

calendar months after the last appearance of the menstrual function.

We may readily comprehend that in this case there was an accidental stoppage of the catamenia for three months, at which period conception took place. Fortunately, the apparently late period at which the movements of the fœtus were perceived, but which, in reality, was the usual one at four months, corroborates the above fact.

Mrs. C— had all the usual symptoms of early pregnancy in May, with total suppression of the catamenia. In this case, also, much surprise was felt that no fœtal movements were perceived until December, and the lady was not delivered till May 20th.

As the mere expectation of perceiving these movements at a particular time will, in numerous cases of non-pregnancy, cause them to be felt, (i. e., in the patient's fancy,) so, in many similar cases to the above, we have another fallacious datum superadded, and the erroneous deductions from such data will afford very supportable cases of extraordinary protracted pregnancy.

The two cases adduced by Mr. Atlee, in the *American Journal of Medical Science*, as proving a twelvemonth's pregnancy, are to be thus explained rather than that we should consider them on such data, as proving a fact so diametrically opposed to the experience of thousands of observant practitioners. In one of these cases, the last appearance of the catamenia took place on August 6th, 1832, movements were felt (?) December 25th, but the patient was not delivered until August 13th, 1833, (372 days.)

In the second case, the last cessation occurred on March 22nd, 1832, quickening took place (?) on August 5th, and parturition on March 22nd, 1833, (365 days;) it is added, that the movements of the fœtus were felt daily after the period of quickening, in both of these instances.

Such cases, however, are valuable in pointing out the great liability to error which exists when we rely, in doubtful cases, on either of the two foregoing signs, as accurate data for the detection of pregnancy.

I have known several instances in which the catamenial appearance had not occurred for many months, and yet impregnation took place.

In the practice of the Northern Dispensary, in May, 1846, at which time I was obstetric physician to that charity, a very respectable woman applied for a lying-in letter, she then being advanced to the seventh month of pregnancy. She assured me that she had not had any menstrual appearance for the two previous years,—i. e., since the birth of her last child, which had been weaned for more than a year.

In another case of a patient, there had been no catamenial appearance for the last six years, during which period she had been either pregnant or suckling.

Some women even appear to conceive more readily when there is any accidental stoppage of the menstrual function. I have known several such instances. Thus, for example, a lady who had been married some months without becoming pregnant, was alarmed, during the night, whilst the catamenia were on her, by a cry of fire in the country mansion where she was at the time on a visit. She, with several other inmates, ran out of the house, and remained standing with naked feet on the damp lawn, until the fire had subsided. The catamenia were suddenly checked, and this occurrence was regarded as the natural consequence of the cold, and was for some time treated as a simple case of amenorrhœa. It proved, however, that she must have become pregnant immediately after the accidental stoppage from cold, and the date of her confinement corroborated the fact.

Two other instances similar to the above have also come under my notice, in which pregnancy speedily followed an accidental and sudden check of the menstrual discharge by cold, although in both cases no pregnancy had occurred previously for some years. Zacchias states, that he attended a lady who was in general very regular as to her monthly periods, but who never became pregnant until she had passed three or four periods previously.

In some rare exceptions a monthly uterine discharge has occurred *only* when the female was pregnant. This was exemplified by a case which I published, some years since, in the *Medical Gazette*.

The following table is the result of 500 cases, in which the exact number of days intervening between the last day of menstruation and that of parturition is shown. With the exception of about fifty, they were private cases, in which the data were most correctly kept; and the others were selected from upwards of 1000 hospital and dispensary cases, presenting

an equal certainty as to date, in females superior to the usual class of hospital patients.

	Days.	Cases.		Days.	Cases.
37th week.	252	4	42nd week.	288	17
	253	1		289	8
	254	3		290	9
	255	1		291	14
	256	2		292	6
	257	4		293	3
	258	4		294	6
	259	4		295	2
38th week.	260	6	43rd week.	296	5
	261	5		297	8
	262	3		298	6
	263	9		299	1
	264	10		300	2
	265	5		301	4
	266	10		302	1
	267	9		303	1
39th week.	268	13	44th week.	304	2
	269	5		305	1
	270	13		306	0
	271	12		307	1
	272	13		308	2
	273	16		309	0
	274	21		310	1
	275	20	45th week.	311	1
40th week.	276	16		314	1
	277	16		315	2
	278	22		316	1
	279	21	Total, 500 cases.		
	280	15			
	281	18			
	282	25			
41st week.	283	14			
	284	15			
	285	14			
	286	15			
	287	11			

In the case which occurred 314 days after the cessation of the catamenia, I find it noted that quickening did not happen until the sixth month,—proving, in my opinion, that conception had taken place later than had been thought. Had minute investigation been made, at an early period, into the remaining five cases, which went beyond the forty-fourth week, it is most likely that some similar facts might have been observed.

It will be seen that the above table agrees with that of Dr. Merriman (114 cases) in showing that the greatest proportion of women complete the period of gestation in the fortieth week after the cessation of the catamenia, and a very considerable number in the forty-first week.

In Dr. Murphy's table of 182 cases, the numbers born in the thirty-ninth and fortieth weeks were about equal, being twenty-four and twenty-five, whilst the greatest proportion (thirty-two) were in the forty-first week, and twenty-five in the forty-second week—equal to those in the fortieth.

In my own tables the 282nd day was that on which the largest actual proportion of the patients were delivered; but the numbers from the 274th to the 282nd day run so near to each other that we must rather take that as the average period. If we allow of a range of from two to six days after menstruation, as elapsing probably before conception takes place, it will then appear that about the thirty-ninth week after impregnation is more probably the ordinary duration of pregnancy; and this will coincide with the results of the table taken from cases of single coïtus.

CASE OF STRANGULATED INTRA-ABDOMINAL HERNIA, RELIEVED BY OPERATION, BUT ENDING FATALLY.

WITH REMARKS.

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(The substance of a Clinical Lecture.)

JAMES J—, aged thirty-five, a porter and a muscular individual, was admitted into the London Hospital on the evening of Wednesday, the 26th of June, labouring under urgent symptoms of strangulation of the bowels. It appeared that he had been subject to a rupture on the right side, and had worn

a truss for some years, but it was not ascertained that a descent of any size had recently taken place, or that force had been employed at any time in returning the protrusion. On the morning of the 20th inst., two hours after going to work he was suddenly seized with pain in the bowels, and from that time up to the period of his admission his sufferings had continued to increase. He had been attended by a surgeon who had administered purgatives, and applied leeches and mustard poultices to the abdomen, without any benefit. When admitted the patient had a feeble pulse and anxious look—he had hiccups, and vomited stercoraceous matter, but the pain in the abdomen had ceased. I was immediately sent for, and on my arrival made a careful examination of the groin. There was a considerable fullness in the spermatic cord on the right side, but no hernial protrusion could be detected anywhere. On learning, however, the history of the case, and perceiving the urgent character of the symptoms, I determined on performing an exploratory operation without delay. The man was carried into the operating theatre at eight P.M. An incision having been made into the spermatic cord, it was found to be swollen chiefly from cedematous effusion. The inguinal canal was next freely laid open, but no protrusion could be made out. A further examination was then made of the spermatic cord, and at its back part and behind the vessels a sac was found and opened. It was about two inches in length, and quite empty. It extended upwards to the abdomen in the course of a direct hernia, but was entirely closed. As no communication with the abdomen and nothing like a hernia could be found in this direction, I made a further search in the situation of the internal ring. The spermatic cord was pulled down, but without bringing any sac into view. Something like a tense sac, however, was felt in the abdomen. Grasping this between the fingers I endeavoured to draw it down into the inguinal canal, but without success. A portion of the abdominal muscles was consequently divided in order to enable me to reach this part. An opening was next made into what seemed the peritonæum, when a quantity of dark grumous fluid, having a gangrenous smell, escaped, and a portion of intestine of a deep chocolate colour just came into view. The finger was introduced, and seemed to pass into the cavity of the abdomen; but after groping about I distinguished towards the median line a small aperture just sufficient to admit the point of the finger, and I felt as if something like intestine was constricted within the orifice. I was quite unable to make out the nature of this opening, or in what way it was formed within the abdomen, but as there was no doubt of the existence of strangulated intestine I determined to enlarge it at all risks. This was no easy matter, owing to its depth and direction at a right angle with Poupart's ligament. I was obliged to divide the peritonæum for some distance towards the pubis, and with some trouble managed to guide a blunt bistoury along my finger through the opening, which was dilated so as to allow the finger to pass with great freedom. As nothing further seemed called for, the wound was lightly closed and the patient sent to bed in rather an exhausted state. After his removal fluid continued to ooze out of the wound, exactly like that which escaped at the operation; but four hours later it became evident that the bowel had given way, for a quantity of fæculent matter also passed out. With the exception of vomiting he remained free from suffering until his death, which occurred twenty-six hours after the operation.

On examination of the body, there were marks of general peritonitis, the folds of intestine being glued together by lymph. In the right iliac fossa, between the peritonæum and fascia transversalis, there was a large hernial sac, which was found to contain a coil, of about five inches, of small intestine. The intestine was collapsed, its coats being gangrenous and ruptured. Fæculent matter was mixed with the fluid in the sac. The neck of the sac appeared to correspond with the internal ring, but to have become displaced, so that the opening was at an angle with Poupart's ligament, and presented laterally; and the dilated pouch of peritonæum which formed the sac, instead of descending along the inguinal canal, had passed into the iliac fossa, and remained within the bounds of the abdominal walls. The stricture had evidently been very tight for the intestine, as the part corresponding to the opening was much constricted, and its coats partially ulcerated.

The case, the particulars of which I have just detailed, belongs to a class of obscure and rare forms of hernia, which are calculated to embarrass even a skilful and experienced surgeon. One chief point of interest is the circumstance, that, by perseverance, the source of the danger was at length reached, and the constricted intestine liberated; and had the

patient been admitted into the hospital at an earlier period than after six days' strangulation, and before irreparable mischief had taken place, a different result might have ensued. It is true that only the stricture was divided, the intestine being left in the sac; but had the exact nature of the case been satisfactorily made out at the time of the operation, the protrusion was so situated within the abdomen that it would have been impossible to have employed manipulation, so as to have returned the intestine through the neck of the sac into the cavity of the peritonæum, without, at least, a large division of the abdominal muscles. The mode in which the hernial sac was developed in so unusual a situation is a matter also worthy of consideration. I regret that I have not been able to obtain such particulars of the early history of the case as might enable me to arrive at a satisfactory conclusion on this point. It must still remain in doubt, whether the rupture for which the truss was applied was a direct inguinal hernia, which had been cured by the pressure of the truss causing adhesions of the sac at its neck, the intra-abdominal hernia being a more recent occurrence; or whether the sac in the spermatic cord was accidental, and the original descent an oblique hernia, which, by the repeated manipulations of reduction, had, in the course of time, been pushed upwards into the abdomen.

In the *Guy's Hospital Reports* for 1847, (vol. v., New Series,) there is an excellent practical paper, by Mr. Cock, on some obscure and difficult forms of hernia, in which two cases, very similar in character to the above, are related. Both were operated on, but in neither did the surgeon succeed in reaching the stricture, and dividing it. Mr. Cock is of opinion, that the pouch or sac is the result of frequent and protracted manipulation to reduce an old hernia, which has been in the habit of descending through a lengthened period of time. He states, that the repeated application of the taxis, and the means constantly used by the patient to return the intestine as often as it is protruded, appear at length to have had the effect of separating the arch of peritonæum constituting the mouth of the sac from its connexion with the margin of the internal ring; a portion of the sac becomes pushed upwards from the inguinal canal through the opening in the fascia transversalis, and gradually dilates into the cavity or pouch. The case I have related differs from those of the reduction *en masse* in the important circumstance, that no strangulation had taken place out of the abdomen. The rupture may possibly have been originally extra-abdominal, for on this point my experience does not enable me to come to any proper conclusion, but the recent changes giving rise to strangulation undoubtedly occurred within the abdominal walls. It must be observed, too, that in the operation it was quite impossible, either by dragging down the spermatic cord, or pulling down that portion of the sac which presented near the internal ring, to force the protrusion out of the abdomen, so as to facilitate the steps of the operation; hence the difficulty of detecting the nature of the case, and of relieving the stricture, was far greater than in a case of "reduction *en masse*." There was also the complication of the sac in the spermatic cord passing close up to the abdomen, which tended to render the case still more puzzling, and to mislead me as to the real seat of the mischief. Unfortunately, the patient was almost in a hopeless condition at the time of his admission, and it was only my anxiety that he should not die with a strangulated bowel unrelieved whilst a chance remained of his life being saved by an operation, that induced me to venture on its performance.

New Broad-street, July, 1850.

ANÆSTHESIA IN NATURAL PARTURITION.

WITH AN ANALYSIS OF TWENTY-SEVEN CASES WHERE CHLOROFORM WAS ADMINISTERED BY DR. SACHS IN THE BERLIN LYING-IN HOSPITAL.

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(Concluded from p. 42.)

WE now approach the consideration of a question, the affirmative of which has been too hastily assumed, viz., Does chloroform save the patient from pain and its shock upon the system? Those who contend for the use of presumed anæsthetic agents in parturition were at least bound to substantiate this opinion. I submit that they have signally failed to do so.

In the first place, what mean those groans, that agitation, those suppressed mutterings or open cries which all have ob-