of consciousness. As the definition or type of the slight seizure is vague, it may be well to give an example.

A boy, ten years of age, had been subject to fits for two years. At the first, they were so slight that his mother said they thought he was "in a deep study"; he used to sit "as if he were thinking". The attack was only of a few seconds' duration, and no alteration of complexion was noticed. Later on, in his fits he would turn up his eyes—slight spasm of very small muscles. Later still, there was, besides this, occasional shaking of the body (imitated before me by tremor). I saw one. Whilst sitting, the boy blinked his eyes; he did not alter in colour; he then drew in a deep breath, and rapidly all was over. The paroxysm was so sudden and short that I could not make any special investigations.

Now for cases which do not fully resemble the type, for cases which are subtypical. This boy lost consciousness, but it is to be insisted upon that there may be epilepsy proper without loss of consciousness. There may be defect of consciousness only. For example, a patient may say that he is not unconscious in his attacks, but add that he does not know where he is; that he hears people talking, but does not know what they say; this shows that he has not *lost* consciousness, but it shows also that he is defectively conscious; in popular language, he is "confused", becoming so suddenly, and remaining so for a few seconds only. "To be confused" and "to be defectively conscious" are synonymous expressions.

Then we have evidence from what we observe that a patient in an epileptic paroxysm is not absolutely unconscious. The patient may not only utter incoherent words, but may reply to questions vaguely, as, "What do you say?" or, more definitely, he may reply quite correctly. There is the difficulty here that the talking, vague or definite, may be just after a very slight and transient fit; but in an empirical or clinical inquiry for diagnosis the distinction is of little moment.

So our first exception or subtype is that the patient may be only defectively conscious, or, popularly speaking, simply confused.

More than this, there may be, according to the patient's report, no affection of consciousness at all. It is well known that a common precursor of affection of consciousness in a paroxysm of epilepsy proper (slight or severe fits) is a strange sensation at or near to the epigastrium, "in the chest", or "at the heart", some patients may say. Children call it "pain", or "stomach-ache". The sensation is variously described by adults as "sinking", "nausea", and it is frequently said to be accompanied by fear. Now, patients will tell us that, for months before they had any affection of consciousness preceded by an epigastric sensation, they occasionally had that sensation alone, and thought little or nothing of it; some thinking it to be owing to disorder of the liver, although it was sudden and transient (paroxysmal). This, then, would be a second subtype of epilepsy; it would be for a time all the paroxysm.

It may be said, your "definition" by type here breaks down utterly for practical purposes, for in this subtype consciousness is not affected at all. If literally taken, it does break down; we ought and must take such cases as subtypes. It is certain that these sensations may occur paroxysmally for months without anything further. The fact that such a paroxysmal sensation is often epileptic in nature is shown by the future of the cases: undoubted epileptic affections, attacks with convulsion and tongue-biting come on, and each of these severe paroxysms is preceded by the sensation. What was once all, is now the so-called warning of the severe fit. Strictly speaking, it was when it occurred alone a rudimentary or incipient fit of epilepsy, and when it becomes what we call a warning, since a severe fit follows, it is the inception of the severe fit. Besides, I should doubt whether consciousness was not affected to some trifling extent. However, I state what patients say, and at any rate the affection of consciousness is insignificant or most transient, or both.

Then there are cases of slight epileptic vertigo. By epileptic vertigo, I do not mean any sort of "confusion in the head", I mean real vertigo; a sensation of movement of external objects, or a sensation of turning of the body itself; at the beginning of some severe attacks of epilepsy proper, the patient does actually turn to one side, "turns right round", as the friends say. Hence I do not use the term epileptic vertigo as the name for a slight *degree* of seizure, but specially for a slight seizure of a particular *kind*, or for a seizure beginning in a particular way. I will not say there is no defect of consciousness in cases of epileptic vertigo, but it may be very slight and transient, and according to the patient and his friends none. I do not believe that consciousness is entirely unaffected in any sort of giddiness, however caused. Let a man try to think, when giddy, of something complex and difficult to him, to tackle some problem he could just manage to solve when well; he would fail when giddy.

So, then, whilst we take paroxysmal *loss* of consciousness as our type, there are subtypes. There is not only loss of consciousness, there is defect of consciousness, and there is also no affection of consciousness.

The paroxysmalness of the seizure and the sequel show that each is truly epileptic; they occur at different periods of the same case. Indeed, we may have each of the three degrees at different times in one case.

REPORT

ON

THE OUTBREAK OF DIPHTHERIA IN THE GRAND DUCAL FAMILY OF HESSE-DARMSTADT.

By J. OERTEL, M.D.,

Professor of Laryngoscopy in the University of Munich.

I HAVE undertaken the more readily to publish a report in an English Medical Journal on the outbreak of diphtheria at the Grand Ducal Court of Darmstadt, because I feel only too deeply that, before all others, the people of England have a right to demand an exact account of the morbid process, and of all the circumstances connected therewith, by which they have lost a much-beloved member of their august royal family.

From the very beginning of the epidemic, a general interest has been evoked by the fact that a most severe form of diphtheria attacked, within a very few days, not fewer than six members of a family, which appeared to be much less exposed than others to obnoxious influences, and to the common modes of infection. Rapid limitation or destruction of the poison, which had been imported from without, might have been expected as another consequence of the advantages which their superior social position seemed to offer to them. Again—and this fact has not yet been much emphasised—it deserves attention, that the disease, in spite of its malignant form, has been limited to the Grand Ducal family, whilst not a single member of the numerous household or of the entire staff of nurses has been attacked. It will be seen that the facts enable us to draw quite special conclusions as to the nature of the contagium acting here, and as to its mode of dissemination. Finally, the intensity which characterised the diphtheritic process in all the patients without exception, and the increase of malignancy which took place *pari passu* with the propagation of the infection, are two most remarkable features. It is not at all rare for an entire family, or the greater number of its members, to be attacked by epidemic diphtheria; but, as a rule in such instances, there will be graver and slighter cases, the latter generally being even more frequent than the severe ones. In this instance, however, all the cases belonged, from the very beginning, to the worst forms which the disease can take, and in each individual case there was danger to the patient's life within a short time after the outbreak of the affection.

The commencement of the infection, and the propagation of the local process in the first affected members of the Grand Ducal family, were described, on the 25th of November, in the *Berliner Klinische Wochenschrift*. From this, the report passed into several German and foreign journals, and an abstract was given in the *BRITISH MEDICAL JOURNAL* of December 14th.

The first case (Princess Victoria, aged sixteen) was diagnosed by Dr. Eigenbrodt on the 6th of November. The patient was at once isolated, and the Grand Ducal family most carefully watched. There was no new case during the next five days, but after that, at intervals varying from five and a half to eight days after the first outbreak, the other members of the family were attacked, between the 11th and the 13th of November. Princess Alice, aged six, fell ill in the night of the 11th-12th of November; Princess Mary, aged four, in the course of the 12th; Princess Irene, aged twelve, in the night from the 12th to the 13th; the Hereditary Grand Duke Ernest Louis, aged ten, on the afternoon of the 13th; and finally, on the 14th, eight days after the beginning of Princess Victoria's, and two and a half days after the beginning of Princess Alice's illness, His Royal Highness the Grand Duke himself was attacked.

After this, no further propagation of the disease seemed likely to take place; the whole of this large household, the numerous staff of servants and nurses, and all others having intercourse with the palace, were spared. Suddenly—all the previously infected persons (with the exception of Princess Mary, who died during the night of the 15th) being already convalescent—on the 7th of December, twenty-three days after the last infection, Her Royal Highness the Grand Duchess fell ill, with high temperature, and all the symptoms of the most malignant croupous form of diphtheria. In the previous cases, the

intensity of the disease, the febrile symptoms, and the local affections, had been quite extraordinary. There were extensive fibrinous exudations on the tonsils, the soft palate, the uvula, and the posterior wall of the pharynx, in the form of greyish, white-and-greyish, red flocculent membranes, which varied in thickness from two to three *millimètres* (.08 to .12 inch). The infiltration and coagulation of the fibrinous matter took place so deeply in the structure of the mucous membrane that, after the expulsion of the membranes, there remained everywhere extensive ulcerations, which healed but very slowly from their borders, the suppuration being limited. In the case of the Hereditary Grand Duke, not only the pharyngeal tissues, but the larynx also, was attacked; and there were hoarseness and complete aphonia. The expulsion of the diphtheritic membranes was accompanied by hæmorrhage, and was quickly followed by the formation of new and more extensive ones, until at last the intensity of the disease was vanquished, and slow convalescence took place. When I saw the Prince last, on the 24th of November, there were still, in some spots, remains of the former membranes and extensive ulcers, which became cleaned and healed up during the following days. This is an important and remarkable fact with regard to the ensuing disease of the Grand Duchess.

In her case, the morbid process was still more malignant, and more destructive to the entire system. Her Royal Highness had a chill, with subsequent febrile symptoms, on the evening of the 7th of December, and the physician sent for at once recognised the beginning of the diphtheritic process in the pharynx of the august patient. The fever soon reached a dangerous height; the temperature varied between 102 and 105 deg. Fahr., and fell only for a few hours to 101 deg., reaching immediately afterwards again 103 and 104 deg.; the pulse, which was small and empty, participated in these conditions, varying generally between 120 and 130, and having a slight remission to about 100. The diphtheritic patches appeared first on the right, later on the left, tonsils, all the pharyngeal tissues being highly inflamed from the very beginning, and the cervical and submaxillary glands, first of the right, afterwards of the left side, becoming swollen and painful. When, on the 10th of December, I saw the patient for the first time, both tonsils were covered by thick flocculent greyish-white membranes; the uvula had attained a length of three times its normal size; it was highly oedematous, and almost completely filled the space between the two enlarged tonsils. Further, the membranes had extended from the tonsils to the posterior wall of the pharynx, and there was, on both sides of the neck, besides the glandular swelling, considerable swelling of the periglottic cellular tissue. Feeding and deglutition were extremely difficult and painful; the salivary secretion was augmented, and gave the patient much discomfort. There was oliguria with albuminuria. There was nearly complete sleeplessness from the beginning of the disease, and gradually mental depression set in, corresponding to the gravity of the case and to the rapid decline of the vital powers. These had already been considerably weakened before this attack by another chronic affection, which had been present for some years. Thus, she had no hopes of recovery. On the next day, the temperature in the early morning was a few tenths of a degree lower, but soon rose again, and exceeded 102 deg. in the course of the morning. At the same time, there appeared a greyish-white membrane over the whole length of the uvula, and the membranes on the left side became more extensive. Although from this time there were still some fluctuations in the height of the fever, yet the temperature remained now constantly much over 102 deg., and even again reached 104.5 deg.; the pulse varied between 116 and 130, its quality gradually falling. The local process constantly increased *pari passu* with the increase of the fever; and with each new elevation of the temperature, a further extension of the membranes could be observed. Their configuration was noted diagrammatically at repeated periods of duration. The old membranes rapidly increased in thickness from the continuous fibrinous exudation, and from purulent infiltration, and began partially to be detached in shreds on the 12th of December. Corresponding with the progress of the pharyngeal affection, the lymphatics became enlarged, as their vasa afferentia absorbed the lymph from the diseased parts. On the 13th of December, an oedematous swelling, of the size of a hen's egg, had been formed under the chin, between the hyoid bone and the thyro-hyoid membrane, indicating that the lymphatic glands, which lie between the hyoid bone and thyroid cartilage, and into which the lymphatic vessels of the epiglottis, the aryepiglottic folds, and the ventricular bands empty themselves, had become highly infected, and that the diphtheritic process had now involved the upper parts of the larynx also. The voice was at this time somewhat rough and hoarse, but showed especially a nasal and pharyngeal character. An active decomposition had simultaneously begun within the thick crusty coverings of the pharyngeal cavity. The fœtor arising from this was controlled by the energetic employment of disinfectants, and after in-

halations of an 0.25 per cent. solution of permanganate of potash. Together with the further progress of these symptoms, the vital powers had very much decreased; the nights were sleepless; feeding was very difficult; the quality of the pulse became still worse; its irregularity pointed out the danger arising from the severe affection of the heart. On the evening of the 13th, the patient fell asleep. During this sleep, which was very deep, and lasted for several hours, she breathed so entirely freely from her chest, that I then directed the attention of the bystanders to the fact that the disease had not produced any obstacle to respiration in the larynx or in the other air-passages. However, what was to be expected from the participation of the heart soon ensued. There was at midnight profuse perspiration, followed by rapid decline, coma, and unconsciousness. The august patient tranquilly expired on the morning of the 14th of December, at half-past seven o'clock. Thus the series of these grave cases in the royal house was concluded, the epidemic having raged with an intensity certainly seldom to be observed.

One explanation of this extraordinary occurrence has been mentioned by us in our first report, viz., the particular disposition of the tonsils and pharyngeal mucous membranes of all the illustrious patients towards acute and chronic inflammations, as well as an extraordinary susceptibility to this specific contagium. On the other hand, certainly, the virulence of the imported infectious matter, its direct transmission by kisses, and the quantum which was brought into play by this mode of transmission, are of grave importance. We are fully justified in making direct use here of the results of experimental investigations and attempts at inoculation. The intensity of the infection, the degree of the anatomical lesions, and the rapidity with which the process destroys the life of the animal, were, in all my experiments under like conditions, proportionate to the quantity of the inoculated diphtheritic matter. We likewise see in practice that those cases run the most rapid and most fatal course in which *direct* transmission of contagious matter has taken place. Proof is to be found in the fatal cases of Blache, Valleix, Gillette, Seehusen, O. Weber, Heine, and of other physicians who have fallen victims to their professional devotion in attendance on diphtheritic cases.

The treatment was directed, according to the present indications, against the local affections and against the general process. Following the method which I have adopted for more than twelve years, and which I still find the most efficacious in comparison with other modes of medication and therapeutic measures, the tissues attacked by the diphtheritic process were acted on by inhalations of a disinfectant spray in hourly or half-hourly applications, of which each lasted a quarter of an hour or longer. At the same time, it was intended to excite, by the effect of hot steam inhaled at a temperature of from 112 deg. to 122 deg., an accelerated production of pus and the separation and expulsion of the membranes. Any more violent measures were most carefully avoided, as such only lead to a sanguinolent detachment of the still firmly adherent membranes, and consequently to the more easy entrance of infectious matter into the tissues. The apparatus used for inhalation were arranged in such a way that they gave as little discomfort as possible to the patients. Thus it was possible to employ inhalations comfortably and for long uninterrupted periods at night and during sleep in the cases of the Hereditary Grand Duke, of Princess Irene, etc. The patients occupied a lateral position near the edge of the bed, holding the inhaling tube in the mouth, whilst the nurses kept the apparatus at the proper distance. According to the period of the affection, the following solutions were made use of, viz., a 2.5 per cent. solution of chlorate of potash; a 0.1 per cent. solution of salicylic acid; and in the case of the Grand Duchess, when the septic decomposition began to become alarming, a 0.25 per cent. solution of permanganate of potash. Moreover, during the first days, injections of freshly prepared dilute chlorine-water were made, the solution containing from 25 to 30 per cent. of the official chlorine-water. In those instances in which the fibrinous exudations began to involve the larynx, as in the cases of the Grand Duchess and of the Hereditary Grand Duke, the fluids just mentioned were either exchanged for lime water, or the latter was inhaled alternately with the others. Repeated experimental investigations and bedside observations lead me to the belief that lime-water is still the best means for the solution of fibrinous membranes, whether these have been produced as a result of diphtheritic inflammation of the mucous membranes, or whether they have been formed through other causes. For it is possible that such membranes might be formed from the effects of ammonia, perchloride of mercury, boiling water, etc.; further, in certain infectious diseases, such as typhoid fever, scarlatina, and, lastly, especially in laryngitis crouposa as a consequence of atmospheric influences.* Internally, on the one hand, the fever and the septic infection were combated by the administration of reliable remedies, such as

* Oertel, on "Artificial Croup", *Deutsches Archiv für Klinische Medizin*, 1874, page 202.

salicylic acid and benzoate of soda in large doses (the Grand Duchess could not take quinine); on the other hand—and on this we laid the greatest stress—everything was done to keep up the general strength and maintain the energy of the heart's action by the administration of strong wines, port wine, arrack, cognac, ethereal tincture of acetate of iron, etc., in so far as the indications were present for their use, and as it was possible to administer them to the patients, especially to the Grand Duchess. In similar cases, I have given repeatedly from five to seven ounces of cognac with lasting success; and for years past I have come to the same conclusions on this subject, and have obtained the same results, as Dr. Charles West. It is scarcely necessary to mention, that the diet was regulated as much as possible with regard to the fever and to the state of the strength. Besides the administration of the alcoholic stimulants, in the case of Her Royal Highness, when rapid decline of the vital powers threatened to set in, subcutaneous injections of ether were tried as a last resource, without, however, producing a saving effect.

Finally, a remedy is to be mentioned here, which was most urgently recommended to us in more than a dozen letters, especially from England, viz., sulphur. Sulphur acts, according to my numerous experiments and observations, as a mere "scouring powder" in those cases in which the purulent infiltration of the membranes has already taken place and the fibrinous exudation has stopped; in other words, in those cases in which the process is about to expire. In these cases, sulphur, like a scouring powder, wears off by friction the membranes which are loosened by the voluntary and involuntary movements of deglutition, and thus removes them more quickly and without damage to the patient. This is the whole secret of its efficiency, and the explanation of the apparently incontestable successes, which are sometimes observed at certain times of its administration, viz., when it has been noted that the exudation has come already to a standstill. Moreover, the successes are thus explained which quacks have obtained in cases in which a physician has had the patient for some time under his care, and in which the spontaneous expulsion of the extensive membranes in the pharyngeal cavity has not yet or not completely taken place. At the onset of the disease, while the exudation continues, the remedy has no influence whatever, and will even prove damaging in the cases of irritable and sensitive patients by its mechanical effects; in short, sulphur has no *specific* action at all. Nevertheless, it was given in the cases of the Grand Duke, of the Hereditary Grand Duke, and of Princess Irene, with the result stated, but was discontinued later, in consequence of the expressly stated wishes of the patients, there being, in our opinion, no special indication for its employment.

As to the treatment of the fever by means of cold baths, with resulting reduction of temperature, I have not seen any encouraging results from this method in diphtheria; and my former experience in this respect has been just now corroborated in another fatal case, in which I was consulted. One is but very rarely enabled to diminish the temperature of the body with lasting effect, and to reduce in some degree the dangers arising from the local affection, from the septic process, and from the participation of the heart; diphtheria and typhoid fever are not alike in this respect. Generally, one succeeds only in reducing the temperature for a few tenths of a degree; and, as the elevation of the fever depends so directly upon the extension of the local process—which fact we had the opportunity of observing most strikingly in all our patients—nothing is gained by this small result toward a more prosperous issue of the disease. For this reason I did not recommend the employment of cold baths in the present case, not to mention the fact that the considerable renal inflammation might have been still further heightened under the influence of the hyperæmia caused by the cold baths, apart from the disease of the respiratory organs.

Against the glandular swellings, fatty embrocations and poultices were ordered, and morphia, etc., were administered to relieve the sleeplessness. For the disinfection of the objects which had come into contact with the patients, permanganate of potash, carbolic acid, sulphate of zinc, etc., were used. The sick-rooms and corridors were most thoroughly disinfected by burning sulphur, by the carbolic spray, by washing with solution of chloride of lime, by fresh papering of the rooms, etc., according to the prescriptions of sanitary science.

If we now pass to the question of the causes of this remarkable epidemic in the Grand Ducal Court, we are led to consider the question of direct infection and transmission of the diphtheritic contagium, and the cases offer in this respect a certain etiological interest. Up to the present day, it has been impossible to prove that, in our climate, the diphtheritic contagium could originate spontaneously, either in consequence of atmospheric and telluric influences, or by pollution of the soil, or by insalubrity of houses. Apart from former times, we have until 1863 not had any diphtheria at Munich for several decennia. I was for four years clinical assistant of the late Dr. Pfeufer, and I

examined with special care and interest all the inflammatory affections of the throat, without encountering in this long time a single suspected case. Well, at that time, none of the measures for the improvement of the sanitary conditions of Munich had been adopted which since then have been executed. Very little had been done for purifying the soil from faecal matter, for securing pure drinking water, for the ventilation of schoolrooms and dwellings. At the instigation of Professor Pettenkofer, I made the first extensive inquiries on the condition of the air,* dwellings, schools, etc., in Munich, and showed their extraordinarily bad condition. And yet we had then no diphtheria; there was no such infectious contagium formed. It was not until 1863 that the first cases of diphtheria were imported here from without, and ever since then we have had a very considerable quantity of cases in hospital and in private practice almost throughout the whole year. Now, if we consider the excellence of the position of the New Palace at Darmstadt, and the admirable character of its structure, ventilation, and sanitary arrangement, the idea of the autochthonous origin of such a nidus of infection must be energetically rejected. With the same distinctness, we must negative the hypothesis of the development and propagation of the imported contagium through the air of the rooms, through the water, through the closets, through a supposed defective drainage, or the secondary origin of a nidus of infection, which is otherwise met with frequently.

Princess Victoria must have been either infected by some person who was in intercourse with her—and this supposition does not seem to be quite unfounded—or she acquired the disease in the town, in which diphtheria is everywhere propagated. Here especially those slight cases are of importance which I have described as "catarrhal". The patients might move about while thus affected, as with an ambulant typhoid; and the affection might be cured spontaneously without any medical interference. Nevertheless, such a slight affection may convey the germs for the development of the worst croupous forms of diphtheria, if there be a peculiar disposition to such. I have observed this fact repeatedly in the last fifteen years, during which I have studied the disease; and I should like to draw once more very urgently the attention of the profession to this subject. In our case, it has been proved that the six patients first affected had kissed each other shortly before the outbreak of diphtheria was diagnosed in Princess Victoria. By this custom, prevalent among many families, they naturally infected each other with the contagium. Although the Princess was completely isolated immediately after the recognition of diphtheria, and although all kinds of precautionary measures were taken in order to prevent further propagation of the disease, the infection had already taken place, and, after a period of incubation varying from five and a half to eight days, five other members of the family fell ill one after the other, according to their idiosyncrasies and to the receptivity of their system for the diphtheritic contagium. Now, it is very remarkable and deserving the highest attention, that, after the necessary prophylactic measures had been taken and all objects coming into contact with the patients were thoroughly disinfected, not one other person was attacked out of a number of sixty who lived or had business within the palace, and a part of whom, like the staff of nurses, came into immediate contact with the patients. Had the infectious matter been anyhow propagated within the dwelling itself, or could it have been acquired there; had the air, the water, the rooms, been impregnated with it; had defective drainage, to which etiological recourse is, perhaps, too easily now-a-days had; had either been the cause of the development and the extension of the imported contagium, certainly, as is the case in houses infected by enteric fever and by cholera, the propagation of the disease would have been quite different. It would have attacked its victims here and there in the house, amongst high and low, without any regard to the family and to its familiar intercourse. The Grand Ducal family occupied, of course, the best rooms in the palace. Their drawing-rooms and bedrooms were extended in both floors on all four sides of the compass, surrounded everywhere by the sitting-rooms and bedrooms of the numerous household. The staff of servants lived and moved on the ground-floor, in the basement, and in the upper floors; and notwithstanding, out of all these persons, who lived partially under the same conditions as the Grand Ducal family, but occupying less excellent rooms, not a single one has been attacked by the disease. This would be absolutely inexplicable if one of the occasional causes were admitted here. The fact that, out of sixty-eight persons in the house (the Grand Ducal family included), just the members of the family (and no other persons) should be again and again infected by contagium belonging to the house, speaks most decidedly against the supposition (which is, besides, quite contrary to all experience) that the

* Oertel, "Inquiries about the Accumulation of Carbonic Acid in the Air of Dwellings, *Bayerisches Industrie- und Gewerbeblatt*. München: 1863.

formation of an infectious nidus took place in the palace; otherwise the course of the infection and the time of the propagation would have been much more various in every respect. We have, between the sickening of Princess Victoria and that of the rest of the family, a period of five and a half to eight days. This corresponds completely with the period of incubation of diphtheria. The sickening of the other patients took place within two days and a half. We must, therefore, in accordance with the different idiosyncrasies and receptivity, suppose that the time during which the contagium could have been acquired was so short, that the patients must have become infected nearly simultaneously or in very rapid succession; and that after this no further acquisition and propagation of the poison was possible for some time. If not so, there ought to have been subsequent affections amongst the household and amongst the nurses. The time and mode of the infection being most clearly proved, the result agrees with our propositions as in a correct calculation. If we finally consider the circumstances of the illness of Her Royal Highness the Grand Duchess, here again we have the nidus of infection within the family itself. The mother having passed watchful nights at the bedside of the Hereditary Grand Duke, tortured by the fear of losing her beloved child, embraced him, in spite of all earnest warnings, too early, when he was given back to her. When I left the Hereditary Grand Duke, there were still some remains of the membranes and some suppurating ulcers in his pharyngeal cavity. Unfortunately, I have had repeatedly the opportunity of convincing myself that these diphtheritic residua, and the buccal fluid which covers and surrounds them, can act as germs of the disease, if conveyed to suitable soil. Between my departure and the illness of Her Royal Highness, there was but a fortnight's time, and the complete healing of the Prince's buccal cavity had taken several days after my departure. Thus, here again, the time from the proposed infection until the outbreak of the disease completely corresponds to the period of incubation of diphtheria. I observed a similar propagation of diphtheria by kissing, in 1869, in the family of Baron von R., at that time in Munich. His youngest son sat in school next to a boy infected with diphtheria, who died afterwards from it. He got a slight catarrhal diphtheria, and, as in the family of Baron von R. the children kissed their father in the morning before going to school, and in the evening before going to bed, the little boy first infected his father, and the latter again his two other children, who fell ill within the following days with a severe croupous form of diphtheria. These facts were communicated to me at once by the father himself. In this instance also, the necessary prophylactic measures having been adopted, the disease remained limited to these members of the family, and neither any one of the numerous servants was attacked, nor was an infectious nidus formed in this house.

I could quote more instances of the same kind from my observations. The diphtheritic contagium, the parasitical nature of which has become a conviction of mine, is little volatile, is disseminated especially in the diphtheritic membranes and in the buccal fluid, and the infection takes place, in most cases, by its direct transmission either by means of the atmospheric air or by touching objects to which it adheres. Such objects are especially those coming into contact with the mucous membrane of the buccal cavity, etc., as spoons, tumblers, also pocket-handkerchiefs, etc. The possibilities of transmission are so numerous and manifold, that they cannot always be directly demonstrated, even if a careful investigation be made. On the other hand, the propagation of the diphtheritic contagium through the air is much rarer, and we shall have to admit this way of transmission only when we are really in the position of completely excluding all other possibilities. Even when a nidus of infection has been formed—we thoroughly admit the possibility of the formation of such—we shall still have well to consider the possibility of the transmission of the contagium by objects which have been in contact with it, and to which it has adhered. The probability of such an infection is still far greater than that through the air.

I conclude with the hope that this description may give in some degree a picture of the morbid process and of the course of the infection in this illustrious family. If I have spoken about many things more in detail, and laid stress upon others which my expert *confères* probably look upon as being generally known and only needing to be superficially touched, I have done so because I imagine that, in the general sympathy which England, and with England the whole world, has shown to those affected by the tragic event, a paper so widely circulated as the *BRITISH MEDICAL JOURNAL* may be read perhaps by others who have not so complete a knowledge of these conditions, and whose erroneous suppositions and conclusions may perhaps be thereby corrected. May my good intention serve as my excuse in this respect.

DR. JAMES CAITHNESS has been appointed Deputy Coroner for the County of Denbigh.

SOME INCIDENTAL BENEFITS OF ANTISEPTIC SURGERY.

By ROBERT BARNES, M.D., F.R.C.P.,
Obstetric Physician and Lecturer on Midwifery and Diseases of Women, St. George's Hospital.

THERE are virtues attending antiseptic surgery even beyond that of enhancing the patient's prospect of recovery from severe operations. The surgeon secures himself, as well as his patient, from the dangers of blood-poisoning. Had Maurice Colles performed the operation on the cancerous jaw, which cost him his life, under a cloud of carbolic spray, is it not reasonable to suppose that this admirable surgeon would have been safe from infection?

Sidney Smith used to say that doctors encountered the perils of pus and miasm with a heroism akin to that which animates the soldier in battle. But it is not the part of a good general to throw away needlessly a soldier's life. Nor can it be for the public good that a surgeon should, in his endeavour to save the lives of others, heedlessly risk his own.

Again: the still greater danger attending the performance of *post mortem* examinations may be minimised, if not annihilated, by the use of the carbolic spray. This, I believe, will be found far more effectual than the use of the film-gloves. This point is especially important. The performance of *post mortem* examinations is often an unavoidable public duty, as well as a matter of scientific interest. I have known not a few instances in which puerperal fever and other diseases sprang up in the track of the surgeon who had been engaged in this duty. In this, as in many other ways, the welfare of the surgeon is intimately linked with the welfare of his patients and of the public.

Thus, when we have realised the great facts: 1. That the field of surgery is enlarged by making operations hitherto condemned possible and safe; 2. That the success of many recognised operations is greatly increased—we have far from exhausted the beneficial applications and consequences of Lister's great discovery.

The question of the comparative dangers attending the major operations in our large hospitals, and in small special hospitals and private practice, is apparently solved. The special dangers supposed to be inherent in large hospitals are eliminated, and their many natural advantages remain untouched. It is now a matter of experience that ovariectomy especially is, under the antiseptic method, as successful in our general hospitals as in small institutions, or in private practice.

Might not the antiseptic spray be more widely extended to the practice of medicine, and especially for the prevention of the spreading of infection? We are familiar with various vaporisers for diffusing pleasant perfumes and vapours for inhalation. Some put their trust in saucers of Condy's fluid. This might be justified if the contagium, emanating from a case of small-pox or scarlatina, could only be induced to deposit itself in the saucers and go nowhere else. But it is not so accommodating. The subtle particles diffuse themselves throughout the air, enter every cranny, cling to walls, floors, furniture, bedding, the clothes and persons of attendants. A disinfectant, to be effective and trustworthy, must chase the poisonous particles as fast as they start from their source. The true disinfectant is, therefore, volatile. Chlorine and sulphurous acid gas are excellent in this respect; but, if generated in the ordinary way, they are irritating to the lungs, and it is difficult to keep up a supply of uniform strength. A plan that promises better would be to use a vaporiser worked by a spirit-lamp, charged with a solution of one part of carbolic acid in a hundred, and to keep up a constant cloud of steam playing over the bed. The contagium would thus be continually met at its source, and the vapour would spread throughout the room. Having often operated with a stream of one in forty playing under my nose, I can affirm that there is no reason to apprehend annoyance from this plan. But the ventilation of most of our bed-rooms, which sooner or later become our sick-rooms, is but ill adapted to counteract infection. Every bed-room should be provided with an Arnott's chimney-valve to carry off the foul air; and, to maintain a proper current of clean air to wash out the room, it is essential to supply a stream from without. The best contrivance I am acquainted with is Tobin's tube. It acts better than an open window. When there is a fire in the grate, the combined action of the Tobin's tubes and Arnott's valves is very efficient. The doors and windows may be kept closed, and the ventilation goes on perfectly. If we were to lay over the mouth of the Tobin's tube, suspended on wire-frame, a sheet of gauze saturated with carbolic solution, the fresh air would enter charged with the disinfectant, and add to the efficacy of the spray-producers.

Surely, if septicæmia can be stamped out from the surgeons' wards, small-pox, scarlatina, and diphtheria should not be allowed to spread beyond the sick-room.

15, Harley Street, W., January 1879.

SOME REMARKS ON OBSTRUCTION OF THE BOWELS.

By T. CLIFFORD ALLBUTT, M.A., M.D.,
Physician to the Infirmary, Leeds.

As Vice-President of the Section of Medicine at the Bath meeting, I took much interest in the discussion on Obstruction of the Bowels, which was promoted at that meeting by my colleagues Drs. Grainger Stewart and Balthazar Foster, and by myself.

So important were the issues raised at that meeting by Mr. Hutchinson, Dr. Long Fox, and others, and in the urgency of such cases so prompt must be our decisions, that, as President of the Yorkshire Branch, I asked Mr. Teale to open the subject a second time at our last Branch meeting in Huddersfield. I feel that our gratitude is greatly due to Dr. Long Fox, Mr. Hutchinson, and Mr. Teale for their cordial acceptance of a somewhat onerous position, and the profession is not likely to forget the ability they have shown in their treatment of the subject. At Bath, I purposed to be a listener only; but, like the rest of his hearers, I was so stimulated by Mr. Hutchinson's address, that I ventured to speak after him at some length, but in a discursive and inaccurate fashion, as I had no notes by me, nor had I measured my words before rising. I ventured then to quote the practice and opinions of my surgical colleagues in Leeds, if not in opposition to Mr. Hutchinson's views, at any rate with some mixture of criticism; but I could not give any authority for so doing, nor any accurate facts. Some cases of my own were related, necessarily in the same offhand way.

It is my desire now, in the light of Mr. Teale's address, to restate my own opinions in substantially the same terms, but with more premeditation. My remarks at Huddersfield were more or less in repetition of those I offered to the Section at Bath.

When a doctor is called to a case of obstruction of the bowels, he has at once to determine, if possible, the process, the site, and the nature of the block. As regards the process of it, he will try to decide whether the obstruction be due first to enteritis, plugging, intussusception, hernia, or stricture. He may then decide where the disturbance lies, and finally of what nature it is. The enteritis may be primary, secondary, or tubercular, etc. Plugging may be faecal, polypoid, due to gall-stone, and so forth, and may be accompanied by enteritis. Intussusception may be strangulated, or simply incarcerated, and is probably always accompanied by inflammation, simple, purulent, or gangrenous. Hernia may be in the usual external or internal sites; or may be due to other accidental twisting or snaring of a loop of bowel, and is sure to be accompanied by some degree of inflammation. Finally, stricture may be simple, malignant, or due to pressure from without; and is less frequently accompanied by inflammation than the preceding events. With all the other forms of obstruction, inflammation of the bowel is more or less surely associated.

Now, we know that simple local enteritis may of itself cause the symptoms of obstruction, as, for instance, in cases in which a loop of open bowel is involved in an inflammation of parts beside it. So that, if we decide that we have to do with plugging, intussusception, or hernia of the bowels, we may assume that we have to do with some measure of enteritis over and above; and that this enteritis, acting by palsy of the intestine at the part, is to be added to the cause or causes of obstruction. We have to decide, then, whether the obstruction be due to inflammation alone, or to plugging, intussusception, or hernia (internal or external), accompanied by inflammation, and whether this latter be more or less, and in what stage, or whether the obstruction be due to stricture.

The questions of site and nature are not essential to the matters more closely before us. One other very important distinction, however, is closely before us, and was discussed at Bath by several speakers, and at Huddersfield especially by Mr. McGill, namely, whether the obstruction be acute or chronic. Under this head, I wish to draw attention especially to the fact that *early acuteness* depends much upon the nature and rate of the cause; and to this other side of the same thing, that chronic obstruction often puts in an acute appearance. This I may call *late acuteness*. I have found in practice that there is a danger of mistaking late acuteness for early acuteness. I need not record cases, as they are not uncommon, in which malignant disease of the bowel or its neighbourhood is suddenly revealed by symptoms of acute obstruction occurring in a person hitherto seemingly in good health. In such a case, intussusception, volvulus, internal hernia, and the like, may be suspected when the real cause is malignant stricture. Such an error may, I believe, be generally avoided if the possibility of it be before the practitioner. In each case, we shall probably find some symptoms to aid us, and two especially I have found of use; firstly, that, before such obstruction becomes acutely manifest, its earlier degrees have been

counteracted by hypertrophy of the gut above the point, so that in moderately thin people coils of intestine may be seen and felt upon the abdominal surface, and may be brought into still greater prominence by manipulation; secondly, that, as a consequence of this hypertrophy, the injecta are swept too hastily down to the seat of obstruction and are forced through it in great measure unabsorbed. Such persons lose flesh in spite of fair appetite, and will, if questioned, speak decidedly of the great increase in the volume of their stools for some time previously. Violent ejections of wind from the rectum are also noticed in the history of such cases, and for the same reason. Malignant obstructions again in the lower colon may often be reached by a stem and ball, by the hand, or even by the finger in the bowel, and thus diagnosis is made positive. In the majority of cases, however, the nature of the stricture may be inferred with tolerable certainty from the previous history of the case and from other present signs and symptoms. Obstruction from local inflammation is not so urgent nor so complete as in the other cases named.

Moreover, although peristalsis may cease at the spot, matters will drop by gravitation into the length below, and insoluble remnants of recently taken food may be recognised in the motions. Faecal accumulation is generally to be made out by its history, symptoms, and signs. Gall-stone obstructions in my experience are always to be ascertained by reference to past hepatic colic; and polypoid obstructions resemble gradual stricture in their development, are fortunately rare, and are often within reach of touch. Intussusceptions and hernial obstructions alone remain, and intussusception is practically a hernia of bowel into bowel. Yet, happily, the distinction is generally to be made by reference to the age of the patient, and by searching the abdomen for a tumour, and the stools for blood. A past history of peritonitis would suggest the presence of bands, bridles, or dislocations.

In dealing with obstruction of the bowels, the lesson I am never tired of enforcing here, as in all cases, is that therapeutics depend upon diagnosis. Make diagnosis accurate and complete, and treatment will come. This is the reply students should receive time after time, when they will ask what is good for this disease or that disease. Never mind the name of the disease, but make out precisely what state of things you have before you in the individual case, and you will rarely be at a loss for treatment.

Obstruction of the bowels, then, may be really acute or really chronic; or again acute obstruction may occur in the course of chronic obstruction, and may even be the first revelation of it. With care, we can generally clear our case under this head. Chronic obstructions have to be dealt with according to the special features of each case, and we have plenty of time to weigh our opinions and to adjust our healing measures. When drugs fail us, the surgeon comes to our aid and makes an artificial anus. Acute obstruction occurring in the course of chronic obstruction is generally to be reduced to its chronic state by rest and subcutaneous morphia.

Let us, then, dispose thus hastily of all cases of essentially chronic obstruction. They rarely need heroic action or great presence of mind either in physician or in surgeon. We thus clear our way to the essentially acute cases; and we have found that in these inflammation always counts for something, generally for a good deal. Now, whether we know exactly how to deal with the main cause or not, we always know how to deal with the inflammation; and, except in the extremest urgency, our first duty is to simplify our case by lessening this. Unfortunately, means are often used which tend rather to aggravate the enteritis, and of all these injections into the bowel are the most mischievous. Even in the case of faecal accumulation, it is not the faecal mass, but the inflammation set up by it, to which the explosion is due, so that even in these cases it is wild practice to pump into the inflamed bowel and to drag the patient hither and thither in bed. Even for diagnostic purposes, enemata are rated far too highly, and are rarely of much service. Clearly, it is our duty to reduce our case by complete rest, opiates, fomentations, and a leech or two to the abdomen or anus until the enteritis subsides, and then we can deal as we see fit with its cause. But, if enemata are abused in faecal accumulation wherein they are chiefly valuable, what are we to say of the fashion of forcing their employment in cases of internal strangulation? It is certainly conceivable that an intussusception may be unfolded by a forcible and ample injection, or by the insufflation of air, but to force air or water against a knotted or snared loop of intestine is surely outrageous meddling. And yet I have never been called into a case of such obstruction without finding that such a measure has been assiduously employed, to the harass of the patient, to the aggravation of the symptoms, and to the increase of inflammation around the obstruction. I believe no formula can be drawn from our experience of more value than this, namely, that *if rest in every way be sedulously enforced, and the inflammation which palsies the bowel be carefully combated by the use of sedatives such as opium and belladonna, and other means, cases of obstruction of the bowels tend to*

recovery. Experience, written and personal, is full of illustrative cases. At Bath, I ventured to quote the case which follows.

R. J., aged 13, fairly nourished, was said to have enjoyed good health. His family was said to be consumptive. On May 20th, 1878, he was suddenly seized with pain in the abdomen. This was increased by pressure; there was no vomiting. May 21st. Vomiting set in; the pain continued; temperature 103 deg. Those symptoms continued the same, with the addition of increasing tympanites until June 1st. On this day, I met Dr. Wright of Wakefield in the case; Mr. Walker, the family attendant, being unfortunately absent. Dr. Wright and myself found pain, vomiting, tympanites, tenesmus, and an escape of mucus from the anus. Blood had come away at times, but not for a day or two. The anus was deeply congested, wet with mucous discharge, and gaping widely. A large intussusception could easily be felt in the rectum, and the finger passed round it. Dr. Wright and myself consulted anxiously what was to be done, and the use of taxis under ether was much discussed. As the case, however, was of ten days' standing and the pain and fever considerable, we feared the parts were at least greatly inflamed, and possibly gangrenous. Believing that rest and antiphlogistic measures were imperatively called for, we determined for a while to let all alone and to give opium as fully as the symptoms should need, and to apply leeches to the anus if required. Under fuller opiates, the acuter symptoms so much diminished that no further measures were taken, and we hoped the intussusception might follow some natural mode of recovery, as, for instance, by slough.

Mr. Walker wrote to me in September 1878 as follows: "Two days after your visit, the intussusception had passed out of reach; and a day or two later he had an attack of stercoraceous vomiting lasting a few hours. After this, he steadily and continuously improved and regained health and strength. No portion of intestine has ever come away."

Such is the very brief outline of this remarkable case, which helps to prove plainly that the first step to relief in intussusception must be to reduce inflammation and to diminish the forcing downwards of the gut. I have little doubt that forcible measures would have killed the lad.

Finally, as to the therapeutics of acute obstruction due to twists of the bowel, to snaring of loops of bowel in bands or fissures, to knots in its length, and to similar accidents; let us first clearly see that in reality, if not in name, all those events are hernias, and must be treated as such. Do we hustle the patient and his gut with injections in femoral or umbilical hernia? Surely not. Why, then, do we do so in internal hernias? Is it not our business to soothe with fomentations and the like; and, above all, to put the patient fully under opiates?

Meanwhile, taxis can be used at discretion. In such internal hernias, then, as we are speaking of, let us remember that our duty is to put the bowel at rest and the patient under opiates, knowing that in these cases also we shall promote the escape of the bowel more readily than by irritating it. To whip a dog in a leash is to strangle him; he may slip his collar if he be left to himself. It is not by agonised writhings that the bowel will get loose, but by quiet subsidence, if at all. When the patient is under full opiates, gentle taxis upon the walls of the abdomen should be used in all directions. But, if those means fail, the abdomen must be opened, as it must in strangulated hernia. As in the latter case, operation gives the patient his only chance.

Here we reach the issue between Mr. Hutchinson's canons and the teaching of the Leeds surgeons; and I cannot hesitate to support the practice of my colleagues in this matter. I repeat, let us make our diagnosis certain; and if we can do this, and for the most part we can by close attention to the history of the case, the signs, the symptoms, and the age of the patient, then I say the indications for operation will be as clear as in the case of strangulated external hernia. To deny operation to a case of internal hernia is, in my opinion, to deny to the patient some fair chance of life; and for confirmation of this view, I rely upon Mr. Teale's paper, which, I believe, represents the views also of Mr. Wheelhouse, Mr. Jessop, and Mr. Atkinson: the two latter of whom spoke at Huddersfield, and will, I hope, publish their cases and opinions.

ON EXPLORATION OF THE ABDOMEN IN OBSTRUCTION OF THE BOWELS.*

By T. P. TEALE, M.A., F.R.C.S.,

Surgeon to the General Infirmary, Leeds.

THE recent debate on this subject at the meeting of the British Medical Association in Bath, and the expression of an opinion unfavourable to exploration of the abdomen in obstruction of the bowels by one of our most thoughtful and observant metropolitan surgeons (Mr. Jonathan Hutchinson), shows that the question is still far from being settled, and

imposes an obligation on those who have had experience in the subject to contribute what evidence they possess towards the solution of such a serious and difficult question.

There can be no doubt that the subject has been looked upon with much favour by the Leeds surgeons; and I must confess to having myself a strong leaning towards the operation, on the grounds both of theory and experience.

As far as I can make out from my notes, or recall from memory, I have six times opened the abdomen in apparently hopeless cases of obstruction of the bowel; and I do not consider that in any one of them a chance of recovery was taken away by the operation. In one of the six cases, I believe life would have been preserved if I had operated two days earlier.

Two cases, at least, I have seen in which section of the abdomen would have revealed the true nature of the disease and would have given a reasonable chance of preservation of life. In the first, a patient died of obstructed bowels, who had previously recovered from an operation for strangulated hernia. The cause of death was strangulation of the bowel by a band uniting two coils of intestine. This, of course, could easily have been set free. In the second, the patient died from peritonitis caused by rupture of the bladder. Being convinced that the bladder was ruptured, I proposed to explore; but, as my colleagues did not think the symptoms sufficiently pronounced to justify what we then looked upon as a most hazardous operation, I desisted. The man died in about two days, and a rupture in the fundus of the bladder was found. Had an operation been agreed upon, it was my intention, first, to stitch up the rent in the bladder; second, to wash out the peritoneal cavity; third, to perform perineal section, so as to secure a more ready and perfect drainage by a large tube than could be obtained by a catheter in the urethra.

Exploration of the abdominal cavity is, to my mind, justifiable on two cardinal grounds. Firstly, the mere opening of the peritoneal cavity is not necessarily a dangerous operation; secondly, there are many cases of obstruction of the bowels which must prove fatal unless relief can be given, which can only be rightly directed by means of exploration of the abdominal cavity.

As to the impunity with which the peritoneal cavity may be opened, I need hardly remind you how constantly this is proved in operations for hernia. We think nothing of pulling out and handling coils of intestine; and we rarely look for danger unless the bowel have been damaged by too long continued strangulation. It is a common saying, that it is not the operation which proves fatal, but the delay in operating; in other words, the damage to the bowel by delay, not the damage to the peritoneum by the operation.

Again in ovariectomy—What are the dangers there? Mainly from the fluids poured out from adhesions and divided vessels, dangers which, as a rule, are absent in gastrotomy. Look at Keith's thirty-nine cases of ovariectomy without a death. Look at the confidence with which he reckons upon recovery where there is a smooth peritoneum without adhesions. Why need we have such dread of opening the peritoneal cavity?

Let us next inquire what are the teachings of my own cases.

In six instances I have opened the peritoneal cavity in order to search for and deal with obstruction of the bowel; and I cannot do better, perhaps, than take them in detail; and, by analysing each one, draw out such lessons as it seems to teach.

CASE I.—A patient, named Wright, butler to a gentleman in Leeds, was brought to the Infirmary suffering from obstruction of the bowels. At the end of three weeks, he was failing so rapidly and was in such agony that, after a consultation with my colleagues, I determined to search for the obstruction. Having opened the abdomen in the middle line so as to introduce the hand, I traced the distended colon until it terminated at a stricture in the sigmoid flexure. I thus discovered, what before we felt uncertain about, that there was a cancerous stricture at the sigmoid flexure so situated as to permit left lumbar colotomy. Having temporarily closed the abdominal wound, I straightway performed colotomy; and, having relieved thereby the abdomen of its tension from gas, I finally closed up the stitches in the front of the abdomen. The relief was immediate. In two days, he was ravenously hungry; and he rapidly recovered. After his recovery, he discharged his duties as butler for several months; and he died about a year afterwards from a pyæmic attack originating in the cancerous mass.

In this case, the indications of the seat and nature of the stricture were so doubtful, that I thought it would be less risk to explore the abdomen than to do a colotomy when in doubt as to the site of the stricture.

CASE II.—I was called to a gentleman dangerously ill from obstruction of the bowel of two days' standing. Feeling bound at that time (about five years ago) to delay any abdominal exploration until the case

appeared desperate, I decided to wait. At my next visit, two days afterwards, matters were much worse. He was greatly exhausted, and I determined to explore, beginning at the situation of an old right inguinal hernia, which had not descended into the sac for many years. Following the sac, just within the internal ring, I met with an abscess communicating with an ulcerated opening in the intestine, probably the cæcum, and imperfectly shut off from the neighbouring intestines by adhesions. The relief came too late, as he continued to sink, and died in a few hours.

CASE III.—An elderly, stout, short built woman, had suffered from two attacks of obstruction of the bowels during the last three months, lasting ten days or a fortnight. The present attack had lasted one month and two days. Having watched her for about a week, I did not think further delay advisable. The following are my notes. March 9th, 1876. Last night, she passed a little flatus, which rendered her body rather less tympanitic. As, however, she had had no passage for four weeks, and as the repetition of the attacks pointed to a permanent cause, it was decided to operate. The probability was that the stricture was not far from the anus, for two reasons: first, that her symptoms had never been very acute; second, that only about a quart of fluid could be injected into the bowel. The abdomen was opened in the middle line below the umbilicus, so as to admit the hand, and a cancer of the size of a large walnut was felt at the sigmoid flexure. The wound was then closed. An incision was made in the left loin, as for colotomy. There was a great depth of fat, and there was little more than an inch of space between the crest of the ilium and the last rib. The kidney occupied the whole space, almost touching the crest of the ilium, and there was a great quantity of subperitoneal fat. Finally, the bowel was reached, about three inches and a half from the surface of the wound. To open the bowel at such a depth, and through such a narrow chink, and then to drag it three inches and a half up to the skin, seemed a hopeless task. The attempt to open the bowel was abandoned, and the wound was closed. For several days she went on perfectly well; the bowels acted two days afterwards, as if the handling of the bowel had straightened a kink or removed some impediment. No peritonitis followed. Her temperature after the third day became normal, and remained so until the ninth morning, when, her pulse being 108 and her temperature 97.6 deg., she had a violent fit of coughing, and burst open the abdominal wound so that the intestines protruded. From this time, she began to go down hill. Bilious vomiting set in; her pulse ranged from 120 to 130; and she died March 29th, three weeks after the operation.

In this case the exploration did not set up peritonitis; and, though indirectly it was the cause of death, it is fair to suppose that, if colotomy could have been achieved and the abdomen completely relieved of tension, the abdominal wound would not have given way under the cough.

CASE IV.—In this case, as a duty rather than with any confident hope of doing good, I opened the body of a man at the point of death from perforation of the bowel and escape of fæces into the peritoneal cavity.

CASE V.—This patient also was almost at the point of death from obstruction of the bowels complicated with ascites. He had had good health till three months previously, when he was seized suddenly with pain in the abdomen and collapse. From that time, he often had pain, and had difficulty in the action of the bowels. When I first saw him, January 4th, 1878, the bowels had been obstructed about a week; and I was doubtful whether the obstruction resulted from malignant disease, or intussusception, or inflammatory change. Six days later, I saw him again. He was then much exhausted, the body was greatly distended with fluid and flatus, his pulse was stabby, and he was perpetually vomiting. As a forlorn hope, I opened the body, let out several quarts of ascitic fluid, and then discovered cancerous nodules forming constrictions in the rectum, cæcum, and sigmoid flexure, and infiltrating other portions of the bowel. The case, therefore, was hopeless. The patient did not live many hours.

In this case no chance was thrown away by the operation.

CASE VI.—This is the last and by far the most important case that I have had; and, although the patient died, recovery from a most desperate condition seems to have been all but achieved.

The Rev. Mr. N., aged 32, went out fishing June 1st, 1878, after a dinner at which he drank some sour porter. During the night, he was seized with severe colic, and sent for his doctor. June 3rd. He was very ill, vomiting all day, and was in great pain. June 4th. The body was very tympanitic in the morning, and there was intense pain and vomiting all day. In the afternoon, the pain was mitigated by two hypodermic injections of morphia, but the vomiting and hiccough continued unchecked. Temperature normal. In the evening, the pulse, previously natural, began to quicken. At twelve at midnight, I saw him for the first time. Pain was not severe, owing to the morphia. The body was

enormously distended with flatus, hard and drum-like, especially bulging at the epigastrium. There were incessant hiccough and vomiting of fæcal matter in small quantities, and intense thirst, and when he drank a small quantity of fluid he rejected it instantly. There were great *malaise* and restlessness. Feeling convinced that if unrelieved he could not possibly live for twenty-four hours, I explained the state of affairs to his family. Being supported in my views by his medical attendants, Mr. Hicks of Easingwold, his brother-in-law, and Mr. Dowker of Helmsly, I decided to explore the abdomen, fearing that enteritis was causing the obstruction, but hoping that some condition might be found which would admit of relief. On June 5th, at 3 A.M., I made an incision four inches in length below the umbilicus. Distended small intestine at once escaped, and more bowel was drawn out until about six or eight feet of small intestine were outside the body. Then, the abdomen was searched for the cause of obstruction; and it was discovered that about eighteen inches of the lower part of the jejunum was black from congestion, the bowel on each side being pale and tense with flatus. I then concluded that the black portion of intestine was in a state of enteritis and on the verge of gangrene, as the peritoneum was easily abraded when rubbed with the finger; that this was the cause of the obstruction; and that the patient could not possibly recover. Then came the question of replacement of the distended intestines. It was clear that this was impossible unless they could be emptied. A fine trocar was plunged into the bowel, and proved useless. No gas escaped, only a drop or two of thick intestinal fluid. Under the pressure of necessity, I made a quarter-inch puncture with the scalpel at each extremity of the protruded intestine, and caught in a bowl about two quarts of grumous intestinal liquid, and also let off a large quantity of gas. The openings were stitched up with continuous suture, and tied tightly in a pucker, and the thread was cut short. Probably none of the fluid escaped into the abdomen, as the mesentery of the protruding bowel plugged the wound. The bowel, having been thereby much reduced in bulk, was slowly and without difficulty returned, as in an operation for hernia, and the wound was stitched up. After the operation, the breathing was for awhile difficult, and the pulse fair but rapid. As he became conscious from the ether, he was very restless and in great pain. One-fourth of a grain of morphia was, therefore, injected. Mr. Dawson, who administered the ether, noticed that as soon as the opening had been made and the intestines had escaped out of the abdomen, the patient began to breathe well, and his pulse improved. At 6 A.M., I left, saying to his medical attendants that I feared he could not live six hours.

The sequel is told in the following letter of Mr. Hicks of Easingwold. "Easingwold, June 19th, 1878.

"Dear Mr. Teale,—My brother-in-law lived until the ninth day after the operation. On the morning following the operation and during the day and night succeeding, he complained of great soreness of the wound, but this subsided, and no pain was complained of afterwards. The bowels acted freely, and flatus was largely passed on the second day, after which vomiting and all other symptoms subsided (considerable reduction of tympanites). I may observe that at no period of time after the operation was the abdomen tense. On the third day, his temperature was normal, respirations 18, pulse 96, having gradually fallen to this frequency from 130, or somewhat over that, at the time you left him ('with six hours to live').

"On the sixth day, the wound began to exude fæcal matter. On the seventh, the fæcal discharge was decided, and towards the end of the day, and on the eighth, a large quantity of fæcal matter was discharged, adding greatly to his discomfort, and utterly exhausting him. My firm impression is that an earlier operation, with exactly the same mode of procedure, would have saved him; but a foot of mortified bowel, of course, proved fatal."

As there was no *post mortem* examination, doubtless the question may be fairly raised whether the fæcal escape may not have been from the punctures made into the intestine in order to let out gas and fluid, and not from the moribund bowel. The possibility, however, of closing by suture a wound in the bowel is sufficiently established to lead one rather to the belief that it was the inflamed bowel which gave way, and not the punctured bowel.

Deductions from this case are the following.

1. The opening of the abdominal cavity did not shorten life, but actually prolonged it.
2. Could the operation have been done twelve hours earlier, the probability is that the bowel would have recovered, and that the patient might have lived.
3. The operation did not cause peritonitis.
4. When we open the body for obstruction of the bowel, even if the condition causing it be enteritis or peritonitis, we may attain the greatest amount of benefit if we boldly aim at emptying the bowels as far as pos-

sible of flatus and fluid by effectual openings—i.e. with the scalpel—as to replace the bowels perfectly flaccid and free from gas or tension.

5. If the bowels be mechanically emptied of gas and rendered flaccid, the gas is not re-formed rapidly, and meantime the circulation of the bowels, being relieved from pressure, has time to recover itself.

May we not look forward to a time when we shall treat peritonitis, traumatic, ovarian, or puerperal, by opening and washing out the abdominal cavity and emptying the intestines?*

TWO CASES OF INTUSSUSCEPTION OF THE BOWELS IN INFANTS; WITH REMARKS.†

By G. F. HODGSON, M.R.C.S.Eng., etc., Brighton.

CASE I occurred in February 1868, to a well-developed and healthy boy, aged 6½ months, and was ushered in by a sudden attack of belly-ache, which was soon followed by two hurried actions of the bowels, and these again, in a few hours, by a bloody evacuation unmixed with faecal matter. The severe pain recurred at irregular intervals of from half-an-hour to two hours, accompanied by efforts to expel, which resulted henceforth until death in the discharge of nothing but bloody mucus. But little sickness was present. Anodyne medicine by the mouth was given, and, after the second day, a fluid injection was administered, which returned at once without faecal taint. Then copious injection of air was adopted repeatedly, several times in one day, until, on each occasion, visible distention of the belly was manifest; yet, the same pain and the same bloody mucous discharge persisting, the parents would allow no further attempts to relieve the obstruction to be tried, and the child died of exhaustion the fourth day. A necropsy was not permitted.

CASE II occurred at the beginning of November 1878, in a remarkably fine boy aged nearly 6 months. In the middle of the night, he was attacked by severe pain in the belly and quick action of the bowels; the second motion containing some blood; and the third being only bright blood. The medical man who was called to him (in the night), prescribed a teaspoonful of castor-oil, to be followed by another spoonful in three hours, if no motion resulted from the first. Both doses of oil were retained, but without any faecal relief. I saw the child the next evening. Paroxysms of severe pain were occurring, followed by small discharges of mucus, streaked with blood, and quite devoid of even the odour of faeces. There was no sickness. The abdomen was plump from fat, and no tumour was detectable on palpation. An opiate mixture was prescribed, with direction that it should be given freely, unless the child became very sleepy. The next day, the case presented the same features, leading me to suspect intussusception. The opiate was persisted in, and repeated attempts during the day to administer warm water enemata, these returning at once. On the third and fourth days, the symptoms were much the same, with the addition, on the fourth day, of an internal prolapse of the bowel being detectable by the finger in the rectum. Insufflation was then practised, and the wind was fairly pumped into the bowels until (artificial) borborygmi were caused by it, and decided distention of the belly. On the fifth day, the same state prevailed, the insufflation having afforded no relief. Gastrostomy was suggested to the parents, but, as a cure could not be certainly promised therefrom, they would not consent to it; and henceforth nothing was done beyond giving the opiate medicine. On the seventh day, the bowel protruded at the anus, and so continued (about two inches) until death, which occurred on the fourteenth day, from exhaustion. But little sickness occurred throughout, and the child took the breast-milk, in small quantities, and a little cow's milk with brandy, until within a few hours of its decease. The thermometer also indicated no sympathetic fever. *Post mortem* examination was refused.

REMARKS.—Infantile intussusception seems to occur in two forms.‡ In one (the more frequent), the invasion is sudden, the constriction is absolute, the anal discharge soon becomes solely bloody mucus, devoid of faecal matter, and the child, unless relieved, succumbs to its suffering within a week. In the more rare form, the constriction is less tight, consequently some admixture of faecal matter is voided with the other discharge. The case is in all respects less severe, and may linger on for many weeks. Peritonitis is rarely present to any extent in any case.

* See "Ovariectomy in Extremes." By T. P. Teale, *Lancet*, 1874, vol. i, p. 617. "Intra-peritoneal Injuries, etc." By T. Smith, *St. Bartholomew's Hospital Reports*, vol. ix.

† Communicated at a meeting of the East Sussex District.

‡ The second case given above, it is true, held out for a fortnight; but that was very exceptional, and may be accounted for by the child being robust, and by its retaining some food all along.

The invagination generally commences at the ileo-cæcal valve, the ileum being drawn into the cæcum; and as it proceeds, more and more bowel is taken in, until most of the ileum and the ascending and transverse colon disappear from their usual sites, and form a volvulus in the left iliac and pelvic regions, dragging on the mesentery.

There is much reason to believe that both forms of the disease are not seldom overlooked, or rather mistaken for dysentery. My second case occurred during my absence from home, and the gentleman who kindly took charge of it, though a man of years and much practical experience, handed it over to me as one of dysentery. Yet a prompt diagnosis is essential to successful treatment, especially in the acute cases. That treatment would seem to be, first, to avoid purgatives; secondly, to give anodynes frequently; and thirdly, to adopt measures for mechanically rectifying the displacement. For the latter purpose, a copious bland fluid injection has sometimes succeeded, though more often it has failed, the fluid generally returning at once as injected.

Insufflation of air has the credit of having wrought more cures than fluid injections have.*

Both fluid and air failing, after fair trials, no time should be lost before gastrotomy and the untucking of the gut should be attempted; for the earlier done, the more chance would there be of success. The operator must be prepared for a few difficulties: thus, when reached, the invaginated gut is sometimes found so gripped, that it cannot be withdrawn without receiving injury. Again, that being accomplished satisfactorily, such an extent of bowel may have been passed through the surgeon's hands, that the re-stowing of it in the abdomen properly may require much tact and judgment.

In addition to what may be found on this subject in surgical treatises, much useful information is contained in several periodicals. Thus, in the *Lancet*, June 1854, Mr. King gives a remarkable case; in the *Edinburgh Medical Journal*, October 1864, Dr. Greig gives four cases cured by insufflation, and a fifth fatal; in the *Glasgow Medical Journal*, February 1870, Dr. Wilson quotes a fatal case; in the *Lancet*, November 22nd, 1873, is a valuable paper by Mr. Jonathan Hutchinson; and in *St. Bartholomew's Hospital Report*, 1876, a paper by Mr. Howard Marsh.

CASE OF INTUSSUSCEPTION IN AN INFANT, SUCCESSFULLY TREATED BY DISTENTION OF THE LARGE INTESTINE WITH THIN GRUEL, UNDER CHLOROFORM.†

By N. P. BLAKER, M.R.C.S.Eng.,

Surgeon to the Sussex County Hospital, Brighton.

A. B., a well-nourished infant aged seven months, nursed by a healthy wet nurse, was in perfect health till twelve o'clock on September 4th. She then suddenly screamed, and immediately afterwards became collapsed, pale, and cold. She was put into a warm bath, after which she lay quiet in the nurse's arms for an hour and a half, the bowels acting slightly once or twice. At 3 p.m., the child had become warmer and was sleeping quietly, occasionally, however, waking up with a scream, and drawing up her legs with an expression of severe pain. She took the breast at intervals, and then vomited, without much effort, the whole or a portion of what she had swallowed. Nothing abnormal could be detected in the abdomen, and in the intervals between the paroxysms of pain, a casual observer would have thought the child in perfect health. A mixture containing nearly one minim of tincture of opium for a dose was given and retained; and the child was afterwards more quiet till 6 p.m., when she passed two motions consisting of blood-stained mucus. She continued much in the same state during the evening, occasionally passing a small quantity of mucus after a paroxysm of pain. At 9 p.m., she was seen by Dr. Moore, and a large injection was proposed; but as Dr. Gee was expected at 11 p.m., it was decided to postpone all further treatment till his arrival. A distinct but ill-defined oval tumour, about an inch and a half in its longest diameter, could then be felt through the parietes, at a spot about two inches to the left of the umbilicus. A considerable quantity, perhaps a drachm, of dark blood came away. It was determined to distend the large intestine with thin gruel. The child was, therefore, put thoroughly under the influence of chloroform and placed on a table, with the nates well raised on a pillow. The gruel was then slowly injected by means of a Higginson's syringe, the

* Judging from the case just narrated to us by Mr. N. P. Blaker, the child being rendered anæsthetic prior to the administration of either the fluid or the air is calculated to materially promote their desired effect.

† Read at a meeting of the East Sussex District.

upper part of the nozzle being pressed firmly against the anus to prevent any from escaping. After a pint or more had been injected, the abdomen became somewhat tense, and the distended bowel could be felt like a hard rope an inch in diameter, across the upper part of the abdomen, almost as far as the right iliac region, and considerable force would have been required to inject any more of the fluid. When the nozzle of the syringe was removed, a portion of the gruel escaped, and soon afterwards, a much larger quantity. The child slept at intervals during the night, and took the breast well, and there was no vomiting nor pain. Next morning, at 8 a.m., the skin was a little hot and the pulse a little quick, and one small healthy motion had been passed. The tumour which had been felt in the abdomen had now quite disappeared. At 1 p.m., all the feverish symptoms were gone, and the child had passed a copious motion of a green colour, and there had been no symptom of pain or spasm. At 4 p.m., she passed another copious motion of the same character. From this time, the child appeared in perfect health, but the motions retained their green and unhealthy appearance till September 8th, four days after the commencement of the attack.

The success which attended the means adopted for the recovery of this little patient was due, perhaps, first, to the chloroform, which, by allaying muscular spasm, and preventing any struggling on the part of the child, allowed the treatment to be efficiently carried out; then the injection of gruel was had recourse to early, before the intussusception could have extended far, or the invaginated bowel become congested and swollen; and lastly, the position of the child, lying on her back with the nates raised, favoured by gravitation the introduction of the fluid.

CLINICAL MEMORANDA.

THE EFFECTS OF ATTENTION ON THE BODILY ORGANS.

DR. CARPENTER'S remarks on this subject in the JOURNAL of December 14th, will, it is to be hoped, be read by every member of the Association, not a few of whom could contribute similar cases. A special interest must be felt in such influences by teachers of therapeutics, who, as experience tells me, are often consulted on the problems involved.

I offer one or two cases which seem parallel with Dr. Carpenter's.

A few years ago, a lady who had watched a case of cancer of the breast, which had been excised, became convinced that she was suffering from the same disease. The pain, she said, amounted to agony, and its character was just such as she had witnessed. As an intimate friend, I was spoken to by the relatives, and took her to the surgeon who had operated on the case—Sir James Paget. He re-assured her so completely, that peace of mind was soon restored. Pressed to say, if not cancer, what could cause such agony, he declared that it was not a surgical case at all, and would never require the knife. She was soon well.

Another lady, who had been under my care for laryngeal disease, travelled a long distance to confide to me her conviction that she had cancer of the breast. The history was similar. She, too, rapidly recovered on being assured that there was no such danger.

No part of the body is more likely to be affected by "expectancy" than the throat. Of this I could give numerous examples, but will add only one, as parallel with Dr. Carpenter's.

A few months since, a lady succumbed to cancer of the throat. I saw her in the later stages every week. A few weeks after her decease, a young lady, a near relative, who had nursed her, came to me in the utmost distress, affected, as she believed, with the same disease. This single consultation sufficed to restore her to health.

PROSSER JAMES, Dean Street, Park Lane.

ON THE MODIFICATIONS OF CANCER BY HEREDITARY TRANSMISSION.

ALTHOUGH a family history of cancer is found in a very large proportion of the cases of malignant disease which are met with in practice, yet very few show such a clear history of the disease occurring in members of several generations of the family as the case recorded below.

Elizabeth T., widow, aged 40, was admitted into the North Staffordshire Infirmary on November 16th, under the care of Dr. Arlidge, complaining of swellings on both sides of the neck and in the left armpit. About four months before admission, she first noticed a small tumour, about the size of a marble, in the left parotid region. Three weeks afterwards, she discovered a similar swelling in the right parotid region and one in the left axilla. The tumours were soft at first, and tender

on manipulation. They all increased rapidly till admission, and the patient lost much flesh and strength. She stated that her grandfather died at a very advanced age from a "bleeding cancer", which affected the right eye and cheek. Her uncle died about the age of sixty from cancer of the tongue, and her mother had a portion of the tongue excised for cancer about thirty years ago; but there has been no recurrence of the disease since. Her sister had one of her breasts removed for "hard cancer" a few years ago.

Present Condition.—The patient is very feeble and emaciated, with well marked cachexia. A hard tense swelling is situated in each parotid region, extending upwards as high as the ear, and in front slightly overlapping the rami of the inferior maxillary bone. The tumours are about four inches in their vertical and antero-posterior diameters; their outline is rounded and even. The left submaxillary gland is hard and tender, and about the size of a pigeon's egg. All the cervical glands are enlarged and hard. In the anterior part of the left axilla is a firm mass of the size of an orange; posteriorly, its outline is rounded and circumscribed; but anteriorly it infiltrates inwards and upwards beneath the pectoral muscles, to within an inch and a half from the sternum and half an inch from the clavicle. Over this mass the superficial veins are dilated and tortuous. The patient suffers much from sharp shooting pains in all the tumours, which are extremely tender on pressure.

The chief interest in this case lies in the history of cancer in three generations of the patient's family. In the grandfather, mother, and uncle, it occurred in the form of epithelioma; in the sister as carcinoma; while in the patient herself it has appeared in the form of adeno-sarcoma. Paget states, in his *Surgical Pathology*, that, in hereditary transmission, the cancer-material may be modified, so that the form of the disease in the offspring may be different from that in the parent; and also that the cancerous disposition may gradually cease or fade out in the production of recurrent and other tumours, whose characters are intermediate between cancerous and simple growths. But here we find in the grandchildren that the cancerous disposition, instead of fading, shows itself in the one case in a more, and in the other in a not less, malignant form than in the parent and children.

It is not the form of the cancer alone which is seen to vary in this family. The sites of the disease offer a not less striking variation. In the first generation, it occupies the cheek; in the second, it attacks two members of the family on the tongue; while, in the third, it appears in the breast in one member, and in the lymphatics of the neck and axilla in another. It illustrates very forcibly the remark of Paget, that "it seems unreasonable to call a disease local, which can pass by inheritance, with so complete a disregard of localities".

ALEXANDER MCALDOWIE, M.B., Resident Medical Officer to the North Staffordshire Infirmary.

FALSE DIPHTHERIA.

I WAS much pleased to read Dr. Cornelius Fox's interesting communication in the JOURNAL of December 7th, on "False Diphtheria". For some years I have recognised an affection almost identical with that so ably described by him, and for the last three or four years have been rarely without a case under treatment. I say almost identical; for, in the cases described by Dr. Fox, the tonsils appear to be the parts first and chiefly affected, whereas, in mine, they are more frequently only secondarily affected by extension of the inflammation. In the majority of my cases, the pharyngeal mucous membrane is the part first and chiefly affected; the follicles are very prominent, and the mucous membrane is studded with granulations. Indeed, but for the highly contagious nature of the complaint, there would be no necessity to make special note of it, unless to trace its origin, as follicular inflammation of the throat would pretty accurately describe it. The affection differs from influenza, in that there is no tendency to discharges from the nostrils or eyes. The symptoms are, indeed, more like those of scarlet fever; but one point alone distinguishes it from this latter affection, and this is, that a patient who has once had the disease, and remains exposed to the same kind of infection that originally produced it, is almost certain, before the year has closed, to have two or three fresh attacks. Moreover, the affection I wish to describe is not accompanied by any kind of rash; and the temperature, so far as I have seen, never rises higher than about 103 deg., the average being, for the first three or four days, from 100 to 101 deg. I have never seen erysipelas mistaken for or mixed up with it. The complaint, in the majority of cases, commences with frontal headache, pyrexia, and dry skin. Adults and children are alike affected, and I have seen a few infants at the breast take the complaint from the mother or from elder children.

The disease usually lasts in an acute form for three or four days, when the headache and pyrexia subside, then becomes subacute so far as the throat-symptoms are concerned for about ten days longer, when,