with; it would neither depend upon degeneration of the muscles, upon disease of the nerves going to the paralysed parts, nor upon disease of the nerve-centres in which the structural arrangements for bringing about the necessary coordinations exist; so that all the anatomical essentials for the production of muscular movements are present, if the volitional stimulus be forthcoming. But, in these cases, this would be the very thing wanting. The centre whence the volitional stimulus emanates, which calls into action the muscles of the opposite half of the body, being extensively diseased and more or less destroyed functionally, no such stimulus is produced; and, Will being absent, the nervous and muscular apparatus pertaining to the voluntary movements of the opposite half of the body are thereby rendered functionally inert and useless, solely because their appropriate stimulus is not forthcoming.

The solution of a question of this kind is a matter of the utmost difficulty, and it seems wise not to be too hasty or exclusive in one's views on the subject until more data have been accumulated. Whilst admitting the value of the reasoning, based upon the most careful clinical observation, advanced by Dr. Hughlings Jackson in support of his theory, it seems to lack that anatomical and physiological warrant which would be desirable. It is perfectly fair to admit, however, that the clinical facts may be truly explained in this way, although anatomy and physiology have not yet advanced so far in this direction. Still, in our present state of knowledge, we may be allowed to ask whether the different arterial regions are presided over by independent sympathetic nerve-centres, with which are connected afferent and efferent fibres having a similar distribution, so that an irritation of these nerves in any part of a vascular area may be conducted centrally to a vascular ganglion, and thence reflected so as to produce spasm and contraction in all the branches of this particular artery. This being true, we have still to consider whether the spasm so produced could be so intense as to cause an almost complete negation of function in the part—not for a few minutes only, but for days and even one week—and yet at the same time that the vascular supply should be adequate to preserve the nutrition of the part, and obviate all tendency to *ramollissement*. All these things must be admitted, if Dr. Jackson's explanation be a valid one. But, whilst alluding to apparent difficulties which stand in the way of our accepting it, I must not omit to call attention to the fact that, in the *post mortem* record, it is distinctly stated that the veins in the pia mater over the right hemisphere were "very full and turgid, presenting a marked contrast to those of the anterior two-thirds of the surface of the left hemisphere, which were even less distended than usual." At the time this observation was made, I did not know of Dr. Jackson's theory, and unfortunately no special observation was made with regard to the relative vascularity of the two corpora striata. This marked difference in the amount of vascularity of the surface in the two sides, though in accordance with Dr. Jackson's theory, does not seem to be otherwise easily explicable.

The position assumed by Dr. Wilks seems to be theoretically unsailable; and, moreover, in his admirable paper in the *Guy's Hospital Reports* for 1866, he records a case which seems to bear out his views. Here almost the whole of the surface grey matter of the right hemisphere was most extensively diseased; there was a considerable diminution of intelligence, and also the gradual supervention of paralysis on the left side of the body. In this case it is said "the most remarkable symptom was the fact that she never seemed aware that she had lost the power of the left side of the body. On being asked about her arm, she always declared she could move it, although it lay helpless by her side." But in the case before us the question seems to be, whether the amount of convolitional grey matter involved by the disease in the right hemisphere, was sufficient to account for such a diminution of volitional power, as would practically leave the right side of the body paralysed. Doubtless other cases have been recorded in which injuries of surface grey matter have been met with quite as extensive, and yet which have been unaccompanied by hemiplegia. But the mode and rapidity of production of the morbid change must always be taken into account, as well as its extent. It seems possible that chronic changes, as extensive as those met with in the present case, might gradually supervene, and yet produce no paralysis; whilst, if the lesions were suddenly, and, as it were, violently, produced all at the same time, it is quite conceivable that the result might be different. A violent and sudden interference with such subtle functions as are presumed to be carried on in the surface grey matter, may not only annul the functions of the part injured, but may also extend a perturbing or inhibitory influence over the remainder of the grey matter of this hemisphere. Thus, there would be a supplementary reflex influence, such as Brown-Séquard considers adequate for the production of the most definite and varied combinations of cerebral symptoms.

It is interesting to call attention to the fact that, although, in this case, there was a sudden and extensive injury to the surface of the brain,

no convulsions of any kind were produced. In other cases of disease at the surface of the cerebrum, convulsions are frequently met with; and Dr. Wilks strongly insists upon the fact that in many instances of cerebral disease, accompanied by epileptiform convulsions, some morbid change is met with in this region. Also, it is interesting to note that the onset of a sudden destructive change in the convolitional grey matter, not only gave rise to no motor phenomena, and seemed unaccompanied by pain, whilst it did produce marked mental phenomena which revealed themselves by unmistakable symptoms of terror, agitation, and bewilderment. This is as might have been imagined, so far as the absence of sensation is concerned, and is fully in accordance with the results of experiments, which have shown that the substance of the cerebral hemispheres may be cut or handled without the production either of sensation or of movement.

It is reasonable to expect, also, that irritation of a part concerned in the carrying on of intellectual operations, should lead to mental perturbations of some kind, just as an irritation of the optic nerve, for instance, leads to subjective sensations of flashes of light.

It is quite certain, moreover, that, in this case, there was, from the first, not the slightest trace of that partial paralysis of the facial and hypoglossal nerves, which almost invariably exists when hemiplegia is produced by a lesion in the central ganglia. Dr. Alderson told me that this particularly struck him during the life of the patient, and that he called the students' attention to this fact. Of course, if the paralysis in this case had been produced after the manner indicated by Dr. Hughlings Jackson, then the symptoms, if due to an interference with the functions of the corpus striatum, should have resembled those ordinarily produced by a hæmorrhage into or softening of this body. But the absence of facial paralysis may have been exceptional in this case, even though the paralysis were due to the lesions in the convolutions; and such paralysis may perhaps occur in other cases where the superficial lesions are more extensive. Still the absence of such paralysis in this case must not be lost sight of. Unfortunately, no definite observations were made as to the condition of sensation in the paralysed extremities, or as to their temperature. Physiological data would lead one to expect that in a paralysis so produced, sensation would be unimpaired, and the temperature of the paralysed parts unaltered instead of raised.

In cases, therefore, where hemiplegia exists from the first without any paralysis of the facial or of the ninth nerve—more especially where the onset of the symptoms has been sudden, and attended with some evidence of mental agitation—where perhaps sensation is unimpaired in the paralysed limbs, and their temperature is normal, may we not suspect the existence of extensive disease of the grey matter of the convolutions of the opposite half of the cerebrum?

A CASE OF FRACTURE OF THE TIBIA AND FIBULA IN A PATIENT AGED 88.

By JOHN GRANTHAM, F.R.C.S. (Hon.), Crayford, Kent.

In relating the following case of fracture of the tibia and fibula, I am prompted to do so by the fact that tubular bones can unite at such an advanced age as 88 years. If my memory serve me rightly, I think it is the only case of fractured tibia on record as having been completely united so as to support the body without assistance at that age. Mr. Erichsen says, in his work on *Surgery*, that he has witnessed two cases of fracture of the femur in patients upwards of 90 years of age, but does not state the results: only, that the bones united.

My case occurred to a lady, of short stature, very temperate in her habits throughout life, an early riser. She had slipped off a step that had just been washed, causing a transverse fracture just above the ankle-joint, representing the appearance of a dislocation of the ankle-joint backwards, but which, on extension being made, proved to be a fracture of the tibia and fibula. Knowing how much is due in effecting the union of a fractured bone by maintaining tranquillity of the general system so as not to depress the law of life, I resolved in the treatment to leave no principle that could produce ease and quietude unperformed; I applied, therefore, Dr. Neil Arnott's method of making equal pressure (as reported in my work *Facts and Observations in Medicine and Surgery*, page 2), by means of an India-rubber case of the shape of a box, of the length of the leg, without ends or top, supplied with air from the upper end into three compartments; the same secured on the limb with Cline's splints; the whole suspended on one of my suspension apparatus from the top of the bed, the four ropes converging to one point, not being checked by any other machinery, thereby giving full effect to the simple action of the "ball and socket joints".

The accident occurred on Jan. 26th, 1867. The patient is at this time in perfect health, taking carriage exercise and walking without aid.

On reflection, I cannot help feeling how much the science of surgery has been benefited by the invention of Dr. Arnott's mode of making equal support on an uneven surface; also the invention of the "swing" by M. Sauter of Constance. I, on my part, only claim the merit of having been the first to publish the principle of suspension in the *Medical Gazette* in the year 1832; also in adding the curved groove to the board so as to allow the limb to revolve on its own axis.

GERMINATION OF A BEAN IMPACTED SIX MONTHS IN NOSTRIL: REMOVAL.

By THOMAS SMITH, F.R.C.S.,

Assistant-Surgeon to St. Bartholomew's and the Children's Hospital.

A LITTLE girl, aged 6, was brought to me in May 1865, suffering from discharge from one nostril, with enlargement of the same side of the nose and obstruction to the lacrymal duct; the nostril was quite impervious to air. These symptoms had lasted six months, having lately increased in severity. Something could be seen within the nostril; it was soft to the touch, and covered with purulent mucus; it looked like some polypoid growth. Under chloroform, a scoop was introduced, and a full sized bean was brought away, of the kind called, I believe "scarlet runner". The mother at once recollected that the child had, early in the previous October, picked up some of these in the garden. The bean had thus, in all probability, remained in the nose six months under all the conditions most favourable to germination; it had, in fact, both germinated and grown, having thrown out a plumule, a state of affairs fully accounting for the recent aggravation in the symptoms.

OBSTETRIC MEMORANDA.

[UNDER this head, we shall, from time to time, as materials come to hand from correspondents, publish records of cases remarkable in themselves, or illustrating points of interest in obstetric practice, therapeutic or manipulative. We shall probably in this way preserve from oblivion the notes of very many useful and instructive occurrences in private practice; for the great obstetric experience is that—for the most part hitherto unwritten—of the great body of general practitioners throughout Great Britain. We will only ask those who may forward cases for record, to relate them with the utmost brevity, and equally to condense any appended remarks.]

CAUSES OF DEATH IN FOOT- AND BREECH-PRESENTATION.

By T. O'CONNOR, Esq., March, Cambridgeshire.

I READ a paper on the Ergot of Rye in Midwifery before the Cambridge and Huntingdon Branch of our Association on June 27th, 1865, which was published in the *JOURNAL* on Dec. 16th following. In that paper I gave the result of turning, with rapid delivery, in four cases of placenta prævia, in which I was fortunate enough to preserve the lives of the children as well as the mothers. I also advanced the opinion that, by adopting this manœuvre in foot- and breech-presentations, we might save the life of the child; whereas, it must be admitted that in a large percentage of those presentations the children are born dead, the prevailing opinion being that death results always from pressure on the umbilical cord. Let me say here that I have always had great doubt about the soundness of this doctrine, as it appeared to me there was little difficulty in protecting the cord from injurious pressure during the pains, by simply placing a finger on each side of it at the point where it suffered the pressure. I am now convinced that the death of the child in foot- and breech-presentations is due in some, I believe in a large majority of cases, either to the negligence of the practitioner in not giving to the cord that protection which is certainly in his power—a negligence which, I hope and believe, rarely occurs, and therefore pressure on the cord has to bear only a small share of the sin—or to the great extractive force required to bring down the head, where there is any considerable disproportion between it and the passage through which it has to emerge. To this latter cause we are to look, therefore, principally for danger to the life of the child.

The following three cases, which occurred in my practice in the latter part of 1866 and beginning of this year, will throw some light on the question. Mrs. H. had been in labour four hours in her second labour. It was a foot-presentation; the pains were feeble. I gave cold brandy and water. In five hours, the whole of the body except the

head was born. There was pulsation in the cord with the last pain, during which the head was extracted. Great force was used; the child gasped several times and in about five minutes ceased to live, notwithstanding all the aid I could afford by simulating respiration; and I prevailed on the friends to allow a *post mortem* examination—a difficulty not easily overcome. I found the odontoid process of the axis thrown back on the spinal cord; the rudimentary transverse ligament was, of course, ruptured. Here, then, was the cause of death. This accident was caused by the force required to extract the head.

It would be a waste of space to give the details of the other two cases, which were breech-presentations, the progress of the labours more or less slow, and the children expiring after a few minutes' gasping respiration. The autopsy revealed the same cause of death; viz., pressure on the upper part of the spinal cord by the odontoid process of the axis.

We have, fortunately, an unfailing remedy against this disastrous fatality to infants, in the long forceps, which may be readily applied to the head after the expulsion of the body; thus substituting the instruments as a lever for the body of the child, and relieving the spine, in this mechanism, from the distension it must always suffer from the tugging force hitherto employed to extract the head. I have used these instruments now in a dozen cases with the happy result of saving the lives of the children. There is no difficulty in applying the long forceps. The short forceps (I use Denman's) has failed in my hands in this operation.

PUERPERAL CONVULSIONS: MERCURIAL TREATMENT.

By EDWARD GARRAWAY, Esq., Faversham.

MRS. N. was seized with puerperal convulsions at the end of the eighth month. There had been pain in the head and vomiting for some days previously. There was much anasarca, and the urine was densely coagulable. She was treated heroically (save bleeding), by blisters, mustard, turpentine enemata, and five-grain doses of calomel every two hours. The object, however, is not to detail minutely the treatment. The patient recovered. The mouth was sore.

The following year, Mrs. N. came to me again just before the completion of the eighth month complaining of stupor. The legs, hands, and face, were anasarcaous; the urine became almost solid by heat. Small doses of calomel were given night and morning. In a week, the mouth became sore; the condition of the urine was unaltered. Labour now supervened, and I prepared the friends for a fatal issue. Notwithstanding that the placenta presented and turning was necessitated, there was not the slightest tendency to convulsion, and the recovery was good. Whether the absence of convulsion this time was a *post hoc* or *propter hoc*, quoad mercurial action, I know not. This I know, that some years ago, when mercury was given as it is not now, we had a run of scarlatina cases, followed by anasarca and culminating in convulsion. My late esteemed partner, an article of whose creed was that "nobody need ever die if they would only submit to a sore mouth", treated these cases accordingly. A very little went a great way, as we know how susceptible the subjects of renal disorder are to mercurial action; and pretty bad mouths we had. But the fact stood; that, whereas the cases of anasarca which were treated by me non-mercurially almost all went on to convulsions, in not a single instance did this complication occur where, as soon as anasarca and albumen were detected, calomel was given and its specific effect obtained.

CONGENITAL MALFORMATIONS.

By W. T. GREENE, M.B., Physician to the Dispensary, Moira, Co. Down.

THAT some cases of congenital malformation may be due to maternal impressions during pregnancy, I am not prepared to deny; but that others, and perhaps the greater part, are hereditary, could, I think, be proved. I shall adduce one instance in support of this opinion, out of several that have come under my own observation.

In August last, I was summoned to attend Mrs. T., a healthy young woman, in labour with her fourth child. She was delivered in a few minutes after my arrival, when the infant, a fine boy, was found to be minus the right hand, which presented the appearance of having been amputated at the wrist, where three small papules, representing the fingers, were loosely attached to the skin.

This woman had been conscious of no impressions whatever during her pregnancy; had not thought about what the child she was carrying would be like, or supposed that it would, in any respect, differ from the others, and yet she might have been excused for entertaining morbid ideas, seeing that one of her husband's brothers is greatly deformed, having been born without legs or feet, or, at least, with the barest rudiments of these members, while his hands are cleft down to the