is represented by the number '35 in. The spot where he found the lowest sensibility (5'0 in.) was on the dorsum of the hand, opposite the base of the fifth metacarpal bone. The palmar surface of the hand was in all parts more sensitive than the dorsum; but this was not the most sensitive part, for next to the tips of the fingers stood in order the two borders, the radial border being more sensitive than the ulnar. As pointed out by Weber, he also found that the sensibility of the hand increases from the base towards the extremity; but the author exhibits this fact by accurate numbers, and demonstrates not only the increase but also the rate of increase on each surface and border of the hand and of each finger separately. He finds the most rapid increase in sensibility to take place at the spot where the fingers actually commence, not where they apparently commence, and thus not at the clefts, but opposite the metacarpo-phalangeal articulations, and again at the middle of the last phalanges on approaching the tips of the fingers. Perhaps the most interesting and important demonstration of all is that which relates to the fingers and their several surfaces. On the whole, the most sensitive finger is the index, and the sensitiveness shades off towards the ulnar side of the hand; and the most sensitive portion of the index, next to the tip, he finds to be its radial side. Of the little finger, the most sensitive part is the ulnar side, and he connects these two facts with those parts entering into the constitution of the borders of the hand at large. Of the palmar surfaces of the fingers, that of the index is the most sensitive; of the dorsal surfaces, that of the ring finger is the least sensitive. As respects the radial sides of the fingers, he finds the radial side of the index to be the most sensitive, and that the sensibility shades off as the fingers are further removed from the radial side of the hand, till it becomes least upon the little finger. As respects the ulnar sides, he finds that that of the little finger is the most sensitive, and that the sensibility becomes less as the ulnar side of the hand is distanced; with this remarkable exception, however, that the high sensibility of the index is provided for by its ulnar side standing next in rank to that of the little finger. Of the three intervals between the fingers, that whose approximating surfaces possess the highest sensibility is the interval between the index and middle fingers. The thumb is, for the sake of convenience and simplicity, considered separately,—and is regarded, from its carpal attachment onwards, as a finger not having a metacarpal element. It is thus compared in the paper with the fingers, from their metacarpo-phalangeal joints onwards. Appended to the paper are tables exhibiting the observed sensibility in each direction at the several spots examined, and four photographs on which are marked the sums of the observations at each spot. The author reserves the consideration of the differences of result according to the direction in which the compasses are placed for a future communication.

The PRESIDENT remarked on the great value of the facts contained in the paper.

Dr. BALLARD, in reply to the President, stated that he had used compasses tipped with cork in making his experiments.

Dr. KIDD said that the plan of ascertaining the amount of sensibility by means of compasses had been extensively tried by the late Dr. Todd, and the results obtained were often very valuable. The hand and forearm were, Dr. Kidd said, more sensitive than other parts of the body, and hence he had found that dashing cold water on these parts was more efficient in rousing a person from the effects of chloroform than throwing cold water on the face. It might be, he said, on account of the more intimate connexion the nerves of the brachial plexus had with the nerves of the thoracic viscera.

Dr. BALLARD said that the paper was physiological, and referred only to the hand. He had made experiments on the sensibility of other parts of the body.

ADDITIONAL EXPERIMENTS ON THE POISONOUS EFFECTS OF COAL-GAS UPON THE ANIMAL SYSTEM.

BY C. J. B. ALDIS, M.D.,

MEDICAL OFFICER OF HEALTH FOR ST. GEORGE'S, HANOVER-SQUARE.

In the paper which the author brought under the notice of the Society a short time ago upon this subject, the experiments were made with the gas as it issued from the main, but in those now to be related common gas, diluted with atmospheric air in different proportions, was used. The experiments were made with Mr. Henry Banister, at the works in Horseferry-road, on the 7th and 14th of February, 1862.

In the first experiment, common gas and atmospheric air, in equal proportions, were administered to a rat placed under a glass vessel. The head dropped in one minute, and it became

insensible, with slight convulsion, in one minute and a quarter; the respiration was hurried, the eyes staring, spasmodic jerking of the body, followed by death in two minutes and a half. The post-mortem appearances were similar to those already described, except a darkish patch on the anterior part of the pleura of the right lung, like that produced by smoke.

The second experiment was made with one-fourth common gas and three-fourths atmospheric air. The respiration of the animal became hurried in three minutes, the head dropped in five minutes, and death occurred in eleven minutes. During the autopsy, a smoky-looking spot was seen on the pleura.

In the third and fourth experiments, the proportion of gas to air was one-fifteenth.

The former experiment was not sufficiently prolonged to show the deadly influence of the gas in this diminished quantity; but in the latter the rat soon began to pant, and the head fell in ten minutes, springing of the body was observed, with twitching of the ear, followed by coma in seventeen minutes. The eyes were open, the respiration became laborious in forty-one minutes, when death ensued. The surface of the skull was intensely red; the brain congested; the blood very fluid and bright-coloured; the pleura of a bright-red colour, and the right side of the heart distended with darkish blood.

Dr. HODGKIN said that he supposed the experiments had been made in reference to the condition of miners, and as far as they are concerned the experiments were very proper. There was, however, a manner in which they might advantageously be modified. He said that the late Mr. William Allen, in studying the phenomena of respiration, made a mixture of hydrogen and oxygen, and placed animals in it. The mixture differed from atmospheric air in that it contained hydrogen instead of nitrogen, but the same proportional quantity of oxygen. It was found in this case that the animals died, but death was not due to the presence of hydrogen or to the absence of nitrogen, but because the oxygen was consumed, and carbonic acid resulted, the result of respiration.

Dr. WYNN WILLIAMS said that in 1857 he had under his care two cases of poisoning by coal gas. Two old women went to bed between the hours of eight and nine. Next morning they were both found unconscious. One of them died. The other said that in the night she heard her sister get up, but she remembered nothing more, as she then fell back insensible. The patient who died remained for forty-eight hours unconscious, and then became partially conscious; but in three or four days she again became comatose, and occasionally violent, and ultimately died. The one who recovered suffered from pain exactly like muscular rheumatism, and after recovery to a certain extent she had an attack of acute rheumatism. At a previous meeting, he (Dr. Williams) understood that Dr. Kidd believed that the effects of chloroform and of coal-gas were analogous. He (Dr. Williams) did not find that the symptoms in his cases were at all like those produced by chloroform.

Dr. KIDD said that all he intended to say was, that chloroform and carburetted hydrogen were both anæsthetics, and not that their action was identical. The experiments of Perrin, Lallemand, and Duroy had shown that anæsthetics differed very much in their special actions.

OBSTETRICAL SOCIETY OF LONDON.

MARCH 5TH, 1862.

Dr. TYLER SMITH, PRESIDENT, IN THE CHAIR.

ON OVARIOTOMY; THE MODE OF ITS PERFORMANCE, AND THE RESULTS OBTAINED AT THE LONDON SURGICAL HOME.

BY I. B. BROWN, ESQ., F.R.C.S.,

SENIOR SURGEON TO THE LONDON SURGICAL HOME.

AFTER some preliminary observations upon the operation of ovariectomy, its now recognised character, and the mortality from it being much less than that from many other capital operations, the author alluded to the statistics of Mr. Clay, of Birmingham. He believed that, highly favourable as were the results given, there was good reason for supposing that they would become still more so when the statistics were hereafter collected, subsequently to the period to which Mr. Clay's cases extend—namely, February, 1860. The conditions rendering the operation justifiable were next dwelt upon. The difficulty of diagnosis was shown, especially in cases complicated with cancer. Much would, however, depend upon the history of the patient and her family, in arriving at a correct conclusion. Adhesions at

the present time rarely prove an obstacle to the completion of the operation; they are either broken through with the hand, or divided with the knife or *écraseur*. When necessary to secure any by ligature, he advised the use of silver wires instead of thread or twine, allowing them to remain within the abdomen after cutting them short and close.

The author next fully described his mode of performing ovariectomy. He advises the pedicle to be enclosed in a clamp—the ordinary carpenter's calipers—as near to the tumour as possible, and kept externally. Its advantages are, that it can be removed in from one to three days; the wound heals quickly, and convalescence ensues in two or three weeks. If the pedicle proves to be very short, and pain is complained of, the clamp is to be removed in a few hours.

The preliminary measures to be adopted previous to the operation were then described, and their importance shown as bearing on the subsequent results. Amongst others, the author advised the observation of certain atmospheric conditions, and the avoidance of any proceeding when the atmosphere was low and heavy, with an absence or deficiency of ozone, and in that condition generally which we describe as depressing. If greater attention were paid to atmospheric changes, the author thought that there would be much less of gangrene, pyæmia, low fever, &c., so frequently witnessed after operations.

The after treatment was also dwelt upon, and, finally, the analysis of the cases. Of the latter, ovariectomy has been performed nineteen times by the author in the London Surgical Home up to the present time. Of these, thirteen have been recoveries, and six deaths. The details and special particulars of the whole of these cases were given in a series of tables. The ages varied from eighteen to fifty-six. Of the successful cases, eight were under the age of thirty, and five above; whilst of the unsuccessful, one was twenty-one, and five were thirty and upwards. The duration of the disease in the successful cases was from four months to six years; six were within the first year, or ten within two years, and three over the latter period. Nine were single, and four were married, and of the latter, two only had had children. Five had undergone tapping from one to three times. In the unsuccessful cases the duration of the disease was from two to ten years; four were married, three of whom had had families of from three to six children. Four of the fatal cases were tapped from one to six times. The general health was very good in five of the successful cases; in six it was but middling; in one it was shattered, and in another bad. In the unsuccessful cases four had bad health; one was in good health, and another had good health up to six weeks before the operation.

With respect to the operation, the incisions varied from three to seven inches long; in eleven it did not exceed five inches. The tumours were multilocular in eleven, and unilocular in two of the successful cases; in the unsuccessful they were multilocular in four, unilocular in one, and more or less solid in the sixth, containing hair, teeth, bones, &c. and no doubt congenital. Adhesions were found in all except four of the successful and one of the unsuccessful cases. These varied very much, being either very slight and easily broken down, or firm, strong, and unyielding; some were numerous in all directions, requiring to be cut or ligatured. Chloroform was given in all the cases; in two it had to be discontinued, but the patients suffered no pain; in two instances ether was substituted during the latter part of the operation. The pedicle was retained outside of the abdomen in all but two of the cases, the calipers being used for the purpose of holding it. In all the operations performed lately, the wound had been closed by silver-wire sutures, simply twisted. Of the causes of death in the six fatal cases, in two instances it clearly arose directly from the operation itself: in the other four, conditions were found which chiefly brought about this result; in one case—that of the solid tumour—there was much old disease found; in another, the patient had been a hard drinker, was tapped five times, the belly being filled with forty-five pints of ascitic fluid, independently of the contents of the ovarian cyst, there was softening of the liver, and death occurred in six days. In the other two cases, diarrhoea carried off both—one in eight days, and the other in eighteen after the operation. In one of these, cancer of the duodenum was found, wholly unsuspected during life. The author concluded by stating that all these operations had been witnessed by gentlemen from various parts of the world, of Great Britain, and the metropolis, many of whom had watched the results from day to day and week to week.

Mr. SPENCER WELLS said that the recommendation of the author of the paper to tie adhesions with silver wire, to divide the adhesions, and then cut off the wire short, leaving a portion of dead tissue strangulated by the wire within the peritoneal

cavity, was advice which he should not be disposed to follow. The material of which the ligature was composed could be of little importance. Either wire or twine would equally cause gangrene of the strangulated part, and there would be great danger that the whole system might be poisoned by absorption of the fetid matter of the decomposing slough. He felt convinced that if this practice were followed the mortality after ovariectomy would increase. He preferred a ligature of wire-rope, tightened by a screw, to the caliper-clamp; and he thought (after trying both seton-wire and harelip-pins for the closure of the wound) that harelip-pins were generally to be preferred. The lateral pressure made by the cotton twisted round the pins fixed the two wounded surfaces more closely and securely than the wire. He differed entirely from the author in his preference to flannel over sponge in cases where it was necessary to cleanse the peritoneal cavity from blood or ovarian fluid. A soft, clean sponge did the work more effectually, and there was not the danger, as there was with flannel, of leaving portions of fine wool, or *fluff*, adhering to the peritoneum. He also thought that the author's advice would lead to a want of care in thoroughly cleansing the peritoneal cavity from ovarian fluid. In a recent case he had been led by some such advice, and by what he had heard of the practice of other surgeons, to leave some ovarian fluid in the cavity, thinking that as the intestines were exposed it might be more dangerous to sponge away the fluid than to leave it; but the result had very much disappointed him. There was little else unfavourable about the case, yet the patient died, thirty hours after operation, of acute peritonitis, and the whole membrane was covered by a layer of the albuminous portion of the fluid. The peritoneum seemed to have acted as a dialyser, allowing the water to pass through, and leaving a coating of albumen on its surface. He (Mr. Wells) could hardly suppose that either inhalation of the vapour of turpentine after the operation, or the exhibition of arnica before it, could have any great influence upon the result. He had never used either, and his results were not less satisfactory than those of the author. It was singular that he had had exactly the same number of cases in the Samaritan Hospital as Mr. Brown had had in his Home—namely, 19. Mr. Brown's returns showed 13 recoveries and 6 deaths. His (Mr. Wells'), 11 recoveries and 8 deaths. His cases in private practice had been 15, of whom 8 recovered and 7 died. The total gave 34 cases, with 19 recoveries and 15 deaths. He trusted that Mr. Brown would also give the Society the result of his *whole* experience of ovariectomy. He (Mr. Wells) had not been able to do more than compare their first 20 cases; and it was singular that with these 20 the number of recoveries and deaths was directly reversed, for Mr. Brown had had 13 deaths and 7 recoveries; while he (Mr. Wells) had had 13 recoveries and 7 deaths. He was also of opinion that in some of the fatal cases brought before the Society this evening, the operation ought not to have been performed. The patients died of serious organic disease of the heart, or of some other important organ—cancer of the intestine, for example; or the uterus was so closely surrounded by adhering pelvic portions of the ovarian tumour that separation could not be effected. Such conditions could generally be detected during life, and ought, with rare exceptions, to lead the surgeon to refuse to operate. It often became our duty to operate in very unfavourable cases, and many of his own fatal cases had occurred under conditions so unfavourable that a good result could not fairly be expected; but the operation was decidedly contra-indicated in any case of serious organic disease of heart or lungs, or of malignant disease in any important organ.

Dr. TANNER made some observations on the importance of not resorting to ovariectomy in cases where the operation was almost certain to prove fatal. There were many patients afflicted with ovarian tumours, where extirpation was quite impracticable.

Dr. HALL DAVIS observed, that the success recorded in the paper, of thirteen recoveries out of nineteen ovariectomy operations, was highly gratifying. Of those cases, he (Dr. Davis) had had the opportunity of watching the course of twelve, of which number eight recovered. It was his opinion that such success as that presented by the nineteen cases now reported by Mr. Brown, and the equal success of Dr. Tyler Smith, Mr. Spencer Wells, Dr. Clay, and of other recent operators, of 67 per cent. of recoveries, as compared with the unfavourable results of former years, was an advance in surgery of which we might well be proud. We are now, therefore, no longer justified in opposing the wishes of our suffering patients, anxious for the operation, as affording them a chance of escape from a miserable death,—at least in any case which furnishes a hope of a favourable issue. For many years he had taught, in his lectures,

that ovariectomy was not justifiable; but the mortality which then attended it, of at least two in every three operations, as fully authorized his so doing as the present improved method of operation, with its happy results, has justified a change of opinion. He still did not think we should take upon ourselves the responsibility of actually recommending the operation, but after fairly laying before our patient the proportion of recoveries from it, we should then, in a case which offers no obvious objection to the operation, leave the important decision to the patient herself.

Dr. ROUTH said he had seen a large number of cases of ovarian disease; he spoke, therefore, from experience. First, he believed we could never be certain beforehand on some of the most important points of diagnosis in ovarian disease. Extensive adhesions had been often diagnosed before operation, which were afterwards found to be limited or absent. In other cases their absence was predicted, while the operation revealed that they were very firm and numerous. The presence of coexisting ascites greatly increased the difficulty, so as to make a preliminary tapping necessary before ovarian disease could be made out at all. Secondly, he had known some of the most eminent men in the diagnosis of ovarian disease mistake a large cyst of the kidney, extra-uterine pregnancy, and fibroid disease of the uterus, for ovarian dropsy. Thirdly, Mr. Wells objected to the expediency of operating in cases of cancer of the intestines, which he stated was always to be made out before operation. In one of Mr. Brown's cases there was cancer of the duodenum with a large ovarian cyst in front of it. How could such a disease be made out? It was simply impossible. In honesty he (Dr. Routh) was bound to add that post-mortems often revealed the coexistence of other disease which not only could not have been made out before operation, but which could not even have been suspected. Fourthly, his impression was that the non-existence of adhesions, and a perfectly healthy peritoneum, were not necessarily favourable to recovery. He had seen such cases followed by severe peritonitis; while others, in which the peritoneum was almost entirely disorganized by extensive adhesions, which needed very great force to tear them asunder, recovered without one bad symptom. Lastly, he thought that the practice of sponging out the contents of a cyst, if accidentally effused during the operation, was better than leaving the fluid in the cavity of the peritoneum.

Dr. GRAILY HEWITT thought the facts contained in Mr. Brown's paper interesting, as showing the improvements which had occurred of late in the results obtained in the operation of ovariectomy. If he recollected aright, Mr. Brown's former experience, as summed up in Mr. Clay's work on Ovariectomy Statistics, exhibited cures only to the extent of 31 or 32 per cent., whereas in the series of cases now brought forward the mortality was 31·5 per cent. and the cures 68·5 per cent. It was very satisfactory to find that the attention paid to the subject had produced improvement such as that now referred to—an improvement which had more or less attended the operation in other hands also. He thought it would be interesting to the Society if Mr. Brown would state to what he considered the very marked difference in the results of his recent and former experience was due. Further, would Mr. Brown inform the Society whether the results related included all his ovariectomy operations, both those carried out and those only attempted, as was done in Mr. Clay's statistical account? With reference to the existence of adhesions, and their influence on the mortality of the operation, his own observations induced him to think that the point was comparatively unimportant. Any danger attending the use of sponges in the operation was, he believed, connected with want of attention to cleanliness, and consequent introduction of noxious matter into the system.

Dr. TYLER SMITH observed that the remarks of the author upon the semi-recumbent position of the patient during the operation evidently applied to himself. He could only state that he believed this position to be superior to the horizontal, inasmuch as it facilitated the removal of fluid from the abdominal cavity, was convenient to the operator, and it certainly did not produce syncope. In some patients the horizontal position was impossible, as they could not lie down without danger of suffocation. He was glad Mr. Brown had modified his plan of giving opiates as the rule after operations, as he thought it could be shown in some cases to lead to bad results. His own plan had been not to place ligatures upon adhesions when bleeding took place immediately after their division, but to wait till towards the end of the operation, by which time all bleeding from adhesion had generally ceased. He used warm flannels to prevent the protrusion of the intestines, but thought soft and perfectly clean sponges preferable to flannel in remov-

ing fluid and coagula. The sponges should be large, so as to render it impossible for them to be left in the abdomen. He thought neither good nor harm could result from the use of arnica and turpentine in the way recommended by the author. It appeared to him that we were quite able to estimate beforehand the chances of recovery in particular cases; but in the interests of humanity, and when death was inevitable without operation, he thought we were bound in some cases to operate, even though the chances of recovery were slight. The general recognition of the value of the operation would diminish such cases, and also those which were now met with of a hopeless character, by leading to its timely performance. He could not but remark that the present discussion had been chiefly occupied with the minor but still important points relating to the details of this great operation. The subject had now been several times before the Fellows, and, as far as the Obstetrical Society of London was concerned, no voice had been raised against the principle of the operation. It must be held to have been accepted as a recognised operation of the highest value. His own cases had been ten in number, of which seven had been entirely successful.

Mr. BAKER BROWN, in reply to the observations of Mr. Spencer Wells, stated that he had used the silver wires as ligatures with the best results. In one instance, in the presence of M. Nélaton, of Paris, and others, he had used four silver sutures to a wound in the uterus, and the patient made an excellent recovery. The theoretical opinions of Mr. Wells were completely upset by practical facts, as proved both by Mr. Brown and by Dr. Tyler Smith. Mr. Brown repeated, that his experience of the injurious effects of sponges on the peritoneum was founded on actual facts, first suggested by observing that Mr. White Cooper avoided the use of sponges in operations of the eye, and by also hearing the practical experience of Mr. Webber on the same subject. Mr. Wells' observations about leaving any of the ovarian fluid within the peritoneal cavity were repeatedly controverted by the non-injurious results of spontaneous rupture of ovarian cysts. Mr. Brown had recommended the use of the inhalation of turpentine, after the operation, in cases of pain, upon the well known principle of its vapour being an excellent anæsthetic; and he felt sure that if any gentleman would administer it to patients suffering pain he would be speedily convinced of the advantages arising from its use. Mr. Brown had no intention in this paper of comparing the results of his operations with those of Mr. Spencer Wells; but as that gentleman had alluded to the number of his own cases, he had not the slightest objection to state that the results of his whole practice, in private and in St. Mary's Hospital, had been nineteen cases, with six recoveries; and in the London Surgical Home, nineteen cases, with thirteen recoveries: total, thirty-eight cases, and nineteen recoveries. Mr. Brown was surprised at the opinion expressed by Mr. Wells as to the facility of detecting other organic diseases during life in patients suffering from ovarian disease, because he knew that Mr. Wells had operated on a patient, with a fatal result, which he and others of great experience had condemned as totally unfit for operation. The Society had also heard from Dr. Tanner of another case of like nature; and Mr. Brown believed that in two other cases Mr. Wells had operated, in his hospital, where the adhesions were so great as to forbid the completion of the operation. Mr. Brown believed that the greater beneficial results obtained in the London Surgical Home over his former practice were due to the skilled nursing and perfect organization of that establishment affording such facilities in the after-treatment. He had mentioned the minute details of the preparatory and after-treatment because a large experience in operative surgery had convinced him that success arose from a strict attention to these points, rather than from the brilliancy of the operation itself.

Reviews and Notices of Books.

An Inquiry into the Medicinal Value of the Excreta of Reptiles in Phthisis and some other Diseases. By JOHN HASTINGS, M.D. pp. 103. London: Longmans.

It is now nearly twenty years since the thoughtful and sober-minded men of the profession were somewhat startled by the appearance of a widely-advertised work, entitled "Pulmonary Consumption successfully treated with Naphtha." Its author purported to be "John Hastings, M.D., Senior Physician to the Blenheim-street Free Dispensary." The general character