

THE position taken up in the note to which Mr. Loring's letter refers was that the X-ray evidence was insufficient definitely to establish his claims. Experience with apparatus similar to that used by Mr. Loring shows that the difficulty is not one of obtaining foreign lines but of eliminating them. Unless special precautions are taken, prolonged exposures give lines due to various sources in addition to those corresponding to the radiations from the anticathode. Two of the most common sources are zinc and mercury. If the exposure and conditions are such as to permit of the detection of small quantities of any element in the anticathode, then the zinc line must appear from the passage of the X-rays through the brass slit of the spectrometer and probably also from the brass of the tube itself. If we accept Mr. Loring's view that the line γ_{43} is not the zinc $K\alpha$ line, then the necessary conclusion is that he could not hope, under the conditions of his experiments, to detect small quantities of elements in the material under investigation. The photograph to which reference was made in the note was made for the purpose of that note and showed both zinc and mercury lines.

Mr. Loring states that a comparison of the line γ_{43} with the zinc $K\alpha$ line obtained on another film showed a slight difference in position between the two. It is possible to suggest various explanations for such a shift based on purely geometrical considerations, but a critical discussion of the point is impossible until a detailed account of the experimental arrangements is published. A better test would be to obtain both lines on the same plate. If the amount of element No. 75 present is so great that the $L\alpha_1$ and $L\beta_1$ lines appear with exposures insufficient to bring up the zinc line, then it should be relatively easy to obtain also the γ_1 line, which lies well outside the bromine absorption band.

The results claimed by Mr. Loring are so important that the X-ray identification of the lines should be placed beyond doubt, and this cannot be said to be the case until a satisfactory explanation is given of all the lines found on the films, and not merely of a selection of them, as has been done up to the present.

THE WRITER OF THE NOTE.

Genes and Linkage Groups in Genetics.

I FIND it a little difficult to deal with Prof. MacBride's reply to my letter, in NATURE of December 26, since it seems to me not to take any account of the numerous facts bearing on the point which I raised. I feel that the only course is very briefly to enumerate the chief of these facts, and leave other biologists to judge if Prof. MacBride is right in his strictures, or in his views of linkage. I do this at the risk of becoming tedious, because it appears to me very unfortunate that, when delicate quantitative methods are at length introduced into a difficult biological field, they should be attacked on what I consider wholly insufficient grounds by prominent authorities such as Prof. MacBride.

As I see them, the facts are these: (1) There are numerous pairs of characters of organisms which "breed true" and which, when the strains showing them in pure form are crossed, reappear in the F_2 generation in the ratio 3:1. This has universally been taken to indicate material *somethings* in the germ-plasm connected with the appearance of the characters in question, and segregating in a clean-cut way into the F_1 gametes—in other words, *genetic units*. These doubtless are "disturbances of the chromosomes," but they are equally genetic units, and we may continue to use the non-committal term *factors* for them.

(2) When two such pairs of factors (call them A and a, B and b respectively) have been separately identified in a species, we can then test them together in one experiment. If so, it is without exception found that one of two things will happen:—

(2a) However the factors are introduced into the cross, the results in all later generations are the same. The F_2 gives the ratio $9AB:3Ab:3aB:1ab$, the back-cross to the double recessive the ratio 1:1:1:1. This has been universally taken to imply *independent segregation* of the two factors.

Or (2b), the result is different according as to how the characters are introduced into the cross. Taking, for simplicity's sake, only the result of back-crossing F_1 to the double recessive, then (i.) if the cross was $AB \times ab$, we get $nAB:1Ab:1aB:nab$, where $n>1$. (ii.) If the cross had been $Ab \times aB$, precisely the *reverse* result occurs, namely, $1AB:nAb:nAB:1ab$, n having the same value as before. This has been universally taken to indicate genetic linkage (some degree of absence of independence) between the two factors. (iii.) In *Drosophila* there is the further fact that the results under (i.) and (ii.) differ according to the sex of F_1 . The female gives the formula stated above, but the male, without exception, gives n infinite—i.e. linkage is complete in the male. How these facts bear any relation to Prof. MacBride's definition of linkage, I fail to see. The same factors in the same proportions are present in F_1 in both (i.) and (ii.), and in male and female under (iii.); yet the proportions of the normal type and what Prof. MacBride regards as the doubly pathological type (with both mutant characters) are entirely different in accordance with the way in which the factors entered the cross, and with the sex of the individual tested. That this reversal of proportions can have anything to do with the weakening of developmental energy of which Prof. MacBride speaks as the basis of linkage, is to me unintelligible.

(3a) Finally, we can define as the "percentage cross-over value" (N.B., *pace* Prof. MacBride, as a purely genetic concept, with *no reference* to possible cytological interpretation) the expression $\frac{100}{n+1}$.

(3b) If by the methods of (2b) above, A or a has been found to be linked with B or b with cross-over value p , and with C or c with cross-over value q , then it can now be prophesied that on testing B or b with C or c (a) linkage will be found between them; and (b) if p and q are small, the cross-over value will be the algebraic sum of $p \pm q$.

This prophecy has been repeatedly verified; and no exceptions occur to the rule, provided naturally that conditions are kept constant, as in any physical or chemical experiment. It is on this last general result that the hypothesis of linear arrangement of the factors is based. I can only repeat that if Prof. MacBride can advance any alternative explanation of these facts (which he has not yet done), it will be of great interest.

With regard to points in Prof. MacBride's reply, he is not accurate in stating that Jennings started by assuming chromosome-breakage. Jennings assumed nothing but the facts provided directly by certain breeding experiments—namely, the proportions of certain visible characters appearing after certain crosses. The genetical fact that linkage is broken in a regular percentage of cases is entirely independent of any cytological hypothesis advanced in further explanation of it; and I repudiate the suggestion that to state this is to show confusion of thought. I have tried hard to grasp Prof. MacBride's own definition of linkage. He states that the basis of a linkage-group is "an impairment . . . [of] 'develop-