

I have the impression, and I suppose everybody has, that Germany, since the beginning of its new regime, has made some steps nearer to independence, nearer in any event than Herr Stresemann's Germany ever was. I am sure that my country will reinstate the full academic freedom of its universities and science, as soon as political sovereignty in our own country is assured.

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It may well be that Prof. Woltereck's restrained and courteous letter will produce upon his colleagues in Great Britain a more painful sense of alienation than the disturbing utterances of those who now control German academic life. We English do not need to be reminded that political excitement often betrays wise and good men into strange company. We are the last people in the world to deny that, in times of panic or excitement, we have said and done things which in retrospect are recognised by us to be wrong and humiliating. But what seems to men of science most deplorable is the elevation of national passion into a principle, the acceptance of a policy which teaches that to attempt to find and hold truth is but a secondary and subordinate activity of the human mind to be postponed or slighted for *any* reason whatever.—Editor of NATURE.

#### Inheritance of Habits

DR. S. MAULIK<sup>1</sup> has well pointed out the necessity for distinguishing between experiments, like maze threading, in which the nervous system of the animal is primarily concerned, and other experiments in which a foreign substance or a new food material is introduced into the organism—when we attempt to decide whether the new experiences “produce any physical change in the organism”.

In regard to the second group, some information can be obtained from a study of the immunity reaction. In the report of the Medical Research Council “On the Chemistry of the Antigens and Antibodies”<sup>2</sup>, Dr. L. R. Marrack describes certain experiments by Landsteiner and others, in which an artificially prepared substance—atoxyl azo protein—when injected into the blood, confers on the serum of the animal so treated a capacity to precipitate any other protein, if it is coupled with the diazotised atoxyl.

A physical change is thus brought about in the organism by the introduction of an artificially prepared protein antigen, of which neither the animal nor its ancestors can have had any previous experience.

It is also significant that the organism modifies the molecular composition of the foreign antigen before assimilating it, somewhat as it breaks up, and resynthesises, ordinary food material, though after the incorporation of the foreign protein the constitution of the organism itself also becomes altered.

The immunity so acquired is, however, not transmitted to offspring, at any rate in the human subject, because experience shows that acquired immunity against subsequent attack by the same disease

organisms, for example, measles, is not hereditarily transmitted in man.

Prof. MacBride's feeding experiments were carried out on the Ceylon stick insect<sup>3</sup>.

It is possible, as I have elsewhere<sup>4</sup> suggested, that human germ cells may be more isolated, that is, more fully protected against influences from the internal environment, than the germ cells of insects, or some other animals. Hence it is not wise to argue directly from one case to the other without further experimental evidence.

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<sup>1</sup> NATURE, 133, 760, May 19, 1934.

<sup>2</sup> M.R.C. Special Report Series 194, Chap. iii.

<sup>3</sup> NATURE, 133, 598, April 21, 1934.

<sup>4</sup> Withering Lecture II, University of Birmingham, 1932, “On the Making of Use Acquirements, etc.”

PROF. MACBRIDE<sup>1</sup> and Mr. Maulik<sup>2</sup> have raised in the columns of NATURE the important question of the inheritance of acquired habits. Mr. Maulik, if I understand him correctly, states that the offspring of mice trained to run through a maze acquire the same habit more easily than their parents. A reference to the journal in which this remarkable result is published was not given. Mr. Maulik regards it as necessary, before conclusions are drawn, to obtain information as to the nature of the physical change produced by habit in the organism and its reproductive cells. While such information is desirable, it is surely a biological fact that some habits are inherited, even if we do not know the nature of the process of their inheritance. Thus the statistical laws of inheritance of human stature are known, though we have no idea, for example, how many genes are concerned in the process.

Such an excessive demand can only obscure the important issues raised by Miss Sladden's<sup>3</sup> demonstration of the transference of an induced habit (namely, that of feeding on ivy) from parent to offspring in *Carausius morosus*. At least three possibilities suggest themselves. The young insects on hatching may be so saturated with bitter substances from the ivy eaten by their mothers that ivy is less repugnant to them than to insects not so saturated. They may be affected by a *Dauermodifikation* inherited from the mother only and disappearing in a few generations, such as those described by Jollos. Or they may have acquired a character transmissible by both parents, as are most interspecific differences, or such inter-varietal habit differences as that between broodiness and non-broodiness in poultry, or wildness and tameness in mice.

Only in the latter case would the transference of an acquired habit have the relevance for the problem of species formation which Prof. MacBride claims for it. Nevertheless, it should be perfectly possible in suitable cases to test such claims without the very complete knowledge which Mr. Maulik demands.

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<sup>1</sup> NATURE, 133, 598, April 21, 1934.

<sup>2</sup> NATURE, 133, 760, May 19, 1934.

<sup>3</sup> Proc. Roy. Soc., B, 114, 441; 1934.