

the shrinking of the tumour, so this want is less felt in the recumbent posture. It is only necessary to mention, that this woman has derived great relief from the operation, is stronger than she was, and has been freed from all feeling of distention, pain, and dyspnœa. Of course this relief can only be regarded as temporary; for it is much to be feared, that not only will the sac soon refill, but that the smaller cysts will not long remain dormant.

Charlotte-street, Bedford-square, Nov. 1852.

CONTRIBUTIONS TO THE SURGERY OF RUPTURE.

By JOHN GAY, F.R.C.S.,

SURGEON TO THE ROYAL FREE HOSPITAL, ETC. ETC.

FOUR years have now elapsed since I proposed to the profession a new method of operating in cases of strangled hernia.* I was induced to do so in obedience to my conviction, that the rate of mortality was (and it is still) unnecessarily and fearfully high, and that this fatality was in great measure due indirectly to the operation usually resorted to for their relief; its severity and the comparative difficulty of its performance being such as to have led to the conclusion that the herniated parts, under almost any circumstances, had better, if possible, be returned without, than by division of the stricture. I propose an inquiry into the merits which these two methods respectively possess, and trust I shall be able to show that a reversal of the rule of practice, which still very extensively prevails, would, if acted upon, lead to a considerable saving of human life. If we reflect for a moment on the conditions of a portion of intestine or omentum strangled by its having found its way into a straightened ring or canal, it must be conceded that these could rarely be such, *per se*, as necessarily to give rise to fatal consequences; but that, on the other hand, if the constriction be promptly and effectively remitted, its effects would, in the great majority of instances, as quickly and completely subside.

The causes of death after strangulation of some portion of the abdominal viscera are almost invariably either shock, a low form of peritonitis, or mortification of the strangled parts in some one of its stages. The former of these causes have been known to dispose of the sufferers within twenty-four hours, and the fatal termination cannot perhaps in any case be averted, inasmuch as other and still more formidable lesions are found to co-exist in some department of the organic system, which contribute the greatest share in determining the common result. With regard to the other cause of death—disorganization of the strangled parts—this would appear to be a consequence not so often of the mere act of strangulation as of its persistence, and is induced with a rapidity generally proportionate to the tightness of the stricture, and in some degree to the power of the parts involved to resist the encroachment of the fatal change. Cases have occurred in which sphacelus of a portion of bowel has been induced in the course of two hours;† whilst in other cases this result has not followed, although symptoms of strangulation have existed for a period of ten or twelve days. I am not aware of a case in which such changes have been known to take place in a less period than that just mentioned; but I am inclined to think that in those cases, which must be familiar to every surgeon, in which the coats of the bowel have been cut through by the stricture, as though by a cord tied tightly around it, this result must have obtained in a still shorter period of time. If the circulation be completely arrested in a knuckle of intestine, I quite conceive, with Sir Chas. Bell, that half an hour would determine its destruction. With these facts before us, is the rule of practice which generally prevails such as their urgency can upon reflection justify? I think not.

The first remedy, in every sense of the word, according to the place it holds in the general routine of practice, is the taxis, repeated again and again, with the aid that one or more of its ordinary adjuvants, the warm bath, bleeding, ice, enemata, &c., can give it. Every effort is strained to procure a reduction of the hernia by these means; and it is not generally until the patient is supposed to have reached his climax of toleration both of the disease and of the remedy, that chloroform

and the operation are called into requisition. During this period of experimentalizing, be it remembered, some of the most important and delicate structures of the body are exposed to the inroads of the most rapidly and surely fatal lesions to which they can be subjected. Let us look for a moment at what the taxis has achieved under the best auspices, and endeavour to arrive at a true estimate of the value of this mode of reduction in cases of strangled hernia. For this purpose I might have appealed to the results of my own practice, which certainly are most unfavourable to its employment; but in order to avoid any doubt as to the premises, I have constructed a table from Mr. Poland's interesting and able report of the cases of hernia that were admitted into Guy's Hospital, from September, 1841, to December, 1842.* (See opposite page.)

From this table it will be seen that the taxis was employed in twenty-six cases of hernia, in which strangulation had existed for periods varying from two hours to seven days. In five of these cases the taxis is reported to have been successful, so far as reduction was concerned; whilst in the remaining twenty-one, it failed altogether of its purpose. In one of the five successful cases (38) strangulation had existed about twelve hours; the taxis (which had been employed by the patient himself) was again used; then a warm bath and an enema; and afterwards ice, which was kept applied apparently for two hours, when the parts were reduced "without difficulty." The man "had been labouring under some abdominal complaint for the previous fortnight." He died the following day; and the post-mortem inspection revealed the existence of general peritonitis, with considerable bloody and serous effusion into the abdominal cavity. Three feet of intestines were almost black; (the tumour on admission was "enormous" and "elongated.") The intestines, both large and small, contained a quantity of grumous blood, and their lining membrane was much congested and soft. The symptoms of this patient on admission were not severe; the tumour was "rather tense, but not tender," and he suffered from sickness, considerable pain, and general distress. There can be little doubt but that in this case the taxis proved worse than abortive—that, in fact, it caused those lesions which terminated the patient's life; and, moreover, that if the stricture had been relieved by operation as soon as symptoms of strangulation showed themselves, the life would have been saved. In case 39, a portion of the contents of the sac, probably the intestinal, was reduced by the first manual effort; the patient obtained relief, but the whole was not returned until after the expiration of eight days. The case was not severe at first; but to the taxis certainly belongs the credit of its reduction. In the third case (41) the patient had been operated upon five months before. On this occasion the tumour became rather large, tense, and moderately tender; the constriction at its neck being very tight. The taxis was first employed, then the warm bath with calomel and opium, and subsequently ice. The contents of the sac were returned without much difficulty, when the opening of the ring was found to present an oblong slit, with hard, unyielding edges. In all probability this state of the ring, the result of the former operation, furthered the good offices of the taxis in this case. In case 43 the symptoms were moderately severe; and after the application of ice for the space of two hours, the taxis proved successful.

In the fifth case (44), the symptoms of which were moderately severe, an enema was given, and ice applied to the tumour, after which reduction was effected by taxis. Four cases only could be said to have had a favourable termination, and in one of these it might be questioned whether this result was not in great measure attributable to the accidental and peculiar condition of the ring at the time the taxis was employed.

In twenty-one instances the taxis failed altogether, although it was, with a few exceptions, undertaken by most competent surgeons; in cases of, for the most part, moderate severity; and persisted in, with judiciously ordered intermissions, for periods varying from two hours and a half to twenty-nine hours. In eighteen the stricture was ultimately divided; but of these nine died, and the inspection discovered in each those irreparable lesions which are well known to be occasioned by violence or protracted strangulation, or by both causes combined. There were no grounds for suspecting in any one case that the operation itself had the slightest share in bringing about the fatal result; but much reason for thinking that the lives of most of these patients would have been saved had the operation been substituted for the first manual efforts at re-

* Although averse to the use of new terms, I cannot but think the term "strangled," applied to hernia, much more suggestive than "strangulated." The import of the former is self-evident; whilst the latter derives its force almost entirely from its alliance.

† Larrey, Mémoires de Chirurg. Militaire, tome i.

* Guy's Hospital Reports, April, 1843.

CASES OF STRANGLERD HERNIA.

From Mr. Poland's Report in Guy's Hospital Reports, No. I., April, 1843.

Case.	Variety.	Period of Strangulation.	MEANS EMPLOYED.							Time occupied in the use of these means.	RESULTS.		Operation.	Condition of Parts, &c.	Termination.
			Taxis.	Hot Bath.	Ice.	Leeches.	Bleeding.	Tobacco.	Other Emmata.		Successful.	Non-successful.			
1	Oblique inguinal.	About 12 hours.	Several times.	—	—	—	—	—	—	A few hours.	—	—	—	Ileum highly congested	Recovery.
4	Oblique inguinal.	15 hours.	Ib., violently, by patient.	—	—	—	largely.	—	—	12 hours.	—	—	—	Post-mortem.—Ten inches of ileum excessively congested; coated with false membrane; two rents in coats of bowel.	Death.
8	Femoral.	48 hours.	Moderately.	—	—	—	—	—	—	24 hours.	—	—	—	No post-mortem examination	Death.
10	Ib.	48 hours.	Repeatedly.	—	—	—	—	—	—	4 hours.	—	—	—	No post-mortem examination	Death.
11	Ib.	32 hours.	—	—	—	—	—	—	—	29 hours.	—	—	—	Intestines healthy at time of operation; no post-mortem examination.	Death.
12	Ib.	4 days.	—	—	—	—	—	—	—	2 hours.	—	—	—	Intestine of a dark colour, with ash-coloured spots; slit up during operation.	Death.
14	Scrotal.	59 hours.	—	—	—	—	—	—	—	4 hours.	—	—	—	A portion of omentum submitted to taxis congested, black, and crisp; intestine also congested; post-mortem examination, low peritonitis.	Recovery
15	Femoral.	34 days.	By patient.	—	—	—	—	—	—	3 days.	—	—	—	At first refused; afterwards assented to.	Death.
16	Ib.	20 days.	—	—	—	—	—	—	—	17 days.	—	—	—	Operation refused; gangrene and fecal infiltration.	Death.
18	Ib.	11 days.	—	—	—	—	—	—	—	10 days.	—	—	—	Operation refused; gangrene and peritonitis.	Death.
19	Ib.	7 days.	—	—	—	—	—	—	—	A very short time.	—	—	—	Omentum in a state approaching gangrene; bowel dark and rough, not gangrenous.	Death.
21	Ib.	34 hours.	—	—	—	—	—	—	—	10½ hours.	—	—	—	Omentum approaching gangrene; knuckle of intestine black.	Recovery.
22	Ib.	43 hours.	—	—	—	—	—	—	—	17 hours.	—	—	—		Recovery.
23	Oblique inguinal.	27 hours.	—	—	—	—	—	—	—	13 hours.	—	—	—		Death.
25	Femoral.	5 days.	—	—	—	—	—	—	—	17 hours.	—	—	—		Recovery.
27	Ib.	2 days.	—	—	—	—	—	—	—	3 hours.	—	—	—		Recovery.
28	Ib.	19 hours.	—	—	—	—	—	—	—	4½ hours.	—	—	—		Recovery.
31	Ib.	20 hours.	—	—	—	—	—	—	—	6 hours.	—	—	—		Death.
32	Congenital scrotal.	5 days.	vigorously.	—	—	—	largely.	—	—	About 26 hours.	—	—	—		Death.
36	Scrotal.	—	—	—	—	—	—	—	—	—	—	—	—	Intestine healthy	Recovery.
38	Ib.	About 42 hours.	—	—	—	—	—	—	—	About 10 hours.	—	—	—	Post-mortem.—Acute peritonitis; intestine fissured at the seat of stricture, evidently by traction used in drawing it out for examination; sac black, approaching gangrene; its contents, omentum and bowels, sphacelated; the latter perforated.	Death 23 hrs. after reduction.
39	Femoral.	About 15 hours.	—	—	—	—	—	—	—	—	—	—	—	Post-mortem.—General peritonitis; intestines nearly black; a portion of intestine not discoloured still in the sac; grumous blood in the intestines; lining membrane congested and soft.	Recovery
41	Scrotal.	11 hours.	—	—	—	—	—	—	—	10 hours.	—	—	—		Recovery.
42	Femoral.	7 days.	—	—	—	—	—	—	—	5 hours.	—	—	—	Portion of omentum gangrenous, and removed.	Recovery.
43	Scrotal.	About 20 hours.	—	—	—	—	—	—	—	About 2½ hours.	—	—	—		Recovery.
44	Femoral.	5 days.	—	—	—	—	—	—	—	2½ hours.	—	—	—		Recovery.

duction. In those cases in which recovery followed the operation, the periods of time allotted to the trial of desultory measures varied from two hours and a half to (in one case) seventeen hours; clearly showing that the success of the operation was, to a certain degree, associated with its comparatively early adoption.

It is evident, then, that but little confidence can be placed in the best efforts at manual reduction of the parts in cases of strangled hernia, apart from division of the stricture; whilst, on the other hand, the evil effects of the force used, as well as of the protracted constriction which its employment necessarily occasions, give rise to an amount of fatality that fearfully counterbalances the presumed advantages of its being occasionally successful. The truth and importance of these observations are well attested by the following extract from Mr. Hancock's interesting and able work on Hernia:—

"Between the years 1834 and 1839, M. Boyer never operated until he had made prolonged attempts at reduction, and during that period nine cases were operated upon, of which eight died and one recovered. From 1839 to 1843 he employed the taxis to a much more limited extent. Seven cases were submitted to operation, of which four died and three recovered. From 1843 to 1846 he had almost entirely abandoned the use of the taxis; and out of fourteen cases upon which he operated, four died and ten recovered. Mr. Manee, on the contrary, during the same period, almost always proceeded to operation without employing the taxis. *Out of twenty-eight cases operated upon, two died and twenty-six recovered.*"—(p. 65.)

But the results of the taxis are often disastrously fallacious, as Mr. Cock, Mr. Teale, and others, have, in common with myself, observed. Not only has the sac with its strangled contents been together returned within the abdominal or pelvic boundaries, but it has frequently happened that a portion of the fluid contents of the sac, or of the intestine, has been forced back beyond the seat of stricture; the patient has been relieved, and the surgeon has been led to entertain a hope that the urgency of the case is at an end. A few hours, perhaps a night, have been allowed to pass, the symptoms have recurred with greater severity than ever, and, despite of the release of the parts by operation, the life has been lost. What then should be the rule of practice? Clearly, that in cases of strangled hernia the parts should be freed without the least possible delay; and by the use of those means which give the greatest promise of success. Is a trial of the taxis justifiable in any cases? 1st, Clearly not in those in which, however recently the contents of the sac might have become strangled, symptoms of constitutional disturbance have developed themselves; 2nd, Neither in those in which, from the existence of local pain or tension, we infer the encroachment of those lesions which, if unchecked, must prove fatal, and that possibly in the shortest space of time; 3rd, Nor in those cases in which strangulation has been protracted beyond a few hours, seeing that of all the circumstances that are known to compromise the safety of the patient, none is more insidious, more replete with peril, than delay.

From the table adduced, which certainly places the taxis in the most favourable light, as well as from my own and the concurrent testimony of those who have seen much hernial practice, I confess myself indisposed to place any reliance upon it in a case of strangled hernia, especially whilst I am prepared with a surer remedy, being persuaded that that surgeon will save most lives who trusts to the division of the stricture, and to that alone. If the taxis be tried at all, it should be tried on cases exceptional to those I have just classified, whilst the patient is under the influence of chloroform, and after both the patient and friends have been warned of the nature of the case, and of the course it is necessary to pursue, so that in the event of its failure the operation may at once be proceeded with. Moreover, this trial should be limited to the most gentle pressure, continued but for a very few minutes, and concentrated towards the axis of the ring. The ordinary adjuvants of the taxis are unequal, in point of efficacy, to chloroform vapour, and would thus be dispensed with, together with the mischievous delay that their trial occasions.

For a case of strangled hernia, then, there is but one remedy on which the surgeon can depend—the division of the stricture. This is no discovery of mine; it has again and again been reiterated, but every day's experience, and the revelations of practice in the various journals, show that the doctrine is still far from being generally believed in; or if believed in, is not acted upon. The rule of practice which appears very generally to prevail, even at this moment, is, as I have already

observed, that it is better to reduce the parts by any other method than by an operation; and that consequently this should be delayed until every other shall have proved futile.

The fact is, a very strong and general antipathy exists against the operation abstractly; and probably by those who have a lingering impression on their minds of the large incisions, the tedious dissections, and the wholesale exposure of parts which its performance some years since was wont to involve, this antipathy might be entertained with some considerable show of reason. It is not, however, warranted by the more than necessarily elaborate mode of dividing the stricture, which was adopted in the time of Pott and Hey; for these surgeons averred, that the operation, if well done and in due time, did not prove the cause of death once in fifty times. Even now the usual operation has many defects; and in order to remove the prejudice referred to, it appeared to me, that in its place a mode of dividing the stricture was required, which should be simple of execution, free from danger, and still effective for all purposes; a method which should claim to be preferred to all other methods of effecting the reduction of a strangled hernia.

The defects of the old operation are—that (if I may so express it) it is too anatomical, and not sufficiently surgical; and that in securing a very simple end, it is too prodigal of the means. The knowledge and skill required for its performance have not only served to lull all suspicion as to the actual necessity of so elaborate a procedure, but have so captivated as well the aspiring student as the practised surgeon, that its real elements of danger are commonly looked upon as its greatest charms. The simple objects of the operation should be, of course, the division of the stricture, wherever situated; the method, that by which a knife can be brought most readily to bear upon it. According to the old operation in femoral hernia, the form and extent of the preliminary incisions are to be determined by the size and shape of the hernial tumour; but what relation these latter circumstances have to the seat of stricture, so as to necessitate such a modification of the operation for its division, I cannot divine; seeing that under every conceivable condition of the tumour, the seat of stricture, according to the variety of hernia, might be, and is for the most part, the same. This, and other unintelligible absurdities appertaining to the method referred to, quite suffice to account for, if not to justify, the dread that has generally been entertained with respect to it.

The method of operating to which I have called the attention of the profession is, I believe, free from these objections. By it the seat of stricture is reached through a small wound made in healthy structures, immediately contiguous to the neck of the hernia, and so as not in any way to interfere with or to disturb those parts of the protrusion not directly implicated in the act of strangulation. It has nothing to do with the size, shape, or even direction of the tumour, for it deals with the stricture, and with that alone, so long as that comprehends the seat of the mischief; whilst, at the same time, it allows of any modifications that extraordinary circumstances or conditions may in the course of the operation be found to demand. It is not, as some have alleged, a species of subcutaneous section, for the whole process may, if required, be distinctly seen; although, with a moderate acquaintance with the anatomy of the region, the sense of touch is alone necessary for its safe execution.

I have now operated according to the plan advocated many times, and have only had to repent of the use of the taxis and of delay. In favourable cases, where the stricture has been outside the sac, the operation has only taken from one to a very few minutes for its performance; whilst the recoveries, in comparison with those after the old operation, have been, on an average, as seven days to thirty. The operation has been, I am pleased to learn, adopted by several of our leading surgeons, but it is not without the utmost gratification that I extract the following remarks from a clinical lecture by the distinguished Professor of Surgery at King's College; for not only do they confirm the opinion I have never ceased to entertain of the method of operating which I have advocated, as well as of the principles upon which it is based; but, coming from so high an authority, and after so long and deliberate a trial as that which Mr. Fergusson has done me the honour to give to my views, they abundantly justify me in still claiming for them the serious attention of my professional brethren.

Mr. Fergusson says, after giving a very clear account of the method of performing this operation: "Now this I consider a most simple proceeding, . . . and I think it deserves much more attention than it has already obtained at the hands of practical surgeons. I have for the last few years rarely performed any other operation.

The incision in the skin admits of the sac being opened or not, as may be found advisable. I feel certain that those who see this operation performed, will confess that there is some advantage over that which is usually done, both from the facility with which it is accomplished, and from the simple aspect of the whole process. I shall avail myself of other opportunities of calling your attention to this method, as I deem it worthy of your careful consideration; in fact, I should not be doing my duty as your teacher, if I did not bring before your notice what I myself deem a very great improvement in connexion with hernia, as the proceeding referred to is really not much more than the simple operation of the taxis."

OBSERVATIONS ON THE IMPULSE OF THE HEART.

By ROBERT CARTWRIGHT, Esq., Surgeon.

SINCE my last communication in *THE LANCET*, Oct. 23rd, I have opened another dog, and Mr. Blaikie agrees with me, that in this case, also, the first sound occurred during the diastole, and that there was not the slightest tilting or turning of the body or apex of the heart during the systole, but there was a slight jerking movement during the diastole. The systole commences instantly, the very moment the ventricles are fully dilated; it does not begin at the base, as stated by Valentin, for that would be the very means to prevent the expulsion of the blood; nor is it by a simultaneous contraction of all the fibres, for that would tend to have a similar effect, though in a less degree. Some physiologists consider that the heart, by its contraction, is shortened, and the base, by the swelling of the muscular substance, becomes broader and larger, so as to raise a weight, and give a distinct impulse to the finger; but the error, so far as there is any, can be satisfactorily explained. The contraction begins at the apex, squeezing the blood onwards to the base, which thus becomes enlarged during the contracting of the heart, not by the swelling of the muscular substance, but by the additional blood passed into it; the base then contracts, and when the heart is completely contracted, it is diminished in all its dimensions. The systole of the heart is a successive act; even in its quickest motions, there is still a successive, and not a simultaneous, contraction of all its fibres.

The diastole begins about the middle of the heart. I am inclined to this opinion, because in the two rabbits which I examined last year, and in which the heart's action was not only vigorous, but identically the same in each, the spiral fibres about the middle appeared first to unroll themselves, and then the apex shot out,—and then the whole heart appeared to be suddenly enlarged in all directions; in the two dogs the action was very similar, though not so distinctly marked. The diastole may be divided into two stages, but I consider that the dilatation in each stage is active, and that the contraction of the auricle has merely the effect of slightly increasing the tension of the walls of the ventricle, and thereby perhaps serving as a stimulus to its immediate contraction. The first stage of the diastole is to admit the blood from the auricle towards the apex; in the second stage, the muscular fibres at the base and along the septum unravel themselves, a beautiful illustration of which statement I saw in an ass; the animal being only six weeks old, sank under less than a drachm of chloroform, so that on opening the pericardium there were only the apex and auricle in action, passing the blood backwards and forwards between them; there was not the slightest action perceptible at the base, nor along the median line.

In the diastolic theory of the impulse, the movements and sounds of the heart, and the course of the circulation, all harmonize together;* the dilatation of the ventricles instantly followed by their contraction, and then a pause; the first sound instantly followed by the second, with a slight interval, sufficient to separate the two sounds from one another, and then the pause.

DIASTOLIC THEORY.

Diastole.	Systole.		Pause.
	Contraction of apex.	Contraction of base.	
1st sound.	{ Slight interval between the two sounds. }		2nd sound. Pause.

What is this pause? It appears to me, that the heart remains after the systole in a state of contraction, thereby serving as a base to the column of blood, which would otherwise immediately fall back, the body being in an upright position, by its own

* The first sound is caused by the passage of the blood from the auricle into the ventricle towards the apex; and the second sound by the forcible propulsion of the blood against and through the semilunar valves.

gravity, and not in consequence of the elastic reaction of the arterial walls. When the contraction of the ventricle ceases to dilate the commencement of the aorta, the elastic reaction of the artery begins, and exactly at that point where the muscular substance ceases; consequently, the sole effect of this elasticity is to pass the blood forwards, and not one single drop backwards. The closing of the semilunar valves takes place during the second stage of the diastole, just when the ventricle is completely dilated.

I take the liberty of again referring to Professor Skoda's valuable case, where the sternum was deficient in a child a few days old. "By applying the hand, one could easily perceive that the heart was vertically placed, and moved with each systole downwards and forwards, with each diastole upwards and backwards. The impulse was felt with each systole of the heart immediately above the insertion of the diaphragm; with each diastole, on the contrary, as high as the second rib, if the fingers were sunk sufficiently deep towards the spinal column, &c.; the heart during each systole glided about an inch downwards." Cruveilhier says, that in his case the heart projected downwards during the ventricular diastole. It is evident that Professor Skoda did not see the naked heart, but merely felt the movements through the pericardium, and it is equally evident that he has misapplied the terms systole and diastole. I have seen the naked heart, the pericardium having been slit up, in five rabbits, two dogs, and two asses; and in each instance, without the slightest deviation, during the diastole the apex projected downwards, and during the systole it was again drawn upwards; that is to say, taking into consideration the normal position of the human heart, the heart moves with each diastole downwards and forwards, and with each systole upwards and backwards; an impulse being felt during the diastole at the apex, and during the systole at the base, for the base is very slightly enlarged during the diastole, but considerably so during the contracting of the heart, as before stated, by the passage of the blood, giving a distinct shock to the finger.

It is unnecessary to recapitulate my reasons for believing that the impulse occurs during the diastole; the proof hinges entirely on the point—Does the first sound occur during systole or diastole? Dr. C. J. B. Williams's extraordinary "fifth Observation," as quoted in my last letter, must prevent, in the present state of our knowledge, any man, however prejudiced, from placing any reliance on his and Dr. Hope's experiments with reference to the first sound.

November, 1852.

ON A SIMPLE BOOT FOR ATROPHY OF THE LEG.

By ROBERT CHARLES CROFT, Esq.

THE following description of a simple contrivance, in cases of atrophy of the leg, has been written in the hope that it may prove useful to some of the readers of *THE LANCET*, particularly to those who, living in the country, and having such cases to treat, have to depend upon their own mechanical skill for the manufacture of such appliances.

It not unfrequently happens in infancy, that paralysis of a limb occurs during dentition, but, generally speaking, as soon as the irritation caused by teething has been removed, the limb thus affected resumes its proper functions. Sometimes, however, paralysis takes place without any perceptible or assignable cause; and it is to a case of the latter kind the reader's attention is invited.

M—, a little girl seven years of age, has always enjoyed excellent health from her birth; the period of dentition was passed through easily, without any unusual constitutional disturbance, and, to the best knowledge of the mother and nurse, she has never had a convulsion or fit of any kind. When she was a few months old, her right leg was observed to be somewhat smaller than the left; and when she began to walk, it was noticed that she appeared to have less strength in it. From this time the right leg seemed to be imperfectly nourished, and grew very little compared to the left. When the writer first saw her (now six or eight months ago), the condition of the right leg was as follows:—It was an inch shorter than the left. The thigh, although considerably atrophied, was not so small in proportion to the other thigh, as the lower part of the leg, from the knee downwards, which was very small in proportion to the other leg. In walking, she threw the leg forward, swinging it over, as it were, and bringing the inner part of the heel to the ground. At this time, too, she had been wearing for some time two iron bars, running for a short distance up either side of the leg; an apparatus resorted to, to cause her to put her foot flat to the ground. On carefully examining the leg, it was found, tha