PANGENESIS.—MR. DARWIN'S REPLY TO PROFESSOR DELPINO.[1]

I COULD say something in answer to most of Delpino's ingenious criticisms, but I will confine myself to one point; remarking, however, that I cannot see the force of his objection to the belief that the gemmules have the power of self-division. The analogy of the multiplication within the body of an infected person of the contagious atoms of small-pox or scarlet-fever still seems to me fair. The criticism which at first struck me as much the most forcible, relates to the re-growth of an amputated limb or other organ. I have assumed, from what we know of the ordinary laws of growth, that gemmules become developed only when united with the nascent or very young cells which precede them in the due order of successive development. Now, Delpino remarks that when the limb of a mature salamander is cut off, nascent cells would not exist on the stump, but only mature ones; consequently, as he urges, the gemmules, according to the hypothesis, could not be there developed and form a new limb. But have we any reason for assuming that with animals the same cells or organic units endure throughout life and are not replaced by new cells? All physiologists admit that the whole frame is being constantly renovated. The bones, which it might be thought would be least capable of renewal, undergo before maturity great changes in form and size and in the diameter of the medullary canal; even after maturity their shape does not remain the same. If, then, it be admitted that the tissues throughout the body are constantly being renovated by means of the old cells giving birth through proliferation or division to new cells, the difficulty of the gemmules being developed at any point at which a limb may be cut off, disappears; for some nascent cells would exist there; and we must bear in mind that gemmules may possibly become developed in union with cells which are not strictly the proper ones, as shown in the extreme case of nails occasionally growing on the stumps of the amputated fingers of man. It is probable that the renovation of the tissues by the formation of new cells becomes less and less in old age, and so does the power of re-growth. The curious fact that insects which live but a short time after their final metamorphosis, have singularly little power of re-growth, may be partly explained by the deficiency in their tissues of nascent cells.

In the case of plants, the cells composing the tissues endure for life, and are not renewed. The woody cells in the trunk of a tree originally existed as delicate cells in the young shoot. Now, it is remarkable, considering how low plants stand in the scale of life, what little power of re-growth most of their organs or parts possess, notwithstanding their high vitality. Cut off a young shoot, and the next bud below will form a new shoot; cut off an old stem, and hundreds of adventitious buds will be formed. No one, I believe, ever saw a mutilated leaf repaired in the same manner as the limb of a salamander; yet on the cut margins of certain leaves innumerable buds will be quickly developed. In the bark, on the other hand, new cells must be continually forming, for the outer layers are continually being thrown off; and bark can certainly reproduce itself on a decorticated surface (see Professor Oliver in *Transactions of Tyneside Club*, vol. iii. 1855, p. 67), from an isolated point, not connected with the surrounding uninjured bark; so that this case is fairly analogous with that of the re-growth of an amputated limb.

I will add only one other remark. Delpino insists that, according to the hypothesis, the same cells must throw off gemmules at many successive periods of their development. But I can see no such necessity. Certain epidermic cells, for instance, form horn; but their conversion into this substance is apparently due to the chemical nature of the contents of the cells; and of the changes consequent on the absorption of certain elements. An atom or gemmule thrown off at an early period from one of these cells would, when properly nourished, first form a cell like the parent cell, and ultimately be converted into horn, without the necessity of throwing off successive gemmules. As, however, a cell is a complex structure, with its investing membrane, nucleus, and nucleolus, a gemmule, as Mr. G. H. Lewes has remarked in his interesting discussion on this subject (*Fortnightly Review*, Nov. 1, 1868, p. 508), must, perhaps, be a compound one, so as to reproduce all the parts. The attack by Delpino on the weak point of the hypothesis, namely, that it requires the support of several subordinate hypotheses, is perfectly just, though perhaps pushed to an extreme. But I can speak from recent experience, that he who has to consider complex cases of inheritance, as limited either separately or conjointly by sex, age, and season, with the inherited characters themselves and the form of inheritance liable to change from crossing and variability, will be able to disentangle the phenomena much more clearly, if he admits for the time our hypothesis with all its imperfections. He will then have fixed in his mind that transmission and development are quite distinct phenomena,—the gemmules being thrown off from all parts of the organization, and transmitted from both parents to both sexes at the earliest age,—their development alone depending on the nature of the nascent tissues of the individual, whether permanently or temporarily modified by sex, age, season of the year, or other conditions.

CHARLES DARWIN.

Down, Beckenham, Kent.

[1] Professor Delpino's opinions, as translated from the *Rivista Contemporanea Nazionale Italiana*, having appeared very fully in our columns, we have requested Mr. Darwin to reply to them. To this Mr. Darwin has complied, and we believe that the natural history world will be interested in reading the following remarks.—ED. *S. O.*

ON THE PROBABLE SEAT OF VOLCANIC ACTION.

BY T. STERRY HUNT, LL.D., F.R.S.

IN TWO PARTS.—PART I.

THE igneous theory of the earth's crust, which supposes it to have been at one time a fused mass, and to still retain in its interior a great degree of heat, is now generally admitted. In order to explain the origin of eruptive rocks, the phenomena of volcanos, and the movements of the earth's crust, all of which are conceived by geologists to depend upon the internal heat of the earth, three principal hypotheses have been put forward. Of these, the first supposes that in the cooling of the globe a solid crust of no great thickness was formed, which rests upon the still uncongealed nucleus. The second hypothesis, maintained by Hopkins and by Poulett Scrope, supposes solidification to have commenced at the centre of the liquid globe, and to have advanced towards the circumference. Before the last portions became solidified, there was produced, it is conceived, a condition of imperfect liquidity, preventing the sinking of the cooled and heavier particles, and giving rise to a superficial crust, from which solidification would proceed downwards. There would thus be inclosed, between the inner and outer solid parts, a portion of uncongealed matter, which, according to Hopkins, may be supposed still to retain its liquid condition, and to be the seat of volcanic action, whether existing in isolated reservoirs or subterranean lakes; or whether, as suggested by Scrope, forming a continuous sheet surrounding the solid nucleus, whose existence is thus conciliated with the evident facts of a flexible crust, and of liquid ignited matters beneath.

Hopkins, in the discussion of this question, insisted upon the fact, established by his experiments, that pressure favours the solidification of matters which, like rocks, pass in melting to a less dense condition, and hence concludes that the pressure existing at great depths must have induced solidification of the molten mass at a temperature at which, under a less pressure, it would have remained liquid. Mr. Scrope has followed this up by the ingenious suggestion that the great pressure upon parts of the solid igneous mass may become relaxed from the effect of local movements of the earth's crust, causing portions of the solidified matter to pass immediately into the liquid state, thus giving rise to eruptive rocks in regions where all before was solid.[1]

Similar views have been put forward in a note by Rev. O. Fisher, and in an essay on the formation of mountain chains, by Mr. N. S. Shaler, in the *Proceedings* of the Boston Society of Natural History, both of which appear in the *Geological Magazine* for November last. As summed up by Mr. Shaler, the second hypothesis supposes that the earth "consists of an immense solid nucleus, a hardened outer crust, and an intermediate region of comparatively slight depth, in an imperfect state of igneous fusion." In this connection it is curious to remark that, as pointed out by Mr. J. Clifton Ward, in the same magazine for December (page 581), Halley was led, from the study of terrestrial magnetism, to a similar hypothesis. He supposed the existence of two magnetic poles situated in the earth's outer crust, and two others in an interior mass, separated from the solid envelope by a fluid medium, and revolving, by a very small degree, slower than the outer crust.[2] The same conclusion was subsequently adopted by Hansteen.

The formation of a solid layer at the surface of the viscid and nearly congealed mass of the cooling globe, as supposed by the advocates of the second hypothesis, is readily admissible. That this process should commence when the remaining envelope of liquid was yet so deep that the refrigeration from that time to the present has not been sufficient for its entire solidification, is, however, not so probable. Such a crust on the cooling superficial layer would, from the contraction consequent on the further refrigeration of the liquid stratum beneath, become more or less depressed and corrugated, so that there would probably result,

[1] See Scrope *On Volcanos*, and his communication to the *Geological Magazine* for Dec., 1868.

[2] The elevated temperature of the interior of the globe would probably offer no obstacle to the development of magnetism. In a recent experiment of M. Trève, communicated by M. Faye to the French Academy of Sciences, it was found that molten cast iron when poured into a mould, surrounded by a helix which was traversed by an electric current, became a strong magnet when liquid at a temperature of 1,300° C., and retained its magnetism while cooling (*Comptes Rendus de l'Acad. des Sciences*, Feb., 1869).