acute philosophic observation of Denman, that a cry uttered at the acme of a bearing-down effort uncontrollable by the will, by opening the glottis and permitting the escape of the retained breath, at once broke the force of the expulsive nismus, reduced the pressure on the perinæum and uterus, and saved both from rupture. He turned this observation to account in practice. He says, "When the patient has been outrageous, and the danger of a laceration very great, I have sometimes gained a respite by telling her suddenly, in the height of a pain, that the child was already born." The agony of the pain occasioned by the stretching of the external parts frequently fulfils the same indication; and the obstetric practitioner may often have occasion to observe how the strain is taken off the perinæum, perhaps entirely, by a loud cry extorted from the mother by anguish too acute to bear in silence; or gradually, by a succession of explosive emissions of air by partial openings of the glottis; and he will admire how Nature can extract a real good out of an apparent evil.

When the parturient woman reflects that pain contributes directly to her safety, and the integrity of her structure, she will bear the infliction with greater fortitude.

Emotional influence is annulled when anæsthesia is induced; when the sensation of pain is removed, the system takes no alarm at impending danger: the glottis remains closed; the expiratory muscles continue to act with all the blind violence of the diastaltic function; the order of parturition is subverted; one essential link in the chain of the phenomena of labour is wanting; the head is driven with irresistible fury upon the perinæum, before the process of dilatation is complete. It is not surprising that laceration is the result. It is fortunate that at this stage of labour the perinæum more readily gives way than the uterus.

3. In one case, convulsions followed delivery in two hours; in nineteen hours more the patient was dead. In another case, ominous quivering of the lips and shivering occurred during the anæsthetic state, and shivering fits were renewed after delivery. (Case 2.) In Case 17 also, strong shivering followed delivery. Among the other cases also one of convulsion occurred.

In the fatal case, there existed, no doubt, a strong predisposition to convulsions in the previous epileptic attacks. The suspicion will, however, obtrude itself, that the chloroform inhalation was the immediate exciting cause. To my mind it is a well established clinical fact, that in all those states of the system in which the controlling influence of the cerebral or sentient organ is in abeyance, whether from sleep, coma, interrupted communication between the brain and the spinal marrow, as in certain cases of paraplegia, or the action of narcotic poisons, if not carried to an extreme degree, the diastaltic function assumes an abnormal, a morbid activity, which, under strong excitation, may easily merge into convulsions. In parturition, this danger is infinitely increased by the accumulated excitability of the spinal marrow.

The above statement rests upon facts and clinical observation. To refine too much upon mere physiological reasoning might weaken the force of that statement. I am anxious to avoid this. But I think the following remark has a valuable application:—The insensibility of the cerebrum to pain, induced by chloroform or otherwise, prevents the development of that emotional relaxation of the glottis which is the safeguard against excessive diastaltic action. Hence protracted circulation of black blood; hence congestion of the brain and spinal cord; hence coma and convulsions.

There is a fact—a clinical, a pathological fact—of momentous application to this question. Ether and chloroform have been repeatedly given to epileptic patients: a convulsive seizure was the constant result; so constant, indeed, that, until ether and chloroform were tried, no agent was known by means of which a fit could be so certainly and infallibly produced.

The relation between epilepsy and puerperal convulsion is too well understood. Out of a given number of pregnant epileptics it may be predicted with confidence that a certain proportion will fall into convulsions at the period of labour.

Summed up, the case is this:—

Labour is the exciting cause of convulsion in epileptics.

Chloroform is a certain exciting cause of a fit in epileptics.

Combine these two conditions; give chloroform to an epileptic patient in labour. What will be her prospect of escape?

The tenth case of Dr. Sachs offers a terrible illustration.

I propose to relate hereafter the histories of several cases of puerperal convulsions arising after the administration of chloroform.

4. In one case metritis and dysentery followed. Without

pretending to account for the influence of chloroform in inducing metritis, I think it is not unimportant to call attention to the fact, that in the summary given by Dr. Sachs, of the effects of ether, he refers to metritis as one of the disasters following the use of that agent. M. Dubois also witnessed puerperal fever in two cases in which ether had been employed, and he was unable to resist the impression that there existed between those circumstances relation of cause and effect.

5. On contemplating this fearful array of accidents supervening on the induction of anæsthesia in natural labour—i. e., natural until interfered with,—the question arises, and it must be answered, For what purpose is the risk of all these evils encountered? What is the promised boon which is to be sought at such imminent hazard?

The escape from pain, and the consequent shock to the system.

The reply is not convincing. To sanction the doctrine of interference in natural parturition built upon this proposition the following questions must be satisfactorily answered:—1st. Is the pain of labour physiological or pathological—that is, is it injurious or beneficial in its action? 2ndly. Is the abolition of the sensation of pain in the parturient woman unattended by danger? 3rdly. Is it true that the anæsthetic agents in use really annihilate pain, and save the system from the consequent shock?

When an enthusiastic dogmatism and blind experimentation shall have given way to calm reflection, and a rigid analysis of clinical observations, I believe it will be found that neither physiology nor pathology authorize the utter subversion of a natural function, which the induction of anæsthesia in natural parturition involves.

I will take the liberty of referring to a lecture published in the *Medical Gazette*, (Oct. 1849,) for some of the considerations which tend to prove that the pain of labour is physiological, and subservient to a useful end. Other arguments will be found in the present paper.

All we know tends to confirm the popular belief, sanctioned as it is by the most eminent obstetric authorities, that lingering are not, *pro tanto*, more dangerous than rapid labours.

To the second question proposed, the analysis I here present of the unhappy results of anæsthesia in twenty-seven cases of natural labour supplies a conclusive answer. In addition to the dangers which may be expected to result, and which do result, from subverting the order, and impeding the forces of natural labour, it must not be forgotten that the administration of chloroform is attended by dangers inherent in the agent itself. There is a shock, oftentimes a deadly one, from chloroform as well as from pain.

The cases of Dr. Sachs show that at the commencement of the inhalation the pulse is frequently raised twenty or thirty beats in the minute, and the respirations are increased to forty, fifty, or sixty in the minute. The after effect is to lower the pulse and respiration below the normal standard. This is, perhaps, still more dangerous, as it indicates a depressing action on the spinal and ganglionic systems. Does no shock to the nervous centres attend this sudden impulse of blood, and that blood carrying a subtle poison? One fatal case at least proves that apoplexy in the brain may result. The occurrence of puerperal convulsions indicates in what manner the unwonted stimulus may affect the spinal cord. Repeated cases of fatal syncope attest the influence of shock upon the heart. The moral shock has ended in puerperal mania. Candour will surely admit that the reality of shock from chloroform rests upon better evidence and more numerous facts than does shock from pain.

(To be continued.)

REPORTS OF

CASES ILLUSTRATIVE OF THE REUNION OF BONES AFTER FRACTURE, AND THE EXCISION OF THEIR BROKEN ENDS.

By JOHN RUSSELL, Esq., Surgeon, Merthyr Tydfil, South Wales.

CASE 1.—W. P.—, a haulier, aged eighteen years, received a severe compound fracture of the leg in the summer of (I believe) 1837, in the coal or coke yard of the Doulais Iron Company, at Merthyr Tydfil. His house being near, he was immediately conveyed thither, and I was sent for. On my arrival, the limb presented as ghastly an appearance as can be imagined. The wound, which was on its anterior aspect, was about five inches long, and gaping. The upper and lower

portions of bone (tibia) projected, and, as the limb lay, thus crossed near their points, and were denuded of their periosteum to a very great extent, more particularly the lower portion. The projecting parts, with the surface of the wound, were as black as coal itself, and the two portions of bone appeared more like two sticks of charcoal than what they really were. The fibula was of course broken, but it was not comminuted. The hæmorrhage was inconsiderable. Upon examination, I found that the integuments and soft parts generally at the back of the limb were not materially injured, and the pulsation in the posterior tibial artery was distinctly perceptible. The foot and other parts below the wound, as well as those above it, were uninjured. As this account has been written from memory, no notes of the case having been taken at the time, I cannot speak positively respecting the condition of the anterior artery, but I believe it did not pulsate. Having taken into consideration the age and healthy condition of my patient, I determined on an attempt to save the limb, and with this view I removed, with the saw, the jagged ends of the bone from the upper portion. I took away all that was denuded of its periosteum, which reached to where the circumference of the bone was perfect. But from the lower portion, so extensively was it denuded, that I felt myself called upon to remove a portion, an inch in length, and all measuring the full circumference of the bone. The limb was now placed in the straight position upon the heel, on a pillow resting upon a plane surface, and supported on either side by common deal splints, and bricks covered, &c. The ends of the bone did not reach a state of apposition for a few days. They then came in contact, but so slight was the pressure exerted by the muscles, that it became a daily duty to raise or depress one portion or the other about a quarter of an inch, to remedy the displacement. A little sloughing ensued, suppuration was profuse, and portions of bone, about the size of the fingernail, exfoliated from both ends. Union took place in the course of the ordinary period, (about five weeks.) The wound healed readily. The limb became firmly set. There was little or no enlargement from remaining callus, and with the exception of the shortening of the limb, and the more rounded shape of the tibia at the place of union, no difference was perceptible between the two limbs. The young man continued to earn a good livelihood as haulier, and supported his widowed mother till 1841, when he met with another accident, which caused him the loss of both his legs.

In reviewing the foregoing case, the principal features of practical interest appear to me to be, 1st, the quantity of bone removed. This was done in anticipation of extensive exfoliation, and with the view of saving nature the requisite efforts, and the constitution the irritation, likely to ensue, if such a large extraneous substance should remain in the wound. And, judging from the subsequent exfoliations which took place, I think that the anticipations were pretty well borne out. At the same time I would raise the question, whether, if only sufficient bone had been removed to render adjustment easy, and to leave the ends plain and smooth, it is likely that Nature would have been successful in her attempts to effect union, by the same process which we see set up for the removal of necrosis arising from natural causes; although, be it remembered, many of these cases are tedious, and in others Nature's efforts are unavailing. If success should have attended the mode of treatment now suggested, the patient would have undoubtedly had restored to him a longer and more useful limb. But would not the mortified portion of bone have proved a preventive to union, which I fear would have been the case if the necrosis should have included the whole circumference of the bone at the end? On the other hand, in all cases of accident with comminuted fracture, or with seriously damaged integuments, that I have seen, those of the former kind have done by far the best, in which the portions of bone have been carefully searched for and removed, than others in which their removal has been left to be effected by suppuration, but which often is never thus effected. Those of the latter kind which I allude to are injuries requiring amputation, in which flaps have been procured partially, from portions of integument of questionable vitality, and sloughing has followed. In my belief, no cases do so well as those in which Nature's probable efforts are taken into consideration, and the surgeon as far as possible relieves her of her efforts to remove damaged parts, and she has given her what, if you please, I will call a fair start in her restorative and curative career, which I wished to illustrate, without, for a moment, attempting any dissertation on amputations.

2nd. The difficulty experienced in retaining the ends of the bone in neat apposition. In considering this subject during

the course of treatment, I could conjecture nothing likely to effect my object, unless it could be some mechanical agent applied to the bone itself; and, as the integument bears suture, I could not believe that bone, which is far less sensitive and irritable, would not also. And by experimenting with a common brad-awl upon the removed portion of tibia, I found that canals might be easily and quickly drilled. I therefore determined to adopt it in the first case that I should deem applicable, which did not happen till 1843, and was the following:—

CASE 2.—William B—, aged nine years, was brought to me about Christmas, 1843. I was told that his injury had been received twenty-seven weeks previously, in the mines of the Doulais Iron Company, at Merthyr Tydfil. He had then an ununited fracture of the humerus, situated at the point of junction of the middle and lower thirds. It was perfectly pliable, so that when the upper portion was raised to the horizontal position, the lower and forearm hung perpendicularly. A degree of control could be exercised over the forearm and elbow sufficient to enable him to carry it behind his back, which happens more or less in such cases when the joint remains uninjured. The power of motion in the hand and fingers was very limited, more especially in extension of the fingers. I was told that the small portions of bone which had escaped during the progress of the case had been very numerous; many were shown to me. The wound had healed. I told the friends, that I thought the only possible chance for the restoration of power over the limb would be from the perfect excision of the ends of the bone, to the performance of which they readily consented. And upon consultation with Mr. Probert, surgeon to the Plymouth Iron Works, Merthyr, he fully concurred in the proposition, and shortly afterwards I performed the operation, assisted by him.

I made a free incision on the outer side of the arm, where (fortunately, for anatomical reasons) the lower end of the upper portion of bone was very prominent. I had not much trouble in exposing this, and freeing it from adherings of parts, owing to the very pliant state of the arm. I excised the end freely with the saw, endeavouring, if possible, to remove it sufficiently far so that the end might afterwards comprise the full natural circumference of the bone. This I managed by passing a common dressing-case spatula beneath the bone, to protect the soft parts from injury. To expose the lower portion, and release it from its connexions, was far more difficult, owing to the sharp point having penetrated, and become firmly united in the triceps muscle by ligament. Having released, freely exposed it, and passed the spatula beneath it as before, I removed it, endeavouring to attain the same object as before. The ends of both portions were very pointed, and connected by ligament with a third detached portion of bone included in the ligament, of a triangular shape, and about half an inch long. Having, soon after the case of P—, provided myself with a drill, about four inches long, I bored a canal in both ends, commencing at the sawed surfaces, in the centre of the cylindrical part of the bone, and drilling them slightly oblique, so that the point of the drill made its exit about an inch from the extremities; nothing could be easier than this part of the operation. I then passed a portion of platina wire through the canals, and twisted it into a ligature. In drilling the canals, I had in view to form them as nearly as possible corresponding with a natural line of the bone. The operation, I believe, occupied about three quarters of an hour. The wound was lightly dressed, and the patient placed in bed, with the arm on a pillow, supported below and at the sides by common deal splints, lightly tied. A slight degree of fever ensued, and continued for a few days, and the patient complained of a pricking pain, which he attributed, and I believe correctly, to the wire coming in contact with the soft parts, as it must have been angular at points. Both gradually subsided, very little suppuration ensued, and the ligature came away on the thirteenth day, without the least portion of exfoliated bone attached, or being in the least corroded. It appeared to have been thrown off by a process of ulceration, similar to that by which a ligature escapes from integument. The bone united in the course of the ordinary period, (about a month.) No exfoliation followed, and the wound healed without any interruption.

The following is a report of the present state of the limb, as lately sent me by my friend Mr. Probert:—

“Merthyr, August 26, 1848.

“I saw the boy B— yesterday evening. He had just returned from his work when I entered his father's cottage to make my inquiries. He has been enabled to follow the employment of haulier in one of the collieries ever since the

union of his fractured arm. The humerus now appears to be very firmly united, perfectly straight and smooth, flexion of the elbow joint complete. The extension not quite so perfect, apparently owing to a loss of a portion of the triceps extensor cubiti muscle. Indeed, there appears to be an entire absence of muscular fibres, both posteriorly and anteriorly, over the seat of fracture, their place being supplied by tendinous fibres, which united the upper and lower portions of the biceps and triceps muscles. I presume that the loss of muscles was occasioned by the accident which caused the fracture, or arose from sloughing of the parts afterwards. Were it not for this gap all round the limb, I question whether any one unacquainted with the history could tell that the bone had been fractured at all, so smoothly is it united. The boy has a very useful arm, which he says becomes gradually stronger, owing to the increased development of the muscles which move the limb. This case . . . is most interesting to the profession generally, and ought to be made known to it through the medium of one of the medical journals.

(Signed,)

JAMES PROBERT."

The particulars of the above case I communicated to the late Mr. Liston, soon after its termination. Mr. Liston politely thanked me for the communication; at the same time stating that ligature in bone was not new, which led me then to desist from publishing it; but I have since been urged to do so by a very eminent member of the profession. Judging from several cases of ununited fractures, of which I have heard and read, and some of which appear in Mr. Amesbury's work on fractures, as having been treated by excision, or rather paring of the ends of the bone, it would appear that it is far from being a successful mode of treatment. I think that want of success could not be owing to the nature of the operation; it must have been attributable to the mode of performing it; the accidental position of the bones subsequently, or some intervening substance, either of which latter circumstances would be sufficient to prevent contact of the bones; or else the after treatment not being adequate to prevent displacement. There can, I think, be little doubt that many operations fail from the want of freedom in their performance, and the objects sought for are thus imperfectly obtained. If, in similar cases to the last, the openings in the integument should be made barely sufficient to allow the ends of the bones to protrude, and not sufficient to excise them freely, and at the same time thoroughly to examine the state of the parts and correct them, should any unnatural relations or connexions exist, and to remove any intervening substance that may be found, failures would not be at all improbable. Unfavourable positions of the bones, subsequent to the operation, would be likely to occur, either from the disposition to return to the same situations which they had occupied since the accident, or from abnormal unions with injured muscles, which they may have formed during the healing process. An additional inch or two in a clean incised wound, corresponding with the long axis of a long bone, cannot signify much. The opinion of some authorities is, that a certain amount of vertical pressure is necessary to secure the union of even recently severed bones, and much more in cases of established disunion; and the successful results of the plans of treatment adopted under that idea appear at first sight to favour that conclusion. But I cannot see any grounds on which to raise a doubt, for a moment, of the disposition of bones recently fractured, or the fresh excised surfaces of bone, to unite readily by the simplest contact, as well as integument, and other structures, or even in a state of limited separation, to connect themselves by a process similar (if not by the same identical process) to granulation, and afterwards contract closer together, like the lips of a wound. This, I believe, should be the explanation of callus; for we always find, that in proportion as the limb after fracture is more or less distorted, so is the quantity of callus which Nature throws out to connect the bones, and fill up the vacancies caused by the unnatural position of the bones. And we see a similar result in the thickened and unsightly cicatrix which is left after the healing of a wound, in which the space caused by great destruction of soft parts has been required to be supplied by granulation.

I do not attribute more virtue to the wire suture, in the foregoing case, than that of acting as a safe and direct guide for the ends of the bones into exact apposition. I do not attribute any advantage to it from its compressing the ends of the bones together, as any power of the kind must have been surpassed by the action of the muscles; nor do I believe that advantage was gained by its stimulating any new or in-

creased action in the bone, or I think that exfoliation would have attended its removal. And I believe that all necessary action was caused by the excision. I should doubt whether the material of which the ligature is composed would signify, since the bone threw off the platina in the above case so readily. I used the platina, (being provided with it,) from its non-corrosive nature, and anticipating more irritation in the bone than ensued; and I think the irritation which I attributed to the angles of the wire would have been avoided if an ordinary ligature of silk had been used. There is a feature in both the foregoing cases that I wish to call particular attention to—viz., the absence of any enlargement at the site of union. In the case of P—the ends of the bone were exposed to view till union took place; and in that of B—the power of the ligature to retain them in correct apposition will be readily admitted; and these cases are, I think, sufficient to prove that in all cases of fracture the union would be equally perfect and neat if the bones should be placed and retained in exact apposition. I fear that the profession are not yet in possession of means to secure that object in all cases, and probably cannot afford to resign the adopted theory of callus; and in the many very complicated cases of fracture which must occasionally occur, it must furnish the explanation of the subsequent enlargement. But to myself, when I have been driven to that resource, I have ever viewed it as a professional scapegoat, while mentally I felt a galling fetter.

In reviewing the various modes of treating ununited fractures—viz., vertical pressure, excision of the ends of the bone, seton, and chemical agents applied directly to the ends of the bone, (which I take as the principal ones, where no constitutional taint is supposed to exist,) it will appear that they are calculated to remove one obstacle, which is probably the most frequent one—viz., an intervening substance, by destroying its vitality and producing its removal by sloughing, suppuration, direct removal, or absorption. They have each succeeded, and likewise failed in their turn. Chemical agents will most probably destroy a portion of the surrounding parts with those intended. Seton is a very painful and tedious remedy, and I think is the least to be depended upon. Vertical pressure can only succeed in cases in which the portions of bone can be brought to bear vertically against each other, and could effect no good in a case similar to B—'s, in which the bones were so much displaced and pointed. And I imagine that this remedy requires great skill in its management, but I suppose no one can be allowed to plead to be excused from using it on that ground. Excision of the ends of the bones, then, appears the most adequate to secure all the objects desired—viz., the removal of any intervening substance, to release the bones from unnatural attachments, to furnish them with denuded surfaces, and secure their apposition. The vertical pressure or support I would confidently leave to the action of the muscles. At the same time that excision must be viewed as the most bloody and painful remedy, I do not consider its danger great; while the object of restoring to the patient a firm and useful limb is of the highest importance, especially if he should be dependent upon his labour for his sustenance, which is most likely to be the case.

Merthyr Tydfil, Jan. 1850.

ON SOME DISTRESSING SEQUELÆ OF THE DISEASES OF INFANCY.—PURULENT DISCHARGES FROM THE AURAL, NASAL, AND VAGINAL PASSAGES.

By THOMAS WEEDEN COOKE, Esq.,

MEDICAL OFFICER FOR THE DISEASES OF CHILDREN AT THE ROYAL FREE HOSPITAL.

THE very slight reference made in systematic works professing to treat of the diseases of children to an annoying and, in its consequences, serious discharge, to which the passages above named are liable during childhood, has for a long time struck me as an extraordinary and curious fact, considering the numbers of children who suffer from this form of disease. Feeling, some three years since, the want of information upon this subject, and finding no resource but the book of Nature, I was induced to take notes of every case that came before me, and to watch this particular class of cases carefully. The notes thus taken, both in hospital and private practice, have now so accumulated, that I believe I have acquired accurate data upon which to found a short paper, that may perhaps prove of some interest to those who are engaged in the treatment