

me what exactly he intends. If there be any other explanation of the genetical linkage-results than what, following Morgan, I have given above, it is important that it should be fully and clearly set before us.

JULIAN S. HUXLEY.

King's College,
Strand, W.C.2.

THE confusion is in Prof. Huxley's mind and not in mine.

Of course, I recognise that there are such things as 'linkage groups' and that these must have some basis, but what that basis was I thought that I had made clear in my review. It is, I believe, an impairment of what for want of a better name may be called the 'developmental energy' of the organism which leads to an inhibition or slowing down of one or more of the independent processes of growth which make up its development.

The so-called 'genes,' 'unit factors,' or 'mutations' that are linked are so many symptoms or results of this weakening. According to its intensity, more or fewer of them may make their appearance, hence the variable character of the linking. The alleged 'orderly arrangement' of genes within each group is in my opinion a figment of the imagination to which, so far as I am aware, Dr. Bateson has never committed himself.

Jenning's 'proof' that the linear arrangement of the genes is the only one mathematically possible is, like so many examples of 'proofs' given by this school, a fine example of reasoning in a circle, for it assumes the breaking of the chromosome and the 'crossing over' of its pieces, which, as I have shown in the review, is a physical absurdity. I am confident that when the same physiological analysis is applied to the development of *Drosophila* which has been employed in the case of *Vertebrata*, its many mutations will be seen to be the multiform effects of a few simple causes, and I should like to remind Prof. Huxley that Johannsen, perhaps our foremost geneticist, has expressed a desire that the term 'unit-factor' should be proscribed, for the change to which we give this name is merely, he says, a "disturbance of the chromosomes." With this opinion I cordially agree.

E. W. MACBRIDE.

Moulting of Insects.

THE usual explanation of moulting, namely, that the chitinous integument, being elastic only to a limited extent, cannot keep pace with the increase in size of a growing insect and is therefore periodically shed, does not seem to have a very strong foundation. While experimenting upon the effect of starvation on insects, I have incidentally observed the following facts which strongly tell against such an explanation:

As is now well known, the usual length of the larval stage in *Tenebrio molitor* (the common meal-worm) is 7-8 months, during which they moult 14-15 times. When the worms were intermittently starved, the larval period was extended to so many as 18 months, during which they moulted 30-31 times. The interesting point to be noted is that the size and weight before pupation of the 18 months' old larvæ (starved) and of the normally fed 7-8 months' old worms were the same. Since the ultimate size and weight remain the same, then, if moulting is simply to allow growth, there is absolutely no necessity for extra moults. Moreover, in one experiment, in which the worms were completely starved for 5 months and were actually losing weight and shrinking in size, the larvæ moulted 4 times, though, of course, this

number was smaller than that in the normally fed individuals.

A similar state of things was observed in *Pieris brassicae*. In this species the larval stage extends over about 3 weeks in Great Britain (August). Starved caterpillars, which pupated about one month later than control individuals, moulted five times instead of four times, as is usual for this species and as was done by the normally fed individuals. The size and weight of both the starved and the control caterpillars just before pupation was almost the same.

Evidently, moulting cannot be solely and possibly is not mainly correlated with growth, because while growth does not exhibit itself unless there is a moult, the process of moulting can occur without being followed by growth. F. Balfour Browne, while rearing dragonflies, also observed that while some moults were followed by an increase in size, in the case of others the individual did not show any such increase (*Proc. Zool. Soc.*, 1909). That the number of moults is in proportion to the length of the larval life is better explained by considering that moulting is primarily connected with metabolism. Totally starved meal-worms moult less frequently than normally fed individuals, because their metabolism is at a low ebb.

HEM SINGH PRUTHI.

Zoological Laboratory,
Cambridge,
December 2.

Rate of Growth of Fungus Rings.

It is well known that on air-photographs of the chalk downland, fungus rings are often very clearly shown. Sometimes they form the most prominent objects on the photograph and their size is considerable. It has been noticed that they are best developed upon land which has not been under plough for a very long time: in fact they seem never to occur well developed on land which has been ploughed at some time during the last century or two and afterwards reverted to grass.

Before investigating this matter further on the air-photographs here, I should be grateful for information as to the rate of growth of these rings. Is it possible to tell the age of a ring from its diameter? I would gladly lend prints showing fungus rings to any one who is interested in this branch of study and might be able to supply reliable information on the subject.

O. G. S. CRAWFORD.

Ordnance Survey Office,
Southampton,
November 18

Einstein Shift and Doppler Shift.

MAY I ask whether the Einstein shift of spectral lines is supposed to be due to some change of frequency associated with an atomic occurrence while generating waves, or to direct influence of the gravitational potential on ether vibrations after they are generated.

For example, I suppose the observed shift in light from the companion of Sirius occurs in light primarily emitted, and not merely scattered, from that star. The light from a differentially moving dark satellite would exhibit to us a differential Doppler effect, but the satellite would presumably not affect the light borrowed from its primary with an Einstein shift, however concentratedly massive it might be.

I am not sure that this is correct: hence my question.

OLIVER LODGE.

December 8.