operation, so far as Liverpool practice was concerned, was originated by Dr. Wm. Alexander, and had also been extensively performed in Germany. By opening into the peritoneum and severing the lateral attachments of the bowel, the latter could be drawn down extensively, permitting the easy removal of disease high up, if also confined to the bowel, and not implicating adjacent parts. He himself was disposed to view these procedures hopefully, his own few cases having taught him enough to encourage a further employment of these procedures. He was much impressed by Mr. Jessop's allusion to the rarity of obstruction in disease low down. This opened up the question how best to avoid the necessity of colotomy; this was often achieved by an intelligent dieting of the patient and attention to the state of the bowels by enemata, to which he would draw attention as a point of practical use.

XI.—J. INGLIS-PARSONS, M.D.,

Assistant Physician Chelsea Hospital for Women.

DR. INGLIS PARSONS asked some of those present to try his method of treating cancer with a powerful interrupted voltaic current. A year had now passed since the first case was operated upon, and the patient remained well. The diagnosis in this case was made by Mr. Bryant, Mr. Jacobson, and others. As a guarantee of his convictions he had offered to treat some test cases at the Middlesex Hospital and also at the Cancer Hospital, but permission was refused; both institutions, however, intended to try the treatment. The high mortality and unsatisfactory results of treatment by the knife gave very good grounds for trying electricity, especially in those cases where the disease grew towards the urethra or bladder. An electrode could be introduced into these cavities and a second into the rectum in contact with the growth, and the current passed between these two points. It was also far easier by this means to reach the outlying groups of cells, and there was thus a greater probability of arriving at a satisfactory result.

XII.—PROFESSOR TRENDELENBURG, Bonn.

PROFESSOR TRENDELENBURG desired to state that he could not accept what Mr. Cripps had said about German surgery in general. He drew attention to the resection of the os coccygis and the lower part of the sacrum, as recommended by Kraske. It gave a very free access to the higher parts of the rectum, and if the cancer had not yet infiltrated the surrounding tissues it was easy to isolate it from the wound by the fingers, and to pull it down for a great extent. In some cases the lowest part of the rectum might be left quite untouched, and thus a kind of resection of the rectum might be accomplished.

XIII.—A. F. MCGILL, F.R.C.S.,

Professor of Surgery in the Yorkshire College.

MR. MCGILL remarked that it was most desirable to prevent the passage of faecal matter below an artificial anus. With this idea he had in three cases substituted colectomy for colotomy in the lumbar region. The first case was performed for prolapse of the mucous membrane several months after the primary operation. Four inches of colon were removed, the upper end of the lower portion of gut was closed by sutures, and the lower end of the upper portion attached to the skin; the result was in every way perfectly satisfactory. In two other cases where the operation was primary a fatal result occurred. The cause was the same in both cases. The faecal material situated in the gut between the colotomy wound and the disease in the rectum, being unable to escape, had induced a semi-gangrenous condition of the upper end of the gut; faecal extravasation had taken place, and general peritonitis was thus induced. Mr. McGill thought that it was unwise to repeat an operation of this description unless the upper end of the lower portion of the gut was left open.

XIV.—R. LAWFORD KNAGGS, M.B., F.R.C.S., Huddersfield.

MR. KNAGGS drew attention to a pathological condition which was sometimes present when there was obstruction in the upper portion of the large bowel. The distension having overcome the patency of the ileo-cæcal valve, there was a tendency for the adjoining portion of the ileum to run straight across to the left iliac region, the pressure of the distension causing the obliteration of the except the unavoidable angles, just as the fingers of a glove

might be blown out straight. This was a point of importance in favour of the inguinal operation; for if the diagnosis as to the seat of the obstruction was wrong, and it was necessary to open the small intestine, the probability was that the small intestine would be opened within a few inches of the ileo-cæcal valve. He also drew attention to the probability that "short circuiting" the intestines, after the method of Professor Senn, would be applicable in certain cases of malignant disease of the large bowel, and that would do away with the discomforts of an artificial anus.

XV.—T. SYMPSON, F.R.C.S., Surgeon Lincoln County Hospital.

In support of the idea that the progress of cancer of the rectum was favourably modified by diversion of the fæces from passing over the diseased surface, Mr. Sympson adduced the case of a woman who was under his care in the hospital; on admission she was in a wretched condition through extreme pain, profuse uterine hæmorrhage, diarrhoea, and passage of fæculent matter through the vagina. A year after the operation of lumbar colotomy she was found to be free from pain, free from hæmorrhage, and to have gained a large amount of flesh. She lived between two and three years after the operation, and died from a recurrence of hæmorrhage. In other cases, in order to obviate the chance of fæces entering the lower portion of the rectum, Mr. Sympson had divided the bowel, and stitched its edges together.

NOTES ON AN EPIZOÖTIC OF RABIES; AND ON A PERSONAL EXPERIENCE OF M. PASTEUR'S TREATMENT.

By J. G. ADAMI, M.A., M.B.,

Demonstrator of Pathology in the University of Cambridge.
(From the Cambridge Pathological Laboratory.)

TOWARDS the end of July last I was consulted by Dr. C. Scott Kilner, the medical officer of health for the Bury St. Edmunds district, concerning a disease which was rapidly carrying off the fallow deer at Ickworth Park, the seat of the Marquis of Bristol. The disease was supposed to be anthrax, and, as at that time some fresh anthrax material was wanted for our laboratory work, I gladly availed myself of the opportunity to visit Ickworth along with Dr. Kilner, in order to make a *post-mortem* examination upon one of the dead deer, consent having previously been obtained from Lord Bristol's agent.

From the keepers and others at Ickworth I learned that it was only through the chance discovery of an unusual number of dead deer lying in the thick bracken that suspicions were aroused as to the presence of any disease. Living in a half wild condition, deer do not lend themselves readily to accurate and continuous observation. When any member of the herd is ailing it leaves or is driven away from the rest of its fellows, and, seeking some retired spot, dies in hiding. It is impossible, therefore, to state with any exactitude the time of onset of the outbreak.

From the beginning of July up to the 20th of the month twenty-three dead bodies were discovered; during the next seven days, with more careful search of the whole park, ninety-two were found and buried; and now for the first time symptoms of illness were observed each day in a certain number of the does and young bucks (pricketts). The fact that during this first period of the epidemic only does and young bucks were affected is worthy of note, although capable of easy explanation. For the greater part of the year the adult bucks keep themselves in separate herds away from the does, often, indeed, occupying a different region of any park. Accompanying the does are the pricketts, or young bucks, which have not completed their second year, and the haviours or castrated bucks. To this fact is probably largely due the difficulty of arriving immediately at a correct diagnosis of the true nature of the epidemic, or rather

epizootic. The symptoms of the disease which I was able to gather on my first visit were of a very indecisive nature. All that could be learned was that, leaving the herd, the animal affected—a doe, or fawn, or young buck—appeared uneasy and distressed, wandered about aimlessly, at times essaying a run, and then suddenly stopping short. As the disease progressed the gait became staggering, the hind quarters appearing specially weak, and finally the animal dragged itself into the bracken to die. Death occurred, as a rule, within thirty hours after leaving the herd.

Upon making a necropsy the blood was found dark and almost black, remaining liquid; the liver and kidneys were congested; the lungs unaffected; the spleen somewhat larger than normal, dark, almost black in colour, and very pulpy. Decomposition apparently set in with great rapidity. Incomplete though the *post-mortem* examination was (being made in the open during a heavy shower), all these symptoms and appearances seemed not opposed to the idea that the disease was anthrax. I noticed at the time that the skin was not so congested as is usually the case in this disease, but considered that this symptom might be specially marked only in cattle suffering from anthrax.

Returning to Cambridge, in the full expectation of finding anthrax bacilli in the portions of spleen, liver, and kidney, that I had carried away with me, I was greatly surprised at being unable to discover in any part a single typical bacillus, notwithstanding the greatest care in the preparation of the specimens. Nor were the attempted cultivations any more successful. There were several growths of putrefactive micro-organisms, none of the anthrax bacillus. Soon after this I learned that having obtained evidence of the presence of characteristic bacilli, the Privy Council had recognised the disease as anthrax, and that the park had been officially declared an infected area under the Contagious Diseases (Animals) Act. This fact led me to conclude that I had perhaps been mistaken in my conclusions concerning the disease; I therefore thought it desirable to make further inquiries into the subject. In the meantime, in answer to certain questions addressed to Mr. Turner, Lord Bristol's agent, I obtained fuller information about the symptoms that had so far been noted. I learned, namely, that the keeper had observed the affected animals during the early stage of the disease to throw their heads back in a curious manner, to bite their shoulders, legs, and sides very frequently, at times inflicting severe wounds upon themselves; to jump up (rarely) and bite at the boughs of trees. Further, the gait might be described as becoming more and more "groggy," until at last the animal fell down—to rise again, proceed onwards and again fall—the hind limbs becoming increasingly parietic, until at last the animal died, death being very often preceded by convulsions. These symptoms are not those of anthrax. The possibility of the disease being hydrophobia having naturally occurred to me it appeared desirable to make further necropsies.

On August 8th, with the aid of Professor Roy and in the presence of Dr. Kilner and Messrs Johnson, senior and junior, the veterinary surgeons in charge of the diseased animals, I made two more *post-mortem* examinations at Ickworth upon a full-grown doe and a haviour respectively. Nothing very abnormal was found beyond what was seen in the first necropsy. We brought back to the laboratory sealed pipettes containing spleen juice and blood from the heart, which had been filled with the usual precautions, as well as gelatine test tubes inoculated upon the spot. We also preserved the brains and cords of the two animals.

On microscopic examination of the blood and spleen juice no bacilli were discoverable. No traces of the bacilli showed themselves in any of the gelatine tubes, nor again in the tissues could any typical anthrax appearances be made out. It may be mentioned in passing that during the course of the second of these two *post-mortem* examinations I gave myself a slight cut upon the right index finger with a knife which had been used to incise the brain and cord of the first animal. To this matter I shall shortly return.

To obtain proof positive of the nature of the disease, Professor Roy, to whom I have throughout been indebted for much kindly counsel and assistance, willingly made the necessary inoculations. His investigations, a report of which is appended¹, proved to us

conclusively that the disease from which the animals died was not anthrax, but the paralytic form of rabies.

But if further evidence were wanting, we obtained it towards the middle of August. The epizootic, now no longer confined to the does, haviours, and prickets, began to manifest itself among the adult bucks, and we received information that they had been observed attacking each other, and butting violently at trees. One animal was observed to chase a hare, another angrily snatching at and biting a large stone, while a third rushed upon one of the assistant keepers, who happily escaped. Again, it is to be noted that, although a large herd of sheep and numerous ponies had shared the deer park until as late as July 20th, and one of the animals had been observed licking a dead deer, up to the present moment no symptom of a similar disease has evidenced itself among them. Were we dealing with anthrax, this immunity would be fitted to cause surprise.

Rabies among deer had been almost wholly unknown until Mr. Cope published last year his excellent report upon the outbreak at Richmond Park. The manifestations at Ickworth agree in all important points with those described by him save that I have never been able to observe or obtain a description of the removal of the hair from the forehead, as a result of continued forcible rubbing of the head against trees and posts. Ascending paralysis would seem here to be more frequent and to be specially common among the does, while it would seem to be mainly in the adult bucks that what may be termed the typical symptoms of rabies, accompanied by great nervous excitation, make themselves evident. At Ickworth also the disease has spread to all the separate herds of deer, and we have the interesting though lamentable sight of rabies, not in a series of isolated cases, but as a well-marked epizootic. Since the disease was first observed, less than three months ago, over 450 animals have died in a herd of between six and seven hundred animals—a terrible mortality, and one very much greater than obtained at Richmond.

So long ago as 1881 Galtier recorded some cases in which by the intravenous injection of the emulsified cord of a rabid animal he conferred immunity upon sheep and dogs. More recently Nocard and Roux² have confirmed Galtier's results, and have shown that it is possible, by a single injection of a fairly large quantity of a thick emulsion of "rabie" medulla, to render sheep and cattle immune. It would be useful, not to say interesting, to determine whether by following this method some portion at least of what remains of the herd at Ickworth could be saved. But, up to the present, I have not been able to obtain Lord Bristol's permission to attempt anything of this nature.

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The cut upon the finger which I gave myself while making the *post-mortem* examination on August 8th was allowed to bleed freely, and then washed well for some minutes with carbolic acid solution (1 in 20). It healed slowly, but without any suppuration, and I was disposed to think very little about it, especially so long as there was uncertainty as to the diagnosis of the disease. There have been fairly numerous cases in which practitioners have wounded themselves in the course of necropsies upon those dead from rabies, and, almost without exception, no ill results have ensued. Here, for some time after the wound had been inflicted, there remained considerable doubt as to the nature of the disease. When, finally, the diagnosis of rabies seemed perfectly certain, medical friends insisted strongly upon the fact that the knife by which I had cut myself had been used previously to dissect the brain and spinal cord of a rabid animal. Hence, seven-teen days after the event, as the result of earnest solicitations from Professor Roy and others, I found myself beginning the treatment at the Pasteur Institute. From M. Pasteur, Dr. Roux,

trepan hole through the vertex of the cranium. The wounds healed by first intention. On August 27th this rabbit, which, till that date, had not shown any symptoms of disease, was found to be parietic, the weakness being recognisable chiefly in the hind limbs. It screamed for some time after being simply lifted on to a table, although this was done gently enough. On August 28th the paresis was much more evident. On August 29th, in the morning, the rabbit was, so far as could be seen, completely paralysed, with the exception of the respiratory muscles. It died on the afternoon of the same day. *Post-mortem* examination showed no noteworthy abnormality, although the brain and spinal cord were very carefully examined. On August 30th I inoculated two other rabbits subdurally from the medulla of the one which had died on the previous day. On September 15th one of these showed the characteristic paresis, and it died on September 16th with symptoms identical with those of the rabbit from which it was inoculated. The second of the two rabbits inoculated showed paresis on the 16th and died on the 17th September. The conclusion is, of course, that the deer from which these animals was inoculated died of rabie s and not of anthrax.

² *Annales de l'Institut Pasteur*, 1888, p. 311.

¹ NOTE BY PROFESSOR ROY.—The disease in question was officially declared to be anthrax, but in accordance with Mr. Adams's views on the subject it was decided that a rabbit should be inoculated from the brain of one of the deer at the same time that some of the blood of the dead fallow doe was introduced under the skin of one of the thighs. This was done by me on August 9th, 1889. A portion of the medulla having been rubbed up in a clean mortar with a small quantity of sterile decoction of meat, a small quantity of the mixture was introduced under the dura mater, which had been exposed by making a small

and every member of the staff I received the greatest kindness, and I was freely permitted to examine into every detail of the method. Dr. Ruffer's address to the Association at Leeds was so full and convincing that little is left to be said with regard to M. Pasteur's method. I cannot refrain, however, from recording my testimony to the extreme care that is taken to ensure accuracy in the treatment and security to the patients from any untoward results, and my admiration for the wonderful precision that regulates each step in the preparation and inoculation of the virus. For the benefit of those who in the course of their practice are called upon to deal with cases of bites from animals supposed to be rabid, it is perhaps wise to emphasise the fact that, as Dr. Roux has shown, the freshly removed brain and cord of rabid animals may be preserved for long periods, without loss of virulence, in pure neutral, or rather non-acid, glycerine; and that, sending specimens so preserved to the Pasteur Institute, it can there with certainty be determined whether a given animal was or was not rabid.

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In concluding these notes it may be as well to mention a train of symptoms of a somewhat ambiguous nature experienced by me. I do this with no little diffidence; yet so few cases of like nature have been placed on record that I feel it wise not to pass the matter by unnoticed. Within a day or two after the cut I became aware of a sensation in the forearm of the injured extremity, by no means acute enough to be termed a pain, but rather akin to the feeling one obtains in a limb after excessive muscular exercise. In time the same sensation made itself evident along the inner side of the arm. Considering this due to the irritation of some small nerve at the seat of the wound, I paid very little attention to it. It persisted, however, for some weeks, in fact throughout the period of treatment at Paris. From about the twenty-sixth day I woke regularly in the early morning, to find the limb in a state of "pins and needles." The condition lasted for several minutes; then disappearing, it permitted me to fall asleep again. Rarely was one of the other extremities affected, and it was only after the phenomenon had occurred without fail for more than a week that I called to mind that it might be allied to the "tinglings" described as a premonitory symptom of rabies. From September 5th onwards—I had wounded myself on August 8th—I began to suffer from insomnia, not falling off to sleep until between 1 and 4 A.M., and from now I became more and more depressed. The depression was completely in spite of myself. I had no reason to doubt the efficacy of the treatment that I was undergoing, and looked upon it, not as necessary, but as precautionary, and as, on the whole, a pleasant experience. From September 8th this depression was constant; the dull heavy sensation in my arm became more marked, and soon I experienced great distaste for being alone for any length of time. On the 10th, while making my homeward journey, a tightening or constant slight constriction of the throat made itself felt. These symptoms became more and more strongly marked until the 14th, when for one short moment I found I could not swallow the saliva. That night, however, I fell asleep fairly soon; the next morning the depression had passed away, and although during the next few days I at times experienced some slight tightness of the throat, and awoke on some mornings to find my arm in the state of "pins and needles," I very soon felt myself in a state of good health, the last symptom to disappear being the heavy sensation in the arm. Taken altogether, the symptoms were at the worst not very prominent; they were, however, most decidedly uncomfortable.

What is the interpretation of these symptoms? On the whole I am inclined to believe them of nervous origin, pseudo-hydrophobic. In favour of this view it may be pointed out that certain signs of rabies which are generally well marked, such as for instance pain at the seat of the wound, were characteristically absent; other signs, for example, the tingling in the extremities were incomplete in their manifestations, and though present might be otherwise explained. But, on the other hand, not a little can be said against the purely nervous theory. I am of robust health, sound constitution, and in temperament should describe myself as the very reverse of nervous. My belief in the efficacy of the Pasteur treatment is and was throughout complete. I knew at the time that, having been inoculated, the chances were even more than ninety-nine to one that I should not be attacked. So slight did I consider my danger, that on August 22nd, but for the urgent entreaties of Professor Roy, I should have started for the Broads, instead of for Paris; and I fully acquiesced with the authorities at the Pasteur Institute, when upon the thirteenth day of treatment

they informed me that they considered my case such that they did not think it necessary to keep me for the usual fifteen days of treatment.

If indeed I passed through a most mild abortive attack of rabies, then certainly that attack was due to the cut I gave myself, and not to the treatment I underwent. The time of the first appearance of the symptoms, namely, about the twenty-sixth day after the cut, would show this. The pain, if pain it may be called, in the injured extremity only, while the region of inoculation was completely painless, would evidence the same, while the fact that it was not until the seventeenth day after the infliction of the injury that I presented myself at the Rue Dutot would again add to the probability that symptoms should manifest themselves in spite of the treatment. It is impossible to avoid the feeling that, had I not gone to Paris, the symptoms, if only nervous in origin, would, in the want of that sense of security gained by undergoing the treatment, have developed themselves to a most painful extent; if of the nature of rabies, would, failing the treatment, have become lethal; and that I owe to M. Pasteur and those at the Rue Dutot my heartiest thanks, not only for the courteous way in which I was received and treated, but perchance also for the continuance of my days.

IMMUNITY PRODUCED BY AN ALBUMOSE ISOLATED FROM ANTHRAX CULTURES.

By E. H. HANKIN, B.A.,

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THE following is a brief preliminary account of an investigation which I have recently been engaged upon under the guidance of Professor Koch at the Hygienic Institute of Berlin. The results appear to have an important bearing on the theory and practice of preventive inoculation.

It is now generally admitted that acquired immunity against a disease is in reality an acquired immunity against a poison. The phenomena of acquired tolerance of the effects of ordinary albumoses, and of snake poison which contains a poisonous albumose, show a closer resemblance to acquired immunity against a disease than is the case with any other poison. These and other facts led me to expect that if it were possible to isolate an albumose from anthrax cultures, such anthrax albumose would probably be capable of conferring immunity against the disease. Such an albumose I have now isolated from my culture fluids by ordinary chemical methods, and find that it gives the ordinary reactions for albumoses, and that it is capable of conferring immunity.

In the course of my process of preparation, it is precipitated from its solution by addition of a large bulk of absolute alcohol, and well washed in this liquid to free it from ptomaines, since it is known that all such substances are soluble in alcohol. After the addition of alcohol it is filtered off and dried, then it is redissolved, and filtered through a Chamberland's filter. A rough estimate of the percentage of albumose present in the clear solution thus obtained is made colorimetrically, by means of the biuret reaction and a peptone solution of known strength.

In one experiment four rabbits (Nos. 23 to 26) were inoculated subcutaneously with virulent anthrax spores. No. 26 served as "control," and died in about forty hours. The other three rabbits had the albumose solution injected into the ear-vein at the same time. Nos. 24 and 25 each had about the five-millionth of their body-weight, while No. 23 had only the ten-millionth of its body-weight of albumose. No. 25 died in less than forty-eight hours, but Nos. 23 and 24 survived. Ten days later Professor Koch kindly reinoculated these two rabbits for me with very virulent anthrax from an agar-agar culture. Their temperature has remained normal since then, and they are now alive and well a fortnight after this operation.

I have also succeeded in producing immunity in mice against attenuated anthrax. Dr. Fränkel kindly gave me an agar-agar culture of *deuxième vaccin*. From this I made bouillon cultures, and whenever I inoculated a rabbit from these it always died, unless it had at the same time been protected by a dose of the