

The Enclosures: read these after the comments at the end of this letter.

TELEPHONE 47726.

DEPARTMENT OF
ZOOLOGY AND COMPARATIVE ANATOMY,
UNIVERSITY MUSEUM, OXFORD.

My dear Ron,

[mid-September, 1951]

You would no doubt receive from me a letter from Australia, and subsequently one from Berkeley, San Francisco. After acting as President there for the meetings of the Society for the Study of Evolution, I crossed California by motor-car and joined Dobzhansky at his camp in the Sierra Nevada. While there I reached the watershed at Tioga Pass (10,000 feet). On the way up Horowitz, who was with me, and I made interesting observations on the ecology of a dimorphic Colias butterfly (C. eurytheme). I also explored the Yosemite Valley.

Dobzhansky is become a Selectionist-Quile. I am impressed with the way in which he has been prepared to reject his previous point of view in favour of the facts which his data reveal. I had thought him rather a bigoted person, but his outlook proves to be more genuinely scientific than I had realized. Speaking of such a thing he said to me, "A few years ago I should have interpreted that along the lines of Sewall Wright: I now see that the Selectionist-Explanation is the correct one".

While there, our camp was raided by a bear, which smashed seven plates and ate a pot of marmalade and two rolls of laboratory paper — so now I know

Low to feed bears.

On my return to San Francisco I flew to New York via Dallas, Texas. On the way, we crossed, and had fine views of, the Grand Canyon of the Colorado.

At New York I saw something of my kind and delightful colleagues at the American Museum of Natural History, of which they had been so good as to make me a Member when I was in the U.S.A. previously. I then went on to stay with Demerec at Cold Spring Harbor; where I lectured, and saw a good deal of the remarkable work on the genetics of bacteria and of phage in progress there. In bacteria, they showed me linkage data in coupling and repulsion - as if one was looking at mouse genetics!

By that time the dark shadow of England was beginning to loom ahead. I returned to New York and made my way to London (so completing a journey round the World) just in time to rush to Tsan, whence I am recently returned. Our data there will, I believe, prove rather exciting. I will make analysis of them and then show them to you, if I may - and as I always do, my dear Ren. However, the general picture begins already to appear :-

On Tsan we still have the 3 populations of Maniola pirtina. They remain, as they have throughout, unimodal in the ♂, bimodal in the ♀. We now have samples of all three for 1946, 1950 and 1957. The 3 populations are of the order of 15000, 2000 to 3000, and

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500. This year we collected on a number of other islands also. Some (of which Tzan is one) are small, and others large. In Sicily the islands tend to fall into two groups, there being a big difference in area between the "large" and the "small". I have not exactly measured the areas yet, but it is reasonable to say that the small are (perhaps) 880 by 350 yards or less, and the "large" are 2 miles x 1 mile or more. The populations on the "large" islands are all very similar to one another, those on the "small" have each diverged widely in different directions (judged by spot distributions). There is a striking instance in which two large islands have almost identical populations, while a small island with an entirely different type of population lies between them.

The effect of minor ecological barriers in promoting isolation (and so in speeding up evolutionary adjustments) is most remarkable. Between Tzan and the next small island (St. Helena) is a strip of sea only 440 yards wide. The butterflies could cross this in a few minutes: and we have seen them traverse ~~approximately 350 yards~~ ^{350 yards} within the largest habitat on Tzan in less than a quarter of an hour (often). Yet the populations on Tzan and St. Helena are quite distinct.

I am sure that the importance of minor ecological barriers has not been fully appreciated.

Turning now to the two Enclosures, the nature of which you will see. I want to say a word about them in confidence.

Both Hardy and I feel some diffidence in sending them to you: because we feel that if a book ^{of essays} on Evolution is to be "dedicated" to anyone it should be to yourself, who has done more for the analysis of Evolution than anyone else alive.

Entirely between ourselves This proposition really originated from Julian himself who, it seems, has always longed to have a book of essays dedicated to him, and he asked our co-operation now that he is reaching 65. (We are not telling anyone this but yourself). He feels, as I believe you do, that Julian really has done a lot to stimulate work on Evolution (and I personally owe him an immense amount). I don't think he has by any means received his due from the majority of biologists: some have thought he has merely copied other people's work (most untrue), and others that he is a mere popularizer — he rather has himself to blame for this, but of course he is much better than that. Anyway, rightly or wrongly, Hardy and I felt it a duty to help (one would not exactly do it for

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Measure!) and as such we are doing to do so.

We are sending out the notices (for Secretarial reasons); and if you feel you can contribute, I know it is the thing above all others Julian would value.

Knowing and appreciating Julian as you and I do it seemed right to tell you this (and Hardy spec), but no one else is going to know it.

Yours ever,
Henry

P.T.O.

Sheppard I know has sent you a first-statement of
the dominica work, ~~the~~ which this year he had to
do almost entirely himself. I am so glad you like his
latest Inaid paper; which he tells me you are kindly
going to put into Heredity: grand!