

communication by which the specific poison is supposed to spread. Dr. Murray suggests that my remarks to the effect that I know of no case in which the poison supposed to be contained in cholera evacuations has spread by means of water in India, apply only to the experience of 1872; but I apply it in its widest sense, and I assert that in India, even in those parts of it in which cholera has its home, there is absolutely no evidence of anything of the kind; and I would ask Dr. Murray, and all others who believe in this theory, to adduce one single instance, that it may be thoroughly sifted.

I could dwell much longer on these matters, and I should like to discuss others; but I shall not detain you longer. In conclusion, I would only remark that I object to the current theories of the day in regard to cholera—1. Because they are founded on altogether insufficient bases to warrant their acceptance as scientific truths; 2. Because the quarantine and other restrictive measures to which they lead are impracticable; 3. Because, so long as mankind believe in such theories, they will never wake up to a proper sense of the great importance of the real work which has to be done—the improvement of the conditions under which they live; and 4. Because the prohibitions which flow from such theories are so vexatious, that the people are set against everything under the name of sanitary reform. I am quite willing to learn in a very difficult inquiry, and to weigh every piece of fresh evidence that can be brought forward; but let us see that the facts are really what they profess to be. And, as regards India, where there are special opportunities of studying cholera, let there be no error in statements, and no hurried rushing to conclusions.

ON VALERIANIC ETHER.

By W. F. WADE, F.R.C.P.,

Physician to the General Hospital, Birmingham.

SEVERAL years ago, Messrs. Southall, chemists of this town, prepared, at my request, some valerianic ether.

At that time, I was not much in the habit of using valerian or its preparations, and consequently allowed this one to fall into desuetude. Of late years, I have recognised more fully the frequency and importance of neurotic elements in mixed diseases, as well as of neurotic disorders pure and simple.

Some of these conditions are most decidedly alleviated by valerian, but the inelegance and nauseousness of its ordinary forms militate against its sufficiently frequent employment. My mind consequently reverted to the ether, and Messrs. Southall have again, at my request, prepared it; and also, for convenience of dispensing, a spirit of it, containing in twenty parts one of ether. The price is about ten per cent. dearer than chloric ether. Fifteen drops of the spirit is a convenient dose.

This preparation seems to me to possess the good qualities of the infusion and tincture, but to be more agreeable to patients both in odour and in appearance. Messrs. Southall inform me that the ether is free from alcohol, water, and valerianic acid, and is as pure as can be readily obtained. I may add, that Messrs. Southall affirm that both their wholesale and retail trade prove that valerian preparations are much more extensively employed of late years than formerly.

If any of our members try the spirit of valerianic ether, I should be glad to hear, after a due time, their opinion as to its merits as compared with those of other valerianic preparations. It is scarcely necessary to add that it is easy to combine it with arsenic, zinc, ergot, bromides, or indeed any ordinary drug.

THERAPEUTIC MEMORANDA.

AMYL COLLOID IN SHINGLES.

I AM anxious to call the attention of the profession to the great efficacy of the "amyl colloid" in removing the pain attendant upon shingles, which, as every practical man knows, is thoroughly out of proportion to the appearance of the eruption, and is often the cause of sleepless nights and great general discomfort. As far as my experience goes, no drug, applied externally or given by the mouth, can be compared to the soothing effect of the amyl colloid painted for three or four coats round the clusters of the eruption. It may be applied as soon as the pain sets in, and continued twice daily as long as required; but it must not be applied on the eruption. In severe cases, the addition of three or four grains of morphia to each drachm of the colloid will be found advantageous. Of course I do not wish this treatment to banish any general or special treatment that may be required in individual cases, but only as a great help to our means of easing pain.

HERBERT M. MORGAN, Lichfield.

REPORTS OF SOCIETIES.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

TUESDAY, JANUARY 13TH, 1874.

C. J. B. WILLIAMS, M.D., F.R.S., President, in the Chair.

LARGE ADENOCELE, COMPLICATED WITH MILK-CYST. BY F. LE GROS CLARK, F.R.S.

E. H., aged 24, married, was admitted into St. Thomas's Hospital in November, 1872. At the age of sixteen, she first noticed a swelling in the breast, which had gradually increased to its present large dimensions, but unattended by pain. She had been married two years, and had an infant seven months old. The bulk of the tumour was below the nipple, and the entire mass measured twenty-six inches and a half in circumference. It was removed on December 4th, and the true gland, being almost entirely distinct, was only partially removed with it. The patient made a good recovery. The solid tumour weighed between eight and nine pounds, and proved to be adenoid growth, resembling, on section, the structure of healthy breast-tissue. In its interior was a large cavity, containing about two pints of thick, creamy milk. Its lining membrane was ragged and fibrous. Its microscopic characters were those of true gland-tissue. The patient had been again recently confined, and suffered only temporary inconvenience at the early period of lactation.

Mr. BIRKETT said that such cases as that described by Mr. Clark were very rare. He had recorded a case in *Guy's Hospital Reports*, in which gland-tissue was formed and milk was secreted. There was a marked distinction between these and the so-called adenoid growths; the former might be regarded as true glands. In Mr. Clark's case, the growth was first noticed at the age of 16; and in Mr. Birkett's it appeared early, but it was long (in consequence of the patient being either pregnant or suckling) before there was an opportunity of removing it. It was found to be a small cyst containing matter like cream cheese or condensed milk. Hanging from the walls of the cyst were imperfect ducts. These growths might be regarded as developments of true mammary glands without nipples or ducts. In another case, he removed a rapidly growing cyst, in which lay many adenoid growths. It contained thickened milk which, he thought, came from the neighbouring normal gland. Another case was operated on by Mr. Luke, in which milk was found in a cyst like an adenoid growth.

REMARKS ON DISLOCATIONS OF THE FIRST AND SECOND PIECES OF THE STERNUM. BY WALTER RIVINGTON, M.S. LOND., F.R.C.S.

The object of the paper was to give an explanation of the mode of occurrence of dislocations of the sternum, illustrated by cases coming under observation at the London Hospital; and to account for their peculiarities by the anatomy of the superior sternal articulation. That dislocations and fractures of the sternum might be produced by direct or indirect violence, and that they frequently complicated fractures and dislocations of the spine, were well recognised facts; but as to the mode of their occurrence, conflicting and indefinite explanations had been offered. Of fourteen cases collected by Mr. Poland, the displacement was due or was attributed in four to force applied immediately to the sternum; in one, to violent lateral compression of the chest; in one, to muscular action; in one, to flexion of the body forwards as the result of a fall; in three, to falls from a height on to the back; in two, to falls from a height, the part struck not being mentioned; whilst in one the cause was not stated. Precisely similar causes had been assigned to fractures of the sternum, and the two classes of injury had so much in common that some writers included displacements of the sternum under the head of fractures. In the large majority of cases of both, the upper fragment was found lying behind the lower. In displacements, for instance, the manubrium was almost invariably found lying behind the gladiolus, the second pair of ribs remaining attached to the manubrium, and the strong posterior layer of periosteum untor, but stripped up from the bone, holding the two segments together. The only recorded exception to this disposition of the segments known to the author was a case recorded by Sabatier. The position of the manubrium behind the gladiolus had been generally attributed to the direct force of the blow, pushing it backwards; but the same effect might be produced, in all probability, by force applied to the body of the sternum, the elasticity of the ribs and costal cartilages attached to it, causing it to rebound after it had been depressed. This was M. Maisonneuve's suggestion, and it appeared the more probable because the gladiolus was more exposed to violence. The question might arise whether a particular fracture or displacement had been the

result of direct or indirect force. The injuries to the sternum, for instance, which complicated fractures and displacements, were often attributed by some surgeons to a violent descent of the chin on to the bone, and often explained by others as the effect of counterstroke; others, again, regarded them as the result of a doubling forwards of the sternum, caused by forcible flexion of the spine. That violent descent of the chin on to the sternum might occasion fracture was both *à priori* probable and was supported by the records of cases. The author quoted a case which had occurred in the practice of his colleague, Mr. Couper, at the London Hospital, as one apparently of this nature. An acrobat, aged 30, in turning a double summersault, fell about ten feet on to the back of his head. He came down on some tan, and his head was violently flexed on to his chest. The injury caused a separation between the sixth and seventh cervical vertebræ and an oblique fracture of the sternum, such as might have been produced by the chin inclined to one side. The author believed that the descent of the chin on to the sternum was far more likely to occasion a fracture than a dislocation, and that it was only in cases of disruption of the ligaments uniting two of the cervical vertebræ, by which a freer flexion of the head could be effected, that this cause came into operation. Neither Mr. Poland nor Mr. Hamilton mentioned it, and certainly it had been used to explain cases due to forcible flexion and extension of the dorsal spine. Two cases were related in exemplification of the influence of forcible flexion and extension of the dorsal spine on the sternum, in both of which the injury had been attributed by some to the patient's chin. In the first case a man, aged 37, was walking in the street when some scaffolding fell on his head. The vertical force thus applied caused fracture and crushing of the third dorsal vertebra, and the violent flexion of the dorsal vertebræ, acting through the medium of the ribs, produced disruption of the ligaments uniting the manubrium and gladiolus. In the second case a man, aged 55, was standing on the floor of a warehouse when a bag of seeds, falling from a height, alighted on his back between the scapulae, fracturing the six upper ribs on the left side near their angles, breaking off the spinous processes of the six upper dorsal vertebræ, and fracturing the body of the sixth dorsal vertebra. The dorsal spine was thus violently extended, the shock was conveyed through the ribs to the sternum, but, owing to the greater length and leverage of the five lower true ribs, more powerfully to the gladiolus than to the manubrium, and hence the occurrence of a dislocation of the two segments. This driving forwards of the gladiolus, either separately or more powerfully than the manubrium, which was held firmly by the first pair of ribs, would appear to be the correct explanation of some recorded cases explained in a different manner; as, for example, by muscular action, such as fractures or displacements from simple falls on the back, or from falls in which the back struck against a prominent object, or falls on to the feet and nates, the body finally falling over on to the back. The lever-like action of the ribs came into play during compression of the chest, and in cases in which the force was applied unilaterally, as well as in overarching of the back by violent contraction of the muscles during the expulsive efforts of labour. The frequency and pathological peculiarities of dislocations were fully explained by the anatomy of the articulation. Although the English anatomical text-books merely stated that the manubrium and gladiolus were united by a single piece of symphyseal cartilage, there were in reality two distinct kinds of joint found between them—the amphiarthrodial and the diarthrodial. Having noticed this in examining specimens in the *post mortem* room at the London Hospital, Mr. Rivington turned to the authorities, and, finding no mention of the fact in English books, consulted a French "Anatomy" by M. Jarnain, in which M. Maisonneuve's researches were epitomised. Thirty years ago M. Maisonneuve had fully described the two kinds of joint in a paper on Luxations of the Sternum, published in the *Archives Générales de Médecine* (serie iii, tome xiv). In the amphiarthrodial joint there was a single piece of true fibrocartilage uniting the segments more thin and friable in the centre than at the periphery. In the diarthrodial each bone was clothed with a distinct lamina of cartilage, adherent on one side, free on the other; and the cartilage belonging to the gladiolus was continued without interruption on to the facets for the cartilages of the second ribs. The spur of the second costal cartilage was joined to the manubrial layer, thus shutting out the articulation formed between the upper facet on the second costal cartilage and the manubrium from the true sternal joint, whilst the lower chondro-sternal articulation was continuous with it. These anatomical peculiarities explained the adhesion of the second costal cartilages to the manubrium in dislocations, and the presence of a distinct layer of cartilage on the end of each segment. M. Maisonneuve's accurate description of the layers of periosteum clothing the anterior and posterior aspects of the sternum was quoted. The anterior coat was described by him as thicker than the posterior, strongly adherent to the bones, and forming a sort of felt

possessing a great power of resistance, especially in the transverse direction, whilst the posterior layer was composed of longitudinal fibres, adhering but slightly to the chondro-sternal articulations. As the force of injuries generally acted from behind forwards through the ribs, and as the sternum forms an arch with its highest point at or near the junction of the manubrium and gladiolus, the greatest strain usually fell on the anterior ligaments, and when the segments had been separated by the rupture of these ligaments the gladiolus was carried forwards and upwards in front of the manubrium, and the end of the latter becoming inserted like a wedge between the posterior layer of periosteum and the body of the sternum, that layer was stripped up from the bone as far as, but no further than, the level of the third pair of ribs. Violence applied to the sternum in front, and occasioning fracture, caused the ends of the bone to be bent inwards, rupturing the posterior ligament, but not generally tearing the anterior layer. The author differed from some of M. Maisonneuve's conclusions. M. Maisonneuve, without stating the number of his observations, placed the proportion of diarthrodial to amphiarthrodial joints as high as three out of five, and found the diarthrodial joint more often in females than males. In children the diarthrodial joint was rare. The author's observations showed a preponderance of the amphiarthrodial joint, and the greater frequency of the diarthrodial form in males than females. Out of one hundred fresh sterna examined by him, fifty-one were amphiarthrodial, six ossified, thirty-two diarthrodial, and eleven of a mixed nature, the separation between the segments being incomplete. The diarthrodial joint was met with at all ages, and in very old people without a trace of ossification. The author had found it in old people between seventy and eighty years of age. On the other hand, the amphiarthrodial form was subject to ossification at a comparatively early age. The author had seen ossification at thirty-four and thirty-six years of age. He believed that the diarthrodial joint was formed by absorption after puberty. M. Maisonneuve had ascribed to the clavicles great influence in causing dislocation of the manubrium, because fracture of the clavicle was often found accompanying the injury. The author attributed far more influence to the first and second pairs of ribs, which were united to the manubrium much less movably than the clavicle, and must exercise more power over it both in forcible flexion and extension of the dorsal spine, and in cases in which the violence was applied unilaterally. If a fresh specimen were examined in which the diarthrodial joint existed, or in which there was a tolerably thick symphyseal cartilage, it would be found that, in addition to gliding movements forwards and backwards (varying much in degree in different specimens), some rotary motion was obtainable. This must favour displacement from unilateral violence; and it seemed obvious that the first and second ribs must be chiefly concerned in giving the necessary twist to the manubrium. Two cases were referred to in illustration of these views. A man, aged 45, fell from a plank about fifteen feet to the ground on to his back, the two upper dorsal vertebræ coming in contact with a piece of timber a foot square. Great pain was experienced at the upper sternal joint, and tenderness on pressure remained for several days, showing that the joint had been severely strained, although the force conveyed by the second ribs had been insufficient to occasion a dislocation. A man, aged 36, fell off a gate on to the back of his head and neck, the lower part of the neck coming into contact with a large stone. The fifth and sixth cervical vertebræ were separated, and the first left rib was separated from the manubrium. On examining the sternum, it was found unusually thick, with the upper joint completely ossified. Under ordinary circumstances, the force conveyed along the rib would have caused a dislocation or fracture of the sternum; but, owing to the unusual strength of the bone, the rib became separated instead. A *résumé* of the main points concluded the paper, which was illustrated by specimens, showing the two different kinds of joint and the effects of dislocation.

DR. NORMAN MOORE asked whether there was emphysema of the lungs in any of the cases of fractured sternum described by Mr. Rivington. Bulging of the sternum, which would favour the occurrence of fracture, was common in emphysema.—MR. HOLMES could not quite understand how a blow from the chin should cause a fracture or dislocation of the sternum at the upper part. If the injury were caused in this way, there should be some evidence of contact in the tissues about the chin.—MR. BARWELL could not see why the displacement in Mr. Rivington's case was attributed to a blow from the chin. But he had seen a case in which he had no doubt that the injury was produced in this way, as there was a bruise on the sternum and a corresponding one on the chin.—MR. LE GROS CLARK agreed that there was not sufficient evidence that fracture of the upper part of the sternum was produced by a blow with the chin. The sternum was very rarely broken by direct violence from any cause, unless there were also at the same time fracture of the ribs.—MR. RIVINGTON had not attended to the question of emphysema; but, of course, a great arching

of the sternum would favour the occurrence of fracture. He was not certain that a blow with the chin produced fracture of the sternum in his case; but it might do so in certain cases.

CLINICAL SOCIETY OF LONDON.

FRIDAY, JANUARY 23RD, 1874.

PRESCOTT HEWETT, F.R.C.S., President, in the Chair.

PYÆMIA IN PRIVATE PRACTICE.

THE PRESIDENT delivered an address on this subject, which is published at page 129. At its conclusion, he invited a discussion.

MR. JONATHAN HUTCHINSON: I feel great diffidence in rising to begin this discussion; but I have a few facts to add concerning the investigation of pyæmia, as it occurs, not amongst human beings, but amongst the lower animals. The facts I have to add are these: that pyæmia is extremely common amongst the lower animals, and it occurs amongst them when they are not crowded in the least, but when they are placed under the most perfect hygienic circumstances. I have been engaged the last year or two in collecting such facts as I could respecting the diseases of lower animals. I had some under my own care. Last spring, I had the misfortune to lose a considerable number of ewes, after parturition, from pyæmia. I made *post mortem* examinations of them all. Most of them were treated in the open field. One of the most typical cases was one that was carefully treated out of sheds, away from any contagion. The ewe had not been in contact with other animals which had suffered from suppurative disease; she simply had a retained placenta; the placenta decomposed, and from the decomposition she had internal metritis and the most characteristic pyæmia. The lungs showed pyæmic deposits; and we found the same condition in several animals under similar circumstances, proving most clearly that, at any rate, hospital conditions, or anything in the least allied to hospital conditions, are, as you have well stated in your remarks, not in the least necessary for the production of pyæmia. Unfortunately had another fact in the country bearing in the same direction. My managing man at the farm had his finger bitten by a calf. It was not bitten deeply, and he thought nothing of it. He was teaching the calf how to drink, and it gave his finger a grip. The skin was not broken in the least. He was a man living at a farm-house in a very healthy part of the country, and, although he was not in contact with diseased persons in the least, still he was not himself in robust health. The sequel was this. His finger inflamed, as the result of this grip; he had a thecal abscess; he had intense inflammation of the hand, and the thecal abscess opened. He had very severe rigor, and passed into pyæmia. He was better for a while, and was able to walk about, still looking extremely ill, with his wrist and hand very much swollen. Then he suddenly had a swelling in front of his hip-joint, and a very large abscess grew there; he had another abscess in his leg, and he ultimately died of pyæmia. Now, if it should be suggested that I took him the pyæmia, because I opened his thecal abscess after coming from a hospital, I would reply, that he had his first severe rigor the day before I cut it. It was not cut early enough, because I was only there occasionally, and I did not see him soon enough; I cut it on the day after the rigor which indicated the pyæmia. I do not believe there was any contagion. Only recently, I was asked, in the same part of the country, down in Surrey, on one of the most healthy hills in the locality, to see the gamekeeper of one of my friends, Mr. Hudson. This man had been bitten between his thumb and fore-finger by a ferret. The other gamekeeper stated that he had been bitten twenty times before, and had never taken any hurt; and I daresay many gentlemen present will confirm me by their own experience, that there is nothing poisonous in the bite of a ferret. This poor fellow, a man of very temperate habits, died eight days after this very trifling injury, having had a very severe rigor on the third day after the bite, having acute inflammation of the whole of his arm as a consequence. I should like very briefly to add, that my conviction, from all that I have seen of pyæmia, is this—that it will originate wherever bone is implicated in the inflammation, but that the reason why injuries to bone are so dangerous, and of course idiopathic periostitis amongst them, is simply, and I believe has long been believed by many to be, that, whenever bone is inflamed, you have great risk of inflammation of the veins. In another class of the cases in which there was no proof of bone-injury, in a certain number of ewes, periostitis appeared to be the starting-point, leading to inflammation of veins. Then why does it occur in a few cases in which there is no injury to bone, and in which apparently the injury was of the most trifling description? I will take your case of a girl admitted after puncture and injury, and who subsequently got pyæmia from this simple accident. It is obvious one

must seek some special difference in these cases from the general run of similar injuries from which no ill consequences follow. We must not be content to say it is a mere matter of accident whether a patient gets pyæmia, or not. Although it is a mere conjecture, I think it has a certain amount of plausibility in its favour, that, in cases where pyæmia has followed slight punctured wounds of the soft tissues, or inflammation of soft tissues even without punctured wounds, in which no bone is involved, a vein was implicated. My suggestion would be that, in the case in which the man died from a bite from a ferret, it is not improbable that the ferret's tooth went right through some vein, and that he consequently got actual inflammation of the vein and formation of pus within—inflammation which caused his death. That would be my guess; a mere guess, but still worth keeping in mind in the investigation, to suggest what might have been the cause in the girl dying from the puncture of a needle. Then we come to the final question, as to what extent pyæmia is to be regarded as a disease due to hospital air, or to any influences which are incident to the confinement of patients in hospitals. We shall be very premature indeed, if, reasoning from the facts you have brought forward, showing that pyæmia can undoubtedly arise from circumstances extremely different from those of patients in hospital wards, we conclude that, therefore, the patients in hospitals have no special proclivity or have no special liability. My conviction is, looking at the results of my private practice as compared with the results of my hospital practice, that the hospital patients are in far greater danger of pyæmia. I have been trying, since I heard the title of your address announced, to recollect whether I had lost any patients in private after operations from well characterised pyæmia. I have seen a great many cases in private dying of pyæmia, after periostitis, carbuncles, boils, etc., but I do not recollect more than one or two somewhat uncertain cases in which we have lost patients in private after operations. Now I have lost a great many in the hospital; not many of late years, but in former years, at the London Hospital, a considerable number. At the Metropolitan Free Hospital, I never recollect to have lost one, although it was a crowded, ill-ventilated hospital, under the worst circumstances, the London Hospital being one of the best. I cannot, then, help thinking that there is the possibility of the conveyance of something from patient to patient which may be the cause of pyæmia; and my conviction is, that, under many circumstances, the inflammation of bones and veins—of bone, because it leads to inflammation of a vein—is very often a matter of contagion, and that patients confined in hospitals are very much more likely to get the disease, as the result of contagion, than those treated under ordinary circumstances. Hence, if this line of reasoning be at all correct, we come to the principle upon which we should attempt to diminish the risks of pyæmia in hospitals; not by ventilating; there is no use whatever in introducing fresh air into the hospital with a view to prevent pyæmia; at least, that is my conviction. We have had the windows of a ward kept constantly opened where has been pyæmia, and have given the patients erysipelas by letting draughts blow upon them when asleep, and yet have not prevented pyæmia. I am no believer in fresh air at all in preventing pyæmia. It is a matter of contagion. What you want here is to take away the source of contagion; so far as erysipelas and pyæmia are matters of contagion, you must remove the source of contagion; and, if you can prevent any wound from taking on erysipelatous action, you prevent it from becoming a focus from which other cases may spread by its proximity.

MR. BRUDENELL CARTER: I hope that, in the course of the discussion, some of the senior members, who can look back to the time when venesection was commonly practised, will tell us whether they ever saw a case of pyæmia following blood-letting. I think that, if Mr. Hutchinson's view be correct, pyæmia should have been more or less frequent after that little operation.

MR. CHARLES HAWKINS: I must apologise for presuming to offer an opinion upon subjects connected with large hospital practice; but it was my good fortune for many years to watch very attentively one of the largest operating practices in this country, and certainly one of the largest for variety of operations. My experience, spreading over nearer twenty years than fifteen, can bring to my mind many such cases as you have described, resulting from operations and accidents alike apparently simple. I am very glad that your paper has been read, because I think it will lead people to consider a little more than they hitherto have done the bad character thrown upon hospitals and hospital construction, and hospital dangers. There were two cases very lately that struck me as a very good example of the theory with which you have set out to-night. There was a gentleman riding in the Park before going to Lincoln's Inn—a lawyer—who had the misfortune to be thrown from his horse, and to fracture his leg. When I say he was under the care of the President of this Society, it is sufficient to prove that he was well looked after. He was brought, as many of these

cases are, into St. George's Hospital, and was placed, at your request, in a ward in the new wing, which hitherto had not been occupied. He had for breathing space upwards of 12,000 cubic feet. He had a nurse told off to himself; and he had, as you know, anything in the requirement of diet that he would have had had he been your patient in Belgrave Square. The Governors of St. George's Hospital have never placed any limit upon the diet of their patients. That patient died, I think, within about three weeks, of pyæmia. In the next ward, where there were thirty or forty patients with 1,200 cubic feet, where one nurse and assistant were attending them, going from patient to patient, carrying whatever they might have to take, there was a patient who was bringing up a horse from Epsom; and the individuals who follow that life do not, as we are told, live perhaps the best life physically or morally. He had come to grief with his horse, and the result was his thigh was broken, his jaw was broken, and he made such an impression upon his skull, that, I think, before he left the Hospital, there were seven or nine pieces of bone taken from his cranium. That patient did well in the crowded ward. You may recollect some years ago an outbreak of phagedæna in a ward, so extensive that it was brought to the notice of the Board. That was in the ward at the top of the house, in the best ventilated and best placed ward in the hospital. In the wards below there was very little of this phagedæna. There has been in the ward scarcely any phagedæna since, or hardly any appreciable quantity. But we have had the means, through the munificence of one individual, of putting to the test the opinion which has been given with great earnestness for many years, that if operations were performed in the country, and we only had convalescent hospitals where the patients could be sent out of the contamination of London and the closely packed beds, the cases would do well. Through Mr. Morley's liberality, the governors of St. George's Hospital were put in possession of a hospital at the top of Wimbledon Hill, in, I suppose, perhaps the most healthy position to which we could go within the distance. There are surgeons in this room who have operated at both hospitals. We have seen the most serious operations do well at St. George's, we have seen them do badly; we have seen them do well in Wimbledon, and we have seen them do badly. We have seen patients die at Wimbledon having a room all to themselves, nurses for themselves, where there was no disease of any contagious kind, for the patients were almost convalescent, with an air blowing pretty nearly from the sea. That has not solved the question; they died in exactly the same way. There was a hospital in this town which, I suppose, was about the most perfect specimen of what it ought not to have been that could possibly have been produced. It was a discarded workhouse, built in a churchyard crammed full of bodies. There were some great operations performed in this hospital, and there was a very considerable amount of success. Then a new hospital was set about; the best opinions were taken, and the hospital was built; all the dead bodies were removed, and carried to a distance. I should like those who have practical experience to tell me what alteration has taken place in the results of the accidents and operations in those two buildings; whether in the new building it has been necessary to shut up any wards for a time, or whether the accidents and operations are so very much better. I believe that, when you find these fatal cases occurring in public practice, you will find them in private practice. It was a result of Sir Benjamin Brodie's great experience, that, under certain circumstances, he was exceedingly loth to operate—in north-east winds, and in the great heat of summer; he never much liked an operation when there was a sudden change of weather. But I could relate to you a great number of cases which occurred very much as you have depicted to-day.

Sir JAMES PAGET: I am sure that not the Society alone, but the public, are greatly indebted to you for bringing this matter very prominently before them; for, after all, the question which has been raised by two of the previous speakers is mainly this, and most important of all, whether there really be conditions in hospitals which render pyæmia more frequent in them than in private practice. The question is very difficult to answer; because, even for those who have had large experience in both hospital and private practice, the classes of cases with which they have to deal are so different, and the classes of persons so different, that it is very hard indeed to compare them. I suppose that very few surgeons, even of the largest practice, have had many cases of amputation in private practice for what used to be at least a common cause of operation in hospitals—diseases of the knee or elbow-joint. In private practice these hardly ever come for operation: they are by long care remedied; and we have not the number of cases of amputation in private practice which we can at all compare with those in hospitals. The classes of patients and of diseases are in the two cases so widely different, that it is hardly possible to get the exact numerical results by which the frequency of pyæmia in the two conditions could be determined. We can have only a very general impres-

sion, which may, after some years of experience, be considered almost as true as to which would result from a comparison of numbers. My own experience exactly accords with your own, that pyæmia in private practice is, making a certain deduction for the different class of persons with whom we have to deal, just as frequent, arises from just as trivial causes, occurs after the same class of injuries, and leads to the same fatal results, as ever it does in hospital practice. I tried once to compare my cases in private with those in hospital; and for six years, while assistant-surgeon to St. Bartholomew's, the number of cases was pretty nearly equal. The result, so far as it could be reckoned with reference to what may be called "plagues of surgery", erysipelas, gangrene, etc., was to find that there was no marked difference between them, except that, as one had to do with the well-fed in the one place and the starved, the comparatively temperate and the drunken in the other, there was an advantage in favour of private practice; but, barring this difference, there was nothing that could be fairly established. Among private patients, I have seen pyæmia arise from more trivial causes than I have ever seen in hospital practice. One case I may mention, where an ordinary chilblain in a young nobleman led to pyæmia and rapid death. He was at school in one of the healthiest districts in England, near Bagshot, with every condition of health. A young lady living at Kilburn, well placed for health, manner of life, house, and all circumstances about her, had a slight chafing from the lining of her boot above her tendo Achillis; and from that, in a fortnight, she died, with abscesses in various parts of the body, and with all the characteristics of pyæmia. Again, I may say in the only two cases in which I have seen pyæmia follow operations for piles, both were in private patients. Happily neither were fatal, but both were very well marked. I am sorry, too, that my experience after operations has not been so favourable as Mr. Jonathan Hutchinson's. I have seen quite as great a frequency of pyæmia after operations in private as in hospital practice. Of the very few amputations for disease of the knee-joint which I have done in private, one was for a wound of the knee. It was done at Croydon, on a fairly healthy lad, in a perfectly well arranged house, well aired, well nursed, with every condition of health. He died of well marked pyæmia. After operations upon the breast, I have seen pyæmia as frequent in private practice as in hospitals; and I may here add, as it is, after all, the chief point to which the paper must tend, that not only with pyæmia, but with the other accidents, as they are called, of operations, I have seen no reason to believe that hospitals are places of greater infection, as it is called, or of greater unhealthiness, than what is met with in private practice. I have seen three cases of hospital gangrene in private, and only three under my own care at St. Bartholomew's; and of those three in private practice, one occurred in one of the best houses in Harley Street, another in the best house in Regent Street, and another in St. John's Wood, all three at distances of two or three years asunder, and with no cases of hospital gangrene or any disease of the kind, as far as I know, within reach of the patients. I therefore come to the very clear conclusion that there is really nothing, I will not say in any hospital, but nothing in a fairly well managed hospital, which contributes to the production of pyæmia. My experience is limited entirely to my own private practice and to St. Bartholomew's Hospital; and I would say for certain that, with the exception of a few times when I believe certain wards were under the management of careless sisters or house-surgeons, I have not seen anything in the hospital which would hinder the prevention of pyæmia more than it may be hindered amongst one's patients in private practice. I am therefore very earnest in the hope that the term "hospitalism", which is being applied not only to this, but to many other diseases that occur to surgeons, will be at once and for ever abolished. It seems to me a name altogether unfair. I believe there is not any fair evidence whatever that these maladies following surgical operations are more frequent in hospitals than anywhere else. But it is not on the ground of unfairness alone that I would speak; but because I believe a term such as "hospitalism", or such as another which was invented of "surgical fever", does lead astray altogether those who would inquire fairly into the truth. If pyæmia is to be studied only in hospitals, everyone would be studying it in a wrong direction. The thing must be studied, as you have yourself suggested, equally and with equal fairness in private practice and in hospitals, with an exclusion of all foulnesses of air such as we may have in private houses, and an exclusion completely of all influences supposed to minister to it in hospitals, and then we may come to a fair knowledge of the truth. Before sitting down, in answer to Mr. Carter's question as to the frequency of pyæmia after venesection, I would say that I fear I am one of the few who have had any large experience in venesection. I am just old enough to have known it when it was rife in country practice; for, in the town in which I was apprenticed, it was customary to bleed on every market-day a considerable number of persons who

came with no definite malady except that they believed it would be better to be bled. They came especially in the spring and fall of the year, both of which were elastic terms, extending over two or three months. I do not remember the total number bled, but it was as large a number as liked to come and pay a shilling apiece ; but, so far as I remember, pyæmia never did follow. If it did, the patients must have retreated into the country, and there been lost sight of ; but certainly there were a large number of townspeople bled of whom I should have known the fate. At the time, I never saw a case of pyæmia at all, except after very severe operations. I therefore think venesection can hardly be charged with producing great liability to pyæmia. But then it should be said that the wounds were always treated in the manner in which pyæmia, if arising from atmospheric causes, was likely to be averted ; they were very carefully bandaged, shut up, and very quickly healed.

Dr. ROBERT BARNES : I also remember the time to which Sir James refers, of country bleeding, and have bled nine or ten people in the morning on market-days as quite a common practice. I have on one occasion seen pyæmia follow ; it might have been from a foul lancet, and I cannot say what the cause was, but it was by no means a common event. I was also for sometime physician to the *Dreadnought*. There was a hospital in very peculiar circumstances, isolated from everything on land, and surrounded by water. That hospital had been prone to pyæmia and to hospital gangrene to, I believe, a considerable extent. It came under the charge of Mr. Tudor, the resident surgeon, who was a man of great energy and painstaking, most particular in everything he did ; nothing could be allowed to be done upon which he did not have his eye. He went round dressing his patients in the morning with a pupil behind him ; and, instead of using sponges or anything of the kind, a piece of clean tow was used for every patient to wipe up the secretion, and was pitched overboard instantly. Nothing was ever used to two patients, I believe, for the three years or more during which the system was followed, and there was scarcely ever a case of pyæmia or any infectious disease in the ship. He attributed it, and I believe with very great reason, to the care he took in dressing the patients, putting aside everything that could possibly be carried from one patient to another. In hospitals on shore this is scarcely possible. You have a number of dressers, a large number of students who are using all sorts of things ; and it is with horror I see sometimes a case of ovariectomy in a hospital where the fingers of half a dozen persons are poking into the peritonæum. So far from assenting to the proposition of Sir James Paget that cases of operation are not likely to be more dangerous in hospitals than in private practice, I entertain a profoundly different conviction, as to this particular operation, on account of the enormously increased risk of infection, from the surroundings of the patient in a hospital, as compared with private practice. That cases do arise in private practice, apart from hospital influence, is a matter that cannot be disputed, especially after the evidence of yourself and others in this room, and what one sees in the practice in my department especially, analogous to that which Mr. Hutchinson has mentioned among the lower animals. There undoubtedly retained placenta, or some cause of the kind, may get up symptoms of pyæmia, and patients die from a form of fever which I call autogenetic, as arising from the patients' own conditions, apart from everything external anywhere. There is a point also bearing somewhat in favour of Mr. Hutchinson's idea that the veins are concerned, certainly in the uterus. If the placenta be not removed, it goes into decomposition, and there you have a series of veins of a kind peculiarly liable to be affected by the imbibition of foul matter. There is no department of practice which does not yield some facts and some experience to illustrate this subject. It cannot be looked at entirely from one side, either inside a hospital or out of it, from one practice or another. It is one which should engage the earnest attention of every member of the profession.

Mr. CAMPBELL DE MORGAN : I think that there are perhaps two questions mixed up together, with reference to the occurrence of pyæmia in hospitals. There is no doubt, I suppose, that if several patients be put together who have had operations, and one get pyæmia, let it be introduced how it may, the others may be liable to it if exposed to any contact whatever ; and that therefore, in the event of pyæmia occurring in the wards of a hospital, it is more than probable you will get more than your average of cases in a certain number. I suppose concurrent testimony would show that the observations which you have made are correct—that it does occur in private, dotted here and there, not communicated from one to the other as it might be in hospitals, and without any apparent cause, such as is supposed to exist in hospitals. But may it not be from very much the same causes in both ? I should doubt whether we may not have even in the best private houses much the same sources of contamination which we have in hospitals, and some-

times even more. The most fatal case of blood-poisoning I ever saw, was in a lady living in what appeared to be a very well ventilated house in the north of London. She died of most intense blood-poisoning, after the removal of a tumour in the neck, without any indications of phlebitis, and certainly no bone exposed. It turned out afterwards that there was a general feeling of *malaise* now and then on the part of some of those who inhabited the house, and they occasionally found a certain amount of uncomfortable smell about it. There was no doubt that here there was imperfect drainage. If you come to look to the connection of the various ventilating apparatus connected with the sewers and so on, I doubt whether there are many houses in London in which you would not find an imperfection which you would not allow to exist in a hospital, if you knew of it. You may have this undoubtedly without any condition, of which you or I or the patients themselves would be conscious. We had a very curious illustration of this, which has been quoted so often, that I am almost ashamed to mention it again. In the Middlesex Hospital there were two beds, one on each side of a window, and patients in them almost invariably, if they had a cold or were operated on, had erysipelas or pyæmia. There was nothing in connection with the window in the part of the ward, so far as smell was concerned, to call attention to it ; but it was found that at some distance below, in the area, in a line communicating with the window, there was a dust-bin. The dust-bin was ordered to be cleansed out, and the window was not allowed to be opened again, and for two or three years we never had a case of erysipelas or pyæmia in those beds. Then the precaution was neglected, and the thing again occurred. I do not quite agree with Sir James Paget as to the notion of covering up the wounds in cases of bleeding having anything to do with prevention of pyæmia. I think it was the immediate closing of the vein, so that nothing could be carried up : it was not from the exclusion of air. I am becoming sceptical as to the effect of air upon wounds in producing decomposition, or giving rise to unhealthiness. I think that free exposure to the air, even of a hospital ward, seems to agree with wounds just as well as treating them in the most careful manner by dressings. I have adopted it in several cases of late, and certainly with very admirable results, and with very little trouble indeed. I think that pent-up matter is very often the cause of all the mischief you get ; but if it can drain away, and not be collected in the dressings, you thereby would prevent contamination. I agree also with what Mr. Hutchinson has said, that pyæmia is connected a good deal with the conditions of the veins of the bone. There again it is from pent-up matter, as you get in cancelli of bone : the matter is collected and decomposing from lodgment, not from exposure to the air, but because the air does not pass freely enough to it. Anything that has a septic tendency, I take it, will have the effect of producing pyæmia or its allied condition, erysipelas, either in hospitals or in private. I quite think the notion of "hospitalism," or hospital disease, has been carried a great deal too far ; that it is altogether a mistake to suppose the hospitals are so very much more prone to these diseases originally than private houses. That they will spread, is another question.

Mr. BRYANT : In listening to your paper, the chief point that attracted my attention was the very interesting one in which you threw the weight of your authority towards the idea that pyæmia is not a purely hospital disease. This has long been very much my opinion, not simply based upon the theory of the question, but upon the observations which I have been able to make both at Guy's Hospital and elsewhere. A few years ago, when I was working at the subject of pyæmia, it was an object of interest to me to find out whence these cases of pyæmia came ; and I was very much struck with the important fact that the worst cases of pyæmia which we saw in Guy's Hospital were admitted with the infection ; and that these cases all followed trivial accidents, such as small wounds, slight ulcers, boils, and carbuncular affections of the milder form ; and, in analysing the results found after death, I find that after the trivial accidents the pathological results were far more severe than when they followed the graver accidents which we are in the habit of seeing in a hospital ward. That is to say, in patients dying from pyæmia following simple contusion, or abscess, or boil, we were more apt to find multiple abscesses. We would find suppuration in a larger number of glands, in the liver, the kidneys, the spleen, and more frequently in the heart. Of course, in the lungs we expect to find them. All this was in cases trivial in their origin, being almost all cases of simple suppuration, or of suppuration in the cellular tissue following contusion. That fact was a most striking one ; and, I confess, led me to feel that pyæmia could not be regarded fully or solely as a hospital disease, and that we should find as much pyæmia probably outside, if we could get a fair statistical comparison, as we find in the hospitals. I was very much struck, on hearing your cases, that so many of them were attacked first by erysipelas ; and I suppose we must class erysipelas amongst the so-called hospital diseases. I have

been taking for the last three years considerable pains to find out really how far such an inference is correct; and I find, in looking at our erysipelas ward in Guy's Hospital, that two thirds of the cases of erysipelas admitted into the ward have been admitted from outside the hospital; and that only one-third of the cases have been cases drafted from the surgical or medical wards. Erysipelas, taking it merely from these statistics, cannot be looked upon as simply due to hospitalism. Of course, erysipelas and pyæmia are connected with hospital work. There can be no questioning the facts that Dr. Barnes has given, and the facts which we are in the habit of noticing in hospitals, where, from the want of cleanliness or attention on the part of the dressers in the matter of sponges, erysipelas and pyæmia are more apt to spread; but, to say that they are simply due to, or connected with, hospital practice, is very unfair. Dr. Barnes gives us the most rigid test, that connected with puerperal patients, and more particularly with a severe operation of surgery, such as ovariectomy. But, unless unusual care or extra precautions be observed in the performance of ovariectomy, we cannot expect the same success with which we should meet were we to exclude all those extraneous influences which we know to have some kind of influence in producing erysipelas and pyæmic affections. Feeling, as I do very strongly, the great necessity of observing extreme care as to cleanliness, air, and so on, in hospital work, I must say I should not like to sit quiet without expressing my conviction that pyæmia and erysipelas are not really hospital diseases; that they are in their fullest extent, if we could get a fair amount of statistics, quite as common outside the hospitals as they are in them.

Mr. T. HOLMES: The observations which Mr. Charles Hawkins made with respect to the hospitals at Wimbledon and St. George's, brought to my mind a rather striking circumstance that occurred in my own practice at these two hospitals, and one to which I have no doubt he was alluding. Mr. Morley, as Mr. Hawkins justly observed, left us the opportunity of trying the experiment as to whether operations would succeed better in the country than in town; and I thought at one time of testing this by performing a series of amputations at Wimbledon and comparing them with a series of similar amputations at St. George's. I commenced with two cases, which seemed to me to be very appropriate for the experiment. One was a middle-aged man suffering from chronic disease of the tarsal bones, a perfectly healthy individual who had never, as far as I could find, had any serious disease in his life, and certainly never suffered from erysipelas. The other was a man broken down by all kinds of dissipation, and no doubt, to a certain extent, a bad subject for an amputation, but otherwise there was nothing very remarkable about the case. I sent these two men down to Wimbledon, and performed amputation upon both on the same day, a few days after their admission into the hospital. They were treated in separate rooms, neither room having even been used before; one was in one of the wards of the hospital, the other in one of the private rooms. They were separate from each other, but attended by the same nurse; otherwise they were in exactly the same conditions as a man would be in private practice. The rooms were entirely free from all possibility of contamination. They were not attended by medical students at all, but by the resident medical officer of the hospital. There were no other cases in connection with them whatever, and all the other cases in the hospital were simply convalescent cases. I never saw two cases more likely to do well. One of them was certainly a case of amputation which anyone would have expected to recover—merely Syme's amputation for chronic disease. Both of these people died; one of pyæmia, and the other of erysipelas. The erysipelas did not attack the stump at all, but simply the head. This appeared on the fourth or fifth day, and was followed shortly afterwards by sloughing of the skin of the back to an enormous extent, a piece as large as a soup-plate sloughing a few hours before his death. The patient died the fifth day after the amputation. The other man died on the seventh day of pyæmia. I must say that, although the occurrence of two isolated cases of the kind proves nothing, still it discouraged me so far, that I ceased to think it worth while to spend the great amount of time that would be necessary to carry the experiment further.

Mr. CROFT: I had the pleasure of being Dr. Barnes's colleague at the Seaman's Hospital when he was physician, being then assistant-surgeon. I do not think that I gained, during my five years' experience of surgical work, the same impression which seems to have adhered to Dr. Barnes's mind since his leaving the hospital. I think that it would be scarcely fair to the reputation of those gentlemen whom Mr. Tudor succeeded, to leave the Society under the impression that Mr. Tudor found a system of dressing at the hospital which could be condemned. When I say that it was under the care of Mr. Busk, I think that it is a sufficient guarantee that the utmost care was taken both surgically and hygienically. Mr. Busk had Dr. Rooke under him as resident surgeon, who worked there in, I think, no less energetic a manner than Mr.

Tudor; and I think it is only fair to Mr. Busk and to Dr. Rooke, who is dead now, that I should say that I think they had done all that they could do. I know that, after some alterations had been made in the state of the hospital, an improvement took place in the number of cases of erysipelas, pyæmia, and phagedæna; but, after a time, while Mr. Tudor was there—I am sure, if he were here, he would bear out what I say—both pyæmia, erysipelas, and hospital gangrene were rife. I had many cases of sloughing bubo under my care to treat—hospital phagedæna, and not simply syphilitic phagedæna. I do not wish to say anything about hospitalism beyond this, that I am extremely glad Sir James Paget has said openly at this Society he hopes the name will be dropped. The sooner the name is forgotten, I think, the better, for nothing is clearer in my mind than that it is a most unfair term. I think that the comparison between the experience at hospitals and the experience in private practice has perhaps taken up, I was going to say, too much of our attention; at any rate, it seems now that we have arrived at a point when our attention should be, as you have proposed, turned into a new way of investigating pyæmia. One is too apt to take a number of cases of pyæmia in the lump, and say that so many cases of pyæmia have occurred, without studying each particular case. It seems to me now that we have arrived at this stage when accurate reports should be presented. When I say "accurate," I mean reports which will extend not only to the general condition of the patient, the general condition of the house in which the patient was, or the hospital in which the patient was, but which will give details of the juices in the patient's body; I mean the condition of the nutritive fluid in the patient's body. When we have a series of reports of cases including such statements as I have referred to, we shall then have a good basis to go upon. At present, it is only here and there that we get an account of the condition of the patient's blood or lymph in pyæmia.

Dr. BARNES: The conclusions of Mr. Tudor were carefully published for two or three years, and I think that, if the Fellows refer to the reports, they will see that what I have said will be borne out.

Dr. BASTIAN proposed that the discussion be adjourned till the next meeting of the Society.

Mr. HUTCHINSON seconded the proposition, which was agreed to.

DUBLIN OBSTETRICAL SOCIETY.

SATURDAY, DECEMBER 13TH, 1873.

LOMBE ATTHILL, M.D., Vice-President, in the Chair.

Preventive Treatment of Post Partum Hæmorrhage.—Dr. A. H. MC CLINTOCK read a paper on the prophylactic measures which may be employed, where there is reason to expect *post partum* flooding. The conditions which determined the production of hæmorrhage after delivery were—1. Deficiency of muscular contractility of the womb; 2. Vascular excitement; and 3. Want of coagulable power in the blood. Vascular excitement, towards the end of gestation and during labour, always foreboded hæmorrhage. This had been especially pointed out by Madame La Chapelle and by Gooch. A rapid and jerking pulse, at the end of labour, indicated the probable occurrence of flooding. The experience of a woman's past labour afforded some clue as to possible inertia of the uterus. The presence of any tumour in the womb, or unusual distension of its cavity, from plurality of fœtuses, or from dropsy of the amnion, should also awaken suspicion. As pointed out by Dr. Whittle and Dr. Atthill, sharp, quick, abruptly ceasing pains, in the second stage especially, were also commonly forerunners of hæmorrhage. The interval between the pains was generally increased under these circumstances. Extreme mental depression, and chloroform sometimes, exerted a paralyzing influence on the uterus. Chloroform, however, in other cases was of advantage in allaying the influence of mental depression, but only then was its use judicious where there was reason to dread the occurrence of flooding. Sometimes *post partum* hæmorrhage took place without any warning whatever, and in such cases particularly the woman's previous lying-in history was of great importance. The prophylactic measures, to secure quietude of the circulation, and to allay local vascular excitement, were open-air exercise, abstinence from stimulants, and regularity of the bowels. Digitalis and cooling medicines might be given, and in plethoric persons, blood might be drawn from the arm, as strongly recommended by La Chapelle. But sometimes rapidity of the circulation depended on anæmia; and here the opposite treatment must be adopted. Where the character of the pains, or the previous history of the patient, furnished grounds for expecting hæmorrhage, reliance must be placed—in addition to the slow extraction of the fœtus, and the following down of the uterus with the hand, etc.—on two measures; the letting off the liquor amnii by artificial rupture of the membranes, and the administration of ergot of rye. The time for puncturing the membranes was when the os was

nearly fully dilated; the presentation, of course, being known as a head or pelvic extremity. The most effectual resource was the use of ergot. If it failed to excite uterine action, it would have no toxic influence on the child; and if it stimulated the uterus, the child would probably be born before any danger to it could arise. To be of real service, the ergot must be given some little time before delivery, and the unlikely presence of morbid adhesions of the placenta might practically be disregarded. Two drachms of the liquid extract of the *British Pharmacopœia* might be given. In primiparæ, however, a smaller dose was sufficient; and the soft parts, as well as the os uteri, should be pretty well dilated before the drug was administered. Where deficiency in the coagulating power of the blood appeared to be a predisposing cause of *post partum* bleeding, Dr. Mc Clintock had sometimes given gallic acid for days or weeks previously, and with advantage. Dr. Bassett, of Birmingham [BRITISH MEDICAL JOURNAL, November 22nd, 1873], had used iron with equal success. Denman, lastly, under similar circumstances, commonly administered some tonic medicine, such "as one grain of the zincum vitriolatum two or three times a day," for several weeks before delivery, and he strongly recommended the cold bath as an additional preventive.—The CHAIRMAN believed that the condition of the pulse was a most useful guide as to the possible occurrence of flooding after delivery; at the same time he considered that bleeding in plethoric women was not called for. In anæmic individuals, or in patients of feeble muscular habit, liable to nervous exhaustion or the subjects of mental depression, a quick pulse was of very serious import. Here the forceps was valuable in addition to ergot, a drug which he never administered without previously rupturing the membranes. He thought that *post partum* hæmorrhage was sometimes induced by the too early and forcible removal of the placenta.—Dr. CHURCHILL said that if the pulse continued at a high rate after the second stage, we might anticipate hæmorrhage. He strongly objected to the forcible extraction of the placenta, but, at the same time, he thought it should come away within a very short time. By grasping the uterus firmly, he found that the after-birth generally came away within five minutes. He agreed with the author of the paper as to the time for giving ergot.—Dr. JOHNSTON was in accord with Dr. Mc Clintock and the last speaker, as to the administration of ergot. Out of 686 cases delivered at the Rotunda Lying-in Hospital during the year ending November 5, 1873, there were seven cases of flooding—six slight, and only one requiring the injection of solution of perchloride of iron (the patient recovering). In 419 of these cases, the second stage was concluded within thirty minutes, 337 being pluriparæ (in two, slight flooding), and fifty-six primiparæ (no flooding); four were twin cases (no flooding), and two suffered from severe hæmorrhage on former occasions, but not on this. He considered that these figures proved that a short second stage did not predispose to hæmorrhage. With Dr. Churchill, he advocated the early removal of the placenta, but without unnecessarily active interference.—Dr. CRONYN laid great stress on the importance of judicious manipulation of the uterus, in obviating the likelihood of flooding. He also thought that hæmorrhage was apt to follow where portions of the membranes were left behind in the uterus.—Dr. DARBY considered that quick pulse was an important sign, but he could not agree that hæmorrhage after one delivery predisposed to a recurrence of the accident after subsequent labours.—Dr. DENHAM attached little value to rapidity of pulse in the second stage. But he never left a patient's room without feeling the pulse. If it then continued fast, it was a bad sign. Two rules should be carried out in removing the placenta. One was to make pressure over the uterus, the other, to use gentle traction on the cord. Ergot, given towards the end of the second stage, certainly prevented hæmorrhage; but its exhibition, after the expulsion of the placenta was useless.—Mr. A. H. RINGLAND mentioned a case of a pluripara in whom *post partum* hæmorrhage recurred after each of three pregnancies. Morbid adhesions seemed to be the cause.—Dr. Mc CLINTOCK replied, and the society adjourned.

PATHOLOGICAL SOCIETY OF DUBLIN.

SATURDAY, DECEMBER 20TH, 1873.

ROBERT D. LYONS, M.D., President, in the Chair.

Colloid Cancer of Ovary.—Dr. KIDD exhibited a very large tumour which he had removed on December 17th. When the patient was seen for the first time on November 22nd, a swelling extended to the pubes from two inches above the umbilicus; the abdominal walls were lax and pendulous; and an irreducible hernia existed at the umbilicus. A distinct fluctuation could be excited in the tumour, which appeared to be composed of a solid movable mass floating in fluid. Over it, there was dulness on percussion. An examination of the uterus, *per vaginam*, showed that this viscus was movable and empty, three

inches in length; a sound introduced into it moved through an arc of ninety degrees, when the abdominal tumour was manipulated. The diagnosis was made of a multilocular ovarian tumour, attached to the uterus. The patient, a lady aged 36, stated that she had been twelve years married, had two children, the younger nine years of age; had never miscarried, and was regular in menstruation. In December 1872, she imagined she was pregnant, but the catamenia did not cease until the following March. In August, a scanty flow recurred. On December 4th, 1873, the principal abdominal measurements were—circumference at line of umbilicus, forty-seven inches; length from pubes to umbilicus thirteen and a half inches; ditto from xiphoid cartilage to umbilicus, eight and a half inches. Two days later, eight pints of fluid were drawn off by an aspirator, the size of the lower part of the abdomen being thus reduced. On December 12th, the umbilical tumour burst, and, subsequently, the size of the upper part of the abdomen diminished somewhat after the escape of a quantity of serous fluid. The hernia became semi-solid, and irreducible, the pain in the epigastrium lessened, and the appetite returned. In operating on the 17th, the peritoneum was found much thickened, and the tumour was universally adherent to it. The ovarian tumour consisted of a multilocular cyst, each loculus filled with gelatinous material similar to that which had escaped through the umbilical opening. The disease was a good example of the alveolar or colloid cancer of Cruveilhier. Dr. R. J. Harvey had carefully examined the structure, and reported that it presented the histological characters of colloid carcinoma, many of the cells embedded in the stroma apparently undergoing fatty degeneration, and so giving to portions of the mass its whitish look. The patient sank in eighteen hours from the time of operation.

Cerebro-Spinal Arachnitis.—Dr. GRIMSHAW showed the brain and spinal cord of a railway porter, aged 23, who had died of this affection on the previous day, after an illness of six days' duration. On Saturday, December 13th, while walking in the street, the patient was suddenly attacked with green vomiting, followed by faintness and pain in the head and back of the neck. When admitted to Steevens' Hospital, his head was retracted, he had herpetic patches on the left chest and lower jaw, complained much of pain in the back of his neck, and of hiccough. His pupils were now dilated, but finally became contracted. He was delirious, although easily recalled to consciousness. The pulse was not jerking, there was nothing peculiar about the respiration, and the temperature never rose above 103 degs. F. The existence of hyperæsthesia was rather doubtful. A vertical tremulousness of the eyes was noticed. Opisthotonos became very marked, and an attack of convulsions ushered in death. At the necropsy, a number of characteristic small black spots were observed over the back. The spinal cord was softened at its origin and about the lower part of the cervical region; there was also some effusion of lymph within its membranes. The surface of the brain was generally hyperæmic, and localised deposits of yellowish-green lymph were found, especially about the circle of Willis. Most of the cerebral nerves were bathed at their point of origin in this lymph. There was softening of the cerebellum also. The specimen presented most of the pathological appearances observed in the epidemic of 1867.

Enteric Fever; Profuse Intestinal Ulceration with Constipation.—Dr. A. W. FOOT showed the intestines of a young man, aged twenty-three who had died on the twenty-fifth day of enteric fever. During the period which he had been under observation in hospital, there had been marked constipation. After twelve days inaction of the bowels, an enema of turpentine brought away some fecal matter from the large intestine. The cause of death was hypostatic pneumonia; the lung complication had been serious and striking from the beginning. In the last six feet of the ileum there were forty-seven ulcerations of various sizes, from cup-shaped ulcers of solitary follicles of the size of a hemp-seed, to oval patches an inch and a half in length. There were many small ulcerations in the cæcum. There was neither perforation, hæmorrhage, nor peritonitis. The eruption was confined to three or four rose-coloured spots.

MEDICATED CANDLES.

MESSRS. FIELD and Co. have introduced, for medical purposes, candles medicated with the balsams of which the vapour is most often beneficially inhaled by persons suffering from bronchial irritation and spasmodic asthma. Among these are benzoin, storax, etc. The candles give out while burning aromatic fumes in abundance. Among the various modes of diffusing balsamic vapours in the air inspired by patients with various affections of the breathing, this novel method is one of the most ingenious yet proposed. It is clean, effective, and free from all trouble. It deserves, and no doubt will receive, trial by medical men.