KINMAN, M. L., AND SRAGUE, G. F. 1945. Relation between number of parental lines and theoretical performance of synthetic varieties of corn. *Jour. Amer. Soc. Agron.*, *37*, 341-351.
NILSSON-LEISSNER, G. Relation of selfed strains of corn to F_1 crosses between them. *Jour. Amer. Soc. Agron.*, *19*, 440-454.
POWERS, LEROY. 1952, Gene recombination and heterosis. *Heterosis*, 298-329. Iowa State College Press, Ames, Iowa.
QUINBY, J. R., AND KARPAR, R. E. 1948. The effect of different allels on growth of sorghum hybrids. *Jour. Amer. Soc. Agron.*, *40*, 255-259.
RICHEY, FREDERICK D. 1931. Experiments on hybrid vigor and convergent improvement in corn. *U.S.D.A. tech. bull.* no. 267.
WILLIAMS, WATKIN. 1959. Heterosis and the genetics of complex characters. *Nature*, *184*, 527-530.

THE FUTURE OF MAN: A REPLY

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Professor Darlington's review of my broadcast lectures on *The Future of Man* appears in the December 1960 issue of *Heredity*. In the main, he construes the lectures as an attack upon a " bogeyman ", an " enemy ", an " unidentified antagonist " who practises what I have chosen to call " geneticism ". The length and style of his review, its misrepresentations, and its agitated appeals to an unseen audience (Darlington asks no less than twenty rhetorical questions) combine to suggest that he has identified my unknown antagonist with himself.

Of my first lecture Darlington says that I advocate cohort analysis because " it is likely to succeed where other methods have failed in predicting the future numbers of our population ". The point of the first lecture has therefore escaped him. What I actually said was that " predictions founded upon cohort analysis are somewhat more exact in the sense that one can foresee a little more clearly what follows from one's assumptions; and if these predictions are wrong, as to some extent they surely will be, it will be easier in retrospect to see which assumptions were faulty and which factors changed in unforseeable ways ".

Darlington reviews my second lecture at great length. His philosophic reflections upon it have, for me, a certain self-taught quality that make them hard to follow, but he is particularly contemptuous of my saying that " it is impossible, indeed self-contradictory, that an animal should have evolved into the possession of some complex and nicely balanced genetic make-up which rendered it unfit ". This statement is true, and the term " self-contradictory " is to be taken in its strictest sense. Darlington apparently deplores the ambition to cure phenylketonuria, for if we achieve it, " shall we not in some sense be arranging for a particular type of hereditary imbecile to breed ? " As he does not answer the question, I shall do so for him : No. Darlington must distinguish between the genetic singularity and its somatic manifestations of the first or second order. He writes as if he thought the genes themselves were mentally deficient. Potential victims of phenylketonuria whose metabolic disorder has been circumvented will still suffer from a genetic disability, for they cannot eat what they choose. It is a restriction of freedom, then ; certainly worse than being obliged to wear spectacles, but not so much worse that we should be content to see them die. There is much to be said for trying to prevent the genetic conjunction that leads to phenylketonuria in the first place (for example, by identifying the heterozygotes and dissuading them from marrying each other), but it should be realised that such a policy will allow the offending gene to increase in the population unopposed.

I forbear from comment upon Darlington's homespun anthropological homily, which begins with the remarkable sentence " Primitive societies are made up of men and women who are able to do everything they need for their own survival and propagation ".

In studying my third and fourth lectures, Darlington finds me to have said that " we must not be sure . . . of any genetic conclusion ". He himself is confident of his ability to control evolution in man, in the sense of directing it towards a predetermined goal. Some of the principles that would underlie his attempt to do so are made clear by his views on the genetic consequences of birth control. Darlington attributes the fall of the birth rate, and its subsequent rise, to genetic agencies :

> " Previously children had been born to parents merely in accordance with their ability to beget and bear them. Now they were born to parents in accordance with their willingness. Thus, for the first time in evolution, parents who did not want children, or want so many of them, were selectively disfavoured. Conversely a selection began which favoured specifically the property of wanting children. We should therefore expect a sag in the birth rate ; and after the sag a recovery. As indeed seems to have happened."

" For Professor Medawar this would be perhaps a worthless speculation ". I fear so. " A piece of geneticism, he might say." I do. The facts are that in this country the average size of completed families fell smoothly from about six for couples married in the 1860's to about three for couples married in 1910, and then to about two for couples married in the 1920's. Furthermore, cohort analysis has shown that a large fraction of the specially numerous births that occurred towards the end of and shortly after the war can be attributed to the decision of married couples to have *then* the children they were disinclined to have during the dark days of the war itself. To attribute all these rapid secular changes to the action of natural selection upon inborn differences in the inclination to have children is, in my opinion, ludicrous.

I reaffirm my contention that the discovery of the conservation of genetic variance that is made possible by the Mendelian system of heredity was a discovery of Newtonian stature. For some reason Darlington feels that the concepts of stabilising selection, of the effects of disuse and of the stability of chromosome numbers all derogate from the importance of this discovery. They do not.

My fifth lecture dealt with the possibility of a secular decline of intelligence. The rational case for a decline of intelligence is based upon the negative correlation between a child's score in an intelligence test and the size of the family he comes from. I said, and I repeat, that for the development of this argument, " there is no need to assume that professional men are innately more intelligent than labourers ". The whole approach bewilders Darlington, for he takes it quite for granted that intelligence tests measure " innate " intelligence : " the intelligence tests which the lecturer accepts for his argument have indicated that professional men are innately more intelligent than labourers ". And so again he misses the point. Darlington does not realise how *difficult* these problems are, how many highly capable people have wrestled with them, how uncertain our inferences still are.

It is equally clear that Darlington has not grasped the argument of my last lecture ; indeed, he confesses himself utterly entangled in it. He speaks of an inversion of metaphors, of heredity being called instruction and instruction heredity, and of many other difficulties of his own making. This is beyond remedy from me.

Finally, Darlington takes it upon himself to speak for science and for genetics generally; " the scientist ", he tells us, will think this, " the geneticist " that. I dispute Darlington's claim to speak for all scientists and all geneticists. He speaks for himself.