

23 April 1932.

Major L. Darwin, Sc.D.,
Cripps's Corner,
Forest Row,
Sussex.

Dear Major Darwin:

I see that I have been actually a month in answering your letter, which I am beginning to do this evening. After first reading it I set it aside until I could get hold of a copy of Hogben's book, which I expected to be able to do at once, though in fact, it has only arrived to-day. You must have been tempted to think me intolerably inconsiderate in sending no acknowledgment in the meanwhile, but it always seemed that the acknowledgment would be followed at once by a proper reply, and would have been a mere nuisance.

I cannot think what Hogben means by his correlation coefficients on p. 103, of which the probable errors have evidently got reversed between that page and the following tables, one of innumerable indications of how hastily the book has been thrown together. I think he must have taken a correlation merely between size of family and order of birth, which seems incredibly fatuous, but he evidently takes them to mean that "both relatively bright and

defective children tend to turn up late in the family group". To see if there were anything in this for the scholarship children I have compared, for families of from 2 to 14, the numbers of last children (e.g. 49 for families of 2) with the numbers expected by chance (e.g. 64.5 for these families). The result is really rather interesting, for, for families from 1 to 5, I find 204 expected and 157 observed — a deficiency of 47; while, for families from 6 to 14, I find 29.5 expected and 43 observed, an excess of 13.5. You will see that on the whole there is a deficiency, contrary to what one would expect from Hogben's remark; but the discrepancies are both big enough to make me think that they are not accidental. In view of Professor MacBride's well known opinions I hesitate to suggest that the youngest children of the well-spaced small families are for this reason at a disadvantage compared to the youngest children of families with a succession of frequent births! I always find data on birth order particularly difficult to interpret. In view of the large proportions of families that are limited, and the general preference for boys, I would expect the last children, especially in the smaller families, to have a very high sex ratio. I do not know again whether the proportion of scholarship children is the same in the two sexes; but if the boys have the advantage in this it only enhances the discrepancy in the L.C.C. figures; but perhaps really the girls win more scholarships. I suppose it is conceivable that there is a psychological reason. One

might imagine that parents are hopeful of their earlier children and run them for all they are worth and that the last children are the last, especially in small families, because disillusion has set in. And, if there is no driving necessity the parents are content with ~~with~~ scholastic mediocrity. I do not know again whether scholarships are not sometimes awarded with a partly charitable motive, and this would favour the younger children of the larger families.

In the case of the defectives there is a real and consistent excess among the last births and, since the actual majority of defectives are born to more or less self-respecting people, this would certainly be explicable if, as I suspect, the recognition of a defective child were an effective reason for limiting the further family. This recognition would of course occur at varying ages so that one would expect to find the excess not only in the last, but in the last 2 or 3 children of the family. The almost complete absence of defectives among the first 12 children of families of more than 15 contains the reassuring suggestion that had such defectives occurred the families would never have reached these proportions; but as I say, I do not like to argue from the data with any confidence, but merely emphasise these effects of conscious limitation, because I think they would probably be pronounced, and because they seem to be invariably overlooked.

One general form of misstatement that one would expect is

that some of the earlier members of large but incomplete families would appear as relatively late members of somewhat smaller families. This would tend to increase the apparent frequency of whatever was being observed in the later members of both groups of fraternity size.

On reaching[^] p. 191 I think there must be somewhere a misprint of "low" for "high", or of "lower" for "raise", but he does not develop his argument far enough to show what he really means. Birthrate, in the first line of the lower paragraph, must certainly be a misprint for deathrate.

In Section 3, I hate his cant about laborious investigation. He is of course energetic enough; but that is a different matter.

You may have noticed in the current Eugenics Review a review of mine on some Dutch data, from Rotterdam, of much the same kind as the Stockholm and Berlin investigations which are cited. I give there some of the reasons for thinking that this kind of enquiry is wholly inconclusive, by reason of its scope, and that the methods employed are hopelessly inadequate to throw light on questions of such delicacy as the relative rate of decrease of fertility in different social classes. Unfortunately propagandists like Mrs. Hodson and, I am sorry to say, Eldon Moore, seem for a time to have swallowed the stuff whole, and it may be long before the misapprehensions that have been spread abroad will be counteracted. This is one of the most serious injuries which I

think we suffer from the lack of good national statistics on the differential birthrate, that there is no means of checking the influence of confident, but almost baseless, assertions.

I had adopted the term "particulate inheritance" partly because I wanted something wider than the ordinary epithet Mendelian, and partly because I knew Galton had introduced it, and had memories of a passage of his, exactly where I cannot now say, in which, in explaining its use, he gave the best ^{early} statement that I knew of, of the contrast between these two possible theories. I did not realise that he associated the idea with the theory of pangenesis. I could, as you say, equally have used the term 'segregating', with an added explanation that I should like to include in the meaning the transmission of extra-nuclear elements, such as the plastids in plants, if they showed particulate continuity, whether or not they segregated in fixed ratios like the nuclear elements. I do feel, however, that even if your father had come to know of, and to accept, Mendel's work, and the generality of its application, it would still be proper to point to an important strand in the argument of the "Origin", a strand, the logical cogency of which has not, I believe been properly appreciated, as evidence that the blending or fusion theory had greatly influenced the form in which he presented the theory of Natural Selection; and, in particular, had led him to give far more weight, than, as far as one can judge, he would otherwise

have done, to the possible effects of non-selective agencies in evolution.

What Hogben says about you, Whetham and others makes me indignant, and at the same time sorry for him. The class conscious prize-examinee, without the tradition of social responsibility sticks out only too plainly when he tries to be clever. I was at the meeting of the Experimental Biologists at Oxford last week, a society in which he was very much at home in the days when he was only a young physiologist of brilliant promise and was really surprised at the change of attitude towards him. He is becoming as much a target for unfavourable criticism, almost, as MacBride has made himself; though as he has certainly infinitely more brains, I hope that he may perhaps see it coming and curb his impulses with some socialistic substitute for reverence.

Yours sincerely,