was no cardiac damage, and where recovery followed removal of eccentric irritation. He therefore recognised other causes for the choreic symptoms, such as impaired vigour and increased irritability of the great ganglia, resulting, possibly, from prolonged arterial spasm, capillary thrombosis, and hyperæmia.

Dickinson's researches demonstrated the presence of hyperæmic foci, spread very generally over the brain and cord, with dilated arterioles and small hemorrhages, especially in the basal ganglia.

Broadbent affirmed that multiple symptoms in chorea were significant of multiple lesions, and hinted that to limit the area of localisation to the basal ganglia, or their vicinity, might be fallacious, since motor areas are now recognised in the cerebral cortex.

At the present time, we have the problem of chorea reduced to the following terms, in accordance with the most recent knowledge. 1. That the disorder is associated with rheumatism in a large proportion of instances. (This is denied by Dr. Sturges and others.) 2. That, consequently, it is also associated with endocarditis, and also often co-exists with signs of heart-disease, even if this be not clearly of rheumatic origin. 3. That the latter conditions predispose to the occurrence of embolisms, and certainly give rise to these in some cases. 4. That the condition of the motorial centres is one of debility and instability, however induced, whether mechanically or diathetically. 5. That, given this peculiar unstable condition, eccentric or other irritants and excitants may readily evoke the irregular modes of motion recognised as chorea.

Now, as in many other cases, where disputants take up opposing sides warmly, there is probably much to be said for both parties, and each is often found to espouse portions of truth. So here, we may, I think, find the truth respecting the origin of chorea to lie completely in no one of the theories I have enumerated. Taking chorea to be always symptomatic, we find it as a result of varying conditions affecting certain portions of the encephalon. Foremost amongst these, I am of opinion that we must recognise the close connection with all that is now signified by the rheumatic habit of body. I believe that closer study of all cases will bring many more of them into this category. The remainder will, I conceive, be found in connection with an ill-nourished and feebly developed nervous system, subjected to unwholesome influences, exhaustion, and unwise nurture. The connection of chorea with other spasmodic neuroses appears, as Dr. Barlow thinks, to be very little marked; and I agree with him. As a neurosis, it is, however, probably capable of heredity and transformation, as are other like conditions.

Let us inquire, next, what may be said respecting the evolution of chorea in members of a rheumatic family. The disorder may occur either during or after an attack of rheumatism; or, again, it may appear long antecedent to such an attack. Yet, again, it may occur in persons whose family only have had attacks of rheumatism. The seat of chorea is certainly in the nervous motor centres, and rheumatism is a disease particularly affecting motor structures—for example, the heart and the joints. The latter are believed to have their nutrient centres in the medulla oblongata, whence arise also the greater portion of the nerves innervating the heart. It is easy to suppose that a common kindred vulnerability, or susceptibility, in the great motor centres should predispose to one or other, or to both, of the disturbances known as chorea and rheumatism. This susceptibility, or instability, may therefore be a part of the diathetic proclivity of rheumatism; and, given this state, an overt rheumatic attack, or some other peripheral irritant, fright or emotion, intestinal or utero-ovarian irritation, may suffice to evoke the peculiar perverted action of the motor centres. I come now to declare my belief that it is in some such mode that chorea arises as a symptomatic disorder; that it is the outcome of a motorial neurosis. This is a speculative theory, I allow; but I think it is a helpful and suggestive view. It accords with much that is gaining acceptance in pathology at the present time, for the wheel has gone round again to neuro-pathology, and diseases of the blood are somewhat at a discount just now. It is perhaps not satisfactory in itself that opinions should thus oscillate, or that fashions should prevail in any science; but it is true, notwithstanding, that progress occurs with each turn of opinion; and, if we are really seeking what is true, no harm will accrue. The nervous theory of rheumatism itself is not new. Addison was wont to bring forward many facts in favour of its being primarily a disease of the nervous system, and my own belief is strongly set in this direction; for I accept the arguments in support of a basic arthritic diathesis, or potential state of certain nerve-centres, from which, under certain conditions, may be evolved either rheumatism or gout. But I none the less ac-

cept the associated humoral conditions, believing that these in themselves are, however, insufficient to explain all the phenomena of these states. I term these conditions, therefore, neuro-humoral. Let me ask you to discuss, as the outcome of this expression of my views, whether it is, or is not, probable that there is a peculiar basic or fundamental condition—a neurosis, if you will—which is capable of hereditary transmission or transformation, in the subjects of which may be set up at one time rheumatic, and at another choreic manifestations. This view will explain many cases of chorea, in which, even after skilful inquiry, no history of rheumatism, personal or family, can be obtained. The rheumatic evolution has not occurred; but the central motorial instability has given token of its presence in the form of chorea. The future life-history of some of these so-called non-rheumatic cases shows that rheumatic manifestations may supervene later, thus confirming the connection. If we accept this view, it is not difficult to conceive, further, that eccentric sources of irritation may excite chorea in subjects thus predisposed to motorial instability. Some light is also thrown by this theory on the value of arsenic in chorea. It is recognised as a most useful remedy in many cases of chronic rheumatism, promoting better nutrition and more stable condition of nerve-centres; and thus it counteracts the conditions on which these states mainly depend. Guided by this conception, you will not fail to understand the full value and importance of completing the inquiries already set forward on this subject by the Collective Investigation Committee of our Association, in whose behalf I appear before you on this occasion.

THE TREATMENT OF INTUSSUSCEPTION.

Read before the Medical Society of London.

By FREDERICK TREVES, F.R.C.S.,

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IN order to give precision to the data upon which the various modes of treatment adopted in intussusception are based, it is necessary to consider briefly certain points in connection with the anatomical and clinical forms of invagination, the morbid changes, the prospects of spontaneous cure, and the general mortality of the disease.

If all forms of intestinal obstruction—exclusive of those due to hernia and congenital malformation—be classed together, it will be found that the cases of intussusception will form 30 per cent. of the whole. The affection, therefore, may be considered to be a common one.

From an anatomical point of view, intussusceptions may be divided into four kinds—enteric, colic, ileo-cæcal, and ileo-colic. The relative frequency of these varieties may be represented respectively by the following proportions in every 100 cases of invagination—30, 18, 44, and 8. In the enteric form, the small intestine, and usually the lower jejunum, is alone involved, and the resulting tumours are generally small. In the colic, the colon is alone implicated, and most commonly that part to the left of the transverse colon. In the ileo-cæcal intussusception—the most frequent form of all—the terminal part of the ileum, carrying the ileo-cæcal valve at its apex, is turned into the cæcum; while, in the ileo-colic, the ileum is prolapsed through the valve, and following upon such prolapse is a secondary invagination of the cæcum into the colon. The colic form is, as a rule, chronic, while the remaining varieties usually assume an acute or subacute course.

From a clinical standpoint, intussusception may be separated into four classes: 1, the ultra-acute, in which death follows within twenty-four hours; 2, the acute, in which the disease runs its course in from two to seven days; 3, the subacute, in which the period is extended to between seven and thirty days; and, 4, the chronic, in which the duration of the malady has extended beyond thirty days. The first named variety is exceedingly rare, and would appear to be invariably fatal. Of the remaining varieties, out of every 100 cases about 48 will be acute, 34 subacute, and 18 chronic.

Intussusception is most commonly met with in children; and, indeed, no fewer than 50 per cent. of all cases occur under the age of ten years. It is among the young also that the acute forms are especially met with, while chronic cases are most frequent between the ages of 20 and 40.

With regard to the principal pathological changes in invagination, it may be pointed out that they tend to lead to two grave conditions—to obstruction of the bowel, and to strangulation of all that part involved in the intussusception. These two conditions need not coexist;

¹ Laycock held views much resembling these, and I imbibed them from his teaching. They have been elaborated and enlarged by others, notably and ably by Dr. T. J. MacLagan, and by Dr. Barlow.

one may be present without the other. Moreover, mere invagination does not of necessity lead to either condition, as is shown in the earlier stages of some cases of chronic intussusception, where for weeks it may be said that there is neither gross obstruction of the bowel, nor strangulation of the intussusception.

The strangulation, when it exists, depends primarily upon compression of the mesenteric or mesocolic vessels, and, secondarily, upon the constriction offered by the neck of the intussusception, a constriction that becomes potent as the intussusception becomes more and more swollen and engorged.

Among the principal causes of obstruction of the bowel in these cases, the following may be mentioned. 1. The orifice of the intussusception—as representing the lumen of the bowel—is rendered slit-like and narrow by the dragging of the mesentery, and is, moreover, frequently opposed to the wall of the intussuscepting part. 2. The inner and middle cylinders are much bent upon themselves, especially in cases where the small intestine is involved. 3. These cylinders may become enormously thickened by congestion and inflammation. 4. The lumen of the bowel may be plugged by coagulated blood or by ingesta. 5. There may be a polyp at the apex of the intussusception.

The two circumstances, however, in the pathology of intussusception that bear most directly upon the question of treatment, are those having reference to the conductivity of the invagination, and the conditions that lead to spontaneous cure. It is evident that, as soon as the intussusception has become irreducible, all treatment by means of forcible enemata and insufflation ceases to have effect; and that, even if laparotomy be performed, it may prove futile without further operative procedure. The term irreducible must be accepted in a relative sense. There are intussusceptions that can be reduced after the expenditure of considerable and undesirable force. There are others that are absolutely irreducible. From a practical point of view, however, the two classes of case must be placed together.

The chief causes of irreducibility in an invagination are the following. 1. Adhesions form about the neck, or over the whole or part of the opposed serous surfaces of the inner and middle cylinders. So far as I know, the existence of these adhesions is not to be certainly diagnosed. They may be absent in invaginations that have lasted for weeks, and present in others within three days of the onset of the malady. They are met with in about 80 per cent. of the cases of chronic intussusception, and in about 45 per cent. of such cases as are acute. It will be unnecessary to point out that quite recent adhesions are too soft to offer much resistance to attempts at reduction. 2. The intussusception becomes greatly swollen. The swelling is met with principally in two places—at the apex of the intussusception, and along its convex side. Such swelling may offer an absolute bar to all attempts at reduction, even when no adhesions exist. It is the chief cause of irreducibility in acute cases. 3. The inner and middle cylinders may become acutely bent upon themselves, or much twisted or contorted. 4. In the ileo-colic form, the valve offers a serious obstacle to reduction; and, in cases where a polyp has led to an invagination, the tumour may form a bar to the restoration of the parts to their normal condition.

The next matter concerns the question of spontaneous cure in intussusception. The question is one of considerable interest, and one that has conspicuously influenced the whole subject of the treatment of this affection. It is well known that, in any cases of invagination—excepting those that are ultra acute and some that are following a persistently chronic course—spontaneous cure may follow the elimination of the gangrenous intussusception. Such cure has taken place in cases marked with symptoms of the gravest character, in cases where the patient has been lying *in extremis*, and in a condition that has been considered to forbid any operative interference. Indeed, it may be said that, within certain limits, no case may be regarded as so desperate as to be beyond all hope of relief by spontaneous elimination. Patients thus relieved have made excellent recoveries, and have apparently suffered no inconvenience, even in cases where several feet of intestine have been lost.

It has been stated that patients who have recovered after the elimination of a portion of gangrenous bowel, are liable to a stricture of the intestine about the line of separation. An extended examination of the literature of intestinal obstruction, and of a large series of museum specimens, shows, however, that this possible sequela must not only be regarded as uncommon, but as quite rare.

It remains now to consider what are the precise prospects of spontaneous cure that may be held out to a patient in any given case.

Spontaneous elimination takes place in about 42 per cent. of all cases of intussusception. It must not, however, be for a moment assumed that all cases of spontaneous elimination are followed by re-

covery. With the mortality that attends such elimination, I shall deal subsequently.

Spontaneous elimination is greatly influenced by the site of the invagination. In the ileo-cæcal forms, it occurs in about 20 per cent. of the cases; in the colic form, in 28 per cent.; and, in the enteric variety, in 61 per cent. It will thus be seen that the chance is most unfrequent in the commonest form of intussusception.

It is much influenced also by the patient's age, as the following table, drawn up by Leichtenstern, well shows.

In patients under 2 years of age, it occurred in 2 per cent. of the cases.			
Between the 2nd and the 5th year	6	“	“
“ 6th “ 10th	38	“	“
“ 11th “ 40th	40	“	“
“ 41st “ 60th	44	“	“
Above the age of 60 years	46	“	“

Here again it will be seen that spontaneous elimination is most rare in the patients among whom intussusception is most common.

As to the period of time, in the course of the malady, at which it occurs, it has been observed at the end of the third and fourth days of the disease on the one hand, and as late as the sixth or seventh month on the other. The great bulk of the cases fall between the tenth and the thirtieth days of the affection.

Now, in over 40 per cent. of the subjects of spontaneous elimination, death follows from effects directly connected with the original lesion, or with the process of elimination. In some, the gangrene has led to rupture or perforation, or to an ulcer about the line of separation that has subsequently perforated. Others have succumbed to hemorrhage or to diarrhoea, or the gangrenous mass has blocked the intestine, or a portion of the intussusception left behind has led to a new and fatal invagination.

The mortality is much influenced by age, as the following table shows. In cases between the ages of 11 to 20 years, death, after spontaneous elimination, occurs in 28 per cent. of the cases. Between 21 and 40 years, it rises to 32 per cent., and between 41 and 50 to 36 per cent. In patients between 51 and 60, the mortality is no less than 50 per cent., while, in those over 60, it is 85 per cent. Thus the mortality is higher relatively at the very ages when spontaneous elimination is most common.

Spontaneous elimination, therefore, holds out somewhat delusive hopes, and affords but the feeblest support to the expectant treatment. If 100 cases of invagination be taken in children under 11 years of age, it may be safely reckoned that not more than twelve out of that number will be the subjects of spontaneous cure. Yet no fewer than 50 per cent. of all forms of invagination occur in patients of this age.

Before leaving the subject of spontaneous cure, it may be noted that there is much evidence to support the belief that some intussusceptions, of short standing and of moderate degree, may reduce themselves, especially when the patient is under the influence of opium. The data, however, that are available in connection with this subject are not yet of a character to definitely effect any schemes for treatment.

The general mortality of intussusception is no less than 70 per cent. It is highest in the young; and in infants under one year the disease is exceedingly fatal. In over 60 per cent. of all fatal cases, death occurs before the seventh day.

From these facts, I think that the following axiom may be deduced: the treatment of intussusception should be prompt and active, and no reliance is to be placed upon expectant measures. In dealing with the detailed treatment of intussusception, it will be most convenient to limit the matter to the treatment of the acute and subacute forms.

I think that, as the very first element in the treatment, opium should be given. It has been shown that intussusception depends upon disordered peristaltic movements in a limited segment of the bowel. This may be considered to have been proved by the remarkable experiments of Nothnagel for producing artificial invaginations in animals. Certain, at least, is it that the intussusception increases by the sole aid of the muscular movement in the bowel. Opium stills all peristaltic movements, and places the bowel in a condition of physiological rest. When a patient is under the influence of the drug, the intussusception cannot well increase in size, although the process of strangulation may still progress. The pain, moreover, is checked, the symptoms of shock are relieved, the pulse improves, the temperature rises, and the vomiting becomes less frequent and less distressing. There is, as I have already said, little doubt but that certain cases of intussusception have yielded to the early and vigorous use of opium, although, in such instances, but slight changes can have taken place in the intussusception. By the administration of the drug, moreover, the patient is placed in the most favourable possible position for the employment of further treatment. If attempts be

made to reduce the invagination by enemata, the injections will be brought to bear upon a bowel whose walls are inert and not responsive to irritation. The enemata then excite no undue peristaltic movement, but can act with their full force upon the invaginated parts. Or, if, again, laparotomy be performed, the intestines will be found to be quiet and still, and not in a state of turbulent unrest. The drug must be given with caution, and its effects closely watched. It must not be forgotten that opium may mask the principal symptoms, and may bring about so great a relief that the surgeon may be misled into believing that a permanent cure has followed.

With regard to the question of feeding, no nourishment should be given by the mouth in acute cases. At the most, the patient may have a little ice to suck. In acute cases, the question of feeding does not really arise. If any treatment be adopted at all, it must be adopted early, and, before the question arises of keeping the patient alive with food, he will be either convalescent and well able to take nourishment, or on his way to death and beyond hope. Much harm is done by pressing food upon the patient in acute cases. The food is rejected almost as soon as it is swallowed. If retained in the stomach, it will not be digested; and if it pass into the bowel, it will merely excite peristaltic action. It can do no possible good; it may do much harm. If much thirst be complained of, it can be relieved by enemata of pure water; and in certain exceptional cases nutriment may be given by the rectum. In subacute cases, when the vomiting is not marked, small quantities of food must be administered by the mouth or by the rectum. In chronic cases, the feeding of the patient is one of the most important elements in the treatment.

The next element in the treatment consists in attempting to reduce the invagination by enemata. In acute cases, this measure should be adopted as soon as the patient is under the influence of opium. In a really acute case, no benefit can be expected to attend the use of enemata after—as an extreme period—the second day. Forcible enemata given at a later stage, in acute cases, have led to rupture of the bowel; and even when such an accident has not occurred, they have appeared to do little but harm. In subacute cases, successful reduction by injection has followed at almost any period of the disease, even after ten, fourteen, or twenty days have elapsed. With every day that passes, however, the chances of such reduction very rapidly diminish. In this treatment, some use enemata of water, and others insufflation of air. The former means is certainly to be preferred. In infants, and quite young children, the enema should be administered while the patient is under the influence of chloroform. In older subjects, no anæsthetic is required, and the patient's sensations are of the greatest value in estimating the amount of force to be employed. In any instance, opium should have been previously administered. Pure water should be used at a temperature of 99. Cold water merely excites peristaltic movement. The injection should be effected slowly, either by means of a siphon apparatus, or the very excellent instrument for air inflation, introduced by Mr. Lund. By means of the elastic pad and handle of the last named instrument, all escape of fluid from the anus can be well prevented.

No rules can be given to determine the amount of force to be employed. The more recent the case, the more considerable may it be. In subacute cases, the degree of pressure employed should be at least moderate. In any case, the injection should be retained for at least fifteen minutes. The best positions in which to administer the enema are the knee and head, knee and elbow, or lateral abdominal. It is difficult to understand how inversion of the patient can be of the least assistance in applying this treatment. For an inflation, there is no instrument so admirable as that introduced by Mr. Lund.

Enemata of carbonic acid in these cases are, I think, to be decidedly condemned. A considerable degree of success has attended the treatment by enemata and air-insufflation; and it is probable that the results would be still more fortunate if more careful discrimination were exercised in the selection of cases suitable for these methods. In not a few instances, the invagination has been reduced, with the exception of the part about the neck. Some relief has followed for a while such partial reductions; but it has been temporary, and the disease has progressed, after an interval, with its original force. As the result of this treatment is best to be estimated by a repeated examination of the invagination-tumour, it may be observed that such a tumour is to be discovered either through the abdominal parietes or the rectum in nearly 50 per cent. of all the cases. It is most common in the ileo-cæcal and the colic forms; most rare in the ileo-colic and enteric. It is usually more distinct in children than in adults.

Failing reduction by these means, I would urge that, in acute and subacute cases, laparotomy should be performed without delay. It is the delay, and not the operation, that is so serious in these cases. As well might a surgeon hesitate to perform kelotomy in a case of

strangulated hernia after all attempts at taxis have failed. Laparotomy is regarded as a last resort in these cases, whereas it should be looked upon as almost the first resort. There is no middle course open. Of certain other modes of treatment, such as that by massage, electricity, the use of metallic mercury in large doses, it can only be said that they waste precious time, and are merely useless when not harmful. Their employment is in opposition to the chief teachings to be derived from a study of the pathology of the disease. Modern surgery has shown that the opening of the abdomen is by no means a serious undertaking; and in discussing laparotomy in these cases, there is this operation on the one hand, and a disease with a mortality of 70 per cent. on the other. I have already pointed out how slender are the prospects of spontaneous cure. Still more slender are the prospects of the acute or the subacute disease becoming chronic. It is only in a very small percentage of cases that this change from acute to chronic disease has been noticed; and it must, moreover, be remembered that the mortality of chronic intussusception is exceedingly high. Among 59 examples of the chronic disease collected by Rafinesque, there were no fewer than 51 deaths. It is true that the present mortality after laparotomy in intussusception is very high; but it can be shown distinctly that this is due to the delay in the operation, to the custom of regarding it as a last and desperate resource. A like high mortality would attend ovariectomy if that proceeding were, as a rule, postponed until peritonitis had set in, or until the cyst had become gangrenous, or had ruptured.

When it is remembered that, of those who die of intussusception, no fewer than 80 per cent. die before the seventh day, it will be obvious that the surgeon is dealing with a disease that will not brook much delay.

I would urge that, in really acute cases, the operation should be performed within the first forty-eight hours, and, if possible, within the first twenty-four hours, when the patient is an infant or a very young child. The frightful mortality of the disease among such patients would sanction almost any operation.

The procedure, when undertaken, should be carried out with strict antiseptic precautions. In all but exceptional cases, the incision is most conveniently made in the middle line below the umbilicus. The whole area of the abdomen can be well explored through such an incision, and any form of invagination dealt with. If the incision be made over a tumour at any other part than the middle line, the surgeon is rendering himself dependent upon a very precise diagnosis, and in cases of extensive invagination may find his manipulations much hampered by the position of the wound. The intussuscepted mass should be, as far as possible, exposed in the wound, and attempts at reduction made in cases where the state of the gut would encourage such attempts. Reduction of the invagination is best effected by dragging upon the entering bowel with one hand, while the intestine about the lower end of the intussusception is gently squeezed with the other. If the bowel be found to be in a viable condition after reduction, the coil may be replaced in the abdomen, and the parietal wound closed. I am strongly of opinion that a drain should be introduced into the abdominal cavity when any evidences of more than limited peritoneal inflammation exist. The principal feature of the after-treatment should be the maintaining of perfect rest in the bowel—an end effected by the administration of opium, and by feeding the patient, as far as possible, by the rectum only.

The general mortality of laparotomy in intussusception is 72.7 per cent., as estimated from 33 recorded cases. In the instances, however, where the reduction was easy, the death-rate was only 30 per cent.; while, in the cases where it was difficult or impossible, the mortality was 91.3 per cent.

It is difficult to too strongly condemn violent and long-continued attempts at reduction in these cases; and it is needless to criticise certain cases in which a portion of gangrenous bowel has been allowed to remain in the abdomen after the operation. Should the reduction of the invagination be difficult or impossible, or should the bowel be severely damaged, or in a state of partial or complete gangrene, the whole of the involved parts should be at once resected. The involved segment should be drawn out of the wound, and placed upon a flat sponge, so that any escaped matters may be absorbed. The opening into the abdomen also, all around the involved loop, should be plugged with sponges, to prevent the entrance of faecal matter into the peritoneal cavity. The intestine, above and below the part to be resected, should then be secured by one of the many clamps invented for the purpose. The diseased bowel should now be excised, together with a triangular piece of the mesentery, the base of the triangle corresponding to the portion of bowel to be removed. The mesenteric arteries will need to be secured. The edges of the gap in the mesentery should then be approximated by means of many points of minute suture;

and, finally, the divided ends of the bowel should be secured to the margins of the abdominal wound, and an artificial anus established. This artificial anus can, at a future time, be closed by the now familiar resection operation, and the loop, so united, returned into the abdomen. The practice of uniting the divided ends of the bowel, immediately after the resection, is, for many very pressing reasons, to be condemned.

It may be well to point out that neither enterotomy nor colotomy can lay claim to be of value in the treatment of non-exceptional cases of intussusception. These operations certainly relieve the obstruction; but they leave in the abdomen an invaginated intestine, in which the process of inflammation and gangrene can still advance.

The operation of resection has now had an extended trial. Reichel, in a recent paper, collected one hundred and twenty-one cases in which the procedure was carried out. The technical details of the operation have been elaborated, and this method of treatment bids fair to play an important part in the future of abdominal surgery.

THE MANAGEMENT OF THE THIRD STAGE OF LABOUR.

Read in the Section of Obstetric Medicine at the Fifty-Second Annual Meeting of the British Medical Association.

BY WM. J. SMYLY, M.D.,

Gynaecologist to the City of Dublin Hospital, etc.

THE third stage of labour is the period between the birth of the child and the exclusion of the after-birth; including in the latter term the placenta, funis, and membranes. I have been asked to open, or rather to re-open, the discussion of this subject, which, though apparently simple, has always been an obstetric battlefield.

The subject is one of the greatest importance both as regards the mother and her child, and it cannot be doubted by any one who has seen much midwifery practice, or who has read the cases published in the medical periodicals, that, even at the present time, numbers of lives are lost through mismanagement of this stage of labour.

The subject divides itself into two parts; first, as regards the mother; and secondly, as regards the child. I shall confine myself to the former of these, namely, What is the best method of conducting the third stage of labour so as to insure the safety of the mother? And here I would state, as the great fundamental truth, the aphorism of Professor Credé, that, "the uterus itself should expel the afterbirth, and the sooner it does it, after the expulsion of the fœtus, the better. If it do not do so it must be made to, otherwise it may be too late, and the dangers of retained placenta come into force." If this be true, and I cannot imagine any grounds for thinking that it is not so, it altogether eliminates such practices as pulling upon the cord; attempting to express the placenta from a relaxed uterus; "the imitation of the pains when absent," as recommended in one of our most popular text-books; or the introduction of the hand into the uterus, unless in cases of absolute necessity. Of all these methods, perhaps the most disastrous is that of pulling on the cord, resulting, as it has done, in its avulsion, entire or partial, with retention of the placenta and membranes; irregular and inefficient contraction; and partial or complete inversion of the uterus, and violent hæmorrhage. All these dangers have been so frequently dwelt upon, that one might have hoped that by this time the practice would have been entirely abandoned; but this does not appear to be the case, and amongst a series of communications on the subject of hour-glass contraction of the uterus, which appeared this year in the BRITISH MEDICAL JOURNAL, is one by a gentleman who not only admits having pulled upon the cord, but adds that "anyone else would have done the same." Neither does it seem to have occurred to him that the anomaly which he describes was the result of his own mismanagement. Such practice destroys the natural mechanism of placental delivery, and is almost invariably followed by considerable loss of blood.

If, then, the uterus is the proper agent, not only for the completion of the birth, but also for the extrusion of the after-birth, why not regard the third stage as a physiological process, and, just as in the other two, observe, unless in case of absolute necessity, a purely expectant attitude? This inactive method has been adopted in some of the Continental hospitals, and has been advocated by Drs. Ahlfeld, Teuffel, and Kabierske, the last of whom has set forth the following propositions. 1. "The natural powers of normal labour are sufficient for the perfect separation of the after-birth, and the completion of the placental stage, and do so much better and more completely without

artificial aid than with it. 2. The expectant treatment of the placental stage is free from danger."

The first of these propositions is true, and is borne out by statistics. The method adopted in the Strasbourg clinic was, in normal cases, expectant: the state of the uterine contraction being from time to time observed, as well as the amount of discharge; and the bladder and rectum were carefully attended to. Thus hours were often allowed to pass, without any attempt to remove the placenta, even though lying in the vagina. Out of 100 cases treated thus, the placenta was expelled in 69 within three hours. This is more rapid than the observations of others would have led us to expect, and was probably, in some measure, due to the action of the bladder and rectum; for it was usually during an effort to relieve either of these viscera that the placenta was expelled. The old wives' plan of giving the woman a pinch of snuff would be equally efficacious and more agreeable.

By this method it is alleged—and this is the strongest point in its favour—that the membranes, and especially the decidua, are much more completely separated than by a more active one. Instead of being expelled as a thin, and in many places imperfect, membrane, the decidua was found to be by far the thickest portion of the coverings of the ovum. Thus the expectant plan appears to be followed by a better separation of the decidua; but, on the other hand, it is liable to be attended by violent hæmorrhages, which not only immediately imperil the patient's life, but subsequently tend to relaxation of the uterus, permitting the formation of clots within its cavity, which by decomposition might occasion putrid infection. Even Kabierske himself admits that the average amount of blood lost in his cases was greater than that recorded by others; and, though he makes light of this circumstance, there is good reason to believe that it was often sufficient to occasion alarm. It also permits irregular contraction of the uterus, and incarceration of the placenta, necessitating more frequent introduction of the hand, and thus increasing the liability to septic inoculation. Besides, the long delay is not only irksome to all concerned, but is calculated to excite and alarm the patient and her friends. The second proposition, "that the expectant treatment is free from danger," must therefore be rejected as not borne out by facts.

The relative results of the expectant and a more active methods of treating the placental stage are clearly shown by the following statistics by Dr. Weir of Copenhagen.

Cases treated.	Expectantly.	Expression.
Post partum hæmorrhage	5.78 per cent.	2.3 per cent.
Manual removal of placenta	1.33 "	0.64 "
Retention of membranes	1.78 "	2.3 "
Secondary hæmorrhage	0.77 "	0.32 "

From these statistics, it appears that the only advantage of the expectant method is that retention of the membranes is less frequent; but, in spite of this, and contrary to what might have been expected, secondary hæmorrhage is more so.

The results of the expectant method should teach us to keep a constant control over the uterus, by holding it with the hand, and never allowing it to relax. This is the method which has been practised in the Dublin Lying-in Hospital for upwards of a century, and is similar to that introduced into Germany by Professor Credé. A good deal of controversy has arisen as to whether these two methods, namely, the Dublin method and Credé's, are the same or different. I shall, therefore, describe them separately, and afterwards compare them with each other.

The Dublin method has been followed in the Rotunda Hospital certainly since the mastership of Dr. Clarke in 1789. It was described by Dr. Dease, of Dublin, in 1783, and by Drs. Hardy and McClintock, in 1848. I shall quote their description (*Practical Observations on Midwifery*, p. 221).

"Having placed the hand on the fundus uteri, friction and slight pressure are to be made, and, if the amount of contraction thereby induced be not sufficient to repress the hæmorrhage, it will be necessary to expel the placenta from the cavity of the uterus. In doing this, the organ must be grasped firmly, and pressure exerted upon it in the axis of the brim of the pelvis. If the uterus have fallen to the left side, as not uncommonly happens, it must be raised into its natural position, before commencing to exert compression upon it. It will also tend much to the success of the manipulation, if it be performed during the presence of uterine action. Indeed, we have sometimes been surprised at the ease with which the placenta was pressed off during a contraction of the uterus, while previously it had withstood our best directed efforts. These measures we have seldom found to fail in getting away the placenta, unless it be morbidly adherent."

In 1853 Professor Credé first published an account of his method.