4 DARWIN-FISHER CORRESPONDENCE 1930-1942

Fisher to Darwin: [late-March 1930]

I am sending herewith a copy of my book, which I hope will not be injured in the post, as seems to happen too frequently.

I know you will be as eager as I am to know whether it is going to exert any real influence, but of course we can scarcely hope to form *any* opinion about that for a year, and no very confident opinion, I suppose, under five years. ...

Fisher to Darwin: 29 March 1930

Many thanks for the new edition of the *Descent*. It is a nice little volume, and I agree that it is wise to separate Part I and the general summary from Parts II and III; albeit I value the fact that your father felt sexual selection of such special importance for Man that he chose to treat them in one volume.

Darwin to Fisher: 9 June 1930

I have been rather busy of late *for me*, with the Twitchin bequest,[1] and other minor worries. I have not had time or brains, therefore, to tackle your book in earnest. I have read the first chapter, and turned over some of the papers, with the result that my impression is confirmed that it will be slowly recognized as a very important contribution to the subject. But I am afraid it will be slow, because so few will really grasp all that it means. You must not, therefore, be disappointed at the reception which it receives, but trust to ultimate results. I remember that I criticized to some extent what you said about my father's views, and I think you did make some changes. It rather depends on what is meant by the word 'theory'. I can imagine my father saying, if reading your first sentence about what he accepted, 'but, hang it all, I have not got a theory of inheritance. I wish to goodness I had. Cases like the mulatto show that blending does take place, and other cases show that individual characters are inherited. How and why this is, I do not know. But I have come to see that in 1842 I stressed blending too much.' But this is a minor point, as I say depending largely on one word. I have noted two letters, which I enclose as reminders, showing how fogged people are about evolution, and how a thorough knowledge of the particulate theory would help them. Salisbury's letter[2] is interesting and stimulating. ... Is not the death rate at different periods of growth an entirely erroneous basis for comparison? ... Cunningham[3] is too weak for words. He cannot have ever thought it out carefully. ... The whole theory of growth depends, I believe, on genes acting differently in different circumstances. ...

Fisher to Darwin: 12 June 1930

Many thanks for your letter. As a matter of fact Salisbury's letter had stirred me to a criticism, a copy of which I enclose; it is very much on the lines of your letter, but sticking closely to one central point, in the hope of making it at least clear.

I like immensely your point that the theory of individual development depends upon genes acting differently in different circumstances. I was surprised, too, at the calmness with which Cunningham assumes that all the structure and instincts of worker bees could be ascribed to their solitary ancestor. There is a whole series of reactions connected with swarming, the location by scouts of a new site, the instinct to follow the scouts, and to guide the queen's flight, the swarming itself, the preliminary gorging with honey, which seems to be unknown in existing solitary bees, and very improbable in ancestral ones. Then there is the whole set of behaviour mechanisms, which Frisch has found, by which news of new food sources is conveyed and acted upon always by workers. The only loophole for the Lamarckian here is the possibility of social organization prior to the development of a neuter caste. However, the objection is so obvious that Cunningham ought to have discussed it. ...

I am wondering if *any* biologist will follow the argument of the first chapter [of *GTNS*]. First, because a first chapter is always expected to be not only elementary but trite, and secondly, because we have all grown up in the greatest confidence that we know all about what Darwin meant. I am very tired of having some excessively loosely expressed truism, such that 'all defective deer must be devoured by tigers', put forward as 'the ordinary Darwinian argument', and I believe now I ought in the preface to have hammered in the statement that in biological circles Darwin's views are usually grossly misconceived—though this would annoy many people.

Darwin to Fisher: 16 June [1930]

... I am glad you like my remarks that individual development depends on genes acting differently in different circumstances—but I am not much surprised because I am nearly certain that I bagged the idea from you. I am glad also that you have replied to Salisbury's letter. I feel rather muddle-headed to-day and can only say that I think I understand the situation. The elimination of an individual increases in importance in your three stages A, B, and C.[4] This is also true of the survival of individuals. Hence the proportion of elimination in each stage is of importance. A very high proportion of seedlings is eliminated but then the causes of their elimination have very little to do with inborn qualities. ...

Fisher to Darwin: 21 June 1930

The point of my letter to *Nature* [*CP* 88] is quite a negative one, that one cannot gain any guidance from the mere fact that only one egg in 10 hatches to a larva, and only one larva in 100 pupates and emerges successfully. The fact that of the eggs laid, 90% die unhatched, 9.9% die as larva or pupa, while only 0.1% die as adult insects, cannot be taken to imply that Natural Selection is more potent on eggs than on larvae or on larvae than on adults. You are wanting a much more positive contribution, but I was only trying to show the fallacy in the simple argument stated above, by saying that a freshly emerged adult is, on the premises, worth 1000 newly laid eggs, which serves to counterbalance the apparent disproportion.

Your point about how much of the mortality is selective is of course a much more subtle one, and could not be dealt with by a mere enumeration of the number surviving to different ages. One fairly simple step towards a more positive statement is that if **A** and **B** are two groups of genotypes into which the species is divided, then if I kill one in 100 of group **A**, and none of group **B**, I exert the same selective influence at whatever stage I operate up to the commencement of reproduction; thereafter I exert a diminishing effect, and none after reproduction has completely ceased. To do this I should have to kill (supposing the groups are equal as regards other selective agencies) 1000 times as many new eggs, or 100 times as many newly hatched larvae, as if I killed them off at emergence. That is, I must destroy equivalent amounts of reproductive value, but this statement holds even after reproduction has commenced and up to the end of life, or at least as long as there remains 1% of the reproductive value of genotype **A** for me to destroy. ...

Darwin to Fisher: 24 June 1930

I have been reading your book in a somewhat desultory manner, not carefully enough to make it the basis for writing anything on the subject. I have had some little jobs to do which have taken up my rather small available energy. I should like to try to put in as plain language as I can what I feel as to the value of your work in showing how Mendelism is capable of putting the lid on to the theory of natural selection. I don't want to write about any minor criticisms, and if such occur to me I shall write straight to you. Some day you will have a second edition, and the notes you now make will then be very useful. But you are *not* to trouble to say whether *my* remarks are helpful.

Here is one criticism—page 138, line 15, [*GTNS*, p. 153] '... the dates of the breeding ... could only be stabilized if ...'; my ending would be, 'if there was a period of the year at which breeding would produce a *maximum* number of offspring who would survive till the next breeding season.' The effect of the seasons on births and deaths would, I believe, produce such a result, more especially if selection had resulted in a definite limited number of offspring being normally produced. It would at first sight appear that the beauty of males would be harmful up to the period of the optimum date, because it would tend to make them have first choice and thus breed too early. After the optimum date, beauty would be helpful in making them breed as soon as possible. And it might seem that these two influences would cancel each other. But selection would result in both sexes not wishing to breed before the optimum date, and in such circumstances the beauty of the male would do no harm before the date; whilst after the optimum date, when the desire for breeding had commenced, the beauty of the male would be beneficial by hastening breeding. It seems to me that beauty can be explained thus more or less in the way in which my father suggested. Monogamous drakes, which have no period of eclipse, must not be tempted to mate before the proper time, in spite of their fine clothing. What you say in the next para. is, I think, certainly true, but might make the optimum date a little earlier, I think. I wonder if this is all to the point!! ...

I have just got yours of 21st in answer to my last. ... you must *not* take so much trouble in answering me.

Fisher to Darwin: 27 June 1930

I must write more clearly about the non-genetic early nesting theory, especially as I am sure I got it from you.

Supposing the date at which breeding phenomena are initiated, e.g. by migration etc., to depend on the female only, there must be an optimum date, appropriate to the average bird, for these phenomena to start. The date of starting is partly determined by a heritable variate x, partly by other circumstances. We must suppose x to vary among different females and, to make it more concrete, we might imagine x to be determined experimentally by giving a number of young females exactly the same nutrition and climatic experience and noting the date at which they show the first sign of the reproductive sequence.

Now my first point is that the average value of x must be the same, generation after generation, so that the average number of offspring left by females with a high x, and therefore congenitally prone to start breeding early, must be the same as that of females with low x, congenitally prone to start breeding late. Of course I don't doubt that the medium values are favoured over the extremes, but the net effect of selection on the mean value of x must be zero, if the distinction between a winter feeding period and a spring breeding period is maintained at all. How then can it be that the males who breed early[5] gain an advantage? Partly because more of them breed (this is my suggestion in respect of death-rates), and partly, and this, I think, was your father's theory, because those that do actually breed early (as contrasted with those who are only congenitally prone so to breed) really

do leave more offspring than those breeding later. This is possible if we imagine the actual breeding date to be modified by environmental factors which are also influential in favouring reproduction so that a group of females having identically the same values of x might start migrating at dates from 15th to 30th March, those moving earliest being destined on the average, by reason of their better nutritional condition, to rear the largest families; but no larger, perhaps, than are reared by birds with a lower value of x who start on March 30th.

Here is a chart; 6^4 means 4 pairs of birds each rearing 6 young. If on consideration you think this is a fair representation of your father's theory, I should like to put it in any further editions [of *GTNS*] so as to make it explicit.

You will see that the selection for increasing x due to the larger families of those mated early is exactly counterbalanced by the selection for small x among those breeding at a given date. ...

[Enclosed chart][6]

	Actual Starting Date →									Average
					7^{1}	6^{4}	5^{6}	4^{4}	3^{1}	5
				7^{4}	6^{16}	5^{24}	4^{16}	3^{4}		5
Increasing x ↑			7^{6}	6^{24}	5^{36}	4^{24}	3^{6}			5
		7^{4}	6^{16}	5^{24}	4^{16}	3^{4}				5
	7^{1}	6^{4}	5^{6}	4^{4}	3^{1}					5
Average	7	6½	6	5½	5	4½	4	3½	3	

Fisher to Darwin: 23 July 1930

... I have noted one point where I think you have misunderstood my letter.[7] I am glad you think the table rightly expresses your father's theory. I have made all values of x equal in average fertility, though I should be doubtless nearer the facts if I made the middle values somewhat more successful than the extremes. This would complicate the table by introducing fractions, and I should like to know if you thought it was worth doing to avoid the misapprehension that I am denying the existence of an optimum x.

On the enclosed page I have drafted an argument on which I have long wanted to have your opinion, though I never feel I can express it cogently enough.

[Enclosure]

Suppose you have two groups of men placed in very different circumstances, differing not in the kinds of actions which conduce to prosperity, or in the average prosperity attainable by such actions, but wholly in the certainty with which it is attained.

(A) Every exercise of energy, intelligence or prudence produces with certainty a corresponding increment in prosperity.

(B) The effect of such actions is obscured by chance effects incapable of prediction which, while balancing in the long run, and having no average effect one way or the other, are individually large compared to the average return from the actions concerned.

The contrast is similar to that between an orderly and well-governed country on the one hand, and a lawless or savage condition on the other; it is also similar to the difference between immediate recompense and postponed recompense, for in the latter case intervening events introduce a chance element, e.g. 'Shall I live to reap the harvest?'

Now I am inclined to claim that similar populations exposed to these two environmental systems would react very differently, that a population which in (A) would show itself industrially competent, careful, and prudent, might in (B) show none of these qualities, because the average effects of competent action would be so much obscured by unforeseen chances. Moreover, the psychological differences in the two cases would be much enhanced by example and tradition.

If you agree with this, as I am confident you will, I want to know how far you would think it rational to apply it to the effects of family allowances, and in particular to the inference that such allowances would increase the fertility of the poorest self-supporting class.

To some extent, of course, the economic burden of children must be regarded as distributed from rational considerations. In such cases the parents presumably decide that the satisfaction afforded by the society of the child, or that of doing what they regard to be their duty, is the economic equivalent of the money spent in its upbringing. To a far greater extent, it seems to me that their incidence, or at least its economic effects, is subjectively accidental, and acts just as any other unpredicted cause of fluctuating prosperity. Among the poorest self-supporting class and, indeed, among wage-earners generally, the loss in standard of living occasioned by a single extra child is certainly large compared to any compensating gain which is open to the parents by increased efforts. I infer, therefore, that without family allowances, the incidence of reproduction, whether or not this is excessive, will induce in some degree the consequences of B, and that the introduction of family allowances will change the social reactions of individuals and the social tradition of the group in the same direction as A.

Now with full family allowances equivalent to the actual average cost of children, there will be quite numerous occasions in which prudence would favour family limitation; such things as the health of the mother, or the restricted accommodation of the house will often act in this way; and an unskilled-worker class in which this major chance element in prosperity was eliminated would naturally possess a much more strictly defined idea of what standard of living they could expect, and would be expected of them; if they are therefore more readily influenced by prudential considerations under (A) than under (B), it seems to me far from obvious that we ought to assume any increase in reproduction in this class as the result of family allowances.

Darwin to Fisher: 25 July 1930

It always interests me *much* to puzzle over your conundrums, but I must put this lot aside for a bit. I have been—for me—a bit snowed under with Twitchin

correspondence; for there are financial troubles in several directions. Also a French translation of my little book has been going astray, and I fear time is making my brain no clearer. But I shall have a try before long.

Darwin to Fisher: 31 July 1930

... I am inclined to think that you ought to show the mean values of x in your table more fertile than the extremes. ...

Fisher to Darwin: 7 August 1930

I enclose the sort of thing I had in mind, if ever it seems desirable to elaborate the interpretation I put on your father's theory.

The principal questions are: does it omit any considerations which he would have regarded as essential, and does it introduce any conception which he would have regarded as alien to his views?

For myself I do not judge that he would have objected to the non-inheritance of readiness to breed early, induced by abundant nutrition, even if he were inclined to insist that other efforts [effects?] such as increased size must be inherited.

For your consideration *AT LEISURE.*

[Enclosure][8]

Schematic representation of Darwin's theory of sexual selection in monogamous birds, as interpreted by the author; showing the possibility of a selective advantage of males chosen by reason of superior adornment by early breeding females, without any selective advantage of females congenitally prone to breed early.

									Average brood for given innate proclivity in respect of breeding date
				$(7.40)^{1}$	$(6.34)^{4}$	$(5.16)^{6}$	$(3.86)^{4}$	$(2.44)^{1}$	5.10
			$(7.34)^{4}$	$(6.40)^{16}$	$(5.34)^{24}$	$(4.16)^{16}$	$(2.86)^{4}$		5.28
		$(7.16)^{6}$	$(6.34)^{24}$	$(5.40)^{36}$	$(4.34)^{24}$	$(3.16)^{6}$			5.34
	$(6.86)^{4}$	$(6.16)^{16}$	$(5.34)^{24}$	$(4.40)^{16}$	$(3.34)^{4}$				5.28
$(6.44)^{1}$	$(5.86)^{4}$	$(5.16)^{6}$	$(4.34)^{4}$	$(3.40)^{1}$					5.10
6.44	6.36	6.16	5.84	5.40	4.84	4.16	3.36	2.44	5.28

Average brood for given breeding date

The table shows hypothetical average numbers of offspring reared by females differing in two respects, (a) congenital tendency to breed early, (b) nutritional condition, which favours both early breeding and number of offspring. The indices represent the relative numbers of females in each class, out of a total of 256. Each row refers to a group of females with the same congenital response to the stimuli initiating the breeding sequence, the latest breeders being in the top row, and shows the frequencies of five different nutritional conditions, with the average numbers of offspring reared. Each column refers to birds actually breeding at the same time. The numbers of offspring are adjusted to increase with the nutritional condition of the female in each row, and at the same time to give a small further advantage to those breeding at or near the mean or optimal breeding date as opposed to those breeding late or early in the season. The selective effect upon the cocks is shown in the lower margin of average offspring according to breeding date, those chosen by the hens actually ready to breed early rearing the larger families. The selective effect upon the hens is shown in the right hand margin, there being a slight elimination of hens congenitally prone to breed too early or too late but no tendency to accelerate or retard the breeding date of the whole species.

Darwin to Fisher: 20 August 1930

I have looked at the enclosed again and I really have no criticism to make on it as a representation of my father's views. I guess he would have been a bit surprised that such a complicated explanation was needed. ...

Fisher to Darwin: 11 October 1930

It must be nearly a year ago that I wrote to you that Haldane had attacked Dominance theory on the strength of the dominance exhibited in grouse locusts and the fish *Lebistes*. I thought at the time that his allies might betray him, and give an unexpected support to the theory, as apparent exceptions are wont to do.

So far they have come up to expectations nobly. Of course I need more data to make a complete case, but I think this paper [*CP* 87] may serve to make sure that the necessary observations will be made. I am still quite nervous about my tentative and conjectural last section, because however often one says that he is guessing, there are many people who will take no notice of the difference between a guess and a decided opinion. Personally, I am *sure* that we ought to go on guessing, as intelligently as may be, and if it is an error it seems one on the generous side to do some of it in public.

I feel rather depressed about the Eugenics Society. But I know by experience that I am capable of making a fool of myself, and I suppose I am as liable as anyone else to ascribe that fault to others. This, I believe, ought to comfort me.

Darwin to Fisher: 13 October 1930

Thanks for Dominance theory, which I shall read with interest.

As to the rather dismal last sentence in your letter, I thought you gave the right lead in the right tone at the meeting. As to the Society generally, I am afraid any propaganda is always a difficult and generally an unpleasant job, if any moral questions are involved. That is why it is so generally shirked. We must do our best, and hope for the best, without expecting much comfort out of the job. You will say that my last sentence is even more dismal than yours!

[P.S.] ...

Fisher to Darwin: 15 October 1930

... You have always advocated the claims of propaganda v. research; partly because you think research can look after itself, partly perhaps because our society is worse organized to undertake research than for propaganda. Here is a test case.

A man has developed, by serological methods of admitted excellence, a method of discriminating between samples of blood, even from closely related animals (brothers and sisters, parents and offspring).[9] He is not a geneticist, nor much interested in genetics. I believe, and am willing to put forward at length the case for believing, that his method may lead to a method of discriminating carriers of recessive genes. A good deal of preliminary exploration is necessary, but this will be a direct and progressive approach to the main object, if such an object is in view. He has now undertaken the first step at my suggestion. Now if, as is far from certain, his ability to go on with the work (he is old enough to retire) or to extend it in directions interesting to us, depended on his having an assistant, would you consider it a proper course for the Eugenics Society to provide one?

From the propaganda side you may regard such expenditure as aimed at removing an obstacle to our propaganda or again, making manifestly false the damaging assumption ... that the Eugenics Society is ignorant of the possibilities of genetic research. Obviously, as in all decent research, the object of the grant should be stated in terms of pure science; its success should certainly not be mortgaged.

That is as well as I can state the problem, without details, which I think you will agree do not affect the principle.

Darwin to Fisher: 16 October 1930

... I intended to champion propaganda as against research for our Society only. ... It is because we are the *only* propaganda body, and because I know that that was what Twitchin wanted, that I am inclined to press this view rather heavily. Anyhow, the Twitchin money is coming in badly, and for 2 years we shall have little to spare. Your proposed enquiry seems to me very interesting and valuable, and if money intended for such work was available, I should most certainly like to see the line you suggest pursued.

Fisher to Darwin: 17 October 1930

I think you feel very much as I do that policy should be based as far as possible on a reasoned statement of intelligible considerations, so if I seem troublesomely argumentative put it down to that, but do not trouble to answer me.

... What I am concerned to ascertain is your own feeling, and that only in respect of the Society, and your last sentence suggests that nothing short of compulsion from the testator would make you approve of assistance being given to research, out of the Twitchin bequest or other general funds of the Society. I should regret it greatly if this were your view, but I should be glad to know it, as I am concerned to answer the question, 'Are there any ways in which I can do good through my connection with the Society?'

The claim that research is so much more attractive than propaganda that it can take care of itself would be stronger if one could point to the Galton Laboratory and the Cambridge Scholarship as successfully meeting our requirements in respect of fundamental knowledge. As far as I can see, it is an equally valid objection against research being undertaken by any State or Corporation having material aims (as well as for underpaying such work of this sort as has to be employed).

The fact that abroad and at home this argument has been increasingly disregarded suggests that it does not cover the whole ground. Among other things that it seems to disregard are (i) that there is no sign of diminishing returns or exhaustion of natural resources in quarrying natural knowledge, (ii) even if a fact were bound ultimately to be discovered free of charge, it is often worth much to know it now, (iii) the moral attraction of research to truthful and public-spirited people, which makes it seem possible to get it done at non-economic rates, is also felt by the general public, who even in the most depraved times must in self-defence prefer truth to falsehood, and are not uncertain in preferring to draw their information from the least contaminated sources. These reasons seem at least as cogent for the Eugenics Society as for a business firm.

Of course I confess at once that to reject this claim is not to say that we know how to expend research funds to the best advantage. To the worker it is very often obvious that we do not. I should say that if we make the best use of our experience we could, by examining each scheme on its merits, find some worth acting upon; but it would be preposterous to set out the advantages of a particular scheme before a body which had already decided against it on principle.

Darwin to Fisher: 19 October 1930

Thanks for your long letter about research. I expect that we differ somewhat in views, but not as much as you seem to think. What I now write is only a few first impressions. I shall put your letter aside, and possibly write later. Knowing Twitchin's views, as I believe I do, that we certainly know enough now to move effectively in many directions, and that persuasion is what is most wanted, I do feel strongly about the use of *his* money. As to other money I should feel very differently. If we had a lot of free money, and decided quite legitimately to go in for research to a considerable extent, I should like somehow to divide the organization more or less into two branches, research and propaganda, and in this way to avoid the danger I see of the propaganda being swamped. ... our Review would be a very good place to advocate new lines of research, even if the Society is too poor or itself unwilling or incapable of directing the work. ...

I am rather sorry they picked out an old discontinuous stick-in-the-mud like Punnett to review you in *Nature*.[10] But to get 5 columns is an excellent advertisement. My father would have been much pleased with such a review of the *Origin,* and merely carefully noted the points to answer in his next edition. I think you may be well pleased. *I* never had so long a review.

Fisher to Darwin: 20 October 1930

I ought to have known that my letter would worry you, as indeed I might have foreseen. It was very ill-expressed. I suppose it is useless now to ask you to put the matter out of your mind, so may I suggest that when you do return to consider it, about which I hope you will on no account hurry, you might tell me, what I think would clear the matter up for me, whether you draw any distinction between using the Twitchin money for research, and using it to release other income for that purpose. You will understand that those of us who believe that at least occasional and exceptionally favourable opportunities of furthering research should be seized, might have regarded it as beyond our reach so long as propaganda work was not otherwise provided for, though thinking that when that condition was fulfilled, it became our clear duty.

Thanks for your kind comment on Punnett's review. I think you suggest that a rejoinder would in all probability be unwise. That was my own view before I looked at his statements in detail. I am now doubtful. I enclose a possible letter to *Nature*, which I am inclined not to send in if you so advise.[11]

The distinction I have in mind is between tidying up troublesome trifles now, and leaving serious scientific criticisms to be dealt with later as further facts become known.

Darwin to Fisher: 21 October [1930]

It is true that I am generally opposed to anything in the nature of a controvery in any papers on scientific subjects. My Father always used to rejoice that Lyell had given him the advice to avoid such controversies and that he had always followed it. Your letter, however, seems hardly to come within this description and I have no very strong opinion whether it should go or not. I am in fact not prepared to advise against it. If you send it, it might be worth considering whether it might not be cut down a little and perhaps touched up in a few places. As drafted, you accuse him of misstatements, whereas I think you really mean his errors to come under the heading of 'slighter misrepresentations'. It is so courteous that I do not think he could mind.

When I wrote to you about the expenditure of the Society I think I was rather worried about one or two other things and it was that to a large extent which showed itself in my letter I expect. I should now be inclined to say that if the Twitchin money goes to propaganda and to a fair proportion only of the administrative expenses, I should have no objection to any other available funds being used for research if the Council thought fit. This is written straight away but I don't think I shall alter my mind.

Darwin to Fisher: [*late*] *October 1930*[12]

Here are my first *indecisive* thoughts on the very difficult psychological problem which you set me. I agree that everything which makes the future more clearly foreseen also makes for rational conduct; and that family allowances (f.a.) will have some effect in this direction. This is an *immediate* beneficial result which I had not thought of. ...

I see no reason whatever why the sudden introduction of f.a. should reduce the size of families, and good reasons for anticipating some increases. ...

Fisher to Darwin: 30 October 1930

Thanks for your letter and enclosures; I will only answer now some points of your letter.

I think I agree with your view entirely that family allowances will tend ultimately though perhaps very slowly to exert a direct effect towards increasing fertility in all classes. As you know, I also believe that ultimately, though slowly, it will increase the innate fertility of the well-to-do, and diminish that of the poorer classes. At what point these ultimate effects would balance, if at any point, must depend on a host of different circumstances. What I want to say now is that the effect I am inclined to stress about prudence is a much more immediate one, though a permanent one, to be considered in relation to the initial changes introduced by family allowances.

The general economic prudential motive for birth limitation is, I believe, wholly dysgenic in its effects, but there are prudential motives which I think are eugenic—most notably concern for the health of the potential mother. These motives are, I believe, at present much more active and effective among the well-to-do than among the poor. This contrast is dysgenic, although within each class the action is beneficial. I submit that family allowances would greatly affect the poor, not perhaps much in prudence as you use the term, but in a greater sensitiveness to small differences in comfort and standard of living, and thus introduce a definitely eugenic motive for birth limitation among the poor, and abolish largely or wholly the dysgenic contrast between the effectiveness of these motives among the poor as contrasted with the well-to-do. This is all quite distinct from any general

and, I believe, much slower effect upon the general fertility of different classes.

The important contrast in this respect is between the more and the less healthy; but I believe a beneficial effect would also supervene as between the more or less competent. No one doubts that the management of a family makes calls on general competence, and I cannot doubt personally that the difference in standard of living between couples at different levels of competence is much greater when there is a family than when there is none. If that is so, and family allowances were paid at a rate which on the average allowed an equal standard of living to parents and non-parents, it follows that the standard of living of the competent will rise, and that of the incompetent will fall, with increasing size of family; this supplies a generally eugenic motive which would become effective among the great mass of the population, if they were at all keenly sensitive to differences in standard of living.

I do not object in theory to discussing such proposals as that before the Trades Union Congress,[13] although I am opposed to them. What I do feel is that if the idea of family allowances is introduced to the English public as a political means of catching votes by relieving poverty out of taxation, then we have lost the first round. Perhaps it is inevitable that we should lose this round, but you must excuse me for fighting against it. What I fear is that both its supporters and its opponents will be prejudiced against its proper uses. If, on the contrary, a non-class scheme could be made familiar to the public in the first instance, it would have done much to prevent the adoption, or even the advocacy, of the more ignorant and ineffective variants. ...

Darwin to Fisher: [*early*] *November 1930*

Many thanks for your letter about family allowances. It is all very puzzling, and I feel that you may be right on all points. In short, these psychological problems are so difficult that I must be content to leave the solution to those who will come after me. Anyhow I shall be very ready to back you up in any movement to get contributory f.a. adopted, as the only satisfactory financial method of aiding parenthood. I shall make no complaint however hardly you fight against state systems; but shall continue to believe that to point out the best safeguards in advance might be useful.

I have been reading your Dominance paper, and though it is rather too technical for me, yet I gather that is an admirable example of a theoretical forecast being verified. I think I shall send it to my nephew Charles,[14] in the hope of maintaining his interest in these problems. I said in a paper in our Review that 'the normal aim of natural selection would be to produce' a stable differentiated series. You here prove, I take it, that considerably differing forms can remain in equilibrium as regards survival value; and, if so, is it not probable that this is a very common phenomenon with slightly divergent forms? ...

Fisher to Darwin: 25 November 1930

... What do you think of this? If insanity of a heritable type appears in a family of otherwise good stock, some members will abstain from parenthood from fear they might be tainted, although really free, and others will take the chance of being free although really tainted. Both processes, arising from ignorance, are dysgenic, and could be stopped if it were possible to test the blood of a patient for the defective gene, without its manifesting its defects somatically, either because it is recessive, or because the patient has not yet broken down.[15]

Todd finds he can prepare a serum to which the corpuscles of every chicken, except one for which it has been specially exhausted, will react[16]. If he exhausts for both parents, he finds he has also exhausted for all their joint offspring.[17] I infer that he is detecting primary gene products, for many secondary substances occur in offspring which are not in the parents.

Darwin to Fisher: 27 November 1930

Thanks for yours. It would indeed be grand if a test for a recessive gene could be found, and blood does seem to offer a possibility. But, from the facts you give, I do not see how to get a serum which only reacts to the proper gene. Of course, at first in such cases the answer is not to be expected. But it is a clue. ...

Fisher to Darwin: 28 November 1930

If Todd had exhausted the possibilities of his method there would be nothing left for us to do. But he has not, and I doubt if he ever will. He is not specially interested in genetics, and talks of retiring. He would like some geneticist to take on this aspect of his work, but apart from Haldane, who is doing two jobs and a lot of journalism already, there is no one who could be interested.

Early this year I suggested an experiment to obtain a serum diagnostic of sex, as that is the only visible thing in which his chicks are segregating. In birds the hens are heterogametic, so the thing is to take blood from a number of hens, inject into cocks, draw active serum from the cocks, and exhaust it with the corpuscles of several birds, all cocks, until it reacts to no cocks. If it still reacts to hens, there is a serum for a single factor (or possibly chromosome). Unfortunately, something Haldane had said had discouraged him from looking for a sex discriminant, so though he was, I think, interested, he was not interested enough to fit in a biggish extra job in his programme.

This autumn he sent me a proof of a new paper in which he reported

tests of sera exhausted for different chicks in others of the same broods, and asserted that there was no sex effect. I was so concerned that I tabulated all the cases of positive and negative reactions for the corpuscles of males and females, to serum exhausted for males and females, and was able to show Todd that there was an apparent sex effect in the right direction in all three of his broods, and that in one case it was big enough to be judged significant. This time he was interested enough to cut out the references to sex, and to say that he would do the experiment I had suggested.

Beyond sex, one wants to know, 'Do most genes give an appreciable reaction?' For this, one needs material segregating in single factors, just as any flock does in sex. It so happens that my test flocks for dominance in the wild *Gallus* are just of this kind for, from next year onwards, I shall have lines segregating each in one of 9 different factors. The birds will be smaller than Todd's big Plymouth Rocks, and will therefore yield less serum, but I think this can be got over.

It is quite likely the sex experiment will fail, either because the reaction is too faint to show up with his standard quantities and times, or because there is nothing specific in the female to react to, sex determination being perhaps merely quantitative. In this case, probably, Todd will be disinclined to go any further with this aspect of his work. What in my view is wanted is to offer him a voluntary worker, paid by an outside body, whose programme should be to explore the possibility of detecting single genes.

Darwin to Fisher: 4 December 1930

... I have no very useful—probably useless—suggestion to make about your last point. ... it occurs to me that if you wrote a careful letter and got the Society to send it to the Medical Research Council, it might educate and wake them up, even if it did nothing else. It is, in my opinion, just the job they should tackle. ...

Fisher to Darwin: 5 December 1930

I do not quite know if you will see my point, but I think it would be entirely useless, even if the facilities were offered, to commence research on Man, until (i) the possibility of demonstrating a single gene had been demonstrated in some other species, and (ii) considerable experience had been gained as to the different reactions of different genes.

Only on this basis would it be worth while to set out to build up a test for a specific gene in Man. In Man, too, it may be that a special technique will be needed to circumvent the known isoagglutinin factor, which has a relatively enormous effect, and may not be easy to cut out by 'exhaustion'. I hope, if you happen to discuss it, you will not mention human applications except as a remote possibility.

Half-pay, say £150-£200 a year, would probably suffice to give Todd an assistant with a programme that would suit us, that is, if Todd would take him on. A lot would depend on his getting a sufficient mastery of the technique to prevent a set-back, if Todd should retire. ...

Darwin to Fisher: 6 December 1930

Without having very clear ideas on the subject, I expect that you are perfectly right in thinking that long work on animals would first be needed. I gather you think it would be hopeless to interest the Medical Research Council or the Lister Institute on these lines. One never knows where one strikes oil. They both, I imagine, do work on animals. ...

This is a little problem I have had in my head. Suppose 8 per 1000 are *y* inches in height above the average. Can you say what percentage of the children of persons *y* inches above the average will themselves be *y* inches above the average? If so, does this give some theoretical indication as to the percentage [of children] of mental defectives who would themselves be mental defective? Don't answer if this is a useless idea, which it probably is!!

[P.S.] I suppose the answer depends largely on the amount of assortative mating.

Fisher to Darwin: 9 December 1930

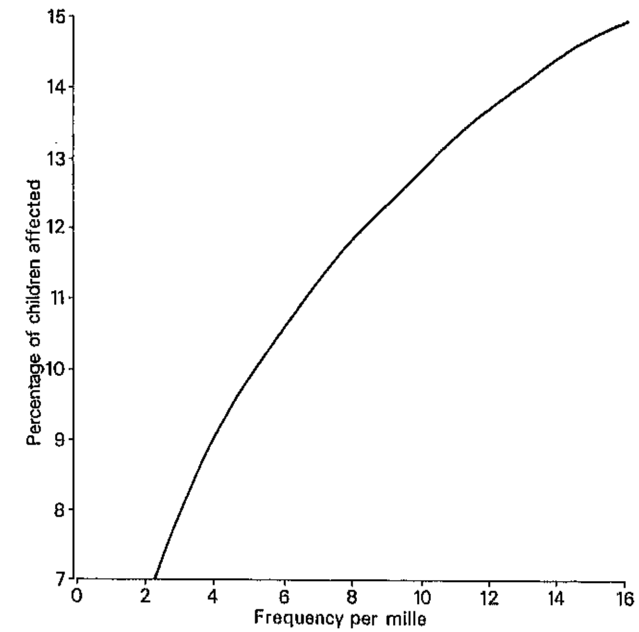
Many thanks for your letter. I must have been writing hurriedly and expressed myself badly. I had only meant that I should not like to try to interest the Medical Research Council in the *eugenic* possibilities opened out by Todd's work. I imagine they are quite satisfied with the value of his work as a serological expert, and are quite prepared to give him facilities for prosecuting the studies he is engaged on in pure (as opposed to medically applied) serology. I believe his discovery arose in Egypt from his work on immunizing cattle to disease; and I suppose the Medical Research Council feels itself justified in expending money on pure research which may make applied serology more efficient. They would presumably give facilities and possibly half pay, if Todd found he could get an assistant to follow up an interesting side line.

Personally I think it is an opportunity of forwarding work of eugenic importance; but I see I have not wholly convinced you, and I am anxious not to be troublesome. So I will not write further on it, unless you decide that you would like to raise the question again.

P.S. Your way of looking at the inheritance of mental deficiency is an extremely attractive one. I wonder why it has not been discussed on these lines before. Perhaps because it leads to rather difficult mathematics.

On the enclosed sheet I have charted the percentage among the children for frequencies of about 5 to 13 per mille, for parental correlation 0.5. I

[Enclosed chart]



calculated the value for 8 per mille independently by another method, and ... it checks well. These percentages give also the diminution of mental defect in one generation, due to sterilization of mentally defective men *or* women. The effect of sterilizing both will be nearly, but not quite double, say 20 per cent at 8 per mille, or 22 per cent at 10 per mille.

A full calculation, involving both parents, would involve frequency tables for 3 variates which have not been constructed. One would need also one further observational fact, namely, correlation between husband and wife.

Darwin to Fisher: 11 March 1931

... Did Huxley ... suggest that to work against individualism was eugenic? I guess not. ...

Fisher to Darwin: 16 March 1931

Yes, Huxley did say that about individualism, but quite tentatively, as an inference from my book. I expected that reaction from Haldane, who has made the same inference, but not from Huxley. I felt, in writing the Chapters on Man, that the great difficulty was to frustrate the people who want to use everything as a handy bludgeon in political controversy, and I reread everything I wrote in that light. It is not only Haldane, but I think also Hogben, who is influencing Huxley in this direction. What a curse it is that some of the few really good brains should be fascinated by sensationalism, but perhaps it would be worse if they all allowed themselves to be quietly ignored.

Naturally, finding class differences to be an essential feature of the dysgenic process in civilized life, I have tried to conceive the possibility of biologically progressive societies in which class distinctions were unknown. At every point this seems to lead to an *impasse.* Man's only light seems to be his power to recognize human excellence, in some of its various forms. From this it follows that actions, powers, and functions cannot be of equal value. Promotion must be a reality, and the power of promotion a real wealth, whether we call our potentates kings or commissars. I do not believe that political capital of any sort can *fairly* be made out of my book; politically biased people will, I think, merely find it inconsistent, and these must be the great majority of readers. ...

Darwin to Fisher: 20 March 1931

Thank you for yours about Huxley's remarks. It seems to me quite illogical, but what you say shows me that you have pursued this line of thought further than I have. I shall keep your letter to re-read if, as I hope, I carry out my plan of re-reading your whole book. It takes not much to fill my days now, but I am always expecting a time of complete leisure. I guess Plato was right in thinking that all babies must be taken away from commissars, or they would pass on their 'wealth' to their children, and the system would break down. I was reminded of my father's words—though not quite to the point—that if a man 'is to advance still higher, it is to be feared that he must remain subject to a severe struggle'. I *think* he put in the word 'fear' in the second edition of the *Descent*, which indicates the kind of criticism he received. ...

Darwin to Fisher: 30 April 1931

I have read the enclosed,[18] and like it very much. It seems to me most valuable at the present time. ...

I wish Smuts could have read it and your book before the autumn. He made the Berg criticism in a very crude form.[19]

I like what you say here better in some ways than as put in pages 1 and 2 of your book. You here divide it into periods. The reader of your book might be led to think that my father held firmly all your (a) to (f) points *simultaneously*, which I doubt. If he were asked in 1881 or so if he believed in the blending *theory* of inheritance, I *imagine* he would have replied as follows:-

I really do not know exactly what you mean by your question. In '42 when considering the indisputable fact that there is normally a good deal of blending when crossing takes place, I was much puzzled because of the apparently obvious tendency thus to wipe out the effects of selection. Now I have to content myself with the *fact* that great changes obviously can be produced by selection, as is proved by domestication. I have *no* theory to account for blending not operating as would seem probable. I wish to goodness I had.

If you were asked to state what exactly was my father's blending theory, which must have fitted in with the reappearance of ancestral qualities and the differences between sibs, would you not find it hard to reply?

I have sometimes felt that one of the difficulties unconsciously felt by critics is due to the half-formed belief that the 'lower' animals have given rise to the 'higher' in an evolutionary process of 'improvement'. All organisms have probably been subject to an equally long period of selective action, and no surprise should be felt at any perfection of adaptation amongst the 'lower' animals and plants.

But all I really want to say is that I hope this will be published somewhere. ...

Fisher to Darwin: 2 May 1931

I wouldn't be sorry to publish something of the kind you suggest, and *Science Progress* would be a distinctly good place, but I did start writing with a view to repairing what is something like an omission from my book. The trouble is, as you see, that while I look, earnestly and diligently enough, for objections to the theory of Natural Selection, the opponents of that theory always fob me off with objections to Lamarckism or to evolution in general, so that I can really find scarcely anything that has any logical place in my book, in spite of the mass of anti-selectionist literature.

I do think, by the way, that you must be reading too much into my first chapter. Your letter reminded me of something C.G. [Darwin] said in his review,[20] to the effect that my argument, had it been brought forward before Mendelism was known, would have disproved selection theory. Now I don't think this is true at all, and as I must have been responsible for the misapprehension, it is up to me to track it to its lair. Tell me if you think I am wrong, but I do not imagine that your father would have found anything new or interesting in my argument, except where I bring in facts which he did not know and had not guessed, and their consequences to an argument with which, without these facts, he would have felt perfectly familiar. The consequences of blending, which I emphasize, do not stop natural selection from working, for, with sufficiently high mutation rates, a supply of heritable variation can be maintained, and I think I could answer your challenge on this point sufficiently to show that your father did believe in these enormously high mutation rates (almost every individual a multiple mutant) in man, and in the domesticated animals and plants; but that, in the case of wild animals, he long kept his mind open to the possibility that they might be for long periods practically invariable and only be made to vary occasionally by changes in their environment. What his final view was on this I do not know, but I have no doubt that after 1860 Wallace did something to persuade him that wild populations were not so constant as he would have been ready, formerly, to admit. Wallace could have produced evidence only of somatic variability and could not have proved that it was heritable.

There seems to me nothing whatever illogical in the theory that heritable variability is maintained by such very frequent mutation, and so the material provided for Natural Selection to work upon, but this theory does open the door to the view that the particular causes to which each mutation must be done (as your father frequently insists) *might* be also important causes of evolutionary change. And though your father could find, I think, very little observational evidence that this was so, against much in favour of the efficacy of selection, yet this possibility he steadfastly kept open. This strengthens my confidence that he had a perfectly clear grasp of the argument I have set out in its esentials, though he might well have preferred other words, and that he was not merely the patient plodding accumulator of observations which one legend makes him out to be.

The case strikes me as remarkably similar to that of Carnot's principle which is the basis of all thermodynamic reasoning. For Carnot developed his theory in terms of the view that the quantity of heat remains unchanged, not knowing that it was quantitatively convertible with work. In bringing in the conservation of energy instead of the conservation of heat Clausius wisely saw that the principle of Carnot's reasoning was untouched, though it led to somewhat different consequences; for Carnot's reasoning was right on the observational facts known to him; and I am sorry that I should have let the point be missed that your father's reasoning seems to me to have been right, even where his premises were wrong.

On the second point you raise as to your father's theory of heredity, would you agree with me if I said that he would have welcomed a view of heredity which could have included reversion, but that, in the absence of such a view, he was willing to accept the fact of reversion, provisionally perhaps, as a principle independent of heredity and possibly due to some entirely distinct mechanism?

Thanks for your point about the lower animals; I must bear it in mind. Certainly Berg seems to think it rather impudence for such a wretched creature as a free-swimming Tunicate to have such a fine 'test'.[21]

To revert to my original purpose, if I can ever produce anything good enough to stand as a review of the difficulties and criticisms raised against selection theory, how do you think such a chapter would go in a German translation? It would be, in its nature, much more provocative, at least to reviewers in a hurry, than the rest of the book, and my wife has just made the scandalous suggestion of using it as a preface, as this is the only part they are likely to read. I have just had a letter from a German anthropologist, working in America, who wants to translate the book into German; quite probably he will fail to find a publisher, but, while the project is under discussion, I should like to know how you felt as to adding something of the kind I have sent you, with perhaps some boldish speculations on the rapidity of adaptive variation in the formation of the great classes.

Darwin to Fisher: 4 May 1931 (a)

... In our Outline,[22] I think *you* put in that 'the Society is strongly opposed to redistribution by means of taxation ...'. Now I have been slowly drifting more and more to tolerate or even approve of such a method of improving the lot of the poor—though my ideas are still very shaky. Now in this *Quarterly* there is an article by Keynes[23] on high wages, which I thought might irritate you nicely. It is true that I neither like him nor trust him, in spite of his being my niece's brother-in-law. But he is very clever, and won't go *far* off the line for any reason. Hence it occurred to me it might do you good to read it. Then read my cousin J. Wedgwood's article, if you have not previously thrown this production into the fire in disgust. Please finally deal with it in that way, for I don't want to see its face again and do *not* write—for I am, I think, going to bore you with another letter tomorrow, on a subject as to which we may possibly agree!

Darwin to Fisher: 4 May 1931 (b)

Thanks for your interesting letter. I am glad to hear of your German edition. I do not see why this new part should not form a preface. It is, I think, very important. Let me know if financial or other difficulties stand in the way of the German edition.

I agree that blending won't stop selection if the mutation rates are high enough. You may be right about my father's views. I incline to think, however, that you have a clear idea of *mutation*, which he could not have had if, as I suppose, the idea of the transmission from generation to generation of quite unaltering hereditary elements had never even occurred to his mind. I remember when, about 1890, I was thinking over Galton's ancestral law, I used to puzzle myself as to how it was conceivable that the average quality of the race could be transmitted from generation to generation as well as particular individual qualities, this being necessary if reversion to the mean takes place. The broad view of the transmission of unaltering genes would, I conceive, have interested my father enormously, as well as all the deductions you draw from it. But you *may* be right in thinking that he saw matters more clearly than I suppose. I shall keep your letter and think over it. It may merely be that *I* did not see it in those days. I think I agree with what you say about reversion to the mean.[24] No more today.

Fisher to Darwin: 6 May 1931

Your two letters arrived together this morning. Thank you for asking me to let you know if financial or other difficulties stand in the way of a German edition. I have not yet heard from my publishers, to whom I sent the offer. ...

I am impressed by the way your father insisted that each variation must have had its particular cause. In saying this I think it is clear he meant by variation, not an ordinary difference due to different heredity, but something new and destined to be inherited, and therefore, to use the modern word, in a broad sense, a mutation. I should guess that he did not think of a mutation as a discrete step, but as quantitatively variable, its magnitude being determined by the intensity of the cause which brought it about, and in this his notion would have been more general, but not less definite, than the modern notion of mutation. One is inclined to wish that he had used some more distinctive term than variation to distinguish the heritable novelty just induced by its particular cause, from the inherited differences between individuals. But I think I see why he did not feel any need for this, for, with blending inheritance and high mutation rates, all differences within intra-breeding groups, such as a single breed of dogs, would be due to quite recent causation, i.e. only a very small fraction could be more than ten generations old. I should be *very much* interested if you are inclined to think over this point of view, for it seems to me an entirely logical position, and fits very well with many of the phrases your father used in writing; I gather, however, that he felt he expressed himself with difficulty, but this was perhaps only because he felt the need of guarding himself against the unintentional misinterpretations which people would put on his words. His spoken words, especially when explaining his dissent from some view, which he felt, rather than saw, to be unsound, might be very illuminating.

Thanks for sending my the Keynes' article. I think I feel as you do about *him*, and heartily condemn his one incursion into theoretical statistics.[25] But he does write well, and is wonderfully clever at characterizing different points of view. As far as I can see, what he says is that as we have got to pay non-economic rates of wages, then we must subsidize industry out of general taxation, and he would like to do this by lowering wages, and giving the wage-earner additional benefit through social services. I don't see any particular advantage in this course as compared to, say, subsidizing the employer to the tune of ten per cent of his wage bill, a process which I, being as you know 'in African darkness',[26] think could be done relatively economically and sufficiently selectively by means of a tariff. This assumes, of course, that an honest tariff is still a possibility.

As to family allowances I do still feel that a state-paid scheme financed out of general taxation would be deadly to the principle of proportionate benefit, unless it were introduced after that principle had become established in separate occupational associations.

In the context of our Outline, I take redistribution by means of taxation to refer only to redistribution as between those with and those without children, and not to taxing the rich for the benefit of the poor. I have just noticed that you say do not write, so now I am done for. Is it too late to say do *not* read?

Darwin to Fisher: 18 May 1931

This is mainly to wish you every good luck on your journey. ...

I am going to write you a long letter about my father's views. I guess you won't want it in America. If I do send it after you, perhaps to read on the voyage home, it will be a *copy*, which if lost will do no harm. ...

[P.S.] Do not overwork yourself *in America*. It is, *there especially*, a *real danger*. Do not mind indulging in many platitudes with your audiences!!

Darwin to Fisher: 24 May 1931

In answer to your letter concerning my father's views on the laws of inheritance, it seems to me that we should first of all look to his theory of pangenesis; for that was his theoretical way of accounting for what he believed to be the facts. We may be disinclined to tread this path once more; for we know in advance that we shall again reject his theory as a whole. Nevertheless, it is the best way, I think, of ascertaining what were his underlying thoughts. If I quote numbers, they will be the pages in *Animals and Plants under Domestication* II, second edition.

According to his theory, each cell gives off gemmules, and 'several gemmules are requisite for the development of each (new) cell or unit' (381), presumably some of them coming from each parent. Though not, I think, clearly stated, I gather that he held that these *necessary* gemmules blended completely, so that the parts, when fully developed on such a basis, in their turn, gave off gemmules which 'ultimately developed into units like those' (370) formed by this process of blending. This blending of the gemmules, though complete, was not uniform; for at all events the gemmules 'derived from one parent may have some advantage in number, affinity or vigour over those derived from the other' (382). These gemmules are also held to be 'capable of transmission in a dormant state to future generations and may then be developed' (370). For both these reasons, organisms will only be 'generally nearly intermediate in character between their two parents' (395).

Now the more unequal the blending of the formative gemmules, the greater the 'host of long-lost characters (which) lie ready to be evolved under proper conditions' (369), the more 'incessantly' (394) reversion acts, the longer would it take for complete uniformity to be created in any inter-breeding group. 'After a longer or shorter period, the species will tend to become nearly uniform in character from the incessant crossing of [the varying] individuals' (262); but the time might be fairly long. And all this time selection would go on whether or not any other changes were taking place. Moreover, 'the dissimilarity of' sibs and the greater variety of sexually-produced organisms would be accounted for by the 'unequal blending of the characters of the two parents' and by the 'reversion to ancestral characters' (239); but all this nevertheless points to a slow but complete process of blending.

In order to account for progressive evolution, he held that the surroundings of an organism affected its development and that, normally, the gemmules subsequently thrown off represented the form thus produced. The transmission of latent gemmules for many generations merely threw back the environmental cause into the past. With 'absolutely uniform conditions of life, there would be no variability' (242) nor any progress.

In passing, may not all mutations be due to some environmental cause? The regularity with which they appear in *Drosophila* no doubt tempts us to look on it as an inevitable natural process. But the more uniform the surrounding, the more regularly will exceptions appear, as a rule. If the mutations in that fly are due to thunderstorms, they would come in one year about as much as in another. And our knowledge of the effects of X-rays on organisms and lightning on our wireless sets makes this illustrative supposition less ridiculous.

My belief is that my father had no clear distinction in his mind such as we have between mutations and Mendelian differences. All variations were much the same to him though I think he says somewhere, I cannot say where, that some are not inherited. Otherwise, in identical surroundings, variations identical in form differ in hereditary effects solely because of differences in the latent gemmules which they carry. The amount of variation depends in the long run on the amount of exceptional environmental effects; this being, he held, proved by the effects of domestication. I think there is more truth in this last contention than you apparently do. But his system allowed for nothing like the accumulated effects of mutations.

In pangenesis an effort was being made by my father in the direction indicated by his letter[27] of 1857, so it seems to me. Fertilization, he then said, is 'a sort of mixture and not true fusion'; and, according to pangenesis, the ancestral gemmules whilst latent do not fuse. This was a step in the direction of Mendelism. He held that 'each minute element of the body' had an 'independent life' (365), and if he had not come under the influence of a few Lamarckians whom he admired so much, he might have hit on a different idea of pangenesis. He might have seen that each cell contained a basis more or less capable of causing the development of the whole organism. He did realize that hybrids, when united, led to reversions to either parental type (395), his explanation being much the same as ours. 'Reversion depends on the transmission from the forefather to his descendants of dormant gemmules' (399). Replace 'dormant gemmules' by 'recessive genes' and we have pure Mendelism. He felt bound, and he was bound from his point of view, to make his theory cover the inheritance of acquired characters and telegony. Pangenesis is, I hold, a logical system, and being 'provisional' it should only serve to indicate his general beliefs. It is hardly more difficult to believe than Mendelism, when we realize what marvellous powers we attribute to the genes of a single cell, enabling them to control the whole growth of an organism through all its many phases. But pangenesis fails to account for the numerical effects discovered in connection with Mendelism, for correlation coefficients, and for the accumulated effects of variation, and it must, therefore, be rejected. ...

All this, however, if accepted, does not seem to me to necessarily alter your views materially. The blending which my father believed in was, I think, less rapid than you seem to suppose; and I do believe that the great variability of domestic animals is partly due to mutations having been exceptionally frequent under domestication.

Possibly we should take a leaf out of my father's book and be sure that our theory accounts for all that we hold to be true. A graft of a plum, which seeds truly, if placed on another stock, loses this power and becomes variable (247). Sometimes 'more than one spermatozoon is requisite to fertilize an ovum' (356). Tubers of a potato 'produced from a bud of one kind inserted into another are intermediate' in their qualities (360). We may yet have something to learn.

P.S. I am inclined to think that 'definite variations' are best translated by 'acquired characters' and 'indefinite variations' by 'mutations'—only no one then suspected the possibility of the segregation of 'genes' unaltered.

Darwin to Fisher: 10 September 1931

I am very glad to hear that your American tour has been so *very* successful—as I call it compared to what I had expected—for I have known of considerable failures. But I am sorry for the fly in the ointment.[28] I forget if you have ever had an operation. If so, you will know that the operation in itself is *nothing* at all. You know that mine was a big one, but I had *no* local pain, only want of sleep, headache, and discomfort. I hope you will be in good hands. ... I wish I could be of any use to you. *Do* let me know if I can be.

I send you a letter written to you in May, which I did not post, as I did not want to bother you then. ...

Well, no more today, except to wish you good luck.

Fisher to Darwin: 15 September 1931

Many thanks for the letter you wrote to cheer me up before my operation. I had not had one before and was quite three parts anxious. What you said encouraged me greatly. I believed it about half, but it seems to have been quite true. ...

Your letter of May 26th I shall postpone until I can read Pangenesis again and get the context of your citations. I can see that I shall value it greatly.

Do not lose any good opportunity of letting me hear your ideas old or new. I have been learning bit by bit that there is generally the germ of something uncommonly well worth thinking about, in what you say. And from my end this is a good opportunity. ...

Darwin to Fisher: 26 September 1931

... I have had a niece here, who is somewhat interested in science. She said, I understood, that there are some distinctions between the *species* of plants, inland and seaside, which are paralleled by the differences in the individuals of the *same species* when grown in the two situations. It seems to me that this latter power of adaptation to different environments by the same species must have arisen much as you account for the origin of dominance. ...

Fisher to Darwin: 29 September 1931

... I had never thought of the plants in quite that way, though I think I know the phenomenon your niece referred to. So the analogy of dominance struck me as new and illuminating. If I have the point right, you may have plants growing in a salt marsh with fleshy leaves and stunted growth, while others of the same species, growing in soil or fresh water, are tall and thin-leaved. The succulent habit of growth is characteristic also of whole species confined to salt situations, but when the succulent plants are transplanted and grown alongside fresh-water forms they revert to the fresh-water habit. Even closer still, Turesson has shown, with I forget what species, that some of the succulent plants are merely modified, while others, growing in the same site, are genetically succulent in that they and their offspring maintain the habit in culture. In this case the plants with halophytic genotypes serve to show that the habit really is adaptive, and enjoys a selected advantage, which is, however, gained by other members of the same species, by an adaptive modifiability. I take your point to be that the adaptive modifiability must have been acquired by the selection of modifying factors, which alter the differential reaction of the plant to its environment, without affecting its habit in fresh water, just as the dominance modifiers alter its reaction to a particular mutant gene, and that the selection in favour of these modifying factors is only effective in individuals containing the mutant gene, or growing in the salt situations. This method of adaptation favours the cohesion of the species, whereas the alternative method adopted by plants which are thick-leaved *wherever* they are grown, would tend to favour its fission; and if the salt marshes were very extensive, and largely isolated, might ultimately produce a distinct halophytic species. Let me know if I've got the point right as you only give me a hint in your letter. ...

I had a letter from Ford by the same post as yours, in which he gives me an amusing account, though at second hand, of the population discussion in Section D.[29] Huxley was good enough to speak in my place, giving an account of my views, and this seems to have stung MacBride into some wrathful allusions to the valuelessness of the opinions of mathematicians *vis-à-vis* biologists. At this there was applause, Baker loudly shouted 'Shame!' and there was applause from another section. Ford expresses great indignation against MacBride, the more so I think, because MacBride evidently has some support, at least for this part of his views. I feel myself that it is very well worth while to be abused if it gets my book read and criticized.

By the way, I was very much surprised to hear from the Oxford Press that the Natural Selection book has sold even better than my one on Statistical Methods did. It was so long before I heard from them that I had quite made up my mind that it was one of those books which everybody praised and nobody read, and would have no influence on biological opinion. I think this is still its danger, but the sales must mean something. ...

Darwin to Fisher: 30 September [1931]

... You have understood what I meant about the seaside plants. But you must not trust to me for facts.

I was much interested and amused at Ford's account of the British Ass. meeting. You need mind it no more than my father need have minded the Bishop's attack at the celebrated meeting at Oxford. ...

Darwin to Fisher: 2 October 1931

... I am inclined to hold that Lamarckism is doing more harm than you seem to think. But I may well be wrong. It is more or less backed up by many men of science, and, so long as this is true, the public will consider it a main factor in evolutionary changes, and will not realize the importance of selection. I can do little these days, but I shall write a letter saying what I want, to an imaginary correspondent, and then consider what to do. I want a book to show how little Lamarckism could do in evolution, even if it is a real factor, and therefore how in any case we must rely on selection for progress. It is the moral of your book, but I want it more definitely brought against Lamarck. ...

If you ever again consider seaside plants, see *Origin*, Chap. V, end of second para. It is quoted as a case where 'conditions seem to have produced' a slight definite effect. ...

Darwin to Fisher: 1 February 1932

As I think I mentioned before, I have been writing some notes on evolution, etc., and I am going to ask you not only to read them, but, what is more difficult, pick them to pieces as much as you can. I have been doing this mainly to interest myself, as I now find writing difficult, and in consequence the results generally unsatisfactory. ... I am also sending you a few separate notes in my own handwriting. Will you also kindly scribble your remarks in pencil opposite them also, and return. As to the typewritten stuff, I may add something as to the way in which the way I have presented the evolution problem reacts on eugenic propaganda. I feel nearly sure that you, and possibly a couple more, will be the only audience for these two efforts. ...

Fisher to Darwin: 5 February 1932[30]

... Regression, as the word was used by Galton and the Biometers, i.e. regression to the mean, must have at least three contributory causes:-

(a) If the relation of the child to only one parent is considered, regression is due to the contribution of the other parent, for the reason that tall men will on the average have not so tall wives, reckoning tallness in each sex from the mean of that sex, and in the same sense a selected group of short men will have wives who may be below the average, but will be not so short as their husbands. To avoid this obvious cause of regression Galton was led to use the 'mid-parent'.

(b) Non-inherited fluctuations due to environment will cause a group of parents selected for height above the average to have more than their share of those whose stature has been enhanced, and less than their share of those whose stature has been stunted by environmental circumstances. Their children, therefore, if reared on the average in an average environment, will be shorter than their parents for this reason. As far as I can judge, this makes a very unimportant contribution to the regression observed.

(c) The main regression from the 'mid-parent' in man seems to be due to dominance, which may be regarded as similar in its effects to environmental fluctuations, seeing that it, like them, disguises to some extent the genetic nature, so that we select a little amiss, and do not find the whole of what we saw in the parents reproduced in the children. ...

I don't feel that we can reject the notion that some qualities making for genius have been harmful. Some geniuses have had so much common sense that one can feel pretty sure that had they been born a medieval serf or a primitive hunter they would have made a very good job of it. But, without a lot of ballast, I should certainly guess that there are qualities, such as introspection, or an excessive concentration on apparent logical contradictions, which may make for greatness in a musician or a mathematician, but which at the same time may have been harmful during the greater part of human evolution. But I do feel these weigh rather light in the balance against the great qualities which make a mind energetic, persevering, and penetrating, and I should guess that these had only been disadvantageous in very exceptional circumstances. I suppose the great difficulty is to allow for the enormously greater facilities which civilization offers for utilizing special and limited gifts, so that if they hit off the temper of the age they are appreciated, like those of Praxiteles or Nelson.

... [Have] we any right to say that the geniuses stray further from the pile than they ought to do by chance? In the Stone Age, I suppose your father might have been a great trapper; perhaps no one would have selected his flints for chipping more carefully, but if carving goddesses in ivory had been all the rage, would he have been able to do much more for his tribe than to leave a few level-headed aphorisms for them to ponder over? ...

Fisher to Darwin: 18 February 1932

I am sorry that I have delayed in returning your typescript on Evolution and Eugenics. I have written in a few verbal suggestions, none being of any importance; in fact I should have written before if I had not found myself is such full agreement with almost everything you say.

The whole contrast you open up between the rapid changes which can be postulated when one or more gene substitutions offer a definite and universal advantage, and the much slower process which you suggest, if I have taken you right, when the advantage is fluctuating and contingent upon a considerable complexity of other conditions, genetic or environmental, is one which I much want to get my own head clearer about. I think you may have formulated it as well as is possible, but there may be other ideas unexpressed which I could take hold of as a stronger clue.

I am not quite sure if it is safe to class the great differences between orders and classes entirely in the group of differences which have arisen very slowly. Of course I agree in the sense that the bats have been so long differentiated from terrestrial mammals that whatever very slow changes are at work in their organization have had time to modify them considerably, and that these changes, being principally or wholly adaptations to their peculiar way of life, will characterize all or nearly all the order, and so be among those characteristics of the order to which a systematist might attach importance. But I should like to keep a mind open to the possibility that what might be called the primary features of the differentiation of the group, the development of the wings and the habit of preying on insects in flight, might have been developed quite rapidly, if we take an evolutionary scale of time.

Existing differences between species of the same genus must often have taken a million generations in their evolution, so that any great change taking place in, say, 100 000 generations would be from an evolutionary or from a geological standpoint extremely sudden, and I much hesitate to say what could not happen in this time, if the environmental conditions imposed a powerful selection in any one direction.

I do not know what are the morphological relationships of hair, but I suppose it is homologous either with scales, or with some structure of the skin between the scales, either in existing reptiles, or at least in those from which the mammals descended. Supposing such rudiments to have existed in an animal in which it was occasionally of great importance not to lose heat rapidly, I cannot convince myself that it would not have a very good pelt inside a hundred thousand generations, or that during this same time, if activity during chilly times of the day or year continued to be important, it would not have gone a long way in reorganizing its circulatory system, and worked in a number of mechanisms for regulating its temperature. But even if you say that a hundred times longer would be needed for developing the primary distinctions of mammals, the paleontologists would still find that the group arose with great suddenness in the history of the rocks.

I think this must be partly because the evidence is based solely on bones, so that it is after all only a guess if we decide to think that the dinosaurs were cold-blooded, and not protected by something like hair. With this limitation of material one is forced to attach inordinate importance to any osteological feature which is exceptionally constant in a whole range of species. But that constancy, I suspect, is most often due to its being ordinarily so unimportant, or so equally suitable to a great diversity of associated structure, that it has never been materially modified, not to its being at all remarkably difficult to modify. I may be wrong, but I should be inclined to guess that it would take no longer to breed a marsupial without the characteristic inflexion of its lower jaw-bone that it has taken to produce a bull-dog.

Now if osteologists are forced to base their principal conclusions chiefly on features of this kind, which happen to have characterized from the first the parents of great radiating groups, they must often be stressing features which have arisen quite rapidly, and in a sense casually, in the sense of being slightly useful to the parent species at a time when its food happened to have some peculiarity, not in the least representative of the types of food prevailing among its descendants. In fact, if a fragment of an animal were discovered in the ancestry of the mammals, which in its major physiological adaptations was really a reptile, I imagine it might be described as unquestionably a mammal from osteological features of exclusively taxonomic importance.

So you see that, from being so far a heretic on the fossil evidence, I am debarred from relying on it in support of what I certainly think may be true, i.e. the rapid origination of the primary distinctions of great classes.

I believe there is a fly whose larvae burrow in human skin, which lays its eggs not on its victims, but along the long legs of a gnat, which it catches for

the purpose, with the result that the gnat runs the risk of being swatted, and the larvae, stimulated perhaps by the warmth and moisture of the human victim, rapidly hatch out and burrow into his skin.[31] I suppose this group of instinctive and physiological modifications may have been elaborated in the last half million years. Whatever brought that about should find no great difficulty in a little problem like adapting a small mammal for flight! However, one's judgement is weak about immensities, and rhetoric is no substitute.

Darwin to Fisher: 22 February 1932

I have not yet sucked the whole juice out of your two letters, so I shall keep them by me for another suck. It is good of you to take so much trouble. If what I wrote stirred you up to reconsider these problems, why it has done something, and may rest content for a bit or for ever in its drawer. ...

Darwin to Fisher: 5 March 1932

I see in today's *Nature* a review of Hogben by Haldane.[32] It reminds me that I have not yet read that book. I have rather definitely avoided doing so, because I hear that he attacks me unfairly. I don't want to be tempted into controversy which, you know, I hold generally does more harm than good. But I think I must anyhow read it soon. At the end of the review in *Nature*, Haldane speaks of the 'unfortunate breach between genetic research and eugenic propaganda'. To what does he allude? Pearson? If not him, what? Haldane has, I think, misrepresented my views, and he may have a down on the Dean[33] and me. But I can't see how we are opposed to the conclusions of genetic research. I see that Haldane is publishing a book on The Causes of Evolution which ought to be interesting. He is, I am glad, generally civil to you.

Do *not* answer this unless the spirit moves you. I am an idle man now. But I may bore you with another letter on evolution some day.

Fisher to Darwin: 7 March 1932

I was quite puzzled as to what Haldane could mean, but after the general anathema of his opening I took it to mean, 'all these old boobies have done nothing but confuse the subject, but now that Hogben and myself are interested there will be some progress.'

I am on quite civil terms at present; he is Chairman of a Committee on Human Genetics of the Medical Research Council, of which Hogben and self are other members. I think I can do something there to forward Todd's work, of which I remember writing to you some time ago[34] (blood tests). I have had to slang him (Haldane) to some extent recently in the *Proc. Camb. Phil Soc.* [*CP* 95] where he has written rather foolishly on some points in theoretical statistics, but I do not think he will take offence.

I am heartily glad of his election to the Royal, for he is shoulder above most geneticists in this country; but he is oddly unreliable, chiefly, I think, because he never knows where he is an amateur and where an expert.

As to Hogben's book I think you would find it cleverly derivative, but superficial, especially in its appearance of originality. But I have only glanced at it.

Darwin to Fisher: 29 March 1932

... I have had to, or did refuse two invitations lately which I was sorry for. One was to take the chair when you discuss Family Allowances [*CP* 100], a discourse I should particularly have liked to have heard. For a few days I have been a bit below par, which confirmed the impression that these things are not for me any longer. The moral is not to take up a new subject when about 60 years of age; for there is then not time to do all one would have liked. I only *thought* about eugenics before I was 59. Up to 55 I regarded my life as more or less of a failure, whatever it has been since.

I forgot to say how glad I am that you are on the Committee with Haldane, Hogben, & Co. It will give you some practice in wheedling to get that lot to move in the right direction. I have just finished Hogben's book, without getting much out of it. ...

He follows you about Darwinian blending inheritance, a subject which I discussed in a letter to you about Pangenesis. I was interested in noticing in Chap. II of Galton's *Natural Inheritance* that he himself seems to have *originated* the phrase 'particulate inheritance'. Again in Chap. XII, 3rd para., he writes as follows: 'I need hardly say that the idea, though not the phrase of particulate inheritance, is borrowed from Darwin's provisional theory of Pangenesis ...' Now if the idea underlying particulate inheritance in the mind of the coiner of that phrase was borrowed from a Darwinian theory, it seems hardly historically accurate to use that phrase to imply what is *not* Darwinian. I suggest that segregating and non-segregating inheritance would be the most useful contrast. My father believed the gemmules could be transferred for innumerable generations unaltered, but I gather that he believed that when once actually married to other gemmules in order to form a cell, some blending took place, and at all events their individuality was lost. He used to point to a scratch on the back of his little *white* dog Polly, and point to the *brown* hairs there as the ancestral gemmules being called into action. But no one, as far as I know, before Mendel ever gave the slightest hint that the hereditary elements could unite in marriage and then separate out quite unaffected. That seems to be the distinction of Mendelian from all previous theories.

Hogben certainly quotes me most unfairly. But Huxley mentioned it in his review of the book;[35] I shall take no notice of it; I am fully content to leave it at that; and I hope my friends will do the same. You will have plenty of chances of saying a word or two about any *theoretical* points where you find you differ from Hogben; and I am convinced that any such quiet hint does more good than all the published controversy *in ephemeral* literature. *All* that is *remembered* is that there was a row of some sort. My father's view was *only* to answer in a book when an answer was worth while. I say this because I own I was much inclined to have a dig at Hogben myself for some weeks after I read what he says of me. And I still should *enjoy* giving him one in the eye!!

[P.S.] Don't answer till and if the spirit moves you.

Fisher to Darwin: 23 April 1932

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.[36] You must have been tempted to think me intolerably inconsiderate in sending no acknowledgement in the meanwhile, but it always seemed that the acknowledgement would be followed at once by a proper reply, and would have been a mere nuisance. ...

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 realize 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. ...

Fisher to Darwin: 22 September 1932

... I hear, by the way, that Pearson is retiring, though I have seen nothing officially. I think you rather doubt if I would do any good with that place,[37] if it were offered me, which naturally Pearson is much interested in avoiding. I think I can see possibilities of getting some real work done, but I am not very confident.

Darwin to Fisher: 23 September 1932

... As to the Pearson affair, I have heard nothing. I certainly had no intention of giving you the idea that I thought you would not do good work there. But I did not think you would get the job, and I wanted to discount your disappointment. ... I have always wanted you to get a professorship, though I don't see where. You ought to have got Hogben's job.[38] I *now* wish you had put it regularly, so that some of us could have had our say. I don't remember what I said then. Anyhow, you can go on writing without such a position, and I feel certain you will continue to add to your reputation. No more today.

Darwin to Fisher: 10 October 1932

... I have been writing some notes on my old theme, the similarity of useless characters, which I shall send you some day, so as to have an audience of one. The point that has come home to me with force, and which I had hardly perceived, is that in nature mediocrity in all characters and fitness are closely correlated. ...

Darwin to Fisher: 12 October 1932

Thanks for your address [*CP* 98]. I cannot remember reading it before, though I may have done so—I think the style very good, and, as [T.H.] Huxley used to insist, that does tell even in science. I like it all. I am glad that you speak of my father's 'early thought', possibly in deference to what I have urged. He certainly conceived a particulate form of inheritance, but one that did no more than mitigate blending. Haldane and MacBride—an odd combination—both say that my father thought that variation takes place 'in every direction' (H. p. 139).[39] I wonder if they have any authority for this. I see Haldane (p. 59) agrees that mutation rate varies with external conditions. There was, I believe, that amount of truth in my father's views about the effects of domestication. These are, however, but idle thoughts.

Fisher to Darwin: 14 October 1932

I am glad you liked the lecture. I think the audience enjoyed it, though I do not know if any mathematicians were present to react to what was intended as a stimulus.

Thanks for rousing me about 'in every direction'; it is just the kind of statement I might make myself, though I do not suppose your father ever used the phrase except to say that he did not assume this. But, what does it mean, and (that answered) is it a plausible guess?

One thing it might mean is that an engineer having a full knowledge of the working of the organism and of the process by which it grows, might suggest some physically possible modification, such as that a mouse should have spiral whiskers. Should we say (a) that we should permit this suggestion only provided that he has assured himself that the modification he has in mind could be brought about solely by a redesigning of the collection of genes, or, imposing a more severe restriction, (b) that it must be brought about by a physically possible substitution of a single gene, or (c) that the transformation of the old gene **A** into the required new gene **A**′ must itself be a physically possible process? On the third restriction it might I think be argued that, since all physically possible processes must have a finite probability of occurrence, there is a non-zero mutation rate in all possible directions. Some changes satisfying (b) could I suppose be brought about by a succession of changes satisfying (c), but if some could not, it would be reasonable to say that these are directions in which variation does not take place. Can your father's view be fitted somewhere into this framework?

A priori, I can see no escape from the view that mutation rates must to some extent be affected by environment, since I suppose all organic activities are. Nevertheless, I do not believe that your father would ever have ascribed the great variability of domesticated races to the effect of their environment on their mutation rates, had he not thought that variations were continually dissipated by blending. His deduction was, I think, the right one from the wrong premise, though wrong in itself.

Darwin to Fisher: 15 October 1932

Thanks for yours; but don't answer this. To make *my* meaning more clear—though that is of little consequence—you *have* convinced me that my father was influenced by the idea of blending. Where I am inclined to think you wrong is in not, as far as I see, admitting the *amount* of mutation caused by domestication. ... Then I think you believe that my father had a clearer idea of a mutation than he really had. I see no signs of his believing that his gemmules ever changed. His mind was often expressed by the words of his quoted by Haldane—'what the devil am I to think'—though not quite in this connection.

Darwin to Fisher: 24 October 1932

I am writing this for my own amusement, so take no more notice of it than you feel disposed. ... My father's theory of heredity was definitely particulate. I should like to ascertain if Johannsen ... when he invented the name 'gene', had pangenesis in his mind at all[40]. ...

Darwin to Fisher: 28 October 1932

This letter will make you swear all Sunday. So much the better, for then you will be in the mood to write 'bosh', 'silly', and other suitable comments in the margin of the enclosed draft of a letter to *Nature.* Seriously I should like your views as to whether it is wise or foolish to send it. I care but little what happens, for I have had the amusement of writing it. I suppose I ought to get it typed if it goes.

I have looked at Hogben[41] on Haldane again and am still puzzled. But I must confess that it gives me the uncomfortable feeling that I do not understand it well myself—I mean the action of natural selection. I shall have to reconsider my views on the uniformity of useless structures, so you are saved from that infliction for the present.

[P.S.] It is rather a shame to make you my confessor on all these matters.

Fisher to Darwin: 29 October 1932

I am sorry you have bothered to read again that long screed of Hogben's as in my opinion it wanders constantly from the point nor is it in any sense a good criticism of Haldane's book, even when it happens to be talking about it. The lectures which Haldane gave to the Welsh agricultural students must have been good and stimulating lectures, though, even in lectures, fewer personal opinions or more reasons for them might have been worth while. But the book they make is structureless and loosely written so that almost every statement made in one place may be found contradicted in another, or is so ill defined that one scarcely knows what there is to contradict.[42] Altogether it was an utter disappointment to me and I understand also to a number of geneticists both in this country and America.

I didn't know Hogben had yet written a book on Natural Selection, but I suppose he inevitably will, and then Haldane, if the spirit of gratitude is in him, had better do his best to puff it.

As regard the word 'particulate', I took Galton's term, in the sense in which he used it, to contrast hereditary carriers which maintain their identity, whatever company they keep, with the other sort of hypothetical carrier. I had noticed the term 'unequal blending' and suppose it to mean that a greater mass of Type A blends with a lesser mass of Type B, but I never could see that there was a clear conception behind it, else, if there were such an idea capable of resolving the contradictions to which the blending theory seems to lead, it ought to have been developed very fully and explicitly. ...

For my part I should not have chosen to use the term, which I wanted solely for the purpose of developing the argument on mutation rates, if I had thought that in Galton's mind it connoted something like pangenesis, in which, if I understand it rightly, the gemmules have no continued identity beyond a single generation.

So you see I see very little point in your sending your letter to *Nature*, but this may be partly because I am writing after a bad night, with a heavy cold in my head, and should not be very easily pleased with anything.

Darwin to Fisher: 1 November 1932

Thanks for taking the trouble of reading my draft letter and writing about it. I daresay you are quite right in all you say. There was one sentence I boggled at, however, You say that you understand in pangenesis that the 'gemmules have no continued identity beyond a single generation'. I believe that he[43] held that they might be 'dormant' for a great many generations. So either I or you have misunderstood the theory. If I have, it is of no consequence whatever. ...

Fisher to Darwin: 2 November 1932

I surrender unconditionally about dormant gemmules, and they are essential to the theory. In view of the considerable supply of fresh gemmules brought in from every organ in each generation, I suppose you would take it that the elimination of functionless gemmules would be rather rapid, I mean a goodly percentage, 5 or 10 per cent at least in each generation. Or is this not intended?

Darwin to Fisher: 3 November 1932

Thanks for yours. It would be mere guess-work on my part to say how rapidly he—my father—thought the dormant gemmules disappeared. In *A* and *P* [*The variation in animals and plants under domestication*] II, 2nd Edition p. 369, he speaks of a 'host of long lost characters lie ready to be evolved under proper conditions'. I think he would have said he was merely groping his way towards the light in his *provisional* hypothesis; and I think he was groping in some respects in the right direction to get out of the difficulties which you correctly show that he had felt. But he could not escape as long as he had to account for his beliefs in regard to use and disuse; and it is segregation which has done most to clear the situation.

Fisher to Darwin: 4 November 1932

Thanks for your letter. With regard to the arguments in your father's mind prior to the *Origin*, I found rather an amusing thing recently, namely [T.H.] Huxley putting forward as one of his main criticisms, on first reading the *Origin,* a problem which your father must continually have considered and resolved in his own mind during the previous fifteen years. You will remember that I am inclined to reverse the common conception of your father's attitude towards use and disuse, and other supposed environmentally induced modifications. It is, I think, usually supposed that he accepted these either as probable *per se*, or on the strength of such evidence as the relative weights of the wings and legs of wild and domesticated ducks, as he subsequently adduced as possible examples of such effects. On the contrary, I believe he felt forced to admit that *theoretically* the environment must be capable of producing evolutionary modifications *because* it must be immediately responsible for the large amount of variation which can be readily observed, while, practically, he was continually impressed with the conclusion that such direct effects have in fact only been of slight or occasional importance; and, therefore, that the great mass of mutation produced by environmental causes must be unbiased in direction, *'mere variability'* as he sometimes says, such as might well arise from the irregularity of disturbance of the working of the reproductive system. Huxley is therefore only raising a very familiar point, reflecting, one might say, fragments gleaned from Darwin's own argument, when he writes, 'And second, it is not clear to me why, if continual physical conditions are of so little moment as you suppose, variation should occur at all.' This is in Huxley's *Life and Letters,*[44] vol. 1, p. 254, but it is almost the only point of scientific interest that I have found in the whole volume. I suppose his son[45] thought the public would not be interested in scientific ideas. ...

Darwin to Fisher: 6 November [*1932*]

... I should like to see a question set at a Cambridge Science tripos asking what [T.H.] Huxley had done for science. I wonder what the answers would be like. He threw stones at religion and talked philosophy, the latter being in my humble opinion not at all sound. On the other hand we found him very delightful company and most amusing. My father must have read his grandfather's views on Lamarckism but they seem to have left no impression on his mind. Does not this fact fit in to some extent with your views as to the development of my father's mind? ...

Fisher to Darwin: 9 November 1932

Thanks for your note on Huxley. I had not before understood what you thought of him. So the son may not be to blame. Thanks, too, for the point about Lamarckism or rather Erasmism, as I suppose your great grandfather antedated Lamarck. It would seem inexplicable if your father had attached the importance to use and disuse (though never to 'slow willing') which he is sometimes represented as having done, that he should not have been impressed by Erasmus. I suppose he (Erasmus) developed the argument from structural affinity quite largely, and perhaps touched on rudimentary organs; and that kind of argument unless associated with something (though perhaps only a too speculative attitude) which your father strongly disagreed with, must have made some appeal. Yet had he felt any strength in use and disuse, as a general principle of modification, it is not easy to see why he should have been put off. But I am only repeating your point; a bad habit, only better than repeating one's own. ...

Darwin to Fisher: 13 November 1932

The next time you happen to write, but not before, [say] if *The Scientific Basis of Evolution*—[T. H.] Morgan (Faber) is a good book and one I ought to read.

Fisher to Darwin: 15 November 1932

I should only read Morgan's new book if you are tempted by curiosity. It seems to me a most interesting example of the way in which a man directly responsible for real scientific advances should retain old concepts and arguments which these advances have really made obsolete. This shows up in the way he speaks of the 'Mutation Theory' of de Vries, as though it were a theory of evolution to be considered as an alternative to Natural Selection. de Vries speculated upon the origin *per saltum* of specific forms, and the Drosophilists have shown that a single species shows many hundreds of mutational variants. But Morgan seems to argue that the occurrence of mutations supports de Vries' theory, as though all evolutionists from Lamarck and Erasmus had not postulated mutations, i.e. heritable changes, to have taken place. In fact, much of Morgan's book, though avowedly based on the Drosophila work, might have been written (and, in effect probably was) in 1905.[46]

Fisher to Darwin: 3 January 1933

I am enclosing a screed which I was induced to write for a composite book having, I fear, some such dreadful title as *Mind Behind It All,* though the Editress has not confessed it to me as yet. Quite possibly she will find it so much against the grain of her other contributions that she will turn it down, or, what will come to the same thing, propose a few tactful modifications. As a title, I think 'A Modern View of Darwinism' might do, as leading no one to suppose that it is genuine Darwinism, as I feel rather guilty anyhow of taking liberties with what your father thought or said. On this point, as you know, I should be guided by your comments.

There does not seem to be much metaphysics in it after all, at least not what a metaphysician would call metaphysics, and I hope it does not open the door to the flood of wishful sloppiness which seems to be called philosophy. However, you will judge best if I am showing signs of drivelling.

Darwin to Fisher: 8 January 1933

... As to our old friend, free will, we have never seen eye to eye, and if I say anything, I shall expect it to be ignored. Modern research appears to me only to show that we have not yet dug down to the foundations and not therefore found where determinism seems to come in.

If men had perfect reasoning powers and knew what they wanted, they would always do the same thing in the same circumstances. If *choice* means the possibility of doing two things at any one moment, then there would be no choice, or free will in that sense, in these circumstances. On such a supposition, you have pushed out determinism from the front door, and find it entering at the back. If choice comes in only because of imperfection of reasoning powers, an element of pure uncertainty in the nexus between environment and action is introduced. This leaves determinism in its original commanding position, and only introduces a variation about the mean in the results. If choice is to be creative, it must not be fortuitous; but the less fortuitously it acts, the less choice there can be. If there is free will in marriage, for example, and if choice is not tied to circumstances, the statistics of marriage would not necessarily be uniform. If choice is tied to circumstances, it is not free. The only way in which indeterminism can come in would be by human beings having a limited power of altering what we call previous events, as we now see we can alter subsequent events. Rather a bold supposition! But still I can [not?] help believing in free will. What you say is interesting and not too long. And I liked it all.

Fisher to Darwin: 16 January 1933

... I am much more likely to combat than to ignore what you say about free will, because I find it interesting and relevant, but, where conclusive, capable of a fundamentally different re-statement. Of course, if determinism were axiomatic we must take the view that scientific research has not *yet* discovered the formulae for exact prediction. But this view, though at present possible, does certainly beg the question.

Next you say, 'If men had perfect reasoning powers and knew what they wanted, they would always do the same thing in the same circumstances'. Equally, if all marksmen aimed at exactly the same point, and shot perfectly, all the shots would pass through the same hole. The variation in behaviour can be regarded as compounded of two distinguishable sets of causes, (i) variation in what is aimed at, and (ii) variations due to imperfect knowledge of, or imperfect control over, the environment. On the moral plane you may say that in given circumstances there is usually one course of action which is better than any other, and that any perfectly wise and perfectly good man *must* choose this course. But this is not the same as saying that he is constrained to do so, otherwise the stipulation of perfect goodness could be eliminated. He can say 'I feel I have no choice', but in saying this the 'I' in his sentence is identified with only part of his personality. It does not include his conscience, which he is speaking of metaphorically as constraining it (the 'I'). Equally, a man can say 'I have a good mind to—' when everybody knows that he won't, meaning assuredly that, apart from his own consideration, loyalty, good-nature, or what not, he feels perfectly free to take and justified in taking such a course. Intricate as such phrases are, I suggest that they emphasize the sense of personal choice rather than tend to eliminate it.

Perhaps you may say that, whatever he feels like inside, the choice of a good man is in fact determined by outside circumstances, and predictable by an outside observer from the stipulation that he must act for the best. So that the outside observer is justified in regarding him as an automaton. There are within this view at least three possibilities all perfectly consistent with human free will. (i) The saints and angels may be automata, without men being so equally. (ii) The saints and angels may each have something of their own to contribute, as to what is good; that is, they may invent new sorts of goodness, like musicians composing different melodies. (iii) Even if goodness can be defined objectively, and equally for all, they may yet be perfectly good only because they choose or have chosen to be so, the phrase to 'choose to be good' merely comprehending in a single clause innumerable particular good choices.

On the intellectual side, you say, 'If choice comes in only because of the imperfection of reasoning powers, an element of pure uncertainty in the nexus between environment and action is introduced. This leaves determinism in its original commanding position, and only introduces a variation about the mean in the results.' What I understand by determinism does not allow a variation about the mean, and if the original commanding position of determinism implies merely that the mean can be calculated *ab exteriori*, then I think determinism is left in this position, on the analogy of marriage-rates, and prediction in mechanics. A good deal seems to turn on your phrase 'pure uncertainty' and on the word 'fortuitous' in your sentence, 'If choice is to be creative it must not be fortuitous.' If, in a long calculation, I introduce mistakes, I do not, of course, choose to do the calculation wrong, but I did at some stage choose to put down a 5 where I ought to have put down a 3, not realizing that it was wrong. In this sense the calculation is wrong because there was something wrong about me; and there is in this case little chance of the accident being a happy one. The neuron system which blundered has probably not such a good idea of mathematics as some other neuron system, which is in charge of and designs the whole operation. But in designing an experiment it might be otherwise. A modification which, at one stage, looked like a mistake, might at another stage look a stroke of genius. The joint process of making the modification and recognizing its merits would be creative, in very much the same way as the joint process of mutation and selection has been in biology. Choice to the mind that chooses, is, of course, never fortuitous, though what presents itself for choice may be. But to an exterior mind an aggregate of choices is a typically fortuitous system. If the probabilities of different kinds of choice were not determined by exterior circumstances I suppose the statistics of marriage would not be in the least predictable, but if circumstances only determine these probabilities there is room for individual choice. And if they determine these probabilities only through the constitution of the human mind, or, strictly, if the only non-empirical method of calculation consisted in the enumeration of the different kinds of mind present, and the probabilities in different circumstances with which they would marry, then there is no reason to speak of the choice as other than absolutely free.

I am afraid my paper was a nasty shock to the good lady, and I don't suppose it will suit her book at all. ...

Fisher to Darwin: 20 February 1933

... A new American or International Journal of Philosophy, Mathematics, and Science has asked me for a contribution to an early number, and I shall offer them something of the kind, though not quite as written.[47]

I do not think really that you *need* disagree with me. Indeterminism is a wider hypothesis than Determinism, and should have precedence, until facts can be found which exclude it. If it did contradict any intuition, which I am not convinced about, that would only make things fair, for Determinism certainly does.

I happened to be at the Linnean at a meeting last year to hear a paper about some plants on African mountains; the main paper given, however, was by Hinton on rodent control, and as he very contemptuously dismissed the plan of killing the feamles, and releasing the bucks, without apparently being aware of their infanticidal instinct, I was led to put on record a few facts, which interested me very greatly when I observed them, and which I think you may find interesting.[48]

Darwin to Fisher: 22 February 1933

... As to free will, my difficulty is to reconcile it with the uniformity of vital statistics. I am trying to put it on paper, but find myself very muddle-headed. I do believe in free will, whatever my reason may say!

I like enclosed. You must some day put *all* such thoughts in a *book*. It seems to me that a male will gain racially by killing the offspring he finds for two reasons. (1) It will shorten lactation; and therefore make the female sooner ready to breed *with him*. (2) Granted that the number best looked after by the parents is limited, *his* surviving offspring will thus be increased, in certain circumstances. I guess this is your idea—unless again I am muddle-headed.

Will not a man in like manner gain racially by killing his father, if he, the son, lives at home? Can we thus account for the Oedipus complex?

I see no reason why a mother should ever gain by killing her *own* children. ...

Fisher to Darwin: 23 February 1933

I think the main factor in infanticide in rodents is that the expectation of life is short, perhaps only about a fortnight for a male. Consequently, the prospect of the doe being ready in 2 days instead of 12 is very important. Against this great gain is the danger, confined to older mice, of slaughtering his own litter; so I suppose the instinct is inhibited by the feeling, experienced only by fully mature mice, of being at home in his own territory. I fancy the means of dispersal are so great that the surrounding population with which there is effective competition is large, some thousand perhaps, so that it is competition for the use of a particular doe rather than for the general means of subsistence which is effective.

I do not think I believe in the Oedipus complex, never having felt any inclination to patricide, or even its possibility as a 'Bad dream'. I doubt, too, if in Sophocles, the tragedy is other than an incredibly awful possibility which might be sprung on a mortal by malignant fate.

Returning to mice, a mother might with advantage kill her own young if it was certain that they would later be killed; she would then save herself some time and expense. Whether the mothers do join in the killing I have never been able to make out.

Darwin to Fisher: 16 March 1933

... I have written something on free will and on the uniformity of useless characters. I want them to be read by at least two persons! And I shall I expect sacrifice you on the altar as one of them! ...

Darwin to Fisher: 23 March 1933

Here is Free Will, to be returned at your leisure—you have brought it on yourself! I never found anything harder to write, and I do not like it now it is written. ...

Fisher to Darwin: 31 March 1933

Wilful modification of the past is a magnificent notion, but in the name of Occam is it necessary? From several passages I believe you are putting on 'the regularity of vital statistics' a burden which it will not bear. There are irregularities, small perhaps in the mass, unimportant to the official, if there is one, who decides how many beds shall be set apart for maternity cases, but quite big enough to the individual to accommodate his freedom of choice. Your argument, as far as I have grasped it, would have weight if the regularity were so austere that one could say 'The number of suicides this year in London *cannot possibly* exceed 150.' For, if that could be said, and the quotum was filled by the end of November, we should really none of us be able to commit suicide before January 1st, which would not be freedom. But in reality the regularities that can be observed do not imply any such individual restraint at all. What is sometimes forgotten about statistics is that, from a vast number of independent facts, after some restatement, we select one or two as relevant to our purpose and reject all the rest as irrelevant. The ones which we choose as relevant are those which depend on general causes, or, in other words, the ones which are useful for predicting future experience. What we reject (if the statistical processes are successful) are the facts which arise from particular causes, and which are useless for the prediction of future events.

You feel that the individual is constrained by the total to which he belongs and so he would be if the total were rigidly fixed. But to give the total a little latitude is to give him a lot; and to give the total what we do give our totals is to give him full liberty.

Even if you admit this, I believe you will still feel that the individual must be constrained in order that the aggregate he belongs to may conform satisfactorily to other aggregates. Suppose we offer 10 000 schoolgirls a choice between pink and yellow sweets and about 7 000 choose pink sweets. In doing this we have our eye on another 10 000 schoolgirls not yet tested. You say, 'they cannot really be free to choose, because you know as well as I that the number choosing pink will be within 100 of what it was last time.' As a libertarian I can be more sceptical, but I admit freely that if there *is* more than 100 difference there must be some cause for it, meaning by that, that with sufficient patience and observation of relevant details some genetic or environmental difference between the two groups could really be found that could account for their difference in behaviour. But I admit that for this back-handed reason, which shows how very far I am from admitting any lack of free choice. The argument goes like this. In the total 20 000 there must be some number who will choose pink, say 14 022 to be exact. It is possible to divide the 20 000 into two lots of 10 000 each in a very large number of ways, which can be enumerated. And each way of dividing the total will correspond to a particular discrepancy between the numbers choosing pink in the two lots. In 99 ways out of 100, or some such calculable fraction, this discrepancy will not exceed 100. If it were to exceed 100, therefore, we must choose between two conclusions. Either something has occurred by chance which we know would only happen once in 100 trials, or the children have been divided into lots by some process which is not quite independent of choosing pink or yellow. To trace the nature of this dependence, if it exists, is what we call finding the reason why one lot behaved differently from the other; and it is clear, whatever view you hold about free will, that to search for such a case would be a hopeless undertaking only in the one case in 100 in which the disparity has occurred by chance.

Consequently if we have reason to know that the second lot of girls is homogeneous with the first, we can predict fairly nearly what their aggregate choice will be simply from experience with the first lot; for by homogeneous we mean in practice either that they have been chosen at random out of the same total, or that they are as much alike as if they had been. In these circumstances our prediction is an example of purely inductive reasoning and is independent of all theories of the causes behind our observations.

I feel I am labouring the point, but it usually happens that one labours the wrong point.

Darwin to Fisher: 3 April 1933

It is very good of you to trouble yourself so much about my free will. I fear we must conclude that we cannot cross over our thoughts, one to the other, and this makes me suspect I must have a blind spot in my mental eye. But it does not much matter, as I have no thought of publishing. I came across this sentence in Bohr's writing in *Nature* yesterday.[49] 'I think we all agree with Newton that the real basis of science is the conviction that nature under the same conditions will always exhibit the same regularities': the reason why, *I* think, that present events are rigidly connected with past events. Some great swells think that nature is indeterminate, but there I cannot agree, and am glad to find some on my side; ...

I guess I shall send you what I have written about the uniformity of useless characters before long. After that I hope I shall not trouble you much more!

Fisher to Darwin: 5 April 1933

I hope you will send me your paper on the uniformity of useless characters when that is ready, and not altogether despair of me as a rational mortal even in the matter of determinism. I know that if I start talking about creative causation I shall shock many of my friends much more than if my wife divorced me; but, like Omar, and I think here you may agree with me, if I thought the whole show had been pre-arranged unalterably I should not be so silly as to bother myself about exactly how it worked, but should rather consider the drama aesthetically as a well- or badly-constructed performance.

On the first point, I have recently been reading a chapter by Ford written for a forthcoming book by Hale Carpenter on mimicry.[50] He gives several examples of non-mimetic forms being distinctly less uniform than mimetic forms of the same species. In these cases, at least, variability seems to go with less intense adaptation, more so, I think, than I should have guessed.

Darwin to Fisher: 18 May 1933

... Thanks for your notes on my production. I wrote it for my own interest, not intending to publish. So many old men write when they had better not, that I feel alarmed, and I have lost confidence in myself. Moreover, I do not know who would consider such a publication. But as you pat it on the back, I will reconsider the matter some day.

Another problem I am turning over in my mind in the same way is death as the result of natural selection. Immortality would put an end to selection, and death is thus favoured. But experience in animals and depth of roots in plants makes the aged, as long as fertile, of increased biological value. Hence the higher animals and trees ought to have the longest lives—as they have. But why are fish such long livers—if it be true?

I wish we could have a talk some day on these things. I am really away from home for a few days at the sea, my first absence for ? 18 months.

Fisher to Darwin: 20 May 1933

I, too, have a good many things that I should like to talk over with you, though some of them are not worth bothering you with. But for the prospect of giving more trouble than it was worth, I could run down to Bognor for a night, so give me a word if you think it a good idea, treating my suggestion as you would have one of your own treated by an old friend.

One thing you will be glad to hear. I had a letter the other day from the Provost of University College [London], telling me that at the suggestion of the College, the official committee unanimously invited me to stand for the Galton Professorship. I hope to discuss the situation with him quite soon. I am not yet quite clear what opportunities the post will provide, as they have first, rather comically, divided off the department of Statistics, in order, I presume, to give Egon Pearson a readership. I think I could work tolerably amicably alongside of Egon, but can foresee the embarrassment that many voluntary workers now come to me, and would come as advanced students to University College if I were there, just because they regard my statistical methods as having superseded Pearson's.

The chair includes a curatorship of a museum of Galtoniana which will interest me (though I am a bad curator of my own possessions). It must have one or two assistants, though I imagine at present no provision for biometrical work on living material, and I do not believe that students could fit themselves for research on man without quite extensive training on biometric-genetic lines on some more manageable animal or plant. I do not know that you will agree with me at once about this; but what other hope is there, to take the best case, for an able graduate in mathematics who has but two years to spend at the place? His mathematical training will, from what I know of mathematical departments, have kept him abominably isolated, not only from biological facts, but from the whole mode of

thought of an experimental science. He may be eager and clever as can be at acquiring the algebraic development of the probability integral of my z distribution and never have a notion of what facts in experimental procedure to make sure of before it is worth applying a test of significance at all. Besides, I want to attract another class of advanced students that I have seen something of, i.e. people who are already biologists, geneticists, and the like, but sufficiently mathematical to want to use the best available statistical methods and biometrical ideas in their own work, and I shall never get the best of these without facilities for handling living material.

And then I won't give up my own chickens, and snails, and mice without a struggle; even if they aren't Eugenics they should do something to get sound Genetics into the heads of students who will certainly need them, and perhaps make a few sound biologists feel that a Eugenics Laboratory is not such a nest of cranks as they had been led to fear. ...

Darwin to Fisher: 22 May [1933]

I am really delighted at your news, which you know is not what I expected. ...

As to coming to see me, I should really enjoy a talk very greatly, and my only doubt is whether it will be worth while for you thus to spend your time. ...

I think I agree with you about animals, etc. I am sure that you are wise to begin at once to make plans in outline but I should strongly advise waiting till a little after you are in the saddle before making definite moves. I hope you will be able to let Egon Pearson know of your friendly sentiments if you can. It might mollify his papa. I write this all in haste to say how grand I feel it will be if you get the job and how warm will be my congratulations.

Darwin to Fisher: 26 May 1933

It will be nice to see you for as long as you can stay. Sat. June 3 suits us perfectly; but do you remember it is Whitsuntide? ...

You will have some hard nuts to crack. As far as I know, no professors as such have personal assistants, which, it has always struck me, must lead to a great loss of valuable time. When will the news be confirmed and *public*? Let me know next time if and when I may say that you have got the job.

Though there are difficulties, I am really delighted—for I suppose, from what you say, it is a certainty. ...

Darwin to Fisher: 31 May 1933

Thanks for yours in which you say you will come on June 17, ... to Forest Row, when we shall be very glad to see you. ... As to what you say about refusing to bind yourself in certain respects, I do not yet understand whether you have definitely got the job. If not, is it not of immense importance to your children that you should get it? Suppose you missed it by making certain stipulations, might not you repent it bitterly? There may be something very foolish in what they ask, but it is not morally wrong, and it cannot be morally wrong for you to agree. You might always hope to reverse any decisions. My point [is] that it is of great importance to your family, to you, and, I think, to Eugenics that you should be selected, and that you will do *wrong* to throw any difficulties in the way. It will be over-conscientiousness on your part, which is an error I can conceive your making. Please forgive me for speaking frankly.

[P.S.] To save time when we meet, I want to put one *un*important matter in writing. You seemed to think that I should differ from you in regard to your programme of work for the professorship. Possibly you were thinking of my views with regard to the functions of the Eugenics Society. I hold that it should confine itself to propaganda for the following reasons. (1) It was originally the Eugenics *Education* Society, which indicates the basis on which members were first asked to join. (2) Twitchin, who helped us so much, wanted his money to go in widespread propaganda, believing that we know quite enough to justify practical action. And (3) research is so much more interesting and advantageous to the individual that it would drive out all propaganda, because of its cost. On the other hand, though I have not Galton's will before me as I write, I am certain that his aim was research together with *advanced* instruction. Hence I think these are the lines on which his money should be spent. How these ends can best be obtained, it is for the Galton Professor to decide, and I see no reason whatever to think that you will come to unsound conclusions.

Darwin to Fisher: 14 June 1933

... Congrats. from an old pessimist like me might bring bad luck, so I shall not send them till you are bang certain. Then they will come quite hot.

Darwin to Fisher: 22 June 1933

Three cheers for the good news. Now I feel that I must celebrate the event, and I am writing to beg you to allow me to do so in the following way.

Only one of my nine brothers and sisters has lived for longer than I now have, and that only for a few months. Hence I had every right to believe that I should have gone away before this. With such thoughts in my mind, I left such instructions as would have had the effect of a codicil to my will passing on at my death a small sum of money to you for the benefit of my godson.[51] I begin to feel, however, that it is rash to count on the money being available when most likely to be useful, and moreover, if I survive for three years, I shall defraud the government of an appreciable amount of death duties if I now pass on the money. Does not that appeal to you? Seriously, will you give me the great satisfaction of helping Harry by now

accepting the enclosed cheque to aid in his education? I am sure that you will. You must use it as you think best, and I will only make some tentative remarks. ...

I have only accounted thus for part of the full sum. Now I should much like, if you will permit it, to celebrate your becoming the Galton professor by helping you to join the Linnean Society, which I presume can be done for some £20. No one will ever know that I have had a finger in the pie, but it would not only be a great satisfaction to me to feel that I have been permanently helpful to you, but also, I believe, of some use to science in this way.

Now if you will agree to all this I shall again throw up my hat with joy.

Darwin to Fisher: 27 June 1933

I was very glad indeed to get your letter. It pleased me very much. ...

Fisher to Darwin: 27 June 1933

I am just writing a note on a small technical point, connected with graded or flat-rate family allowances, because I fancied ... you were taking for granted that the only difference between such schemes lay in how much the wealthier members subscribed towards the support of the poorer. I want to make it clear that this is not so ... Now the economic purpose of family allowances, as I understand it, is to equalize the standard of living between parents and non-parents doing equivalent work. That is to say, that provided the services they actually perform are equivalent, to make sure that parents and non-parents shall, on the average, equally be able to afford the material advantages which constitute the reward society has to offer for these services. It is not essential, at the moment, to my argument that this object is worth pursuing, the only point needed being that it is attainable by a system of graded allowances, but quite unattainable by any flat-rate system within the profession, with or without the addition of a tax on the wealthier for the benefit of the poorer members. ...

Fisher to Darwin: 23 October 1933

You played a nice surprise on me today. I had hurried into my room, after running myself rather late going over some of my snails, and saw with falling heart that there was some more correspondence not yet dealt with. Still, it was only a book parcel; so I put it in my bag to look at in the train, where I found as you will guess that it was Mrs. Barlow's edition of the *Diary*,[52] which has such a big review in this week's *Literary Supplement*.

We had read the review eagerly at home and thought it very good, so your present, which is a very charming one, comes just in answer to our curiosity. I have scarcely looked at it yet, just enough to make me wonder if your caution about reading too definite a meaning into phrases meant to contain some suspense of judgement ought not to be applied to your father's use of the word creation and 'centres of creation' during the 'Beagle' period. The notion that the origination of new species, even though not by generative descent from pre-existing ones, might none the less take place in accordance with natural laws, not yet discovered, must have been familiar to your father from the second volume of Lyell's *Principles*, and I should guess it must have been in this sense that he used the phrases. That is a little less dogmatically, perhaps, than Mrs. Barlow takes them to be. But I must now start my treasure hunting in earnest.

Fisher to Darwin: 10 November 1933

Many thanks for your letter. ... I am exceedingly sorry to hear that the two departments are to be separated,[53] as I believe this will be very injurious to the Scientific reputation of Eugenics in the States, which is already by no means all that could be wished. ... I am more than ever convinced that Eugenics will make no progress either in Academic circles, or [with] Public Officials, or with the Public at large, unless it has widespread sympathy and some active support from Professional men of Science.

Since you resigned the Presidency, in fact, I have been increasingly conscious of the same danger on this side.

Darwin to Fisher: 12 November 1933

... the part played by biologists in the field of Eugenics has been most disappointing. Few have given the movement a real shove forward, and I fear that jealousy has been one cause of their standing aloof. I do not believe in psycho-analysis, but I do think that people are very often influenced by low motives without being aware of the fact. ...

Not many can seek to promote truth without some thoughts of self affecting them. ...

Fisher to Darwin: 13 November 1933

I think I agree with every word of your letter, except your inference in respect of practical policy. The attitude of Biologists to Eugenics has been disappointing, and will continue to be so, I am afraid, so long as they are treated in such a way as to arouse the subconscious motives which you suspect.

Some, of course, are incapable of disinterested action anyhow, and they are no loss to the movement, but there are a great many who could be disinterestedly keen had they not felt on some occasion or other, that they were being lectured on a Biological subject by persons very little qualified to speak upon it.

It is meaner, easier, and generally more consonant with human nature to react as an adverse critic, than to offer hard-won technical knowledge in the service of some ass, who will not appreciate its merits.

Darwin to Fisher: 15 November 1933

I am glad you have taken my letter in the way I intended it. It matters little if I am wrong in my practical policy, as I shall make no move. Something I heard *after* I had written my last letter to you made me fear that you would think I was hinting at things which were not and could not have been in my mind. I shall never *hint* to you, but speak out. The action of some of the biologists has always been a puzzle to me. ... I wish we could pull together better. Of course eugenists make mistakes—I know I do.

Fisher to Darwin: 16 November 1933

I realized that it was just a coincidence that yours about the American Society came just as I was rather bothered about the analogous situation in the English Society, but as it happened my views on the first question are largely influenced by what I heard during my two recent visits to the States. The proposal to abolish Honorary Secretaries, apart from other questions, puts the [English] Society for the first time, I think for at least twenty years, in the position of having no active officer who is a professional Biologist. ... I proposed at the last Council, speaking of what I had seen in America, that the Society had everything to gain by putting a number of Biologists on its General Purposes Committee, which in fact handles all detail matters of policy for the Council. To my surprise and regret, great reluctance was shown from the chair (Bramwell), an attitude which I fear impressed all the Biologists present, which included Poulton, very unfavourably. ... I feel that the attitude of the small group of non-Scientific's which control the Society much more than undid the good feeling I was aiming at. ...

I do hope you will not bother yourself at all about this. If the Society goes to pieces or reduces itself to the status of the American Association, it will not be the end of Eugenics. ...

Darwin to Fisher: 9 March [*1934*]

Many thanks for looking over the extract from my article on The Brock Report, [54] and for your suggestions, all of which I have incorporated.

Fisher to Darwin: 12 March 1934

It was good to get your letter. ...

Did you notice that we carried out a suggestion which I remember your making years ago by ascertaining the mental condition of a large group of children of mental defectives. I put in some work on the results [*CP* 120], though they came to me very late in our deliberations, and they struck me as really remarkable. I was especially struck by the proportion of children from defective fathers and normal mothers being the same as that from defective mothers and normal fathers, since the environmental conditions in the home, especially for infants and young children, must be materially different in these two groups. But perhaps the most remarkable result of all was the actual incidence of defect in these families, which is more, I think, than could be anticipated on any theory of inheritance without strongly assortative mating. I should judge, in fact, that though carriers may well be more numerous than defectives, they must bear to them a much lower ratio than we have all been inclined to suppose. ...

Fisher to Darwin: [*July 1934*]

At the meeting of the Genetical Society recently at Down House, I was delighted, as indeed we all were, to hear your letter read by the President [J.B.S. Haldane]. You will be interested, I think, by Haldane's remarks at the end of the meeting, to the effect that your father knew so many more facts relevant to evolutionary theory than any of us did, that we should hesitate to think that anything we knew disposed of his views.

The day was very hot, and fine, and the meeting well attended. I think we all enjoyed the opportunity of visiting the historic spot.

Darwin to Fisher: 14 July 1934

Thank you for your letter. I was glad to notice what a glorious day you had for your Down expedition, and I wish much that I had been there to wander round some of the old familiar places with you. ...

Darwin to Fisher: 10 October 1934

An uncle of mine, who belonged to a large sibship, wrote the following lines, when young, at some period of family commotion:—

Write, write, write a letter,
Good advice will make us better,
Father, mother, sister, brother,
Let us all advise each other.

He was evidently thinking that too much advice was flying about; and that may well be the case if I give my views about your public lectures. Advice should be readily asked and readily neglected.

I have never, I think, heard you give a regular lecture; but as you can write well, you ought with forethought be able to lecture well. I always wrote out my lectures and had the text before me; but I did not try necessarily to follow what I had written word for word. This is not a good plan, I expect, for most *ready* speakers; though I believe nearly all gain by *writing* it out in advance. ... The spoken word should be decidedly more diffuse and easier to understand than the written.

I decidedly like the idea of your public lectures. You may have to face disappointment in the size of your audiences. ...

Darwin to Fisher: 17 January 1935

I think it is no use searching for the Wedderburn letter.[55] My father used to put a letter, after being noted and read, on a hooked spike. The note was put in some labelled shelf. The spit when full was put in a cupboard under the stairs, and when that got too full, the letters were burnt to make room for more! We protested, but my father could not conceive that his correspondence would be of value! This anyhow is my memory. I return the notes[56] in case of their being of any use to you. And I shall remember the name of Wedderburn in case of a miracle occurring.

Congratulations on the *Annals of Eugenics.* You are right to keep it up to a high standard, though in truth it is so high as to be clear over the top of my head! ...

Darwin to Fisher: 4 May 1935

I have not troubled you with a letter for a long time, though I feel there is a subject on which I should have expressed my feelings long ago. I know that it was you that originated the idea that certain research studentships[57] should be called after me, a fact from which I have derived much pleasure. I did write to the Council to express my gratitude, twice I think; but I felt I was doing it very clumsily. And I think the difficulty of putting my thoughts adequately into words has been the underlying reason why I have not written to you. I think you know that I have always urged that propaganda should be the main aim of our Society, because research is both more entertaining and profitable, and might drive its rival out of the field. Moreover, research is better done by a University than by a Society. But, though these are still my views, I think they need not militate against what has been done; both because it does not go too far, and because of the precautions taken in connection with these scholarships. I think I may, therefore, enjoy my scholarships and stick to my views. ...

Darwin to Fisher: 20 May 1935

... If the research studentships have the effects you hope for, my ghost will be seen wandering about your Lab. at night with a broad smile on its face. ... I do not gather if your Lab. is to be the home of the new serological show based on American money. Anyhow, it is an admirable move. ...

Fisher to Darwin: 21 May 1935

... Yes, the serological work is to start here next October. ... The grant is good for five years, but I presume the Medical Research Council would weigh in to continue it, if there is half as much in it as I hope. I do not at all see why the hidden inheritance of a recessive defect should not be directly detectable in a blood sample; but I ought to say that no one has done this yet, even with animals.

Darwin to Fisher: 11 January 1937

I have heard little of eugenics lately, and what little I have heard is not very encouraging. Judging by the title, M. Keynes's Galton lecture[58] will have no bearing on eugenics proper. I remember rather vaguely a story about an official at the church in Cambridge where they had broad church sermons at intervals. I can paraphrase it by saying that I have heard or read every Galton lecture, but I thank heaven I am still a eugenist. ...

Darwin to Fisher: 26 March 1937

Just a line to thank you for several reprints received at various times. I was glad to see that you confirm my father's generalization about variability and abundance [*CP* 153]. ... As to some of your papers, I can only admire them from a safe distance, whilst making me see how busy you are. ...

Fisher to Darwin: 30 October 1942

I think you will be interested to see from the enclosed offprint [*CP* 192] that the problem in *Lythrum salicaria* to which your niece, Lady Barlow, first called attention, has at last been solved. ...

It has been a great pleasure to me during the last seven years to take some steps in clearing up at last the genetics of the polymorphic situation which so much interested your father. Although the genetics constitute only a small part of the problem, it is, I think, essential to get them right before one can speculate usefully on the manner in which the present situation in *Lythrum* has come into existence.

Darwin to Fisher: 2 November 1942

I was glad to hear from you again, especially as it was to vindicate my niece. But I cannot pretend to make any intelligent remarks on the subject. I shall be 93 next Jan.; my sight, and probably my brains have gone slightly down the hill, and technical Mendelian terms constitute now a considerable difficulty. You seem to be carrying on as hard as ever, on which I congratulate you. I am sure my father would have been very much pleased to know that the *Lythrum* puzzle had been solved.

I wonder if you heard Huxley broadcast.[59] I thought *his* part very good. But such a performance should be either a lecture or a play. I disliked the other performer breaking in. I am trying to read Huxley's new big book but it is so full of technical terms that I don't understand a lot of it. However, I am naturally pleased that he backs up my father's views on some big questions.

I live here very quietly, with my old staff taking great care of me, and occasional visits from nephews and nieces. ...

Notes

1. Darwin was an executor of the estate of Henry Twitchin, a major benefactor of the Eugenics Society. See p. 16.
2. Salisbury, E.J. (1930). Mortality amongst plants and its bearing on natural selection. *Nature* **125**, 817.
3. Cunningham, J.T. (1930). Evolution of the hive-bee. *Nature* **125**, 857.
4. A, B, and C: three stages of development to which 1 in 10, 1 in 100, and 1 in 1000 seeds attain. See *CP* 88.
5. Darwin returned this letter to Fisher with 'early' crossed out and replaced by 'as soon as possible after the optimum breeding date' and the word 'early' in the following sentence replaced by 'thus'. However, Fisher has written in the margin: 'No, I really mean early, before the optimum date for the average bird.'
6. Below this chart, Darwin has written, 'quite right, I think. L.D.'
7. See Note 5.
8. The text of this passage was inserted in *GTNS* in 1958 (pp. 153-4) but unfortunately a modified tabular arrangement was used which does not agree exactly with the given description.
9. See Fisher's letter of 25 November 1930 to Darwin (p. 134) and also his letters to C. Todd (p. 267).
10. Punnett, R.C. (1930). Review of *The genetical theory of natural selection.* (R.A. Fisher) *Nature* **126**, 595-7.
11. See Fisher, R.A. (1930). Genetics, mathematics and natural selection. *Nature* **126**, 805-6.
12. This letter, in which Darwin comments upon the argument Fisher presented in his letter of 23 July, bears the dates 4 August and October 1930. It was evidently returned to Darwin by Fisher after 4 August and then sent again to Fisher, with some changes, in late-October.
13. i.e. flat-rate systems of family allowances payable out of public funds, for children of low-income parents not liable for income tax.
14. The physicist C.G. (afterwards Sir Charles) Darwin, who later discussed various population and eugenic questions in his book, *The next million years* (1952).
15. e.g. with late age of onset for the given defect.
16. See Fisher's letter of 15 October 1930 (p. 129).
17. See Fisher's letter of 23 April 1930 to C. Todd (p. 267).
18. Presumably Fisher, R.A. (1954). Retrospect of the criticisms of the theory of natural selection (*CP* 258). See Fisher's letter of 23 October 1951 to E.B. Ford (p. 202).
19. J.C. Smuts, South African statesman, author of *Holism and evolution* (1926). See also Darwin's letter of 15 September 1926 (p. 80).
20. Darwin, C.G. (1930). Review of *The genetical theory of natural selection.* (R.A. Fisher) *Eugenics Rev.* **22**, 127-30.
21. See *CP* 258, p. 92.
22. A pamphlet entitled, *An outline of a practical eugenics policy,* prepared and published by the Eugenics Society.
23. Keynes, J. Maynard (1930). The question of high wages. *Political quart.* **1**, 110-24.
24. Darwin seems to have taken Fisher's reference to reversion in his letter of 2 May 1931 as equivalent to Galton's early use of this word for regression to the mean.

25. See Fisher, R.A. (1922). Review of *A treatise on probability.* (J.M. Keynes) *Eugenics Rev.* **14**, 46-50.
26. Whereas Darwin believed in free trade, Fisher favoured protection.
27. Darwin's letter to T.H. Huxley, referred to in *GTNS*, p. 1.
28. Fisher had found that he required an operation for a fistula.
29. i.e. the Zoology Section of the British Association for the Advancement of Science. See p. 224.
30. This letter was presumably Fisher's reply to the hand-written notes mentioned by Darwin in his letter of 1 February 1932. Darwin's notes have not been preserved.
31. I am obliged to Dr R.J. Sommerville for the following comment.

    Fisher was probably referring to *Dermatobia hominis,* the human botfly of South and Central America. The botfly catches female mosquitoes and sometimes other flies and glues a batch of eggs, from 10 to 100, on the ventral surface of the abdomen. I cannot find any record of eggs glued to the legs, as Fisher suggests, but it could happen. When the mosquito feeds on its host, the eggs, stimulated by warmth, hatch. The larvae penetrate the skin of the host probably through the lesion made by the feeding mosquito or through hair follicles. They then develop in cysts under the skin.

32. Haldane, J.B.S. (1932). Review of *Genetic principles in medicine and social science* (L.T. Hogben) *Nature* **129**, 345-6.
33. Presumably Dr W.R. Inge, Dean of St. Paul's, London, 1911-34.
34. See Fisher's letter of 28 November 1930 (p. 134).
35. Huxley, J.S. (1932). Review of *Genetic principles in medicine and social science.* (L.T. Hogben) *Eugenics Rev.* **23**, 341-4.
36. A review by Fisher of Hogben's book was published in *Health and Empire,* **7**, 147-50 (1932).
37. The Galton Laboratory, University College, London.
38. The Chair of Social Biology, London School of Economics.
39. Haldane, J.B.S. (1932). *The causes of evolution.* Longmans Green, London.
40. When W. Johannsen introduced the word 'gene' in 1909 in his book, *Elemente der exakten Erblichkeitslehre* (G. Fischer, Jena), he referred to Darwin's theory of pangenesis and de Vries' subsequent use of the term 'pangene' and then suggested that as only the syllable 'gene' was of interest, the simplest thing was to use this for the hypothetical unit of heredity, without any assumption as to its nature.
41. Hogben, L.T. (1932). Review of *The causes of evolution.* (J.B.S. Haldane) *Eugenics Rev.* **24**, 222-5.
42. Fisher wrote a review of Haldane's book and sent it to the editor of the *Eugenics Review* in June 1932 but it was not published—possibly because Hogben's review had already been accepted. See Appendix B (p. 289).
43. Charles Darwin.
44. Huxley L. (1903). *Life and letters of Thomas Henry Huxley,* (2nd edn.) (3 vols.). Macmillian and Co, London.
45. Leonard Huxley.
46. See Fisher's letter of 11 October 1932 to T.H. Morgan (p. 239).
47. See Fisher, R.A. (1934). Indeterminism and natural selection. *Philosophy Sci.* **1**, 99-117 (*CP* 121).
48. See *Proc. Linn. Soc. Lond.* **144**, 124-5 (1932) for Fisher's observations on the infanticidal instincts of male mice when confronted with litters fathered by other males.
49. Bohr, N. (1933). Light and life. *Nature* **131**, 421-3, 457-9.

50. Carpenter, G. D. H. and Ford, E.B. (1933). *Mimicry.* Methuen, London.
51. Darwin was godfather of Fisher's younger son, Harry Leonard.
52. Barlow, Nora (Ed.) (1933). *Charles Darwin's diary of the voyage of H.M.S. Beagle.* Cambridge University Press.
53. Darwin had referred to a suggestion that with the approaching retirement of C.B. Davenport as Head of the Department of Research in Evolution and the Eugenics Record Office in Washington, these two departments were to be separated.
54. Darwin, L. (1934). Analysis of the Brock Report. *Eugenics Rev.* **26**, 9-13. See also *FLS*, p. 199.
55. See Darwin, C. (1875). *The variation of animals and plants under domestication* (2nd edn), Vol. II, p. 319. J. Murray, London.
56. A typescript of Hutt, F.B. (1935). An earlier record of the toothless men of Sind. *J. Heredity* **26**, 65-6.
57. See Fisher, R.A. (1935). Eugenics, academic and practical. *Eugenics Rev.* **27**, 95-100 (*CP* 136).
58. Keynes, J.M. (1937). Some economic consequences of a declining population. *Eugenics Rev.* **29**, 13-17.
59. Huxley, J.S. (1942). Message from another age, an imaginary interview between Julian Huxley and Thomas Henry Huxley. *Listener* **28**, 501.

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