁵ „

The febrile stage might be shorter, and the fall more gradual, but the latter was always great. The elevation was not always so great as in this example, but the curve was characteristic, and the temperature usually gave indications before the actual symptoms appeared. The cause of the temperature in the rabid dog was similar. At first there was no change, then the temperature rose rapidly, and might reach 105° or 106°; the period of acme lasted one or two days, and was succeeded by a very rapid fall to about 84.6°, when death ensued. The fact that the disease was always and invariably transmissible by inoculation was of great importance as regards its pathology, and as distinguishing it from other diseases.

With regard to *post mortem* appearances in the Pasteurian rabbit,

² Dr. Hime stated that he used this term for brevity's sake, without special reference to the fact that the period of incubation and the period elapsing before death had been shortened in the rabbits used by Pasteur for preventive inoculations. This shortening of these periods did not alter the nature of the disease, any more than the nature of the virus in a case of typhoid or scarlet fever was altered because in particular conditions it might kill in ten or three days respectively, instead of in twenty or in six days.

³ The normal temperature of the rabbit and the dog is about 102° F.

⁴ Immediately before inoculation. ⁵ Death.

there was nothing pathognomonic, any more than in the dog or the human subject dead from rabies; all the organs appeared free from disease; the brain was slightly hyperemic as a rule, the heart contracted, and the bladder generally full. The stomach almost invariably contained food; the cæcum was also generally full, and the remainder of the intestine commonly contained but little; the small intestine being most frequently empty. This absence of morbid conditions was in itself remarkable, and was certainly not found after death from injury to the brain in the operation, nor after death from intracranial injection of putrid material. In this case, pus was found within the cranium, and other evidences of purulent absorption were detected. It was alleged by Dr. Dolan (in the BRITISH MEDICAL JOURNAL, September 4th, 1886, p. 475, line 15 from the bottom) that the inoculations were made “into the rabbit's medulla,” and that “the same kind of affection in rabbits could be produced by injecting almost any kind of diseased material into the same region.” The injection was not made into the medulla, nor into any part of the nervous system. It was made into the space beneath the dura mater and pia mater, and the brain was not injured nor touched by the instrument in a successful operation. The medulla, indeed, could not be reached at all (as suggested by Dr. Dolan) from the site of the inoculation, without traversing almost the entire mass of brain. But even if the brain were injured (and it certainly was not, as specimens exhibited proved⁶), mechanical lesions of the brain or medulla did not produce a specific contagious disease indefinitely transmissible by inoculation, and presenting the symptoms of the Pasteurian disease. These were certainly not the symptoms of injury to the brain or of any part of the nervous system. The allegation that injection of “almost any other kind of diseased material” would produce the symptoms could only be disproved by experiment. Intracranial injection of sterilised bouillon did not produce the symptoms of the Pasteurian disease, nor any other morbid symptom, and rabbits so injected did not die if the operation were skilfully performed. Rabbits inoculated according to Pasteur's method with sterilised bouillon, rubbed up with putrid meat, died with symptoms entirely different from those of the Pasteurian disease, and without the characteristic paralysis and temperature. But still more noteworthy was the fact that the disease produced by injecting putrid material was not transmissible, while the Pasteurian disease was always transmissible by intracranial injection. If the same bouillon as was used in these operations, rubbed up with fresh brain matter from a healthy dog, was injected into a rabbit according to Pasteur's method, the rabbit did not die, notwithstanding Spitzka's statement to the contrary, and did not exhibit one symptom of the rabbit inoculated with Pasteur's virus, which always died. Intentional lesion of the rabbit's brain in the performance of a Pasteurian operation, when nothing was injected, did not necessarily cause death, and if it did the symptoms were utterly different from those of the rabbit which had died of the Pasteurian disease or from putrid infection, and inoculations made from such a rabbit did not cause death. Nerve-matter from dogs which had been suspected of rabies, and had died, or been killed in consequence, had frequently been injected intra-cranially in rabbits as a diagnostic. If the dogs were rabid, the Pasteurian disease developed in the rabbits; if not, the rabbits survived without showing any symptoms of disease. This crucial method of diagnosis was due to Pasteur, but it could not be applied so as to settle every doubtful case in England, owing to existing legislation.

The virus used by Pasteur for protective inoculations originated in the rabid dog, and was the virus of rabies. Pasteur had published results showing that the material used for his later protective inoculations of man caused rabies in animals which were unprotected. If virus be taken from a rabid dog and be passed through a series of rabbits, it produced the same disease as was developed in the rabbits from which Pasteur took his matter for preventive inoculations. If the virus from such rabbits were inoculated back into the dog or other susceptible animal, it produced true rabies in them, and if then passed back to the rabbit it reproduced the same symptoms. Human rabies, if inoculated on the rabbit, produced the same symptoms, with such variations only as were due to its origination in the dog, cat, or other animal. These variations were exhibited chiefly in the length of the period of incubation, and the period elapsing before death, the disease invariably terminating fatally. No other known disease produced the same results, nor could the symptoms be caused either by surgical brain lesion or the intra-cranial injection of inert

⁶ One was a rabbit's skull and brain injected, after trephining, according to Pasteur's method, with a coloured solution of gelatine. The gelatine was seen solidified on the surface of the pia mater, the brain being intact, the whole being a distinct demonstration of the method of performing Pasteur's operation. Another interesting specimen exhibited by Dr. Hime was a fresh preparation of a rabbit's brain inoculated in Pasteur's laboratory on December 4th. The skull-cap was removed, showing the brain free from the slightest injury.

material such as sterilised bouillon, or with putrid material. The Pasteurian disease was specific, and very distinctly characterised by its course and symptoms, which were different from those of any other disease; it invariably ended fatally, and was transmissible by inoculation from animal to animal. Dr. Hime did not give the statistics with regard to the success of Pasteur's treatment, but stated that the protection afforded to dogs by Pasteur was proved beyond doubt or question, and analogy would suggest that the same method was applicable to man. Dogs could be protected either by inoculation before infection or afterwards. Further, the protection afforded was against a virus far more active than ordinary canine virus. The reality of this protection established the truth of Pasteur's principle. That his practice was capable of improvement was not impossible; but he had already effected a great deal in a short time. It had been objected that the inoculated prophylactic virus was inert because the patients showed no specific symptoms, but the objection had no weight; it might as well be said that arsenic was inert because a patient taking that drug showed no toxic symptoms. Dr. Hime was inclined to think that there was a decided though slight reaction. He himself had experienced local symptoms and distinct axillary tenderness while under treatment, and in others these were more marked. He had made numerous thermometric observations on persons under treatment, but, so far, could not assert that there was any characteristic thermal change. Some, while admitting that protection had been successful in cases of slight wounds, thought Pasteur's system ineffectual against severe wounds. These persons admitted that the principle of the method was scientific and sound, but found, they thought, a weak point in detail. Finally, from the fact of the undoubtedly low mortality among Pasteur's patients, corroborative evidence was obtained. The rabid condition of the animals in a very large number of these cases was proved either by experimental inoculation or by certificates from veterinary surgeons, the statistics included no doubtful cases, these being purposely omitted. Dr. Hime expressed the opinion that Pasteur had established a true prophylactic treatment against rabies. The establishment of a Pasteur Institute in this country, he thought, was called for; such institutions were already established in Paris, Moscow, St. Petersburg, Odessa, Samara, Varsovia, Naples, and Milan. Turin and New York would soon be provided with such institutions. The English public were still vainly waiting for the report of the Royal Commission of inquiry into this subject. While this report was being prepared other countries were acting, and we were satisfied to send our patients to Paris for treatment, instead of making provision for them in England.

A NEW POSITION FOR OPHTHALMIC OPERATIONS.

By G. M. GILES, M.B., F.R.C.S., Surgeon, Indian Medical Service.

THE supine position is, as most must have felt, by no means a convenient one for eye-operations. It is, however, the only practicable one when chloroform is used as an anæsthetic. The discovery, however, of a local anæsthetic, in the shape of cucaïne, leaves us free to adopt any position that may be found most convenient for patient and operator.

For the last fifteen months I have been employed with an exploring party in the regions lying beyond our North-West Indian frontier. Transport difficulties have rendered it necessary for us to fly very light. Hence, as may be well imagined, an operating couch formed no part of my equipment. A good deal of surgery of all sorts came to my hands, much of it ophthalmic, and I was often at a loss how to improvise a couch of some sort. Not unfrequently even the low country bedstead was unobtainable, so that one had to operate on the ground. Now, to operate even on a low bed is most difficult in ophthalmic cases, the surgeon's position being so constrained that all steadiness of hand is lost. Hence, after adopting cucaïne, which I came to do very early in our travels, I began to try various positions with the view of finding one suitable to the peculiar exigencies of camp life, and finally hit upon the following position, which has been found so entirely convenient that I feel sure it is well worth a trial, not only by such as have to operate under difficulties, but even as a position of election.

The operator sits on an ordinary chair, with the knees well separated so that the patient may be able to sit on the ground between his feet. The position of the latter varies according to the nature of the operation and the side operated on. In ordinary cases, such as iridectomy, removal of pterygia, etc., he is seated with his back nearly to the operator, but with the face turned slightly to the

left for the left eye, or to the right for the right. Seated thus, between the operator's feet, he throws back the head so that the occiput rests firmly on the surgeon's thigh; the left thigh in operations on the right eye, and *vice versa*. This position answers equally well for cataract-extractions on the right side. For left cataract, however, unless the operator actually prefers to use the left hand, the following modification is necessary. Seated, as before, on the ground, between the operator's feet, he faces quite to the surgeon's left, then throwing back his head, and turning it slightly so that the chin points to the surgeon's hip-joint, he rests the occiput firmly on the operator's right knee. With the patient in this posture, it will be found perfectly easy to operate with the right hand.

The method is, no doubt, specially suited for dealing with Orientals, who are accustomed to sit on the ground; but for the limited time required for an operation, I do not think it will be found too constrained for Europeans. Primitive as it may seem, I feel sure that anyone who will give it a fair trial will find the position much more convenient than either an ordinary operating-couch or a dental chair.

The illustration shows the relative position of operator and patient in a cataract-operation on the left eye.

LOOSE CARTILAGES IN THE KNEE-JOINT.

By WILLIAM J. SHAW, M.B., F.F.P.S.G.,
Assistant-Surgeon, Glasgow Royal Infirmary.

W. W., aged 27, was admitted into Glasgow Royal Infirmary, September 6th, 1886, suffering from loose bodies in the knee-joint. His occupation was that of a moulder, which he had followed for eleven years, during which time he has had to work almost continually on his knees. Twelve months ago, his attention was drawn to his knee-joint, in which he felt a very severe pain, or, as he described it, "a kind of grip." This pain increasing in severity, he requested admission to the hospital for the purpose of getting something done, as neither by night nor day had he relief from the excessive agony he was suffering. Examination in hospital showed that the right knee-joint was very much swollen as compared with that of the left side; this swelling being due to the presence of a number of loose bodies of considerable size, one of them being nearly as large as the patella itself, and all being very freely movable from one part of the joint to