SPINAL ANÆSTHESIA.

THE visit of Prof. Thomas Jonnesco, of the University of Bucharest, to the Seamen's Hospital, Greenwich, has brought prominently before the public the method of producing local anæsthesia by the injection of anæsthetic solutions into the spinal canal.

Cocaine was introduced years ago as an anæsthetic fo local application; it was welcomed by the medical profession, and equally by patients, on account of its invaluable services in operative procedures upon the eye, the nose and the throat. By merely placing a drop or two of a solution of cocaine (or one of its salts) into the eye, or by painting a similar solution upon the mucous membrane of the nose or throat, it is possible to produce anæsthesia so complete as to enable surgical operations to be performed upon these parts without inflicting the least pain or discomfort upon the patients. Certain objections to the use of cocaine were not long, however, in showing themselves. Cocaine is a powerful alkaloid; and if the usual dose be exceeded, very grave results follow, a number of patients having actually died as the result of cocaine poisoning. Investigators were therefore led to search for other substances, either like cocaine occurring naturally, or prepared synthetically, which would possess the properties of cocaine while being less poisonous.

In this way a number of anæsthetic drugs has been introduced, including alypin, holocaine, eucaine (alpha and beta), scopolamine, novocaine, stovaine and tropacocaine. Of these the three latter have been chiefly employed in producing spinal anæsthesia. The method consists in injecting, by means of a syringe and needle, a quantity (usually about 1 c.c.) of a solution of one of these substances into the spinal canal. The injection is made in the back, close to the middle line, the needle being inserted between two of the vertebræ. With regard to the details of the method, various procedures have been described, and no agreement has yet been reached as to which of these is to be considered the best. There is no doubt that modifications are desirable to suit particular requirements. Thus, many operators direct that the drug be dissolved in cerebro-spinal fluid or else in a saline solution having the same specific gravity and the same osmotic tension as the blood-serum. Others consider that the anæsthetic solution should be considerably denser or more viscous than the cerebro-spinal fluid, and for this purpose recommend the addition of glucose or of gum-acacia to the solution. These thicker solutions tend to remain at the spot at which they are injected, while solutions in cerebro-spinal fluid or in normal saline tend to spread up and down the spinal canal, and thus have a more widespread anæsthetic effect. It is usual to withdraw a few c.cm. of cerebro-spinal fluid from the spinal canal before injecting the anæsthetic fluid. There are two reasons for this—first, the surgeon is assured that he has actually introduced his needle into the spinal canal, and secondly he is certain to avoid increasing unduly the cerebro-spinal pressure when he introduces the anæsthetising fluid.

On introducing the fluid into a particular part of the spinal column, anæsthesia is produced of all parts of the body deriving their nerve supply from this part of the spinal cord, and all parts below. If the fluid be allowed to ascend the spinal canal (*e.g.* by raising the hips) the anæsthesia rises higher and higher as the anæsthetic fluid reaches the trunks of the nerves arising from the higher parts of the spinal cord. If the patient be placed on one side while the injection is being performed, the anæsthetic fluid can be made to enter one lateral half of the spinal canal, and in this way it is possible to limit the anæsthesia to one lateral half of the body.

The anæsthetic fluid can be allowed to ascend almost to the top of the thoracic spine without fear of untoward consequences. When it reaches the base of the neck, however, the phrenic nerve, concerned with the movements of respiration, becomes involved, and it was deemed impracticable to produce anæsthesia of the head and neck by the spinal method. Prof. Jonnesco, however, has shown that the addition of strychnine to the anæsthetic solution produces so powerful a stimulant effect upon the respiratory centre in the brain that it is possible to introduce an anæsthetic fluid into the upper part of the thoracic spine, and to allow the fluid to ascend the spinal canal in the neck so as to enable operations to be performed upon the neck and throat. But it is as yet too early to say whether this method may be considered a safe one.

Of the three drugs which are now chiefly used for the production of spinal anæsthesia, stovaine is found to produce the most deleterious effect upon the kidneys, acute nephritis having followed its injection in quite a number of cases. Novocaine and tropacocaine are less injurious in this way, while they are equally efficacious as anæsthetics. It thus appears likely that they will supplant stovaine in the near future, and, in fact, tropacocaine in a one per cent. solution is already being largely used for the purpose in this country, the usual dose injected being about $1\frac{1}{4}$ grain.

No doubt further experience will lead to modifications in the present method of performing spinal anæsthesia which will result in its widespread use, as there are a great many cases in which a local anæsthetic is far more advantageous to both patient and surgeon than a general anæsthetic.

A. C. J.

THE CAUSES OF THE GERMINATIVE PROCESSES OF SEEDS.

ONE of the most remarkable phenomena of vegetable life is the occurrence in its cycle of a resting period of varying duration, a period during which the vital functions seem entirely suspended or dormant, and the condition of the organism is hardly distinguishable from death. This stage is most common in connection with the reproductive processes, and can be seen to belong to the constitution of both spores and seeds. The more highly differentiated the structure which shows it, the more prolonged, apparently, can be this resting period, but sooner or later it gives place to the resumption of growth and vital activities.

The interpretation of the occurrence of this phase is rather a matter of inference than proof; probably it was originally concerned in the protection of the reproductive structure from adverse conditions of the environment, for not only is the life rendered dormant, but the resting organ is for the most part protected by modification of its tegumentary covering. In this condition it is able also to bear the severance of its organic connection with its parent, and to subserve the purposes of dispersal. It may, indeed, have arisen with special reference to the latter process alone.

The resumption of the growth and development of the reproductive body after the period of rest may be explained in a similar manner by the reversal of the adverse conditions, these being for the most part secured when its dispersal has been effected.

These superficial considerations are found, however, on reflection, to have others underlying them. Is the resting period of any advantage to the living substance of the reproductive structure, whether spore or seed? Does the cessation of the vital activity afford it any increased power of growth or vigour of constitution? Is it only a condition induced by circumstances, or does it speak of a rhythmic tendency inherent in the plant?

More interesting still—Is the resumption of life which we call germination an evidence of the attainment of such increased vigour, or is it merely the resumption of ordinary chemical change when inhibiting conditions are removed? In the latter case, is the living substance concerned in setting up such chemical changes, or do these arise without such initiation?

If we study the germinative changes we find them associated universally with the existence and activity of enzymes. The resting germ, whether the structure be seed or spore, is surrounded by food material deposited for its ultimate nutrition, but needing enzymic action to render it suitable for actual assimilation. The number of such enzymes known to physiologists has increased most remarkably during the last decade, and though they have been found to be most plentiful in seeds, the study of the spore has shown that it is similarly equipped, though from its unicellular character the distribution of the enzymes is much simpler. At a certain moment the germ starts into life, simultaneously the enzymes are found at work, and nutritive pabulum is presented to it in assimilable form. Which is cause and which effect? Does the living substance, awaking from a sleep, start the enzymic activity, or do the enzymes originate the change? Is the activity of the living substance itself due to enzyme action? In other words, is life a question of the existence and activity of enzymes?

These subjects can be studied more advantageously in a seed than in a spore on account of the physiological as well as anatomical differentiation which it presents. It is easy to distinguish the germ which, after resumption of growth, becomes the new plant, and to separate it from the stores of food which are laid up for its nutrition, and which will be the sphere of activity of the digestive enzymes. These stores may be within it or lying around it, but they in any case are well away from its actual growing points. In such a seed, then, we can distinguish the germ, or embryo, the new plant, and the remains of the parent which has given it origin, this being sometimes large sometimes small, in proportion to the former.

At the outset we may ask, what is the actual condition of these two parts? How far can we find evidence of life in either during the period that elapses between the severance from the parent plant and the resumption of growth and activity after the resting stage? If we study the phenomena of life in the seed as a whole, we are led to apply to it the test of the existence and maintenance of respiration, this being the inseparable accompaniment of metabolic change and hence a constant feature of life in the various conditions with which we are acquainted. If we rely on either the absorption of oxygen or the exhalation of carbon dioxide, however, we shall be obliged to deny the presence of life in the seed at all. Many and careful examinations have been made of the respiratory processes in seeds of many descriptions. Perhaps the most exhaustive of them were those of Romanes in 1893. Seeds of various plants were kept in glass tubes which had been exhausted so completely that they contained only one-millionth of an atmosphere, and were left for upwards of a year. This treatment did not hinder their subsequent germination. Some of them were afterwards immersed in various inert gases, such as hydrogen and nitrogen, others placed in carbon monoxide, sulphuretted hydrogen, vapours of ether and chloroform, and kept thus for twelve months, still without any deleterious effects. It seems hard to suppose them living in the usual acceptation of the term. In 1892 Jodin imprisoned some seeds in ordinary air in hermetically sealed flasks; he kept them so for four years, and examination of the air at the conclusion of this term showed its composition absolutely unchanged, no exhalation of carbon dioxide having taken place. Respiration, as ordinarily understood, thus seems to be quite in abeyance.

The suspension, if not disappearance, of life during this resting period is emphasised by the behaviour of the seeds under exposure to extremes of temperature. Observations made by Wartmann so long ago as 1860 showed that germination was not prevented by preliminary exposure of the seeds to temperatures ranging from -40° to -78° C. This does not seem, however, to prove the point, for the normal temperature of agricultural land in Siberia in winter is almost as low, often reaching -60° C. Experiments were made by de Candolle and Pictet in 1879 which carried the range of temperature a little further, but the most drastic treatment was rendered possible by the liquefaction of air. Experiments with the aid of this powerful reagent have been made in France by Pictet, and in England by Brown and Escombe and by Sir W. Thiselton-Dyer, with the result that such extreme cold had very little effect in inhibiting the power of subsequent germination.

At the other end of the thermometric scale strange results have been found, many seeds having been proved capable of germinating after being exposed for a short time to temperatures higher than the boiling point of water. So long ago as 1877, Just heated seeds of a species of Trifolium to 120° C. without injuring their power of development. Some years later a more extended series of experiments was carried out by Dixon on seeds selected from several natural orders, all of which withstood, without injury, a temperature of about 105° C. prolonged for several hours. Their vitality and power of development were, however, much more easily affected by heat than by cold.

This resistance to great extremes of temperature has been found to be correlated in a considerable degree with the state of desiccation which was characteristic of the seeds. Hence is lent some support to the view of the dependence of germination on enzymic action, for the latter can only be exercised in the presence of water. Some experiments carried out by Acton in 1893 seem to show that even the small amount of water in the wheat grain enables a certain amount of digestive change to take place in both the proteins and the carbohydrates of the grain. In the absence of all moisture the enzymes remain quiescent.

As it is generally accepted that chemical action cannot take place at temperatures so low as those specified, and as chemical change or metabolic activity is an inevitable accompaniment of life as defined by Herbert Spencer, the idea that germination is dependent upon the continued and permanent life of the protoplasm in the resting seed, there have been many efforts made to explain these anomalous manifestations. C. de Candolle concluded that after a certain time the protoplasm of the ripe seed passes into a state of complete inertness, in which it is incapable of either respiration or assimilation, and that while in this condition it can support, without detriment to its subsequent revival, rapid and considerable lowering of temperature. Indeed, the access of cold to a seed seems to be only injurious as it can bring about the freezing of the water remaining in it, with the subsequent thawing as the temperature rises again. This freezing once brought about, further and more intense cold has no effect. Brown and Escombe endorsed de Candolle's idea, suggesting that protoplasm may exist in two conditions, the *static* and the *kinetic* in the former becoming so stable as to be absolutely inert, devoid of any trace of metabolic activity, and yet conserving the potentiality of life.

It is extremely difficult to decide which of the two theories provides the most satisfactory explanation of the observed phenomena. The conditions which mark the commencement of germination help us, however, to come to a conclusion, though difficulties are met with in either hypothesis. For germination to occur, moisture must be absorbed by the seed; absorption of oxygen and exhalation of carbon dioxide speedily follow, enzymic action supervenes, and the digestive changes in the reserve food materials can be readily traced. But what is the first result of the absorption of water is not so clear; is it the resumption of the kinetic condition by the protoplasm, the life of which in all other parts can be seen to be dependent on water, or is it the setting up of the activity of the enzymes, which enables metabolic, and possibly respiratory, changes to take place, such chemical action stimulating the latent life to manifestation?

Certain observations tend to show that the activity commences with changes in the embryo or germ. Van Tieghem, many years ago, endeavoured to excite into activity the endosperm of the castor-oil bean after removing from it every particle of the embryo. In most cases he failed, but in some he claimed to have been successful. The writer, many years afterwards, repeated his experiments, and found that the endosperm could only be quickened when a small portion of the germ was left in contact with it. The changes in this case originated in the embryo. Further observations showed that the earliest sign of germination in the latter is a change in certain cells of its epidermis, which take on the appearances that usually indicate the conversion of a zymogen into an enzyme. The germ appears to start the change by the secretion of an enzyme. It seems justifiable to associate this secretion with the re-assumption of life by the embryo, because, though many enzymes occur in the seed outside the latter, they do not initiate their changes until later. In this particular seed, vital activity is subsequently soon manifested in the tissue of the endosperm, which becomes the scene of very active chemical change, its residual protoplasm growing and secreting certain constituents, particularly sugars, which the resting cells do not contain.

Brown and Morris showed that a somewhat similar procedure can be observed in the barley grain. The first visible changes are the secretion of enzymes by the scutellum of the germ. The germination once started, other enzymes make their appearance in the endosperm, some arising especially from the aleurone layer underlying the testa.

A scrutiny of the results of Dixon's experiments on heating the resting seeds points also to the protoplasm as the initiator of the changes. Exposure of his seeds to 105° C. must have destroyed any preformed enzymes unless the cells were absolutely devoid of water, a condition hardly likely to be reached. The germinative power fell gradually, or nearly regularly, as the heating was raised to this point, but much remained. When, however, a very slightly higher temperature was reached, about 107° C., the seeds lost it with great suddenness and very irregularly. The injury inflicted by the last two degrees was very different from that which was sustained as the temperature gradually rose to 105° C., and was hardly explicable on the theory of enzyme destruction. It did not, at any rate, correspond to the progress of their destruction in the laboratory.

Some experiments recently carried out by Miss White in Prof. Ewart's laboratory at Melbourne bear upon this aspect of the problem. She endeavoured to accelerate the germinative processes in seeds which had but little power of germination by supplying them with additional quantities of enzymes dissolved in the water with which they were kept moist, the coats of the seeds being perforated here and there to allow absorption to take place. Though she examined many in various conditions, the result was always negative. It proved impossible to accelerate germination by supplying additional quantities of enzyme.

Experiments made by supplying resting seeds with reagents such as dilute organic acids, which stimulate their secretion of enzymes, also have been found to be without result.

The idea that enzymes initiate and maintain the process of germination appears, therefore, to be erroneous, and the older view of the sufficiency of the idioplasm of the cells still holds the field, in spite of the difficulties that have been raised by the experiments with temperature. The theory of static and kinetic states of protoplasm explains little or nothing; it is really scarcely more than a statement of the problem in new terms.

J. REYNOLDS GREEN.

DR. W. J. RUSSELL, F.R S.

WILLIAM JAMES RUSSELL was born in May, 1830, at Gloucester, where his father was a banker. He was educated at private schools—Dr. Wreford's at Bristol, and afterwards at Mr. Bache's at Birmingham. In passing, it may be noted that this was before the educational revival that produced and was furthered by the Public Schools Commission of 1859, and that in those days there were very many private schools where scholarship was carried to quite as high a level, and when the conditions of out-of-school life were in some respects much better than in most of the public schools of the time.

After leaving school in 1847, Russell entered University College, London, where he studied chemistry under Thomas Graham and Williamson. In 1851 he was appointed the first demonstrator of chemistry under Frankland in the then newly-founded Owen's College, and helped to plan and superintend the building of the first chemical laboratory of the college. This laboratory, built on what had been the garden attached to the original college building (Mr. Cobden's old house in Quay Street), was the cradle of the great Manchester School of Chemistry, which has become as famous in its way as the Manchester School of Politics. After two years at Owens College, Russell went to Heidelberg, where he worked under Bunsen from the autumn of 1853 to the end of the session 1854–5. During his stay at Heidelberg, he graduated as Ph.D. After his return to England, he lectured at the Midland Institute, Birmingham, and near the end of 1857 came again to London to act as assistant to Williamson, his former teacher, at University College. He was associated with Williamson for several years, a considerable part of the time being occupied with working out and bringing to a convenient practical form a method of gas-analysis whereby the corrections involved in taking account of variation of pressure and temperature were in great measure eliminated. The results of this investigation were embodied in several papers published in the Journal of the Chemical Society and elsewhere, and the form of apparatus finally arrived at was the forerunner of the most improved modern types of gas-analysis apparatus and