

## LETTERS TO THE EDITOR.

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**Synthesis by Means of Ferments.**

THE short article on the synthesis of glucosides by means of ferments in NATURE for November 6 (p. 304) contains the statement, "hitherto it has not been proved that enzymes have anything but an analytical action." "Prof. Bourquelot . . . has, however, obtained results which justify the conclusion that the decomposing action continues up to a certain point only, and that at this point synthetic action begins." Prof. Bourquelot's discovery is by no means new, because in 1898 Dr. Croft Hill, in a paper on reversible zymolysis in the Transactions of the Chemical Society for 1898 (vol. lxxiii., part 2, p. 634) not only showed that the products of fermentation arrested the action of the enzyme which caused it, but also that if these products reached a certain concentration, the enzyme instead of producing further hydrolysis began to reverse its action into a synthetic one, and built up instead of breaking down. These experiments were further extended and described in the Transactions of the Chemical Society for 1903 (vol. lxxxiii., part 1, p. 578), where he also gives an account of experiments made by other authors, and concludes (p. 597) with the words: "These observations, together with my own more recent results, make it increasingly more probable that the view I put forward in 1898 is a correct one, namely that all ferment actions are reversible." LAUDER BRUNTON.

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**Amœbocytes in Calcareous Sponges.**

I THINK there can be little doubt that the Amœbæ referred to by Mr. Orton in NATURE of November 27 are not independent organisms, but constituents of the sponge from which he obtained them. I have been working for some time past at the problem of the origin of the germ cells in the common *Grantia compressa*, and have often found the flagellate chambers of the sponge crowded with amœboid cells, which can sometimes be seen actually squeezing themselves through the layer of collared cells. According to my observations, these amœbocytes are immature germ cells—oogonia and spermatogonia—and they can often be seen undergoing mitosis in the chambers. A similar phenomena has been described in Sycon by Jörgensen. Possibly the amœboid cells squeezed out from the gastral cavity of Sycon by Mr. Orton were either of the same nature or else metamorphosed collared cells. The latter are very readily detached from their proper position in the sponge, and may then put out pseudopodia and come to resemble Amœbæ, as has long been known.

As it is likely to be some time before my results can be ready for publication, I may take this opportunity of mentioning that I find that in *Grantia compressa* the amœboid germ cells arise in the first instance from the metamorphosis of collared cells, and not, as is sometimes stated, from primitive amœbocytes, or archæocytes.

I spent a fortnight in April, 1912, at the Plymouth Laboratory in the investigation of these problems, and in collecting and preserving the material necessary for continuing the work. I have now an almost complete series of stages of the oogenesis, the most interesting feature of which is perhaps the feeding of the

growing ova by nurse cells, the latter being phagocytes which capture other cells and stuff them into the ova. I have also a number of stages of spermatogenesis. The sponge (*G. compressa*) is hermaphrodite, and sperm morulae are to be found (in April), enclosed in cover-cells, wedged in between the collared cells in the lining of the flagellate chambers. Haeckel described and figured the sperm morulae in this situation in various calcareous sponges so far back as 1872, but his results do not seem to have been generally accepted.

The character of the nucleus, to which Mr. Orton refers as a means of distinguishing his supposed Amœbæ from sponge cells, varies greatly according to circumstances, and cannot be regarded as conclusive.

ARTHUR DENDY.

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November 27.

**Intra-atomic Charge.**

THAT the intra-atomic charge of an element is determined by its place in the periodic table rather than by its atomic weight, as concluded by A. van der Broek (NATURE, November 27, p. 372), is strongly supported by the recent generalisation as to the radio-elements and the periodic law. The successive expulsion of one  $\alpha$  and two  $\beta$  particles in three radio-active changes in any order brings the intra-atomic charge of the element back to its initial value, and the element back to its original place in the table, though its atomic mass is reduced by four units. We have recently obtained something like a direct proof of van der Broek's view that the intra-atomic charge of the nucleus of an atom is not a purely positive charge, as on Rutherford's tentative theory, but is the difference between a positive and a smaller negative charge.

Fajans, in his paper on the periodic law generalisation (*Physikal. Zeitsch.*, 1913, vol. xiv., p. 131), directed attention to the fact that the changes of chemical nature consequent upon the expulsion of  $\alpha$  and  $\beta$  particles are precisely of the same kind as in ordinary electrochemical changes of valency. He drew from this the conclusion that radio-active changes must occur in the same region of atomic structure as ordinary chemical changes, rather than with a distinct inner region of structure, or "nucleus," as hitherto supposed. In my paper on the same generalisation, published immediately after that of Fajans (*Chem. News*, February 28), I laid stress on the absolute identity of chemical properties of different elements occupying the same place in the periodic table.

A simple deduction from this view supplied me with a means of testing the correctness of Fajans's conclusion that radio-changes and chemical changes are concerned with the same region of atomic structure. On my view his conclusion would involve nothing else than that, for example, uranium in its tetravalent uranous compounds must be chemically identical with and non-separable from thorium compounds. For uranium X, formed from uranium I by expulsion of an  $\alpha$  particle, is chemically identical with thorium, as also is ionium formed in the same way from uranium II. Uranium X loses two  $\beta$  particles and passes back into uranium II, chemically identical with uranium. Uranous salts also lose two electrons and pass into the more common hexavalent uranyl compounds. If these electrons come from the same region of the atom uranous salts should be chemically non-separable from thorium salts. But they are not.

There is a strong resemblance in chemical character between uranous and thorium salts, and I asked Mr. Fleck to examine whether they could be separated by chemical methods when mixed, the uranium being kept unchanged throughout in the uranous or tetravalent condition. Mr. Fleck will publish the experi-

ments separately, and I am indebted to him for the result that the two classes of compounds can readily be separated by fractionation methods.

This, I think, amounts to a proof that the electrons expelled as  $\beta$  rays come from a nucleus not capable of supplying electrons to or withdrawing them from the ring, though this ring is capable of gaining or losing electrons from the exterior during ordinary electrochemical changes of valency.

I regard van der Broek's view, that the number representing the net positive charge of the nucleus is the number of the place which the element occupies in the periodic table when all the possible places from hydrogen to uranium are arranged in sequence, as practically proved so far as the relative value of the charge for the members of the end of the sequence, from thallium to uranium, is concerned. We are left uncertain as to the absolute value of the charge, because of the doubt regarding the exact number of rare-earth elements that exist. If we assume that all of these are known, the value for the positive charge of the nucleus of the uranium atom is about 90. Whereas if we make the more doubtful assumption that the periodic table runs regularly, as regards numbers of places, through the rare-earth group, and that between barium and radium, for example, two complete long periods exist, the number is 96. In either case it is appreciably less than 120, the number were the charge equal to one-half the atomic weight, as it would be if the nucleus were made out of  $\alpha$  particles only. Six nuclear electrons are known to exist in the uranium atom, which expels in its changes six  $\beta$  rays. Were the nucleus made up of  $\alpha$  particles there must be thirty or twenty-four respectively nuclear electrons, compared with ninety-six or 102 respectively in the ring. If, as has been suggested, hydrogen is a second component of atomic structure, there must be more than this. But there can be no doubt that there must be some, and that the central charge of the atom on Rutherford's theory cannot be a pure positive charge, but must contain electrons, as van der Broek concludes.

So far as I personally am concerned, this has resulted in a great clarification of my ideas, and it may be helpful to others, though no doubt there is little originality in it. The same algebraic sum of the positive and negative charges in the nucleus, when the arithmetical sum is different, gives what I call "isotopes" or "isotopic elements," because they occupy the same place in the periodic table. They are chemically identical, and save only as regards the relatively few physical properties which depend upon atomic mass directly, physically identical also. Unit changes of this nuclear charge, so reckoned algebraically, give the successive places in the periodic table. For any one "place," or any one nuclear charge, more than one number of electrons in the outer-ring system may exist, and in such a case the element exhibits variable valency. But such changes of number, or of valency, concern only the ring and its external environment. There is no in- and out-going of electrons between ring and nucleus.

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#### Philosophy of Vitalism.

IN NATURE of November 6 Prof. E. W. MacBride has made some critical remarks with regard to my proof of vitalism as discussed in the first of the four lectures which I had the honour to deliver before the University of London in October. Will you kindly permit me to explain in how far I feel unable to accept Prof. MacBride's criticism?

NO. 2301, VOL. 92]

I fully agree with him that vitalism has nothing to do with the progress of zoology as a pure science in the narrower sense of the word. As I have said in my "Biologie als selbständige Grundwissenschaft" (second edition, 1911, p. 24): "The problem of the method of biology remains unaffected by the controversies between vitalism and mechanism."

But I cannot accept Prof. MacBride's opinion about the theoretical, or, if he would choose to say so, philosophical importance of the concept of *entelechy*. He believes "that at the best the conception of entelechy is of quite limited application." He speaks of the fact that, under special experimental conditions a lizard may regenerate two tails instead of one, that the egg of Ascidiaceans (he might have added that of Ctenophores, a case well known to me from my own experiments) possesses a very limited faculty of regulation, &c. But, has it not been for the very reason of the fact that there are "limits of regulability" that I have invented a rather complicated theory of the possible relations between entelechy and matter (see my Gifford lectures, vol. ii., p. 178ff., and the second of my London lectures)? Thus it appears, so I hope, that I have never neglected the limited character of regulability and the dependence of the effects of what I call entelechy on matter. Entelechy is *not* omnipotent. But it seems to me that limitation does not mean non-existence.

For, on the other hand, there are very many cases (development of isolated blastomeres or parts of the blastula of Echinoderms, &c., into small but complete organisms, restitution of *Clavellina*, *Tubularia*, &c.) where entelechy acts, so to say, in quite a pure manner. And it is on these cases, of course, that the concept of entelechy was founded in the first place. Would not also a physicist whose aim it is to study the laws of the reflection of light, prefer for his experiments such materials, which do well reflect rays and do not show the phenomenon of absorption, or only in a very small degree? Logically, in fact, one single case of what I call harmonious equipotentiality would suffice to establish vitalism. But there are many cases.

Prof. MacBride does not attack my analysis of harmonious equipotentiality as such. And, in fact, the theory of organ-forming substances, which he advocates, cannot account at all for the differentiation of "harmonious-equipotential systems," though we might accept it, perhaps, if there were *only* eggs, such as those of Ascidiaceans, Ctenophores, &c. Organ-forming substances have to be ordered or arranged during ontogeny; now this could only happen on the basis of a *machine*, if we believe that it happens on a physico-chemical foundation altogether. But just a "machine" is excluded by the phenomenon of harmonious equipotentiality.

Thus I believe that, even if we concede to Prof. MacBride that the conception of entelechy is "of quite limited application," we are entitled to say: In the theory of the harmonious-equipotential system the concept of entelechy *must necessarily be applied*.

HANS DRIESCH.

Heidelberg, November 12.

THE courteous reply of Prof. Driesch to my letter on vitalism which was published in NATURE of November 6 calls for only a few remarks from me. If Prof. Driesch and I were discussing questions of epistemology or of consciousness, questions in which as an amateur I have taken an interest for many years, it is possible that our points of view might not be so far apart; it would certainly be possible to arrange a *modus vivendi* between them. But for me the value of a conception in zoology is its fruitfulness