

Haskins Laboratories
305 East 43rd Street
New York 17, N.Y.

January 11, 1949

My dear Professor Fisher,

Thank you very much for your most kind and most helpful letter of the twentieth of December. If I may, I shall impose a bit further on your time by attempting a very partial answer to some of the questions that you raise. Unfortunately, the answers must be incomplete indeed, for such data as I have that are appropriate are far from complete, but I hope that they may be suggestive. I am attempting at present to amplify them.

First with respect to your comment concerning the distribution of epigamic sex-linked dominants in wild populations, and the statement of Winge that such sex-linked factors occur predominantly in the Y-chromosome. I have also been much impressed with this statement and its implications, and felt it important to check it further. My own impression agreed with the statement of Winge, but I have just this last year completed a series of breeding tests with some wild individuals, tested against some stocks originally obtained from Winge, which seem to me, if I interpret them correctly, to indicate quite otherwise, much to my surprise. These tests, I should emphasize, were preliminary, and they are incomplete and imperfect in a number of ways. The progenies examined were much smaller than I could wish. Also, a final cross to distinguish between certain situations in which it appeared either that homologous sex-linked factors occurred in both the X and the Y of the original male, or that such factors were autosomal dominants, was not made in the cases where this confusion arose. I plan such crosses, of course, in the immediate future, and also to expand the number of tests and the populations counted.

If I may do so, however, I am enclosing a copy of this unpublished data, imperfect and incomplete though it be, because it does seem to me suggestive in respect of the percentage of sex-linked factors which are normally Y-linked in wild Lebistes populations. I question whether we can properly consider that they predominate, unless I have entirely misread the results.

Second, with respect to your very interesting question of what the streams of the Northern Range in Trinidad would look like if mapped for the occurrence of different members of the sex-limited and sex-linked color series. This I shall answer in an even more restricted way, because of the multiplicity of patterns and my ignorance of the mode of inheritance of the great majority of them. This much can be said, however. In all the streams which we have so far examined, with one exception, a mixture of several different Y-bearing, and, if I am correct, also of X-bearing and/or autosomal dominants is to be found in all parts of the stream, provided that fairly small intervals of distance (half a mile or less) are used. Within these intervals, one does not encounter

the situation that, in passing along the stream, one common dominant is replaced by another. For greater distances, I can only say that the frequency of the single factor which we have been able to study at all critically in this respect so far (Maculatus) may vary, but I cannot say much yet as to the significance of this variation. I have never found evidence for the existence of a common universal recessive.

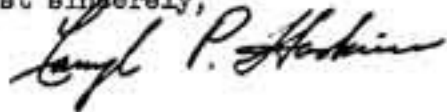
From river to river, the frequency of some of the patterns varies greatly, patterns which are present in fairly high frequency in one river being sometimes apparently totally absent in another. The pattern of a random distribution of many color factors through all parts of the sector of a single river (say of half-mile length) however, is characteristic in all cases, except for one striking exception. This is the Arima River, a tributary of the Caroni about midway of the Island. Here the population, over six miles of river course which we have examined fairly carefully, is remarkably uniform. The variety of patterns is limited and rather constant--so much so that fish from this river can be readily distinguished from those of the more westerly courses examined. There is some indication that the nearer one gets to the headwaters of this stream the more limited the number of patterns becomes, and that the populations are richest in patterns nearest to the point of junction of the Arima with its outlet stream, the Caroni, but the data are not yet good enough to assert this positively. It was interesting to me to notice that, if I interpret the data enclosed correctly, populations from the Arima may show a rather high frequency of non-Y-linked factors relative to the other streams examined--but the numbers are low, and much further work is needed.

I may add that I had tentatively planned to use two sex-linked dominants as "markers" in studying the rates of propagation and dispersal of single genes against these mixed backgrounds in individual rivers. Both seem well suited for this work in a number of ways. The first is Winge's gene Maculatus, which is so closely Y-linked that Winge has reported zero cross-over value among several thousand fish examined, and we have not observed a crossover in sixteen years of work with the stock. The second is Winge's Flavus, which is an X-linked dominant having the unique virtue, among sex-linked genes, of being manifest in the mature female and in half-grown young fish as well as in the mature male, though greatly heightened in expression in the latter. Both genes control phenotypically conspicuous and easily recognized color patterns, readily detected among collected stream fish either fresh or preserved. Further, Flavus does not appear to occur in the Wild Lebistes populations of Trinidad, and Maculatus, though present in a few streams, is almost certainly known to be absent from the Arima River, and has never been found in streams east of that watercourse.

I am afraid that these are unsatisfactory answers to your questions, and they are very incomplete. They seem to me somewhat indicative, however,

and I hope to have more extensive genetic tests before too long. If you should be interested, and if I may do so, I should like to acquaint you with these results as they come in, and would be more than appreciative of any comments you might wish to make on the present data.

Most sincerely,



CPH/hh

Enc. - Data

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P.S. If I may, I will add a word concerning the apparent function of the epigamic male sex-limited color factors in Lebistes. We have recently completed a series of competitive mating tests within Lebistes and between Lebistes and its sympatric species Poecilia vivipara and Micropoecilia parae (now in press for Evolution) which have suggested to me quite strongly that in Lebistes the female is very largely a passive agent in fertilization, and that the evolution of the male color systems is likely to have been very largely on the basis of competitive male repulsion rather than of competitive male choice among females.

CPH