for the conduct of such experiments are more familiar to agronomists than to geneticists, who have had little occasion to use them in the past. Polygenic investigations will, however, require that genetical experimenters become as familiar as agronomists with the principles of experimental design; more especially because these same principles must be used to develop methods as appropriate to the solution of genetical problems as the randomized block, latin square, etc., are to field trials. It would be rash, perhaps, to suppose that all the difficulties of polygenic investigation can be overcome by these means, but suitable experimental design is clearly the primary requirement.

I venture to think that my own experiments[4,5] have been adequate from this point of view, and that, therefore, a certain degree of confidence may be placed in my results. (It should be stated that, for reasons of space, only the outlines of my analyses could be published in some instances.) In any event my experimental technique was sufficient to render it improbable that the results could, as Gordon and Sang suggest, be attributed to the sterility of females in the selection lines, for the following reason. I selected both for increased and decreased number of abdominal chætæ, and obtained a marked response in each direction. Sterility also set in in both lines, with a consequent reduction in the larval population of each culture. It is difficult to see how this reduction could explain the selection results, for in one line it must then be supposed to have resulted in an increase in chæta number, while in the other it resulted in a decrease. Furthermore, a repetition of this selection experiment gave comparable results, even though larval crowding was artificially increased by doubling the number of parent flies per culture.

It may be remarked that, in my experience, reduced fertility appears to be an almost inevitable accompaniment of selective response in any polygenic character of *Drosophila melanogaster*. The reason is probably a mechanical one. Fertility itself must be polygenic and hence its controlling genes will be intermingled, along the chromosomes, with those other polygenes which control the character upon which selection is being practised. Now selective response, on my view, depends mainly on the action of recombination in breaking up polygenic combinations and so releasing heritable variability. Such recombination will affect the polygenic combinations controlling fertility equally with those controlling the character upon which selection is being directly exercised. Correlated response may then be expected to occur, and fertility will be reduced as the chæta number changes. Correlated response to selection would appear to be an inevitable property of polygenic inheritance, and it is of great help to us in understanding some features of evolutionary change, notably the degeneration of unused organs.

Furthermore, correlated response renders it nearly, if not quite, impossible to maintain the fertility of selected females, as Gordon and Sang propose. It also serves to emphasize the absolute necessity of adequate experimental design. K. MATHER.

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Gordon and Sang[1], in commenting upon the important work of Mather[2] on polygenic inheritance, remark that the study of this will only become effective when certain experimental conditions are understood and allowed for. Might I add that there may be a further condition to be fulfilled for its satisfactory development?

It seems to me that it will be necessary first to face and to solve a real methodological difficulty inherent in any work involving the concept of poly genes. Many cases are familiar where several genes independently known, interact to give certain effects. The evidence for the existence of these genes depends upon ordinary considerations such as the increase in variation seen in the F_2, and in the possibility of establishing so-called 'pure lines' of differing qualities out of the original stock.

In order to account for the behaviour in heredity of a variable quality the behaviour of which cannot be accounted for on the assumption that it depends upon the distribution of the members of a single gene-pair, it seems now to be assumed that its behaviour depends upon the distribution of the members of several such pairs, of which there may be no independent knowledge. If the number of such pairs invoked as relevant be increased just until they are sufficient to explain the observations, a weakness seems to appear in the procedure. The explanation offered becomes, it seems to me, simply epicyclic; that is, closely similar to the explanation of planetary motion by the postulate of any requisite number of epicycles. It will, as epicyclic explanations always will, explain anything: any ratio of types, any degree of the expression of a quality. But what is the scientific value of such an explanation?

Surely it will be necessary to base the estimate of the number of genes to be invoked as relevant on something other than an estimate of how wrong the assumption of a single gene-pair would make the results. Where is this other independent criterion to be sought, if the genes concerned have nearly negligible individual effects or even cannot be isolated?

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Reflexion from Paper

THE optical effect described by Mr. Burke in NATURE of May 30, p. 613, is well known to the practical papermaker. In matching the tone of a 'white' paper it is essential to employ direct illumination and direct vision. Any angular deviation in lighting or vision gives rise to misleading tones of red or blue.

Dr. V. G. W. Harrison's ingenious explanation does not appear satisfactory for the following reasons: (1) The effect described is characteristic of plain uncoated papers, not 'art papers', which present a non-fibrous surface; (2) the angle at which the maximum effect is observed does not correspond with that of specular reflexion; (3) it explains a possible red colour but not a definite blue tone.