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Dec. 4, 1939

Dear Grey,

I have indited this long scrawl in the hope that it may be of use to your Committee. I think you felt they would need some such basis for judging whether or not I can properly publish such material under my own name!

Yours sincerely,

Dec. 1. 1939

It is now over 20 years since my first and only previous experience of unfavourable referees' reports upon a paper submitted by me for publication by the Royal Society. About 1916 a paper of mine on "The correlation between relatives on the supposition of Mendelian inheritance" was rejected, on the advice, I understand, of Professors K. Pearson and R. C. Punnett. The paper was published (1918) by the Royal Society of Edinburgh, and has though very long and *badly* constructed, exerted a gratifying amount of influence. Its rejection was apparently due to its *traversing* two opposite but fondly held beliefs of the referees, (i) that Mendelism was inapplicable to human measurements, and (ii) that the biometrical technique of *man* measurement could throw no light on problems of genetics.

In the case of the paper ~~which~~ I have recently submitted, written jointly with Dowdeswell and Ford, the referees' objections, so far as I have been allowed to see them, consist of one which, if well founded, would be of great importance, and many unspecified but obviously trifling differences of opinion as to correct spelling, grammar, and syntax. I believe the paper is clear, and free from any obtrusive error of this sort, but, in the absence of any suggested amendments, I cannot say whether I should agree or not with the usages preferred by the referees.

One referee does, however, make the challenging suggestion that I am not the author of the statistical portion of the paper, and if this were true, it would, in my opinion, be a good reason for rejection. It is, however, totally untrue. In this case

I have not even delegated the calculation of ratios, etc to any other member of the department; the actual arithmetical work, as well as the idea of making the particular calculations I have made, and the theory I have developed for interpreting such recapture data, happen to be both new and my own.

It is a type of data in which I have been interested for more than 10 years. In the beginning of 1930 Dr Jackson, engaged in tsetse research in Tanganyika, spent some months in my Laboratory, and, though he had no data for estimating populations, he had developed a marking technique, and during his studies with me was greatly interested in its possibilities as a means of estimating numbers. He later published some such estimates in cases in which the observational basis could be made reasonably satisfactory. E.g., the area to which the population estimated could be assigned is often well defined only in favourable cases.

Dr Jackson returned to my department at two later periods. In 1935 he had some data from a regular release and recapture programme, and was ^{concerned} ~~concerned~~ with the problem of combining the information supplied by recaptures 1, 2, 3 weeks apart in making a population estimate. I suggested a rough but easy method of estimating the population at date x from the series of recapture ratios at dates $x+1$, $x+2$, etc, based on the supposition that these would fall off approximately, and for some weeks, in geometrical progression. The estimate obtained could be checked if the data were available for the series of recapture/release ratios from dates $x-1$, $x-2$, $x-3$,

I had not at this period data sufficient to determine whether the progression was, in fact, geometric, or whether the estimates from previous releases and subsequent recaptures would really check. On his third visit, 1939, Dr Jackson had ample material giving full ~~reassurance~~ on both these points.

During Dr Jackson's second visit, I suggested to Mr Stevens, then in his second year as my statistical assistant, that he should look into my method to see if it could be improved in respect of what I have called "efficiency". He was able to show that, where the numbers caught and released on successive occasions were constant, the method of maximal likelihood yielded a simple solution for the geometrical progression, but that, when these numbers vary, there is no very elegant method of combining the evidence.

It was not until Jackson's third visit in the early months of 1939 that it occurred to me that the backward and forward progressions supplied separately the rates of birth (and immigration), and death (and emigration). In Jackson's tsetse material, with low birth and death rates, and no great fluctuation in numbers, the chief importance of this step lay in the assistance it gave to the estimation of population movements of a different ^{nature} kind or, possibly, of other kinds. Ford had in the previous Summer, after discussion with me in previous years, obtained some singularly thorough data on an ~~organism~~ ^{insect} presenting quite different problems, namely, irregular emergence, and rapid changes of numbers, almost undisturbed by migration.

Stevens had tried the earlier method on Ford's material, with results which satisfied neither himself nor me. It was

clear that for an organism with rapidly changing numbers, and irregular periods of observation, the method as it stood was quite inadequate. With his entire consent, therefore, I took over the problem myself, with a view to trying out, both the new method of separating the contributions of emergence from those of death, and of weighting the unequal numbers recorded in a different, and as I felt was needed, a much simpler manner.

That this simplicity of approach, represented by the use that has been made of the trellis design, will be adequate for all analogous data is not to be supposed. I hope, however, that the possibility of eliciting the ^{many} ~~various~~ facts by a procedure which involves no more than simple arithmetic, will be a real encouragement to biologists who may be thinking of sampling studies of natural population.

It may be noted that my paper with Dowdeswell and Ford is the first I have published on the subject. It has always seemed to me preferable, and a great saving of redundant algebra, if statistical methods developed in relation to biological problems are published in connection with the material to which they are applicable. In a rather extensive experience I have not been troubled by any unwillingness on the part of biologists^{ists} who have studied with me, to give me any credit which may be due. My referees have, ^{I should judge} ~~perhaps~~, both been pre^oeminently associated with the trapping experiments with Drosophila carried out in recent years by Dr Gordon, and may resent the fact that I do not discuss their contributions to the subject.