divided into two doses, were prescribed to be taken, with an interval of five or six hours, in addition to the mixture.

Sept. 16th. He had about an hour's sleep altogether. He dozed a little, and started up in a fright. He was slightly more composed. An aperient was given, and the morphia was increased to a grain and a half at each dose, with a grain of tartar emetic twice in the day.

Sept. 17th. He had not much sleep, but was more rational, the lucid intervals being more pronounced. He could now lie quite still. The bowels were well open. He was ordered to take two grains of morphia and three-fourths of a grain of antimony twice in the day.

Sept. 18th. He had four hours sleep. The pills were repeated.

Sept. 20th. The pills procured a *fair night's sleep* on the 18th. Last night he did not sleep so well. The narcotic seemed less active. He was ordered to take half a grain of morphia and five grains of extract of hyoscyamus every night.

From this date he improved rapidly, sat up on the 21st, and on the 26th he went to business as usual. In eight days he took twenty grains of hydrochlorate of morphia and four grains of opium, besides some doses of hyoscyamus. Some time ago, he had had no subsequent attack.

I might give other cases in illustration of the principle; but the results in these two are so striking, evident, and conclusive, that it would not be either necessary or desirable to the object in view.

ON THE GENERATION OF NERVE-FORCE.

By RICHARD HUGHES, M.R.C.S., and L.R.C.P.Ed.(Exam.), Surgeon to the Brighton Orthopædic Hospital.

THERE is a tendency in much of modern science to revive upon sound scientific data the guesses of earlier speculators. The atomic theory in physics and chemistry, the reflex doctrine in nervous physiology, will supply examples of my meaning. It will not then be urged, as an objection to the theory I am about to advance, that it may be found adumbrated in the works of Harvey, Willis, and others.

It is generally admitted that neurine, or nervous matter, performs its peculiar functions in the organism by being the seat of a property which, from its analogy with the so-called "forces" of inorganic nature—light, heat, electricity, etc.—is called the nervous force. Farther, that, unlike the contractile property of muscle, which resides in every fibre as such, the gray or vesicular neurine is the generator, the white or tubular neurine the transmitter of this force. These doctrines are so generally accepted, that I may start with them as assumptions.

The common theory of the mode whereby this nerve-force is thus generated and transmitted in the motor process seems to be as follows:—Upon the accession of a stimulus, from within or from without, on the nervous centre, an evolution of force from the gray matter there takes place, which force is then transmitted through the tubular matter of the nerves to the muscles appropriate to the action desired. I would, on the contrary, propose the following hypothesis:—

That the nervous force is being continuously and uninterruptedly generated or secreted from the blood by the cells of the gray matter, and as constantly transmitted therefrom through the nerves; and that any stimulus—whether this be volitional, emotional or reflex, galvanical or mechanical—produces contraction, not by causing evolution of nerve-force altogether *de novo*, but by increasing the intensity of the generation or transmission already going on.

There are three points in this hypothesis which require proof. (*a*) That the nervous force is generated by the gray matter *from the blood;* (*b*) that its generation is *continuous, not interrupted.* If these be proved, another proposition—(*c*) that motor stimuli act, not by producing nerve-force altogether *de novo*, but by intensifying processes already in transaction, will be manifest without farther argument.

a. 1. In the first place, let us consider one of the main grounds whereon we believe the grey matter to be the generator, the the white matter the transmitter, of nerve-force. It is that the grey matter presents precisely the structure which in every gland of the body is characteristic of its secreting portion, viz., a collection of nucleated cells; while the *tubular* form of the white matter presents a no less perfect analogy to the ducts whereby the secretions are transmitted to their appropriate destinations (Solly, *On the Brain*, p. 1-17). But in every gland there is, besides secreting cells and transmitting tubes, a third constituent, viz., the fluid from which the particular secretion is formed. Is the analogy here to fail? Are the nucleated cells of the nervous system to be regarded as generating their peculiar secretion from nothing, or at best from themselves? Shall we not rather say that the blood—the source of everything else in the body, even of the future progeny*—is to this, as to other glands, the material on which its secernent function works?

2. What are the grounds on which, whatever may be our view of the peculiar function which the spleen plays in the organism, we are at least certain that it has some office to perform in reference to the blood? Simply that there is conveyed to it a far larger quantity of blood than it requires for its nutrition. We argue that it must do something, though we may not know what, with this excess of supply. Now the amount of blood conveyed to the brain is enormously beyond that required for its mere nutrition, being not less than one-fifth of the total amount contained in the body; while the weight of the brain in proportion to that of the body is but one-thirty-fifth. And of this blood by far the greater quantity goes to the grey matter, which nevertheless itself exists in the brain in a smaller proportion than the white matter. When, added to this, we have, what in the spleen we have not, a system of nucleated cells undoubtedly generating nerve-force, and a system of tubular nerves undoubtedly transmitting it to its destination, can we doubt that this force is secreted from the largely-superabundant supply of blood?

3. The state of unconsciousness (syncope) which almost immediately follows on the diminution to any great extent of the supply of blood to the brain, is explicable upon no hypothesis so well as upon that which supposes that from the blood that force is continuously generated which is the mind's instrument, and without which it has no consciousness of anything external to itself. For then any interruption, however temporary, to the supply of blood would be shortly followed by unconsciousness, which could not be the case for a much longer time were the loss of nutrition the only result.

In thus speaking of nerve-force as a secretion from the blood, I am by no means committing myself to any theory as to the mode of its production, or as to its nature as a material though imponderable agent. Its analogue in the inorganic world seems to be electricity; and it is by no means necessary to suppose this a material fluid, although we speak of it as generated (or set free) by chemical decomposition and other changes.

b. My second proposition is that this generation of nerve-force is continuous, not interrupted. In support of this I may adduce the following fact:—If a motor nerve be divided transversely, in a living animal, and the distal portion be galvanically or mechanically irritated, vigorous contractions are produced in the muscles supplied by that nerve. The same result follows on irritation for some time after the section, but in a gradually decreasing intensity, until it altogether ceases to be manifested upon any amount of stimulus. Now this change is not to be explained by supposing any loss of vitality on the part of the distal portion of the nerve; for its supply of blood is in no way cut off by the section, and, upon re-union of the divided ends taking place, its function is completely restored; the only altered circumstance is that its connection with the grey matter of the nervous centres has been interrupted. Its loss of excitability simply from this cause would be perfectly intelligible upon the old hypothesis; but not so the presence of excitability immediately after the section, and its gradual decrease and extinction afterwards. The hypothesis I propose will perfectly explain all the phenomena. Nerve-force is being continually generated in the nervous centres, and transmitted through the nerves, so that the latter are kept constantly charged as it were with it; and an external stimulus applied at any part of their course will excite them to produce muscular contraction. Being constantly charged with nerve-force, a separation from the nervous centre at first induces no change in their excitability; but, their connection with the fountain being cut off, their supply of force is gradually exhausted, till at length they become inert cords of nervous matter, insensible to the strongest stimuli.

The preceding explanation of this phenomenon is confirmed by observing the mode in which this exhaustion of excitability takes place, viz., that it does not affect the whole

* For the testis and the ovary can hardly be regarded in any other light than as two glands, secreting from the blood (of an homologous artery, spermatic or ovarian), the one the sperm-cell (spermatozoon), the other the germ-cell (ovum), whose union constitutes the future progeny.

nerve simultaneously, but the latter dies (as a nerve) from centre to circumference. The distal end will retain its excitability for some time after the proximal portion has ceased to respond to stimuli. This is explained by the fact that the continuous nerve-current *sets*, if we may so express it, peripherally, *i.e.*, from centre to circumference. Consequently, upon section of a nerve, the current of force in its distal end still flowing peripherally, and no fresh supply from the fountain coming to reinforce it from behind, the parts farthest from the circumference are soon left bare by the receding tide, and are therefore the first to lose their excitability to stimuli.

It may be objected to this view of the continuous generation and transmission of nerve-force, that a continuous contraction of the muscles ought to be the result, and that the *rationale* of occasional contractions will become almost impossible. To this I would reply by adducing the very thing required, viz., a constant condition of passive contraction in the muscles. For what else is that which is called their *tone*—a state best understood by contrasting it with the laxity of the muscles of a paralysed limb, and whose result is seen in the deformity (so familiar to those, like myself, engaged in the practice of Orthopædic surgery) produced by muscles whose opponents have lost their counterbalancing power. A beautiful instance of the same thing is the contraction of the pupil from the unbalanced influence of the third nerve after section of the sympathetic in the neck. Here is the best of *rationales*; for the continuous *passive* transmission corresponds to a continuous *passive* contraction, while occasional *active* transmissions upon a stimulus being applied, account (as on the old hypothesis) for occasional active contractions.

Further, we have in the phenomena produced by the galvanic current an analogy to this state of things, which, considering how precisely the electrical force in inorganic is the representative of the nervous force in organic nature, is almost an argument in proof of it, and may at least serve as an answer to objections against it—the true value of analogies. The continued galvanic current, when applied to a nerve, produces no perceptible contraction, save at its first establishment, at any sudden increase in its intensity during its continuance, and at its interruption. The second of these phenomena seems to me to represent the ordinary process of active muscular contraction, the stimulus, whether volitional, emotional, or reflex, on the grey matter causing a sudden increase in the intensity of its evolution of nerve-force from the blood, and thence of its transmission of the same through the nerves to the muscles. The contraction which results from a galvanical or mechanical stimulation of the trunk of a nerve itself is explicable on the similar *rationale* of an increase in the intensity of its transmission of force. The contraction produced at the making and at the breaking of the galvanic circuit, may find corresponding facts respectively in the general convulsion which accompanies the return of suspended animation, and that which is so frequently the precursor of dissolution.

The value of a theory is that of a key, that it shall unlock doors which shut us out from hitherto unexplored chambers of knowledge. And its test is the same,—that it shall accurately fit all the wards of the lock, which here are the facts of the case. The deductive part of the process of discovery I must reserve for a future occasion, contenting myself here with having gathered, by induction from a few carefully selected facts, a theory which seems to bind them well together, and to commend itself to physical and physiological analogy.

CURIOUS MALFORMATION OF THE CHEST. At the meeting of the Medico-Chirurgical Society of Edinburgh on May 2nd, Dr. Alexander Simpson introduced Dr. Wojaczek, who is the subject of a peculiar malformation of the chest. Dr. Wojaczek is aged 23, a native of Oslavau in Moravia. In front, the chest presents in the middle, at the lower part, a depression about three inches deep, large enough to lodge the head of a child. The hollow is formed by the inflexion of the sternum downwards and backwards towards the spinal column, which it apparently approaches to within about an inch and a half. There is no deficiency of bone or cartilage, but the cartilages of the ribs are bent back so as to join the depressed sternum and form the sides of the hollow; the skin and soft parts present nothing unusual. Respiration is carried on almost entirely by the diaphragm and false ribs. The malformation appears to have been congenital, and was first noticed by Professor Skoda and Rokitansky, in attending M. Wojaczek during an illness. Casts of the malformation have been placed in the museums of the University and of the Royal College of Surgeons of Edinburgh. (*Edinburgh Medical Journal*, June 1860.)

Reviews and Notices.

A MANUAL OF OPERATIVE SURGERY ON THE DEAD BODY. By THOMAS SMITH, F.R.C.S., Demonstrator of Anatomy and Operative Surgery at St. Bartholomew's Hospital; Surgeon to the Great Northern Hospital. With illustrations. Pp. 139. London: Longmans. 1859.

OPERATIVE SURGERY, ADAPTED TO THE LIVING AND DEAD SUBJECT. By C. F. MAUNDER, F.R.C.S., Demonstrator of Anatomy in Guy's Hospital, and Junior Surgeon to the Great Northern Hospital. Part I. Pp. 144. London: Churchill. 1860.

THERE come before us on the present occasion two authors, engaged in an honourable contest for favour at the hands of the profession, and especially of those who are preparing their hands for skill in the performance of such surgical duties as shall devolve on them in the course of their practice. But, before looking into the books of Mr. SMITH and Mr. MAUNDER, we would beg permission, as a matter of warning to those who may happen not to know or not to think better, to assure them that not all the books in the world, even if written so plainly that all who run may read, and illustrated in the most excellent style, can properly treat operative surgery. They are valuable aids; but the art must be learned, and the manual dexterity acquired, on the subject itself.

The works before us are both written by gentlemen holding important educational positions in two of our greatest medical institutions; and, as to deciding between their merits as surgeons or as teachers in the anatomical theatre, there is nothing to justify us in entering on the task. With regard to the books before us, however, we only express the opinion we have formed of them in considering which of the two we should prefer as a guide if we were about to take a course of operative surgery.

Mr. Smith's book consists of nine chapters: viz.—I. Introductory; II. Some Operations in Minor Surgery; III. Tenotomy; IV. Ligature of Arteries; V. Median Operations; VI. Amputation of the Upper Extremity and Removal of the Breast; VII. Amputations of the Lower Extremity; VIII. Resection of Entire Bones and Joints; IX. The Use of Certain Instruments. The work is illustrated by thirty-two woodcuts.

Mr. Maunder has brought out as yet only the first part of his book. This part contains seven chapters: the first two comprising an account of the Compress, Bandage, Ligature, and some other preliminary important matters; the third, Secondary Operations on the Vascular System; the fourth, Operations on the Arteries; the fifth, the Ligature of Special Arteries; the sixth, Operations on the Bones; and the seventh, Operations on the Surface of the Body. The woodcuts illustrating the work are sixty-six in number.

We might almost say—

"Non nostrum tantas componere lites";

both books are so good; yet we are inclined to think, if a preference has to be made, that Mr. Maunder's work is most likely to be useful to the incipient operator. The instruction which he gives in the early chapters regarding things small in appearance but great in importance, is by no means an unnecessary or a misplaced task; and, in comparing the descriptions given by the two authors of various operations, but especially on the ligature of special arteries and the resection of individual bones, that the carefully detailed instructions of Mr. Maunder are more likely to lead to a right understanding of what is meant than the more brief descriptions given by Mr. Smith.

We anticipate that, when Mr. Maunder's work is completed, it will deservedly be a favourite with the student who is endeavouring to acquire operative skill on the dead body, as well as with the practitioner who, while amputations, resections, and the like, are not things of daily occurrence in his profes-