

produce evidence confirming this last statement. I admit that carefully studied and conclusive instances are not very numerous, but I refer to such cases as the non-transmission (a) of plus or minus variation in pigment produced in individuals by greater or less exposure to sun-light; (b) the effects of dry or moist climate on individual plants; (c) the effects of change of diet on individual animals; (d) the effects of increased use of muscles in men and animals.

It seems that we are driven to the conclusion that the causes which have been active in producing changes the accumulation of which amounts to specific, generic and larger differences, must be causes which are able to act upon the potential congenital quality of the individual, and that there is no reason for associating the somewhat superficial and late responses or reactions of the parts of a growing individual to normal or abnormal forces of its environment with that more subtle and profound disturbance, which is permanent and affects the potential character of the germ, and more or less of all the germs derived from it.

At any rate this is the absolutely unanimous testimony of all those observers, in all countries and in all ages, who have been practically concerned (often with vast pecuniary interests at stake) in the production of relatively permanent new races of animals and plants. Breeders of horses, cattle, sheep and dogs, pigeon and poultry-fanciers, crop-growers, nurserymen, tulipomania, and the like, have never in any single instance put the Lamarckian principle into practice. On the contrary, they laugh it to scorn. Not one of them ever produced a new race by moulding the parents. But, on the other hand, they do subject the selected parents to novel and disturbing conditions, to which the changed characters of the offspring (not of the parents) have no "responsive" relation; they cross-breed here and cross-breed there, until the "specific potential" is broken-down, and strange unlooked for varieties are born and grow up irrespective of normal or abnormal environment. From these congenital variations they select desired forms, and perpetuate them with perfect assurance and security.

For the present I see no evidence of a production of new races on the face of the earth, excepting by the method adopted by these men, viz. by the selection of congenital variations; such congenital variations being produced as the result of (but without any direct adaptational relation to) a disturbance of the material of the reproductive particles of both sexes; that disturbance being increased, if not determined, by changed environment of the parental organisms or the coupling of remote strains.

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Oxford, November 17.

### The Present State of Physiological Research.

THE extracts given in NATURE of November 15, from an article by Prof. Max Verworn, of Jena, on "Modern Physiology," will serve to draw the attention of biologists to the reawakening of interest which is now evinced by many physiologists in regard to the fundamental phenomena exhibited by living things. As the opinion of physiologists is expressly invited in reference to the questions raised in this article, I venture to express my own as being in the main the same as that of Prof. Verworn.

It seems to me an obvious truism to say that the methods which can ensure a real advance in general biological knowledge must be those in which comparative physiology takes the lead. In my recent presidential address to the Liverpool Biological Society I urged the establishment of laboratories for the systematic study of the comparative physiology of the simpler organisms, the end in view not being the elucidation of the functions of organs with an *arrière pensée* as to their relation to man, but the examination of the activities for their own sake, since this inquiry forms the only means of approaching the mystery which enshrouds the essence of living existence.

The determination of the reactions of simple organisms to physical changes (stimuli), and the grouping of such resultant effects carried out systematically, form a line of physiological inquiry of transcendent importance, both because of its large scope and fundamental character, and because it opens the way towards the partial elucidation of the physiologist's real problem. This problem is the one involved in the question, to what extent all living phenomena are to be regarded as reaction phenomena? Are we, on the other hand, compelled to postulate the existence in every living thing of a *deus ex machina*

which can, if it will, act independently of every physical stimulus, a so-called "vital force"?

Prof. Verworn is right in forcing upon the attention of physiologists the paramount necessity for work of this kind. I venture, however, to point out that he has not done justice to the judgment of his contemporary physiologists if, as I imagine, he has been led to infer from the character of the mass of current physiological work, that they do not realise the importance of such comparative physiology. But to realise the importance of an inquiry and to be able to carry that inquiry into effect, are unfortunately by no means identical positions.

There are undoubted obstacles to the latter, however ardently we may desire its fulfilment.

In the first place physiology is, in this country, more or less shackled by its position. It owes everything to medicine. Its laboratories are adjuncts to medical schools, its professors must take their share in the teaching in such schools, and this teaching is essentially connected with human physiology.

The debt, which as physiologists we owe to medicine, is one which we gratefully acknowledge, but even with the thanks on our lips we may be supremely conscious of the chains which still hang on and impede the debtor. It is this close relationship which, in my opinion, has served to accentuate the separation between physiology and the science of which it properly forms part, biology; a separation which is now almost a judicial one, and if unchecked may become an actual divorce. It is rare to find a physiologist who has been highly trained in zoological investigation, and rarer still to find a zoologist who has attempted to perfect himself in the methods used in physiological laboratories. Yet the appropriate blend is essential for the advent of those comparative physiologists who alone can do full justice to the systematic inquiry now advocated.

Another difficulty in this country is undoubtedly due to the scanty pecuniary help afforded to scientific work which is neither technical nor directly concerned with what is regarded as the public good. Physiology, to-day, is maintained in Great Britain solely because it forms an essential part of a specialised technical education, that of the medical student; it is not maintained in order to inquire into the mystery of living things as such.

In order to adequately develop such an inquiry as this, it would be necessary to have a new department furnished with the equipment of both a zoological and a physiological laboratory, and with skilled workers who have leisure to prosecute their investigations. Since this means money, its full establishment may have to be postponed until that pious benefactor appears whose dawn even a Bodleian librarian has now anxiously to await.

Finally, I do not think the outlook is so discouraging as Prof. Verworn seems to believe, nor that "we are making no progress in physiology."

He admits that during the last twenty years we have attained to a precision in our experimental methods such as excites the astonishment of the uninitiated; and surely the mastery of method is the first step, and that an invaluable one, towards its future more fruitful employment. I do not imagine that even the systematic physiological investigation which he advocates, will involve the employment of new methods to the exclusion of old ones; it is the material which will be novel, not the entire experimental technique. Isolated instances of the application to simple excitable organisms, of such physiological methods as have been employed in elaborate detail for the investigation of muscle, nerve, &c., are well known to us all, and to no one better than Prof. Verworn; the real desideratum is surely that the instances should be no longer isolated, but form part of a broad systematic inquiry.

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November 17.

### Wilde's Theory of the Secular Variation of Terrestrial Magnetism.

MR. WILDE'S reply in NATURE of October 11 to my letter of criticism in the same of August 9, with respect to his communication to the Royal Society, contained in the *Proceedings* for March 1894, has just come to my attention.

As the letter consists entirely in an attempt to show the inaccuracy and unreliability of my statements with respect to the inclination-observations made at St. Helena,