

gress; but M. Toussaint informs me that he has already ascertained the existence of immunity against anthrax for three months and a half in both sheep and dogs treated in this way.

I need hardly remark on the surpassing importance of researches such as these. No one can say but that, if the British Medical Association should meet at Cambridge again ten years hence, some one may be able to record the discovery of the appropriate vaccine for measles, scarlet fever, and other acute specific diseases in the human subject. But even should nothing more be effected than what seems to be already on the point of attainment, the means of securing poultry from death by fowl-cholera, and cattle from the terribly destructive splenic fever, it must be admitted that we have an instance of a most valuable result from the much-reviled vivisection.

I have yet one more example to give of researches in this domain of pathology; and this also has reference to the *Bacillus anthracis*. The investigator in this instance is Dr. Buchner, assistant physician in Munich. It is well known that the bacillus anthracis is morphologically identical with an organism frequently met with in infusion of hay, which may be termed hay-bacillus. Such being the case, it occurred to Dr. Buchner that they might be merely one and the same organism modified by circumstances. For my own part, I am quite prepared to hear of such modifying influence being exerted upon bacteria, having made the observation several years ago that, when the *bacterium lactis* had been cultivated for some time in unboiled urine, it proved but a feeble lactic ferment when introduced again into milk. Its power of producing the lactic fermentation had been impaired by residence in the new medium. In the case before us, indeed, the physiological difference between the two organisms seems, at first sight, so great, as to forbid the idea of anything other than a specific difference. The bacillus anthracis refuses to grow in hay-infusion in which the hay-bacillus thrives with the utmost luxuriance; and conversely, the hay-bacillus is utterly incapable of growing in the blood of a living animal, whether introduced in small or in large quantities. The hay-bacillus is remarkable for its power of resistance to high temperatures, which is not the case with the bacillus anthracis. The latter is destroyed by a very slight acidity of the liquid of cultivation, or by any considerable degree of alkalinity, whereas the former survives under such conditions. Both will grow in diluted extract of meat, but their mode of growth differs greatly. The hay-bacillus multiplies rapidly, and forms a dry and wrinkled skin upon the surface, while the bacillus anthracis produces a delicate cloud at the bottom of the vessel, increasing slowly. Nothing daunted by these apparent essential differences, Dr. Buchner has laboured with indomitable perseverance by means of experiments carried on in Professor Nägeli's laboratory, to solve the double problem of changing the bacillus anthracis into hay-bacillus, and the converse. Having devised an ingenious apparatus by which a large reservoir of pure cultivating liquid was placed in communication with a cultivating vessel, so that any cultivation could be drawn off by simply turning a stop-cock, and further cultivating liquid supplied to the organisms remaining in the vessel by a mere inclination of the apparatus, Buchner proceeded to cultivate the isolated bacillus anthracis in extract of meat for several hundred successive generations. As an early result of these experiments, he found that the bacillus lost its power of producing disease in an animal inoculated with it. Up to this point he is confirmed by Dr. Greenfield, who has found that, when the bacillus anthracis is cultivated in aqueous humour, after about six generations it loses its infective property. Then as Buchner's experiments proceeded, the appearance of the growing organism was found to undergo gradual modification. Instead of the cloud at the bottom of the vessel, a scum began to make its appearance—at first greasy-looking and easily broken up—constituting, so far as appearances went, an intermediate form between the two organisms; and in course of time the scum became dryer and firmer, and at length the modified bacillus anthracis was found to be capable of growing in an acid hay infusion, and to present in every respect the characters of the hay-bacillus. The converse feat of changing the hay-bacillus into the bacillus anthracis proved very much more difficult. A great number of ingenious devices were adopted by Buchner, who was, nevertheless, continually baffled, till at last he attained success in the following manner. Having obtained the blood of a healthy animal under antiseptic precautions, and defibrinated it also antiseptically, and having arranged his apparatus so that the pure defibrinated blood, which was to be the cultivating medium, should be kept in constant movement, so as to continually break up the scum, and also keep the red corpuscles in perpetual motion so as to convey oxygen to all parts of the liquid—in this way imitating, to a certain extent, the conditions of growth of the bacillus anthracis outside the animal body, within which the hay-bacillus could not be got by any means to develop—he proceeded to cultivate through numerous successive generations. A transitional form soon made its appearance; but the

change advanced only to a limited degree, so that further progress by this method became hopeless. The modified form hitherto obtained failed entirely to grow when injected into the blood of an animal. On the contrary, it was in a short time completely eliminated from the system, just like the ordinary hay-bacillus. It had, however, been observed by Buchner that spores had never been formed by the bacillus growing in the defibrinated blood; and it occurred to him that, perhaps, if it were transferred to extract of meat, and induced to form spores there, the modified organism might yet grow in the blood of a living animal. The carrying out of this idea was crowned with success; and, both in the mouse and in the rabbit, Buchner succeeded by injecting various different quantities containing the organism in different animals. When large quantities were introduced, the animals died rapidly from the merely chemical toxic effects of the injected liquid; but, in some instances, after the period for these primary effects had passed, a fatal disease supervened—attended, as in anthrax, with great swelling of the spleen, the blood of which was found peopled as in that affection with newly formed bacilli; and the spleens affected in this way were found to communicate anthrax to healthy animals, just like those of animals which had died of ordinary splenic fever.\*

Supposing these results to be trustworthy, and the record of them bears all the stamp of authenticity, I need scarcely point out to a meeting like the present their transcendent importance as bearing upon the origin of infective diseases, and their modifications as exhibited in epidemics.

I trust that these examples may suffice to convey some idea of the work now going on with reference to the relations of micro-organisms to disease.

#### ON THE REMOVAL OF UTERINE TUMOURS BY ABDOMINAL SECTION.†

By T. SPENCER WELLS,

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I WISH particularly to limit this discussion precisely to the consideration of the subject of removal of uterine tumours—myoma, fibro-myoma, or fibroma—by abdominal section. Such a tumour as that on the table, which was removed by Mr. Sherburn of Hull from the uterine cavity and vagina, and the removal of fibroid polyp, or the enucleation of ingrowths which project towards the uterine cavity, are beyond the scope of discussion to-day. And so is excision of the entire uterus for cancer, by Freund's method or any other; and the operation of Porro, so interesting to the obstetrician, where, in addition to the Cæsarean section, the uterus itself is excised after withdrawing the child. All these subjects are well worthy of separate discussion; and I hope they will be carefully criticised as soon as a sufficient number of facts, carefully observed and faithfully recorded, have been collected to form a groundwork for the formation of sound opinion. My object to-day is to obtain from members present any such additions to our knowledge as may assist in the formation of professional opinion upon the removal of fibroid outgrowths from the uterus towards the peritoneal cavity, subperitoneal outgrowths with a more or less perfect pedicle, or fibroid enlargements of the fundus, which may be removed with some part of the uterus itself, and with or without one or both ovaries at the same time, by such a division of the abdominal wall as is made in ovariectomy, but necessarily longer when the tumours are both large and solid. And, as I understand opening a discussion to differ from reading a paper, in so far that in the former case one hopes to elicit information from others, while in the latter we endeavour to relate what we have ourselves observed or thought, I shall now only sketch so much of my own doings and reflections as may induce others to narrate theirs, and thus assist in the removal of the doubts and difficulties which necessarily obscure any comparatively new subject at its rise and during its early progress.

In the Hunterian Lectures at the College of Surgeons which I delivered in June 1878, and which were fully reported in your JOURNAL, I reported all my cases of removal, or attempted removal, of uterine tumours through the abdominal wall; and arranged them in two tables, one containing all the necessary details of twenty-four cases where uterine tumours were removed, with or without one or both ovaries; and twenty-one cases where only an exploratory incision was made, or where, in addition, the tumour was either simply punctured or partially

\* See *Ueber die experimentelle Erzeugung des Milbrandcontagiums aus den Heupilzen*, von Haus Buchner. München, 1880.

† Address delivered in the Section of Obstetric Medicine, in introducing a discussion on the subject, at the Annual Meeting of the British Medical Association in Cambridge, August 13th, 1880. See page 373.

removed. I must refer anyone who wishes to examine this subject more carefully hereafter to the published tables. I can only say now that of the twenty-four cases where the tumours were removed, only nine of the patients recovered, and fifteen died; while of the twenty-one cases of incision, puncture, or partial removal, only one died, and twenty recovered from the operation, some of them more or less relieved by it. I ask your attention to this mortality of sixteen deaths in forty-five operations, because this represents the results of my practice before adopting, in these operations, the Listerian details of antiseptic surgery. Since the delivery of the lectures, I have operated antiseptically, and I have had ten cases of removal, with three deaths and seven recoveries; and five cases of incision and puncture, all recoveries; or three deaths in fifteen operations. My whole experience, then, amounts to sixty cases: thirty-four of removal, with eighteen deaths and sixteen recoveries; twenty-six of incomplete operation, with only one death. The smaller mortality since adopting antiseptic precautions is certainly remarkable; but I do not wish to enter on this wide question now. I rather desire to discuss the indications which should guide us in deciding whether to leave a patient to her fate, or to medical treatment by ergotin or anything else; or to advise her to submit to the risk of abdominal section. It must be remembered that the risk—very small indeed if the attempt end in incision and puncture only—is now considerably smaller than it was a few years ago, and may be expected to become much smaller as experience increases, and the details of the different steps of the operation are more carefully studied and more frequently practised.

I have very little to add to the remarks on the operation which may be found in the report of my lecture (BRITISH MEDICAL JOURNAL, vol. ii, 1878, page 130), beyond attempting to enforce as strongly as I can adoption of the principle of uniting divided edges of peritoneum to each other. Whatever doubt some may entertain as to the value of my experiments on animals, and practice on women, in leading most operators in the present day to bring divided edges of peritoneum together whenever they have been separated by wound or by operation, I myself have no doubt whatever about it. And just as strongly as I assert that it is and must be better, when the abdominal wall is divided, to bring the peritoneal edges and surfaces of the opening together, restoring the complete closure of the peritoneal cavity, than to leave the cavity free to the admission of fluids oozing from wounded muscle, fat, and cellular tissue, and to allow contact of intestine and omentum with anything more than peritoneum, so strongly—more strongly, if I could—would I insist that the peritoneal edges of the divided uterine wall, or of the connecting part of the outgrowth with the uterine wall, should also be carefully brought together. In the only case of Cæsarean section I ever did, I sewed up the wound in the uterine wall by uninterrupted suture; and I still believe that the recovery of the patient was in a great measure due to this protection of the peritoneal cavity from the uterine discharges. Opinions will differ, of course, on this as on every other question; but my own opinion is quite clear. And so, after cutting away an uterine tumour—whether an outgrowth with a pedicle, or a growth which can be enucleated after incision of the fundus, or which leads to removal of so much of the fundus and body of the organ as to open the uterine cavity—I would strongly advise the operator to bring the peritoneal edges of the divided uterine wall together by many sutures, or by uninterrupted suture along the whole extent of the gap. In one of my published cases, where a solid tumour weighing seventy pounds was successfully removed, I put in at least twenty-four points of uninterrupted suture, the gap in the uterine wall and broad ligament having been more than a foot in length.

Schroeder, of Berlin, writes to me that, when he has opened the uterine cavity, he uses two rows of sutures: first, one row which close the mucous surfaces of the uterine cavity, and are left to pass away downwards by the vagina; and another row which brings the peritoneal edges of the fundus and broad ligament together.

Hegar, of Freiburg, writes to me saying that he has returned to the extraperitoneal treatment in these cases, and has had a series of ten successes. But he is especially careful to close the peritoneal edges of the lower part of the opening in the abdominal wall very accurately around the peritoneal edges of the uterine stump. He sews the two surfaces together by passing sutures all round the stump, fixing it securely to the abdominal wall by many points of suture, tying it up like the mouth of a purse. He carefully dries and disinfects the stump which is left between the edges of the lower angle of the united incisions, just as in extraperitoneal ovariectomy; but he believes that if disinfection were imperfect, and putrefaction occurred, the peritoneal union would serve to protect the patient from infection. Before I resorted to antiseptics, I was in favour of the extraperitoneal treatment of the stump after removal of uterine tumours, or of portions of the uterus; but, since antiseptics, I certainly prefer the intraperitoneal

treatment, provided the divided edges of peritoneum are accurately brought together. Unless additional facts alter my opinion, I am not inclined to follow the example of my friend Schroeder, and sew up the divided edges of mucous membrane when the cavity has been opened. Last month, I removed a large fibro-cystic tumour which involved the fundus and part of the body of the uterus. It was not necessary to interfere with either of the ovaries. The patient was over sixty years of age, and had long ceased to menstruate. But I cut away nearly all the supravaginal portion of the uterus, and of course opened the cavity. I carefully stitched together the peritoneal edges of the divided uterine wall, and I believe that the opening left for a little oozing of blood through the vagina, which went on for the first two or three days, was useful. If I had prevented this by suture, something like a hæmatocele might have formed, or blood might have found its way, in spite of the sutures which I had used, into the peritoneal cavity. This patient recovered without any febrile elevation of temperature. My present feeling, then, is in favour of intraperitoneal treatment of the stump or uterine wound; and, if the cautery should not become, as it doubtless may become, the favourite resource, then I would strongly advise complete union by suture of the divided peritoneal edges of the uterine wall.

There were several other operative details which I thought I might bring before you; but the time allotted to me has expired, and I may, at the close of the discussion perhaps, in replying to some question which may be asked, give such further information as may be desired and as I may be able to offer.

#### EXAMINATION OF COLOUR-PERCEPTION AT THE CAMBRIDGE MEETING.

By HERBERT W. PAGE, M.A. Cantab., F.R.C.S. Eng.,

Assistant-Surgeon to St. Mary's Hospital; Clinical Assistant to the Royal London Ophthalmic Hospital; etc.

THE proposed examination by Holmgren's method of the colour-perception of members of the British Medical Association, attending the meeting at Cambridge, was carried out on August 11th, 12th, and 13th, by Mr. Berry, Mr. Frost, Mr. Nettleship, Mr. Pye, Mr. Swanzy of Dublin, and myself. I thank these gentlemen for the invaluable aid so freely given by them—given not without much self-denial, inasmuch as the examinations took them away from the sectional meetings which all had been wishful to attend. Nor can I omit to thank Dr. Brailey for great help given to us in obtaining the sanction of the Executive and the use of the room in the new museums, and for the trouble he took in having had printed and posted the numerous directions and appeals which met the eye at every bend and corner of the buildings where the meetings were held.

The object of the examination, as suggested by me in the BRITISH MEDICAL JOURNAL (1879, vol. ii, page 651), was to discover the proportion of congenitally colour-blind amongst a large number of educated and intelligent men. Want of time rendered it impossible to make more minute inquiries, either into the family history of those who were colour-blind, or as to their ability to distinguish the colours of different signal-lamps placed at a distance from them. At the hour when the examination closed, 920 had entered their names in the reception-room at the Guildhall. There voluntarily presented themselves for examination 745—of whom at least 700 were members of the profession, the remainder being undergraduates (some of them medical students) and other members of the University.

Of the 700 members of the profession, 12 were completely colour-blind, 6 red-blind, and 6 green-blind; and 2 were incompletely colour-blind, one red, and one green—in all 14. Of 4 others, who were not colour-blind, it may be said that their chromatic sense was feeble. In the whole 745 examined, there were 15 colour-blind\*—one of the undergraduates (a green-blind) making the additional number.

All the colour-blind knew of their defect; but it should not be forgotten that they were educated men; and it is not unlikely that amongst ignorant persons, whose faculty of observation is extremely limited, the defect may never be discovered, unless they have undergone colour-education at school. A large number who presented themselves expressed a belief that they were colour-blind, whose colour-sense was yet found on examination to be normal. In nearly all these cases, the belief arose from a difficulty experienced in correctly naming the shades of blue and green, which merge imperceptibly into each other. The readiness with which the colours were chosen was exceedingly variable; but only in one or two instances was there any sign of what is called colour-stupidity, which makes the examination of uneducated persons so much more difficult.

\* Any of the colour-blind who are willing to assist in the scientific investigation of the subject, by submitting to further examination, are requested to communicate privately with Mr. W. A. Frost, 77, Wimpole Street.