

3 DARWIN-FISHER CORRESPONDENCE 1915-1929

Darwin to Fisher: [August 1915 or earlier?]^{1(a)}

Problem Imagine a species composed of a group of genotypes, all of which breed perfectly true as regards their average descendants, or the parental correlation coefficient being 1.0. Imagine these genotypes as regards any one character to be distributed about a central form according to the normal law of error. Then imagine these forms to begin to fluctuate, the fluctuations not being inherited.

What is the law of ancestral descent?²

Does not the parental correlation coefficient merely indicate a relationship between the standard deviation of the genotypes before they began to fluctuate and the standard deviation of the fluctuations? ...

How does such an ancestral law of descent differ from what is found to exist?

2nd Problem If there are mutations and fluctuations, in what conditions if any do the fluctuations help in the action of natural selection? ...

Darwin to Fisher: [August 1915 or earlier?]^{1(b)}

Thinking over your sexual selection point again, I see how *an* aesthetic taste is aroused through the desire to select the healthy; and I am not sure if this has been remarked on elsewhere. But further than that I as yet fail to follow. Take butterflies. The male beauty would on an average be more prolific than the non-beauty. But this would be because he *is* sought after by the female. But take a female who cares for male beauty *less* than the average. Would she not get a mate all the more easily, having the ugly to pick from? Would she not be more prolific on the average? This would only be harmful to her stock if it led her to prefer the unhealthy. If it made her take the male less exposed to danger, it would benefit her stock. But possibly I don't quite see your point. Don't write on this. I am only writing because it comes into my head.

One more word about my problems. I am, as you see, building up ideal conditions and seeing how far they work like nature does work. You say the parental correlation = grandparental = σ^2/σ^2 , on my suppositions. This is, I presume, *ex hypothesi*. ... Take such a relationship between the σ of the fluctuations, and the σ of the immutable characters of the factors as will make the correlation coefficient between father and son = 0.5. Problem: what will be the c.c. between grandfather and grandchildren? If it works out

at 0.3, we have, as it were, imitated nature. I have no doubt that if I was not stupid at mathematics I could answer this from what you have said. Don't trouble to write. I am only suggesting points that I should like to talk about. ...

Darwin to Fisher: 3 September 1915

Thank you for your letter. I will answer your points in the order you raise them.

As to my 'first problem', I gather that you think by a scheme of genotypes and fluctuations you can, as it were, imitate the facts of nature. I do want to 'explain' the regression we know to exist. We will have another talk on this subject, as it interests me *much*.

As to sexual selection, I hope you will get a paper written for the October journal.³ I expect you can make an interesting point. I won't say in advance how much I shall agree with you. But I do want discussion with differences of views, as it is only in that way that the truth emerges.

Then as to the blunder, Pearson's or mine as the case may be. I shall not move in the matter, but naturally I should like to see myself proved to be right.⁴ I want to think mainly of what will do Eugenics good, and at home I was brought up to believe that controversy with individuals was a great waste of time. Supposing Yule⁵ said you were right, should you write something on the point? Where for? It would make Pearson your enemy, I fear, and that should not be forgotten. If for our journal you should write, then I must have some responsibility, as President of the Society. It would be easy to write a note saying that I had in the July 1913 journal in effect expressed a wish that my views should be criticized. Then quote offending passage. Then say that I had said in the Oct. 1913 journal it had been pointed out to me I had made a blunder. This, you could say you presumed from *Biometrika*, was from Prof. Pearson, and it was little wonder that I accepted his Correction, as he is the leading authority. But you hold I was right. Then give your reasons, and show why question is of some importance. Now in all this I am thinking on paper rather than in the least dictating to you. I am wondering whether if you are confident you are right, and if you did intend to write something, whether it would not be best to write out your remarks in their final form, and submit them to Yule, or get us to do so. I should be bound to get assurances you were on the right tack before publishing in our journal. For I cannot profess to judge myself, and Schuster⁶ agreed I had come a howler.

It would be tempting to bring in Galton's name and say one wonders what he would really have thought! But this would only embitter the controversy, which may be bitter enough anyhow. I doubt if P. is honest enough to confess an error.

Darwin to Fisher: 5 October 1915

I want to bother you once more about my inheritance hobbies, as I may now find time to work at them a bit.

To remind you of what I said before, first imagine a number of genotypes of pure lines, the genotypes as regards each character being normally grouped about a centre. Then imagine each genotype to begin to fluctuate. Then we shall have a state of things in which we can account for the regression of the son as compared with the father. ... Now according to Pearson the correlation coefficient of father to son is about 0.5, and ... the correlation between grandparents and children is often about 0.3, and it seems to me the problem is: how nearly can this comparison between the parental and grandparental correlation coefficient be accounted for almost entirely as a case of random mating regression? If there is any selective mating, this will lessen the regression. If the regression cannot be at all nearly thus accounted for, there is something wrong in my views. This is the main point on which I should like your help. I have looked through your old letters, and I think I ought to be able to answer this question myself. But I cannot.

One other less important point: I gather that you came to the conclusion that fluctuations would not assist the action of natural selection. In thinking it over, I wonder if you have assumed that the death-rate is some function (it does not matter what) of the difference of the measure of the character from the median. If so, I agree that fluctuation will not help natural selection. But this is, I suspect, a law which is seldom even an approximation to the truth. Take an extreme case. In certain circumstances it is conceivable that all giraffes would die from want of long enough necks, whilst some would live if the fluctuations of neck length were great enough. Or again, all giraffes below the average in neck length might have equally bad chances of life, whilst the fluctuations of the tall-necked ones might materially affect the distribution of the death-rate in this long-necked half of the species. In short, it seems to me that whenever selection is lop-sided, fluctuations will increase selection, and that selection is generally lop-sided. Where the selection, as it were, centres about the median, does it not mean that the median is the best position, from which no progress can be made? And this is seldom the case. ...

Darwin to Fisher: 11 October 1915

It is good of you to take so much trouble. I am I fear rather stupid at these mathematical ideas, and I do not myself readily draw broad conclusions from what you write. I gather from what you say that your conclusions do not negative the idea that the system of hereditary correlation coefficients, which we find to exist, may be accounted for by scattered genotypes with fluctuations of the individuals composing them, though it does not go far to confirm it. I was surprised at your results before you took the fluctuations

into account, and I wonder if you are breaking quite new ground here. You seem to get far nearer to an explanation of the facts than I had expected, but it still seems to me that fluctuations must be added, and here I do not know if you agree. Anyhow, all this seems to open up an interesting field for research for you Mathematicians.

I am only thinking of these questions in their broad and general aspects. I shall put down my ideas as clearly as I can on paper, and later we might have a talk to see if they seem to be leading in the direction of anything useful.

Did I tell you that I am a convert to your views on Sexual Selection?

Darwin to Fisher: [mid-October 1915?]

I also keep thinking over your problems. It seems to me, if I understand you right, that you are breaking up new and very interesting ground. ...

I am going on writing out my hypothetical views. If you can show that the figures can be explained without any reference to fluctuations, then things will become more simple. But I am provisionally assuming both fluctuations and mutations. ...

Darwin to Fisher: 23 October 1915

Herewith my notes. I suppose all about fluctuations,⁷ or nearly all, will come out. In fact, a good bit of the first eight pages won't hold water. But that illustrates how your conclusions will simplify matters.

I have dealt with certain quasi-mathematical matters in this paper, and as it is by the impressionist method, I am here especially likely to come to grief.

I do not like my suggested definitions on second thoughts. We want the ordinary words to be used for the ideas in most common use. Large mutations will, I believe, figure but little in the future, and I don't therefore want 'mutation' only to mean a large change. How will the following do?

Modifications are differences between individuals which would not have existed if they had been exposed to similar environments.

Mutations are differences between parents and offspring which are due to changes in the germ plasm (Mendelian factors), generally of a permanent nature.

Fluctuations are differences in the members of a sibship due to different arrangements in the Mendelian factors.

Variations comprise all differences between the individuals of the same species.

I don't know that you will agree that you are dealing with fluctuations! It is difficult to get these definitions to run nicely.

[P.S.] ... Do you see Pearson has republished by Cambridge Press that article in *Biometrika*—the one in question?⁸ He is a strange being. Whether

correlation coefficients are a measure of the relative amount of attention to be paid to different questions is an important matter, about which statisticians seem extraordinarily uncertain!

Darwin to Fisher: [late 1915?]

I saw Udney Yule yesterday. He had asked me to write a paper for the Statistical. I have agreed—'On the statistical Enquiries needed after the War in connection with Eugenics'⁹—Heaven knows what I am going to say! But I now write because he told me that both Pearson and Snow have written on the lines you are now working on. Snow's was, I think he said, a paper for the Royal Soc.¹⁰ He himself had written something short for (?) a congress of hybridization some five years ago in London (?).¹¹ I am not sure if you know this. It is a constant story in Science now to find oneself worked on ground already covered. But it is annoying. I thought I would let you know.

*Darwin to Fisher: 18 January 1918*¹²

The enclosed correspondence is very disappointing. There seems no end to the trouble. I think the Eugenics Society could run to £30. Do you see your way to putting up the balance?

Castleton House,
Old Aberdeen.

16 January 1918

Dear Major Darwin,

I have today the enclosed rather disappointing letter from Prof. Cargill Knott. It seems a pity that so rough an estimate was given before, for it led to the expectation that a subscription of £25 to £30 would make publication practicable. I suppose that the question now is whether Mr. Fisher's friends can rise to £43. I dislike the phrase 'or whatever the sum might be'. It is not for a well-to-do society to haggle surely.

Yours very sincerely,

J. Arthur Thomson

Prof. J. Arthur Thomson,
Natural History Department,
University,
ABERDEEN.

Royal Society of Edinburgh,
22 George Street.

14 January 1918

My dear Thomson,

The Council has considered the offer made by Mr. Fisher's friends to advance £25 towards the expense of printing his statistical paper. Mr. Fisher kindly sent the MS. back to me, and I got from our printers an estimate of the cost. ...

As the Council could not see its way in the present financial stress to give more than 10 pages for an abstract of the paper, i.e. about one-fifth of the whole, they are prepared to pay £12 towards the cost of printing this paper, i.e. fully one-fifth of the estimated cost. If Mr. Fisher and his friends could see their way to meeting the

difference amounting to £43, or whatever the sum might be, then the paper would be printed *in extenso*. This is the position which the Council with great reluctance are compelled to take. ...

I shall keep the MS. by me until this matter is finally settled.

Yours very truly,

C.G. Knott

Gen. Sec., R.S.E.

Darwin to Fisher: 6 May 1918

I wrote to Professor Thomson about your paper in the *Eugenics Review*; he tried to square the Royal Society of Edinburgh, but failed. Then I found he was getting rather fussy and so possibly was Professor Knott of the Royal Society of Edinburgh. So I thought it advisable to clench the matter by sending Professor Knott £30, which I described as a practical guarantee that our share in the expense would be forthcoming. Do not trouble to send me your £15 till the matter is quite concluded. I may try to get them to go on with the printing now if I can, though this is doubtful. Why I write is this; I fear we must now break up the type of your article without our putting it in the *Review*. This is the fortune of war and cannot be helped. Shall we get two dozen pulled before it is broken up? It would not cost much and might be useful. For example, I don't know where to lay my hands on a corrected copy, by which to reset up the type in future.

Darwin to Fisher: 20 February 1919

... I have been doing a little work lately—rewriting a paper which you once read on the Postulates of Evolution. I think I shall be sending you one or two mathematical conundrums on this subject in the hope that you will kindly solve them for me.

Darwin to Fisher: 5 April 1919

Here is my paper on postulates. I have found it difficult to write, as it is in parts trying to put mathematical ideas into ordinary words, and it is in these parts I am most likely to have blundered. I should be very glad of your help in detecting howlers, and should also like a frank opinion as to how it strikes you in the broad. You will not like the multiple allel-morphs, but you must bear them as well as you can! ...

Darwin to Fisher: 13 April 1919

Thanks for reading and returning my paper. I have not yet fully digested your remarks, but we evidently don't see eye to eye. This may be want of clearness on my part, or muddling, or both.

I think we are using the words in the same sense, but am not quite sure. By a mutation I mean a change in the gametes from one generation to the

next. You say, 'are not mutations essentially centrifugal?' Certainly not in my sense. But here my words may not be happily chosen. ... I do not see why a random mutation adds to the variance necessarily. ...

The heaping up of species, or the disappearance of intermediate forms, and the creation of new genotypes in the direction in which selection is acting seem to me primary puzzles not yet faced, and what I have been trying to do is to face them. ...

Darwin to Fisher: 13 May 1919

I have now had time to consider your letter of the 21st April, with which I find myself in general agreement. I think that to look at experimental work always tends to focus the mind too much on sudden changes as compared with slow effects. ...

I am sending you a letter which will serve as a testimonial. I believe it is more effective to write it in this friendly way than as a formal document, but I could easily adopt the more formal tone if you prefer it.

Darwin to Fisher: 7 August 1919

I was so sorry that I let you slip away from Cambridge without having a good talk. This arose through a misunderstanding as to the length of your visit. I especially regret it now—I hear through my wife that Pearson has made you an offer,¹³ as I understand. I am astonished at it, as I should think you were too tarred with the Eugenics Society Brush. If you are refusing, I quite understand your feelings, and I would say nothing to dissuade you. But I cannot but be sorry in a way, for I know you are truly Eugenic, and it is very hard to find those that are. You could not be certain to succeed Pearson. I say this because I know something of that strange body, the University of London.¹⁴ One cannot rely on their actions. But it would give a good chance. ...

Darwin to Fisher: [22 August 1919]

I am reading your manuscript,¹⁵ and write as I read. After reading the first chapter I feel that you may make an exceedingly useful and interesting book. I think I did tell you of Brentano's paper¹⁶ ... quoted in Pigou's *Wealth and welfare*. I believe that the Registrar General's office is now at work on the statistics of this question, from the last census, and getting confirmatory results.

Chapt. II does not please me so much; but this may merely be that we don't see quite eye to eye. But I do feel also it wants more orderliness. It is worth taking great pains with your first book, even though a book is an awful grind. I generally write a thing out, make a careful precis or analysis of what I have written so as to get the whole argument clear, and then write

it all again. Huxley said he, often I think he said, wrote a thing out six times before he was satisfied.

Chap. II gives me a feeling—if I may speak frankly—that you are making a case to fit you preconceived conclusions. Limitation ought to, on your hypothesis, increase not only the objections to limitation but also sexual desire, and carelessness about the future, including the future welfare of progeny. Natural selection ought to have lessened such forethought, and to have made us more reckless in certain matters than primitive man. You must not take your facts only when they fit your theories and neglect theoretical conclusions when facts are not available. Civilization has increased our power of looking to the future, and added to the desire to limit offspring. In this respect, though it has not altered the instinct, it may have weakened the effect of the reproductive instincts ...

I do not think you make enough of the existing environmental causes of limitation, such as those I sketched in my statistical paper. It is for this reason I should like you to read Brentano if you have not done so. ... I like Chap. III ...

These notes are hastily written, and will be mainly useful, I hope, to show how much I want you to make a thorough good job of this work.

Darwin to Fisher: 23 August 1919

I am *very glad* indeed for your sake you have got the job.¹⁷ It will, I think, suit you well. I am only a little sad that you may not have much time for Eugenics.

I sent your manuscript back yesterday with what I fear were rather inadequate remarks. ...

Darwin to Fisher: 31 August 1919

Thanks for your last letter, with analysis of Chap. II, which does make the matter clearer to me. With almost all you say, I am in agreement, and I am inclined to accept the importance of this factor¹⁸ in racial decline. Therefore I am most anxious you should go on and make a good job of it. I doubt if I have any further useful criticisms to make at present, but will keep your letter and write if anything turns up. Where I am doubtful is as to your views as to the growth of conscience. ... The care of offspring is the most ancient and most highly developed of all the instincts. The religious ban on infanticide seems to me nothing but an expression of this fundamental instinct. Religion is always backing up the social instincts against the individual instincts, as in regard to murder. A pronouncement about infanticide might indicate nothing new as regards instinct. As to abortion, reason might have made it apparent that it was equivalent to infanticide, and therefore added a new force to the religious support to the very ancient social instinct. If you are right in thinking a great change in innate instincts

can be so quickly developed, why do not celibacy and late marriages come under the religious ban? I believe fundamental instincts take *far* longer to develop, and that historical proofs of a change of innate feeling in such fundamental instincts must be accepted with great reserve. It would only be small changes of mentality I should expect to find. But I confess I am writing this rapidly with insufficient thought, and even if I am right, it affects your final position but little. ... *Don't* take much notice of this, as it is all so crude. But do go ahead. ...

Darwin to Fisher: 25 September 1919

I have no doubt you remember reading a paper of mine,¹⁹ when you fell foul of my ideas of multiple allelomorphs. I should not care for your opinion a straw if you did not express it frankly, and I am grateful for that frankness. I have been going over the whole paper again, rewriting 9/10ths of it, but finding that my opinion on all essential matters remained unchanged. This has made me wonder whether you did fully grasp what I was driving at. You gave me the impression in your letter, if I also may be frank, that you had not fully realized the criticisms of Bateson and others against 'Darwinism'. I have in my paper given a brief account of the criticisms that seem to me valid. Then as to multiple allelomorphs, I have tried to get over the difficulty by dropping the name!! Now I wonder if you could find time to read it once again, with the pledge to be as frank as before if you don't like it. I don't know who would give me a good biological opinion on its merits. I don't want to write anything on evolution over the name of Darwin which can be described as nonsense. Don't scruple to say you are too busy, but if you have a little time to spare, where shall I send the paper? When do you take up your new work? ...

Darwin to Fisher: 14 December 1919

... You quote the *Origin* about each part of an organism being so beautifully related to its conditions that it could not have been suddenly created. But in Chap. II of the *Descent of Man*, my Father says he did not sufficiently consider structures neither beneficial nor injurious, and that this was 'one of the greatest oversights as yet detected' in his work. He then goes on to argue that uniformity of character would nevertheless arise from 2 causes. First, from uniformity of exciting causes. ... Secondly, he speaks of the effects of free intercrossing. But Mendelism has killed that argument.

You ask in your letter in what way specific differences differ from differences between varieties or orders. I don't know what answer men like Bateson would make. I myself think they are only differences of degree, not of kind. But I don't see how the variety heap is formed any more than the species heap, as regards useless characters. And even as to useful characters, where the change seems to have been brought about by changes of

environment and where the different environments still exist, there is no reason why natural selection as ordinarily described should kill out the intermediates. ...

Darwin to Fisher: 20 August 1920

As someone said, one must not treat Pearson like anybody else. I think he means to be civil. But it is an astounding attitude to take up. To allow nothing to be published which does not back him up, or which he personally does not have time to read? & pitch into—it is going far.²⁰

Now as to publication elsewhere; of course I am quite incapable of estimating the merits of your work. Nor do I know anything about the international journal you mention. ... Then how about the *Journal of the R. Statistical Soc.*? I am on the Council, and I could speak to the secretaries, and find out what they think semi-officially, if you like. But I forget who they are, and I feel it had best be done verbally. This I could do in October probably but not before. I think Greenwood is one. Let me know what you think about this, and whether you would like me to do this in spite of the delay it would cause. If so, may I show Pearson's letter confidentially? ...

Darwin to Fisher: 14 October 1920

I saw Dr. Greenwood²¹ yesterday, and had a talk with him about your paper. He says that he fears that the Statistical Society could not take it, because they have to cater for an audience many of whom could not understand it, and they therefore have to limit the number of highly technical articles.

He would, however, be glad to send it on to Professor Gini for insertion in *Metron*, if you would care to modify it in certain respects.

In the first place, there must be some introduction, which I know you have had in view. In the second place, he would like certain phrases modified. He says that a great deal of friction arises between statisticians in consequence of the way they state things rather than because of the substance of what they state. He thinks you might put several sentences in a less provocative way. For instance, you speak of someone's interpretation of your remarks being 'so erroneous etc., etc.' Could not you say that 'this was certainly not the meaning I intended to convey', or something like that? Again, you imply that your opponents have criticized you without reading your paper, and Dr. Greenwood thinks that such implications merely irritate without doing good. In fact, he will recommend publication if all that is provocative is taken out, whilst everything that is mathematical remains in.

If I have expressed this rather clumsily please put it down to me and not to Dr. Greenwood, who spoke very nicely. ...

Darwin to Fisher: 2 April 1921

... As to the uniformity of useless characters, and the need for explaining the disappearance of intermediate types, it is my Father's very strong views on these points that affect me. The shape of a leaf is the type of a useless quality. The leaves of trees are so characteristic, but so unimportant apparently.

Darwin to Fisher: 10 June 1921

Many thanks for your two letters, although as regards some of the symbols it was putting mathematical pearls before unmathematical swine. I am sorry I cannot tell you anything in the way of statistics about the nature of wide ranging species.²² ...

Darwin to Fisher: June 1921

I took away [the] Hagedoorns' book²³ to read and review, when I did not know what it contained. I have written out a few notes²⁴ which may or may not serve as part of a review, and a carbon of these I will send you before long. I think you will want to read the book, and, if you do, you could make a really useful criticism of the 'Hagedoorn' argument. Will you do so for the *Review*?²⁵ If so, I will send you the book by post. They would like the review before the end of July. You could incorporate all or none of my notes. I have hardly touched the Hagedoorn argument. ...

Darwin to Fisher: 14 June 1921

This is how it stands with regard to my blessed old centripetal and centrifugal mutation paper. I got it set up in type. Then I was tempted to send it you to read, but I thought it would be hard luck on you, especially as I know you could only repeat the statement that you did not like the idea. ... I know I shall not convert you, but you might keep me out of quasi-mathematical howlers. I wonder if you could find time to read it. It is about 60 pages of big print, with several pages of typewritten additions. You must not scruple to say you are too busy if that is the case, for I know I am making a serious demand. But I think it will be my last regular scientific paper, and I should never trouble you exactly in this way again.²⁶ ...

Darwin to Fisher: 28 June 1921

Now for your letter about my paper for which many thanks.

As to the inheritance of acquired characters, I was in truth arguing in my mind with those who (like my Father and brother) believe in it without believing in vitalism. ...

Now as to your second point. It is that interbreeding between 2 varieties, when the cross is not so good as the 2 types, will make for the appearance of infertility of some kind appearing. This is a point I had not thought of. But

it is *your* point, not *mine*! ... The use of a paper like mine—if of any use—is to stimulate thought, and create rival suggestions like yours. Your suggestion shows how infertility may tend to arise between divergent types belonging to the same species when the mediocre type is less well adapted to the environment. It seems to me, on first thoughts, to be a very useful suggestion. Is it purely your own?

To put it bluntly, I think the choice before me is to publish, cutting out any regular howlers, or to scrap-heap the whole. I am quite ready to bag some of your ideas, but that must not go too far! That might result in complete disintegration.

Anyhow, can we have a talk, which might to me be very useful?

Darwin to Fisher: [July 1921?]

... Your other point is more difficult. Who but yourself has collected this evidence? And you have not yet published. I think you are very wise not to be in a hurry. The *Origin of Species* was brewing for 20 years. Lots of people have pointed out the decay of ancient civilizations, to which I allude as an incontrovertible fact. Also we have a great deal to show that wealth and infertility are correlated. I cannot allude to you till you have published; for, amongst other things, I must study that evidence before endorsing it. I did read some of it in manuscript, and frankly I felt in some particulars you were a little inclined to jump to conclusions.²⁷ This feeling may all disappear with your more mature work. ...

Darwin to Fisher: [August 1921]

... My present idea is to boil down all my papers into a book during the next three or four years. But I rather funk the task as my memory is, I think, not so good as it was. But I am much impressed with the fact that papers as permanently affecting opinion are of comparatively little use. Thus I hope when you are fully ready—not before—you will put your ideas into a book. But a book is an awful grind. ...

Darwin to Fisher: 5 November 1922

I do not think I ought to trouble you any more on the evolution problem, as I know I shall write no more about it. I hope to stir you up to write a great work on the mathematics of evolution. ...

It is true that Bateson set me thinking about useless characters; but my father's words affected me much more. In the *Origin* he speaks of species where they mingle being 'absolutely distinct from each other in every detail of structure'. In the *Descent of Man* he states that not sufficiently considering useless structures was one of the 'greatest oversights' in the *Origin*. He adds that 'it is, as I can now see, probable that all organic beings, including

Man, possess peculiarities of structure, which neither are now nor were formerly of any service to them.' He gives explanations which do not seem to me to be satisfactory. ...

Darwin to Fisher: 29 January 1923

... If genius had been due to a single factor, it would have been worth millions to try to pick out a male and female homozygous and mate them. But such a thing never occurs. I suppose ... bad single factor qualities are due to something dropping out of the genes. This would naturally lead to recessive qualities as a rule, but I don't see that it would inevitably follow. But why should not there be useful single factor qualities created in equal numbers and therefore often found in nature? I explain this partly by the adaptation of an organ being such an extremely complex and slow business that a number of genes are always, or nearly so, involved in the affair. ... Must pattern be quite as complex an affair as it seems at first? Can there be any lines of growth which help in the distribution of colour, and which remain anyhow? I can only conceive that the genes in these cases have been slowly evolved, and I do not now see how this is to be done without assuming the presence of slightly differing allelomorphs between which selection is possible. ...

P.S. ... I think the Stats. have treated you badly.²⁸ But I hope you will think twice before resigning. The fault lies with at most 2 or 3 individuals, even if more nominally consent. These men go in time and the affair is quite forgotten. If you now protest to the Council or resign, you will get the reputation, justly or unjustly, of being very touchy and easily put out. That reputation will not die out easily. Therefore you will lose by any action. The dignified course is that which makes you appear to say, 'I don't care a damn what you do or say.' Forgive me writing thus plainly.

Darwin to Fisher: 12 March 1923

Thanks for yours. My impression is that it would be useless sending your note again for publication—but it is but an impression.

I forgot exactly what I said in my last letter, and only to make it clear, if it was not so before, my opinion is that resignation is not your wisest course, though I am under the impression that you have been badly treated. ...

Excuse a scrawl, as I am not too fit today.

Darwin to Fisher: 15 March 1923

I feel sure Flux²⁹ is the senior editor, or at all events the man chiefly concerned.

This answers all you ask me, but I do not like to leave the matter there. You may well feel that I preach to you unwarrantably, but it is friendship

to you which makes me risk annoying you. Please remember this in what I now say.

When there is a difference of opinion, both sides in very many cases have, or imagine they have, a grievance. And I am sorry to say Flux considers that your letters to the Society, or your action in some way, has not been courteous. I can't say more, because I only know it from Flux's letter to Mallet.³⁰ Now if you go to Flux, and if you give the impression—an impression he would adopt on entirely inadequate foundations—that all you want to show is how foolish the Society has been to refuse a very important paper by you, the only result of the interview will be a useless and unpleasant row. If, on the other hand, you cared to say that you were much perturbed to find that Flux felt you had been uncourteous, which was the very last thing you intended, and that you would like at a personal interview to put things right, then the interview might do good. And it would do most good if you took up the attitude that you do not care whether the Society does or does not publish your paper; that that is a matter of minor importance.

So many scientific men have destroyed a great deal of their contentment by heart-burnings about the reception that their works receive that I dread anyone starting on that path. I am certain the wise and the pleasant path is to do the work, let the reception of it take care of itself, and push on quietly avoiding as far as possible all controversy.

Thanks for what you say about my health. It was only a passing headache such as you often have.

Darwin to Fisher: 20 March 1923

I am glad you have taken my letter in the spirit in which it was written: that is all that I care about. I fear I can give you no more help. I do not understand Flux's attitude any more than you do. I expect he has got very much the mind of a government official, and looks on contributors to his review very much like subordinates in his office. I suppose a man cannot help being influenced by the life he leads. ...

Darwin to Fisher: [April 1923?]

I have been reading your paper on evolution [CP 26?] with care. It makes me see that mutations may not need to be as frequent as I thought. ...

Darwin to Fisher: 21 October 1925

I have been thinking at odd moments about the problem you told me you were writing about, and I want to put down a few ideas mainly to get them out of my own mind.

As to big mutations, I have no doubt they are generally harmful. But are not they rare and soon stamped out? If so, they are of no great importance in evolution.

As to small mutations, these are what I believe evolution mainly relies on, and it seems to me difficult to prove that they are more often harmful than not. The geological man who spoke about evolution at the British Ass. spoke of perfectly adapted organisms.³¹ ... Perhaps there may be such a thing as an organism which is as perfectly adapted to its environment as selection can make it. In that case, *ex hypothesi*, every mutation must be harmful. ...

Darwin to Fisher: 19 November 1925

You very kindly said something to me a few days ago about reading my proofs,³² and, if the truth be told, I had previously been considering whether I could ask you to do so. But do you know what it would involve? I estimate the number of words as somewhere about 200,000; and if I have made no serious blunder, it would mean reading about 7,000 words a night for a month. Before you decide to renew your kind offer, may I say very plainly what is the part which I should feel it very valuable if you would play? It is no use trying to improve my style. A man's style is himself, and it had better be left to show itself, good or bad. What I want is help in avoiding howlers, such as I am not unlikely to make. These may take many forms, from illogical arguments, statistical mis-statements, etc., down to wrong use of words, bad grammar, etc. ... May I also say that I know that when looking over proofs for another person, one is apt to think one ought to suggest corrections. I remember reading the proofs of one of my father's books, and that I pleased him by making very few observations. The fewer they are, the more the author rejoices. Of course you will disagree with me on some points, and I should be much interested to know where you differ in regard to the arguments I set forth. What I have tried to do is to show the general way in which I hold that racial questions should be approached; for I think that a few mistaken applications of sound principles do little harm. If you will kindly show me where you disagree in principles or arguments, I should be much obliged, but I am sure you will forgive me for not arguing the points, if any, which may arise, and for my sticking to my guns when I do so. These are the chief points to hold in view when deciding whether you will really undertake this tedious task of reading proofs.

One more question I must ask, and that is if you do undertake this job, whether you will allow me to celebrate the publication of my work by subscribing thirty guineas or so to your twin investigations. I should feel it anyhow a privilege to do so. The money must be needed by you in travelling expenses, clerical assistance, etc., and I should much like to push forward your work under the excuse of your assistance to me. Please do let me.

Possibly you will get a chance of reviewing my book somewhere. If so, don't scruple to make it a bit spicy by pitching in to me. ...

Darwin to Fisher: 30 November 1925

I found your kind letter about reading my proofs the next morning after I had seen you. I am very glad that you consent to do so. It was tactless of me to hang the two questions together, the triplets and my proofs; for it is really true that I want to promote your enquiries because of their value, and without reference to any other consideration whatever. I am very sorry that the finances thereof are in a bad way, and that makes it all the more pleasant to send you the enclosed cheque.

As to my book, it will interest me to know how much you differ as to birth control. What I have tried to do is to make it a storehouse of arguments rather than of facts, in the hope that this will make it useful for a longer time. I don't think that we shall differ as much as you expect in regard to what I say in my book. ...

Darwin to Fisher: 6 March [1926]

I understand you have left it to me whether to post the enclosed to the *Morning Post*. This places me in an awkward position. I think your letter is a very good one and temperately expressed. On the other hand I have been brought up with a very strong distaste for controversy and I would do a great deal to do anything to avoid such controversy within the limits of the Society. The balance seems to me to tell against sending the letter and therefore I have not sent it. I fully admit that in this decision I may be quite wrong.

[P.S.] The Dean,³³ who dislikes what we do, blows off steam in the *Morning Post*. I, with the authority of the Council, let fly in the *Spectator*. May it not be quits?

Rothamsted Experimental School,
Harpenden.

5 March 1926

Sir,

It cannot fail to be a matter of grave concern to all who are interested in the future of our race that the Dean of St Paul's should find himself opposed, on humanitarian grounds, to the policy of eugenical sterilization; more especially since in the opinion of many of us who have long studied the subject, this means affords the only practicable remedy for some of the saddest afflictions to which mankind is subject.

'Mutilation' is a hard word, and in certain cases may be a hysterical word. The dentist who pulls out a tooth may be said to mutilate the patient, and certainly this is a more severe operation than the simple section of the duct which is sufficient to render a man sterile. The horrible associations of the word mutilation are inappropriate because the patient voluntarily undergoes the operation and we do not urge the legalization of eugenical sterilization save with the consent of the patient. To this vital fact the Dean makes no allusion, and it has evidently entirely escaped his attention.

Drawing a tooth is a nasty business, but if it causes us suffering we do not hesitate to submit to its loss without a feeling of degradation. Much less should such be felt if the suffering is spared not to us but to our innocent posterity.

R.A. Fisher,
Secretary, Eugenics Education Society.

Darwin to Fisher: 1 April 1926

My dear Elisha,

Next time you see old Elijah,³⁴ give him my kindest regards, poke him in the ribs on my behalf, and say I know how glad he must be to see how much better his mathematical mantle fits you than his χ^2 test fits ... —you will know how to put it to make him laugh heartily. By the by, I hope he won't read my book, or get it, just after he reads this review. If so, I, like you, had better avoid meeting him in a dark lane. But I think you imply your wicked document [CP 49?] has not yet seen the light.

I hope we meet Wednesday.

Yours sincerely,

Leonard Darwin.

Darwin to Fisher: 14 June [1926]

I have just been reading your Essay Review³⁵ with very great satisfaction. I will not pause to enquire whether it is too flattering to my efforts, but I will say without doubt that as a brief general essay on Eugenics it seems to me quite admirable. ...

There is only one line to which I want to call attention, tho' only with reference to the rating of our premises!! You say 'not a science'.³⁶ Possibly you would reconsider this phrase with a view to possibly substituting words somewhat like the following—'not all Science but all its inspiration drawn from...'

Now if you have the slightest objection on the grounds of morals, style, or science to such an alteration please put it entirely out of your thoughts. ...

Darwin to Fisher: 15 September 1926

I wonder what you have been doing this holiday time, if so it is with you. ... I have written a 4000 word essay review on a book by one Berg, a Russian, called *Nomogenesis*,³⁷ with a preface of a laudatory character, by D'Arcy Thompson. It is the most definite and completely worked-out attack on Darwinism that I have seen, giving one plenty to answer; indeed too much, for it seems to me to be very illogical. Now I don't know what to do with my Essay. ... I should much like your frank opinion on it some day, if you should at any time not be too busy to read it. Should it be burnt, is the question.

When last we met, you were saying you might write a paper on the mathematics of evolution and Mendelism—that is badly expressed, but you know what I mean. I do hope this idea will continue to hold good. I have had a few thoughts as to points which ought to be cleared up, and if the spirit moves me, I shall write them down and send them to you.

Darwin to Fisher: [late-September 1926?]

I have read your paper [CP 59] with great interest. ... All I will now say is that it increases my wish that you should deal with the whole problem of selection mathematically. You will have a small audience, but it will gradually be realized that many of these problems can be attacked in no other way.

I don't know why you expect me to disagree with you about men of science and their critics. It is an odd fact that only a week ago I was asking my sister if she did not agree with me that it would be worth republishing the first edition of the *Origin of Species* (you can't now easily get it to read, and I have never read it, I believe) because it was written before my father had been subject to any criticism whatever. His extraordinarily modest nature made him especially liable to pay too much attention to what others said. Somewhere he declared that he had made the mistake at first of paying too little attention to the effects of environment—the direct effects; and it is tacitly assumed that his second opinion must have more weight than his first. I should like the first edition republished with a few notes as to where it would be very generally allowed that the last edition was better, and what the changes implied.³⁸ ...

As to what you say in your letter about the evolution of unpalatableness, I had not thought of the point till you mentioned it. Suppose a bird is in doubt, when food is plentiful, which would be the choicest morsel, a butterfly or a fly. Let him select to go for the butterfly, and to find it a regular tit-bit. Will he not immediately repeat his attempt? On the other hand, let him be slightly disappointed in the taste, and will he not go for a fly next time, possibly returning later to the butterfly hunt? May one not assume that the more quickly the one attack follows the other, the greater the probability that the two victims will be close blood-relations? If so, does not this open the road to selection. ...

[P.S.] ... I will send you *Nomogenesis* by post.

Darwin to Fisher: 5 October 1926

Thanks for all the trouble you have taken over Prof. Berg. I will consider carefully recasting the review into an article of some sort.³⁹ Some of it won't go easily into a general evolutionary talk, I fear. As to your proposal to give *Nomogenesis* to MacBride as a kind of emetic, possibly you might consider ... what are the chances of the poison being assimilated and not ejected,

thus rendering the patient's condition quite hopeless⁴⁰. ... It is a bold proposal. ...

Darwin to Fisher: 6 January 1927

... I thought of getting MacBride to propose a vote of thanks [after the Galton lecture].⁴¹ My question is: who would be the best man to second the vote of thanks and to say with authority that we are still almost entirely ignorant of the causes of mental defect and that our knowledge is entirely insufficient to enable us to found a policy thereon? I can say a word or two, but it is harder when in the chair. It must be all very civil. ...

Darwin to Fisher: 29 July 1927

... I have just been looking at a book by Moore on Evolution and Religion—I forget its title—which made me rather angry. He is so unfair on my father and his views about Lamarck. He says my father never gave any credit to Lamarck. I know of nothing *published* by my father which is not expressive of appreciation of Lamarck as a naturalist. If you ever come across a *published* sneer, let me know. I mean not in letters never meant for publication. These were in truth merely letting off steam to a few intimate friends, who knew well how to discount them. I don't know why I write all this, except to blow off steam myself.

Darwin to Fisher: 1 November 1927

... I look on my letters to you in the light of pins, the pin pricks to urge you on with your great work on the mathematical theory of inheritance!

Darwin to Fisher: 22 January 1928

... We were talking of fecundity when last we met. I want to amuse myself by jotting down certain ideas, though I have a suspicion that they are really your ideas.

When the cuckoo began her nefarious practices, did she lay her eggs in other cuckoos' nests—which must then have existed? Do birds do this trick even now? It would seem the wisest plan, because the foster mother would then certainly be suitable to the task. Now if all birds allowed other birds to drop eggs in their nests, selection would not be brought into play. This is, I believe, the case with the S. American Ostrich with the *result* that they lay a great number of eggs. If some birds of a species allowed it, and others did not, the race of foster mothers would be exterminated by selection. We should expect a strong instinct to arise against such a practice. Can this partly or entirely account for the territory instinct? A pigeon will go 60 miles there and back for its food every day, so I have seen it said. A bird like that cannot mind another bird nesting within a mile of it because of food supply. Then again, if the male gets an instinct to pick the eyes out of any hen,

except his own, who comes near the nest, won't this make for domestic purity? May this also have been the origin of the *very* strict monogamy amongst birds?

Fisher to Darwin: 25 January 1928

... Now for the *really* important part of your letter; of course the cuckoo must have started parasitizing mainly cuckoos, but this is certainly not my idea, and I have never heard it before. A certain amount of such communalism once established would bring in some selective effects, I fancy. Consider the equilibrium which must exist between instincts making for perfect workmanship in the nest, or a warm, or a well-nourished brood, and the instinct to avoid danger with which the former must occasionally come into conflict, sometimes with self-nutrition also perhaps, certainly also, as you say, with fecundity. Start with these in equilibrium in a non-parasitic group, and introduce the communal habit of sharing eggs. You must at once begin to lower the standards of parental diligence, and to increase timidity, perhaps greed, and certainly fecundity. Chick mortality increases (which tends to raise again to some extent the standard of diligence) but it is only when the average cuckoo becomes a materially worse patient than neighbouring birds that an instinctive preference for foreign nests would be an advantage. Parasitism depends, in fact, on the co-existence of two different standards of parental care! At first, the young cuckoo in the foreign nest would do only slightly better than in his own, or some other cuckoo's and presumably would do worse than his foster brothers; but he is in a position to profit by fratricidal powers which would be merely harmful in the host, and can go ahead. The Rhea is excellent in showing that higher fecundity came before true parasitism.

I wonder what means of protection have been evolved. Some birds are particular enough to throw out objects which are not very like their eggs; others will sit on marbles. I understand that both groups are victimized, but the former more skilfully than the latter. This suggests that the method has paid in some cases, but is not a sovereign remedy. Now for a given population of cuckoos, would not the rarer hosts suffer most severely unless specifically protected? Are the rare hosts the more particular? Perhaps you have a fairly recent paper, I forget who by, who contrasts the cuckoos' eggs foisted on these two types of host. If not, I must get the reference from Huxley.

The effect on territory instinct would only work at laying time, though it might have been developed for this time and merely extended, as still useful, earlier and later.

Polygamy would certainly require greater powers of discrimination in the male; it would also give the young a smaller share of his labour. Is the inference that this labour is unimportant in polygamous birds justified? Except

as a guard, or a sentinel. I suppose *Gallus* is chiefly useful as a sentinel, or a lightning-conductor, perhaps, if his conspicuousness draws the danger on himself. Are not pigeons strictly monogamous, and at the same time gregarious in nesting? I suppose the nests are always distinct, and the right squabs always fed by the right parents.

Do you know if the non-parasitic relatives of the cuckoo are gregarious, like rooks? A communal territory might easily be a first step in their degeneracy.

Darwin to Fisher: 26 January 1928

Thanks for yours about cuckoos. ...

Do not ants give rise to some nice selective problems as regards fecundity? The ordinary ideas do not apply to sterile offspring. If the young females originally had their natural instincts developed abnormally young, and began to look after their young brothers and sisters, we see how a beginning might have been made. Then, if some were sterile, so much the better. And does not such a state of things put a stop to the ordinary check on fecundity? Here is a nice thing to think about. ...

Darwin to Fisher: 27 April 1928

Herewith correspondence,⁴² which I have found very interesting. I will hastily jot down my thoughts for what they are worth.

Galton said to me that Pearson can understand Bateson, but Bateson cannot understand Pearson. This seems to me somewhat the same case.

You say that abnormalities in vertebra number are correlated with other abnormalities. (It might be with advantageous differences.) He seems to reply that this indicates that when the vertebra number is normal, these other abnormalities cannot, therefore, exist, and natural selection cannot apply. Of course your argument does not imply this at all. The harmful or beneficial differentiation might be insufficient to bring about the correlated change in vertebra number. That is how I understand you. ...

I did not know my father used the word 'particulate'.⁴³ I thought that was Galton's origination. I guess he would have said that his knowledge only enabled him to look at things more vaguely. It is difficult to get back to that frame of mind. I believe Huxley once said to me that use might produce effects of a hereditary kind only after it had been in operation for many generations, though we could not see how. My father saw contradictions and could only build his theories on generalities. I doubt if he saw distinctions quite as clearly as we can now see them. That is all very hastily written. ...

Darwin to Fisher: 7 May 1928

I think you asked me as to the difficulties I saw in regard to natural selection connected with useless characters. I have little new to say, as I blew off steam on that subject in my Cambridge pamphlet on Organic Evolution. There I think I showed how my father, in *Descent of Man*, Vol. 1, Chap. II, 6th para. from end, said that many useless structures, as now supposed, would be proved to be useful; but that his omission of the consideration of such structures was 'one of the greatest oversights' of the *Origin*. It is probable, he said, that many peculiarities are of no service to the organism. He goes on to suggest an explanation, which does not seem to me to hold water. In the *Origin*, Chap. VI, he speaks of interlocking species being absolutely distinct in every detail of structure.

I agree it is extraordinarily difficult to point to any quality and say that it is certainly not correlated with any useful character. But there are so many where no such correlation appears to exist. Specific characters are, I believe, generally not correlated physiologically with other characters, and we should look to them for most variation. This we find, but why the remaining uniformity, which is often very great? ...

Darwin to Fisher: 14 May 1928

I am not sure that we have caught each other's meaning about useless characters. If not, it does not much matter. You say that the length of the 7th joint of your midge is a by-product of the developmental changes which have been selected. If I could believe that all these unimportant specific characters were necessarily co-ordinated with some other character under the sway of natural selection, I should feel that all my difficulty had vanished. I think I mentioned how my father, in the *Descent*, said he had made a mistake in not considering these useless characters, and how he strove to account for their uniformity. In the *Origin*—I quote from memory—he speaks of the uniformity in the same species of two interlocking species, of every detail of structure. Can this uniformity in every detail be correlated to some useful structure? In Chap. II, 2nd para. of 'individual differences', he suspects that we see in some polymorphic genera, 'variations which are of no service or disservice to the species, and which consequently have not been seized on and rendered definite by natural selection.' But how then have they become sufficiently definite to separate even varieties? In Chap. V, 'Correlated Variation', para. 5, he speaks of modifications viewed as of high value being possibly due 'to the laws of variation and correlation, without being' of the slightest service. Here, I presume, he meant correlated with some useful structure. But this should be read in conjunction with what he said in the *Descent*. What is an 'important structure'? Is it not one generally which is bound up with the whole method of functioning of the organism? If so, it is one tied by ties to other structures, and in such circumstances

it cannot vary much. The systematist is, so I think, on the horns of a dilemma. He must take qualities which do not vary so much as to overlap the two groups he is comparing. But, if descent is the real basis, he must take the more rapidly changing characters, which are the most variable. The colours of butterflies vary very greatly, but are a useful specific character.

I have written out my correction⁴⁴ of my error, as I now think it, in my Natural Selection paper, and I will send you a copy before long. I should like *your considered* judgement some day. ...

Darwin to Fisher: 5 July 1928

Mark Twain tells somewhere how he could only get some lines, which were running in his head, out of his mind by telling them to a friend. I have been thinking over your dominance theories, and I want to blow off steam, and get rid of my thoughts. Mark Twain did not make nonsense of the poetry, and did not get rid of them on to the poet. So the cases are not quite parallel.

You bring in the idea of modifying factors. If these are separate entities, must we not suppose that a species has now modifying factors for every past mutation, if now recessive, which ever occurred? Moreover, why should not there be modifying factors in the mutant also?

Can we get a simpler way of putting your theory by assuming that the original species, **O**, has some individuals (**O**⁺) which are more dominant, and some (**O**⁻) which are less dominant to the mutant **M**. Also that the same is true of **M**, some (**M**⁺) being less recessive to **O**, and some (**M**⁻) being more recessive to **O**. ...

I daresay there is nothing in all this. So don't answer. ...

Fisher to Darwin: 7 July 1928

I will answer your letter in spite of your protest, because you are one of the very few people who will ever appreciate the consequences of my suggestion,⁴⁵ so I shall be especially particular that you shall understand me clearly about its framework.

I take **O** and **M** to be physical organic structures (genes) handed on from generation to generation. For some millions of generations selection has always favoured **O** and we should have long ago seen the last of **M** if **O** had not regularly mutated or changed into **M**, sufficiently often for about one in a million **O** genes to turn to **M** in each generation. This keeps a certain supply of **M** in being, a number proportional to the mutation rate, though also influenced by the intensity of the counterselection.

If **M** possessed an advantage over **O**, no such situation would have occurred, for **M** would replace **O**, apart from back mutation, in a few thousand generations. (I need to think about the case in which **M** is sometimes, in certain places, advantageous.) The case I deal with, and to which I believe

the mutations of our little genetical samples nearly always belong, are the importunate failures.

If the mutations of **O** were of several different kinds, producing **M**, **M**['], **M**^{''}, etc., (as is known in some cases to be the case) from the same kind of **O**, this will not help any progressive change, for the mutants we deal with are those which actually arise in the cultures and are brand new; in any case, the old mutant genes must all fairly soon be extinct, the supply depending upon fresh mutations occurring. However, something does seem to have happened to **O**, supposing it to have been originally a mutant of a proto-original gene **W**, for, whereas the heterozygotes **OM**, **OM**['], **OM**^{''}, all look like **O**, the heterozygotes which we build up by artificial matings **MM**['], etc., are intermediate between **MM** and **M**[']**M**[']. This is my first fact; the original puzzle which set me thinking. For even when **M**['] arises as a mutation from **M**, **MM**['] is still intermediate.

Your more dominant form **O**⁺, I represent by **Oa**₁, and the less dominant form **O**⁻ by **Oa**₂; here **a**₁ and **a**₂ are alternative genes, one of which doubtless arose from the other by mutation. There may be any number of such so called modifiers (all Mendelian factors are modifiers if we choose to think of them as such, though doubtless some only affect the degree of dominance shown in **OM**); thus **Oa**₁**b**₁ may be **O**⁺, **Oa**₁**b**₂ may be **O**⁺, **Oa**₂**b**₁ may be **O**⁻, **Oa**₂**b**₂ may be **O**⁻. All that this means is that **OMa**₁**b**₁ is most like **O**, **OMa**₂**b**₂ most like **M**, least like **O**, and the other two intermediate.

Quantitatively, the effect of the modifying factors on **MM**, if any, is of no consequence, so long as dominance is incomplete, for **MM** will be then so exceedingly rare that no appreciable part of the ancestry of existing individuals will have been **MM**. But I show that an appreciable part will often be heterozygous, **OM**, and in this part the + modifying genes will have been selected, thus tending constantly to produce complete dominance.

It is interesting that such a selective influence acting on a thousandth part of our ancestry should have made us completely dominant to the many importunate mutants which have been shot at our race, and this accords with the view that they have been clamouring at the gates for more like millions than thousands of generations.

Since we distinguish the effects of the factors **a**, **b**, etc., only in the combination **OM**, they cover both the distinctions of your letter **O**⁺ or **O**⁻ and **M**⁺ and **M**⁻.

About the supply of modifiers there is a very satisfactory answer. If I wanted to increase human stature I should select from the mass of modifiers in the existing population, and quickly enough build up a type exceeding the tallest normal variants. At this stage I should expect physiological disharmonies to appear (control of growth, blood pressure, etc.) and selection would be chiefly concerned in remedying these, and if the process had only taken 10 generations or so, I might be held up and have to wait for favour-

able mutants; but if I were content to produce the same change by a mild selection in 10 000 generations, I could never deplete the supply of modificatory variance, and it would always be available well in advance, as it is now in stature. In modifying dominance, natural selection only examines one individual in 1000 or 10 000, and consequently the supply of modifiers is never depleted, and the minute selection at work produces always its full effect.

But what a striking effect for such a minute selection! ...

Fisher to Darwin: 7 August 1928

... I think, in fact I am sure, that we have very much the same picture of evolution in our minds, but the picture in my mind has been changing of late, not in any way in principle, but, by groping after approximate magnitudes, in the proportion of the different parts. ...

My suggestion about dominance makes me think of mutation rates as changing rather slowly, since the mutations which have become recessive in this way must have been very persistent. If, then, there is a possible but exceedingly rare mutation which is slowly increasing in frequency, then it may 'take' if it happens to occur and happens also to get a good start, at an evolutionary stage at which it happens to be beneficent. But I suspect now that its usefulness to the species will change just as rapidly as its mutation rate can be expected to do. That is why I feel that the situation of the species waiting for the lucky mutation to occur may be quite an unreal one. I am inclining to the idea that the main work of evolution lies in the discovery by trial of perhaps rare combinations of its existing variants, which work better than the commoner combinations. A slight increase in the number of individuals bearing such a favourable combination will then set up selection in favour of all the genes in the combination, with marked evolutionary results. Many of these genes would have been previously rare mutant types (not necessarily rare mutations) unfavourable to survival.

I think of the species not as dragged along laboriously by selection like a barge in treacle, but as responding extremely sensitively whenever a perceptible selective difference is established. All simple characters, like body size, must be always very near the optimum, so much so that the average body sizes of two alternative genes must be balanced on either side of the optimum, selection always tending to eliminate the rarer because it is further from the optimum. The selection in this case is proportional to the square of the magnitude of the effect of the gene, and a species affected by mutations making it larger and mutations making it smaller will select persistently against both lots and make both recessive. If now an increase in size becomes desirable, a number of the recessive enlargers will triumph, and the recessive diminishers will remain as rare recessives. So that the prevailing bias of dominance (enlargers being more often dominant than

diminishers) will reveal the direction of the prevailing selection of the recent past. I should like to know if intelligence is less dominant to stupidity among Englishmen than among (say) Afghans.

Is not the case of poultry queer? There must be 8 or 10 factors in domestic breeds, non-lethal and dominant to the apparently wild-like characters. I do not feel it personally as a difficulty to my theory of dominance, because on any view one would want to know why poultry should behave differently from other beasts and birds, to say nothing of plants; and to this we have no clue. That species crosses have occurred is likely, and though all possible species have, I believe, single combs, they may, as you suggest, [be] genetically unlike single combs, which on combination might give Rose and Pea. Is any form of unintentional human selection possible? Were hens only kept at one stage, constantly outcrossed with wild cocks, and so only dominant novelties selected?* (probably some cocks also).

* P.S. I believe this *works*. The primitive fancier would have to be always selecting heterozygotes from wild-type birds in the same brood, and would therefore be constantly increasing the contrast. Dominance of several of these fowl dominants is very variable in its completeness in different breeds. How is that!

Darwin to Fisher: 12 October 1928

... I am glad you are at work on your evolution book. I shall be delighted to be of any use, and could read your chapter any time—not that I expect to be of use. How about your new statistical work? I hope they can go on simultaneously. *Don't* hurry evolution, but *do* go on with it. ...

Darwin to Fisher: 5 November 1928

... I hope now to tackle your chapter in earnest. I have only seen so far as to convince me that it will be a very important book, well worth labouring over. ...

Fisher to Darwin: 13 November 1928

Very many thanks for the care you have given to reading my Chapter [I]. I wish I could believe it was worth the trouble. I have decided to write on, sometimes ahead of my convictions, with a view to subsequent careful revision, which I hope may be less difficult than making a fresh start. I should like first to thank you generally for many smaller hints which I probably shall not mention separately.

I had expected you to demur to my version of your father's reasoning, because I am concerned to reconstruct the earlier and possibly subconscious elements of an argument, which possibly he himself might repudiate in later years, yet the effects of which can, I believe, be traced in quite late opinions.

My belief is that your father was more capable than most men of relatively long logical trains of theoretical reasoning, but that he utterly distrusted his power of giving them expression, and later tended more and more to delete his reasons in favour of his evidence. Myself, I most admire the reasoning (hence quotation (2)).⁴⁶ Ultimately I should like, if you would permit it, to incorporate your testimony as to your father's views in footnotes or otherwise, in cases you consider important.

Instead of saying that your father accepted the theory of blending inheritance, I might have said that he accepted its logical consequences, which no one else seems to have perceived. I take the phrase 'our ignorance of the laws of variation is profound' to mean our ignorance of the nature and particular causes of the mutations induced by the environment, though perhaps he also felt the same about inheritance. I should be very glad of any reference to supplement the letter to Huxley,⁴⁷ which reads to me as though the idea of mixture v. fusion seemed then new and conjectural. I had noticed the term 'unequal blending' and it well shows the kind of way in which he was trying to reconcile the blending theory with the difficulties he felt in connection with it.

Did he go by facts *rather than* theories? May I suggest that he, later in life especially, felt it his duty to, but was far too great a man not to anticipate many facts before they were observed.

You have taken my point about the last quotation. The principle of exclusion is a very great principle. A man is more right in drawing the best conclusion from the facts available than in drawing the right conclusion, if it is not the best on the facts. ...

Did he say the nature of [the] organism is far more important than the surroundings in *causing variation*? I should have expected him to say 'in determining what variations are caused'.

As to any erroneous views your father held, my point is that they all sprang from an assumption for which he was not responsible, and that he was more right in drawing the logical consequences of that assumption than were those who failed to see them.

I must restate one point. I do not argue that mechanisms for causing mutations, by volition, use, etc., do not exist, but that if they do exist they are ineffective in causing evolutionary change. On this view I can afford to deal very slightly with the arguments for and against such mechanisms. I agree that the power of transmitting acquirements might have been attained by Natural Selection, in which case it would not be primordial. I do not need even to exclude blending entirely, only that variance due to such causes is trifling in amount. ...

You will groan to hear that I am going the whole hog about dominance;⁴⁸ any example to the contrary is therefore badly needed. ...

I have finished drafts of Chapters II and III, but not yet started IV. They

are at your disposal but I do not want to press a lot of heavy reading on you. Chapter II is heavy. I want you to read III when you form an opinion on whole-hoggism.

[P.S.] Thank you ever so much for real encouragement.

Darwin to Fisher: 17 November 1928

... Your letter brings us much nearer together. If you say as to blending that he accepted its logical consequences, all right. I do not mean that I had myself thought it out thus, but that I agree. Remember that if you say 'universally', it includes yourself. Also ... [in Chapter I] of *Origin*, VIth [edition], my father wrote that 'the laws governing inheritance are for the most part unknown' [and] in the IInd [edition] 'quite unknown'. I suppose that he saw the difficulty of blending, and until he could see the way out, he must hold that the laws were unknown. I have not now studied the passage and don't know quite what he included in these laws; but, anyhow, you have to reckon with these words. I took the words about ignorance of laws of variation being profound from Chap. V, beginning of Summary. That may help you to judge what he meant. As to supplementing what he said to Huxley, it may be worth noting that ... [in the] summary of Chap. IX, he says sterility depends on the organism of the hybrid being 'disturbed by being compounded from two distinct forms'; wherein he was, I suppose, nearly right; though he goes wrong in the next sentence.

Under the heading of Causes of Variability [in Chapter I], he says that the nature of the organism seems to be much more important, 'for nearly similar variations sometimes arise ... under dissimilar conditions'. I think this supports your view. ... [At the end of this section] he speaks of 'determining each particular form of variation'. ...

I should like to read your other Chaps., and shall be ready for them a week hence. But I shall not understand them! And I shall anyhow do no harm.

Darwin to Fisher: 17 December 1928

I should have written before this to thank you for Chap. III had I not been rather seedy. Nothing much amiss, but it seems to addle my brains. You must not pay too much attention at any time to any of my criticisms, because they are just written straight away, and may easily be erroneous. It may suggest thoughts, that is all.

What I had mainly in my mind about Chap. II was probably the point I tried to make in my article on N.S. in the *Review*, and the letter subsequently correcting it.⁴⁹ It was that the necessity of co-ordinating the different parts of the same organism is the main check on the pace of N.S., and consequently that, with complex organisms, the pace is very slow when co-ordinated changes have to be effected. If the colour of a butterfly can change

without any change in any other quality, it can be quickly made to fit its surroundings. The point which I did not see, and your chapter has made me see, is that the more complex the surroundings, the slower will be the adaptation. If there is only one other butterfly to mimic, N.S. will do the job quickly. But if there are 2 or 3 different butterflies, to imitate each of which would be advantageous, the benefit from imitating any one of them is likely to be diminished, and N.S. made proportionately slower. It seems to me therefore probable that it is generally true that the simpler the organism *and the simpler the surroundings*, the quicker will adaptation take place. Lowly organisms at the bottom of the sea will become almost perfectly adapted to their surroundings, and will, therefore, not alter for vast periods of time. On the other hand a highly complex organism in a highly complex environment will move so slowly, and will have such vast possibilities before it, that it would take a practically unlimited time to reach the stage when no further improvement would take place. I may have been making the assumption that the possible range of mutations is more limited in the simple than in the complex organism. But I want to establish the view that evolution of complex organisms will go on quite indefinitely in an unchanging environment. But please remember my brain is yet a bit addled. ...

Darwin to Fisher: 23 December 1928

... If I have stimulated you to rethink over these problems, that is as much as I hope for. I do *not* mean this to imply doubt, but in these new and difficult regions, reconsideration is nearly always useful.

I will only make a few general remarks. One of your points I could only deal with at all properly if I had your chapter again before me. I agree as to there being an ideal organism, developed from a lion, which would probably be unlike any existing animal; this, I presume, in an unchanging environment. In other cases, with simple organisms, the real and ideal might be much alike. Natural Selection, having a limited scope for action, must concentrate chiefly on the qualities, which, in their range of variations, have the most clearly marked peaks of advantage. These peaks will be most likely to occur where the conditions are most simple, conditions in the organism and in the surroundings; and these conditions seem to us petty. It is here that we get the quickest action, and therefore specific differences. May we not say that 'fundamental' differences, such as those between the qualities of orders, are such as affect the co-ordination of many parts of the organism? I am here putting your ideas, as I apprehend them, into my words. It is going over the same ground again. But it won't take you long to read.

I have not begun Chap. III yet. ...
P.S. The ideal lion can be no further evolved by N.S. What is then to set

evolution again working? It can only be a change of environment. If organisms often reached the ideal stage, changes of environment would be of great importance. Organisms living in the sea ought then to be much less evolved than organisms living on land. This is not markedly the case. They are less differentiated rather than less evolved. There is no land animal which has a lantern on its snout to light up its prey. Hence I think changes of environment are probably not of supreme importance. ...

Fisher to Darwin: 28 December 1928

I had not answered your last letter when Chapter III arrived with your comments.⁵⁰ I am glad you think it is not out of place.

Let me take your numbered points.

(1) and (2) adopted with gratitude.

(3) New loci must appear, I suppose chiefly by doubling whole chromosomes and later gradually specializing the functions of the duplicates, [and] sometimes by attachments of bits of chromosomes to the ends of others. I do [not?] think I can do anything with this though.⁵¹

(4) I of course agree strongly about recessive mental defect. Those who do not must put up a case. What an achievement for a mutation to raise a feeble-minded race up to normal mentality!

(5) I think if you listed the human defects for which there is strong evidence of single-factor inheritance most of them would be dominants, for the evidence in the case of recessives is seldom very strong; hence my remark that albinism, which by analogy everyone would expect to be a simple recessive in man, is still a disputed case on the human evidence. If this seems clear, send it back and I will rewrite the sentence.⁵²

(6) is a subtle point. I do not think it is so much the fault of the wording as of the idea; we have much experience of the relation (Common, Wild, Mother) gene dominant to (Exceptional, Mutant, Daughter) gene. Is the dominance to be ascribed to the relation Mother-Daughter or to the relation Common-Exceptional? The cases which settle this are (Exceptional, Mother) not dominant to (Exceptional, Daughter), (Exceptional, Sister) not dominant to (Exceptional, Sister), [and] (Exceptional, Mother) recessive to (Wild, Daughter). I have called the 'mother' gene the predecessor, and the 'daughter' gene the successor.

(7) About species v. orders, my point is simple, but I cannot say that it is exactly the same idea as Wallace, Bateson, [and] Robson have had in mind on the same theme. I can understand that the dentition of a lion, which is characteristic of his order, is suitable for tearing flesh, as contrasted with that of a goat. But as to the mouths of lions and tigers, which I suppose are somewhat different, as doubtless are their prey, I do not think we know enough to understand the association of the two sets of differences, or other

relevant explanations of the specific distinctions, except in colour, where we have a glimmer, only because we are better placed to appreciate it.

(8) I do mean that a mutation might have an effect if the pupa were kept at 20° but none if it were kept at 30°.

(9) I must write more (probably in Chap. II) on intensity of adaptation.

(10) I do assume the maladaptation to be capable of repair; is this all?

(11) I think this is much to the point, but it is a very elusive question. The leaves of trees are the best example you have given me,⁵³ but do we know enough even now to think about plants? An engineer finds among mammals and birds really marvellous achievements in his craft, but the vascular system of the higher plants, which we do not understand, has apparently made no considerable progress. Is it like a First Law, not a great engineering achievement, but better than anything else *for the price*? Are the plants not perhaps the real adherents of the doctrine of marginal utility, which seems to be too subtle for man to live up to? We can understand that a leaf must catch a lot of light, must not snap out quickly, should be distasteful to parasites, but we understand nothing of the *workings* of each of these desiderata. Can we judge well without this knowledge?

I am sending back [Chapter] III so you shall have it in reading this, not to worry you with it again; also V⁵⁴ which I hope will interest you, even if it does not please, which latter must always take its chance, though I am more confident some times than at others. IV is unwritten, and I am labouring almost vainly at making it clear.

Darwin to Fisher: 2 January 1929

Thanks for letting me see III again, with your counter-notes. ... I feel a little alarmed that you take my remarks so seriously. If *you* think the matter over again, and stick to your point, then *I* shall be satisfied.

I made, before getting your last, some rough notes, partly for my own edification on some of these points, and I send you a copy which my wife has written out for me by dictation. Now *don't* bother to comment on them, nor return them.

I have not attacked V yet.

Darwin to Fisher: [early-1929?]

... The question of overlapping species gives rise to some nice problems. See *Origin*, ... beginning of Chapter VI. If two species in the same area are equally well adapted to their surroundings, then the contest between them depends solely on their relative rate of multiplication. Does not this tend to make each species take a definite area for itself? My Father seemed to see this, but not with mathematical exactitude, and I am not quite sure that I see it either. ...

Fisher to Darwin: 15 January 1929

I am answering your last letter piecemeal, so do not answer unless you feel so inclined. I now return the Galton letter, which I thought so interesting that I had it copied, so now I ask your permission to keep a copy.⁵⁵

It is perfectly true that village communities may be much isolated, but I wonder if Galton ever considered (or people like Fleure, who find 'neolithic' villages all over the place) how complete the isolation must be to be worth anything genetically.

If only one in 10 filter in from outside in each *generation*, in seven generations half the population comes from outside and in 70 generations all but 1 in 1000. Isolation would be very extreme at this level, in the ordinary course of events, and catastrophic events, war raids, famine, plague, are not so uncommon as to be ignored in the case of such habitual isolation.

King Solomon lived 100 generations ago, and his line *may* be extinct; if not, I wager he is in the ancestry of all of us, and in nearly equal proportions, however unequally his wisdom may be distributed.

You see I shirked the problem of optimum mutability for asexual organisms [in Chap. VI], merely proving that there must be an optimum. The problem has a very beautiful general solution in operative form, but I cannot make it manageable for any simple case. I will try again owing to its importance for single loci, where I believe (at the optimum) most would be absolutely uniform (at least apart from the rare defects always being eliminated); perhaps all loci have a few lethals going.

Darwin to Fisher: 16 January 1929

I thought you would like Galton's letter, and am glad you have a copy.

As to Bateson, if I had to write, I should write something like the following. But I am not well up in what he did do, and may well blunder. ...

In the future the great merit of Mendelism will be seen to rest on the proof that the ingredients of the germ plasm on which heredity depends are located in pairs in each organism, one of each pair selected by chance disappearing at each sexual union. On this fact a rational system of evolution can be based, and it is, therefore, of enormous importance. The merit for this discovery must mainly rest with Mendel, whilst amongst our countrymen, Bateson played the leading part in its rediscovery. Unfortunately he was unable to grasp the mathematical or statistical aspects of biology, and from this and other causes, he was not only incapable of framing an evolutionary theory himself, but entirely failed to see how Mendelism supplied the missing parts of the structure first erected by Darwin. Nothing but harm can come from following Bateson in regard to evolutionary theory, though his name will come to be honoured for his pioneer work in Mendelism when what he failed to do as regards theory has been accomplished.⁵⁶

Having written it, I daresay I should tear it up, and advise you to do ditto. ...

Fisher to Darwin: 21 January 1929

Many thanks for the note on Bateson; it puts the point admirably, and though I have already altered the wording somewhat, it seems to me just what was wanted.

The only thing to do is to commend Bateson's enthusiasm for genetics, without saying, which would rather comfort my conscience, 'while greatly retarding its progress in his own country'. But it is difficult to be sure. How far did he alienate the better biologists, e.g. Poulton, Goodrich, from Genetics, and how much did it matter? I wish one could deal frankly with peoples' ideas without seeming to asperse their august persons, but then a man's value *as a man of Science* lies in his contribution to Science.

I have just been reading Samuel Butler's *Luck or Cunning*; what a malignant knave he must have been, yet Bateson borrowed his sneers and quoted his opinions.

Fisher to Darwin: 18 February 1929

... I am sending you a copy of Chapter IV, which will have to be Chapters IV and V, as it has grown so confoundedly long. Do not try to read it, except the summary and any points which the summary makes you want to look up in more detail. I have made an abominable mess of the whole thing and failed to get out an adequate solution of nearly all the problems, but I hope it may at least show what further work is needed.

I have made a start with Mimicry (Chapter VII), which will contain little more than a paper on the subject [CP 59] which I think you have. The rest of the book will be essentially Man, and I hope about four Chapters may do it. Do not tell me that this is unintelligible and, when examined, so incomplete as to be scarcely worth understanding, for I know that already.

Darwin to Fisher: 23 February 1929

I was delighted to see the R.S. [Royal Society] list⁵⁷ in *The Times*. You have won a well-deserved honour, and you may justly be proud and pleased. I am so glad that the R.S. is beginning to realize the place that statistical investigations must play in science. ...

Fisher to Darwin: 25 February 1929

I knew you would be glad, and your pleasure is as good to me almost as though my own father were still living. He lived long enough to see me fail in two occupations,⁵⁸ and to hear me say that I was on my feet in research. That is nine years ago, and it has gone well.

I wonder if you have any words of wisdom on a contingency which I suppose is not now too improbable to be considered. If I were offered Pearson's Chair,⁵⁹ what in your opinion should I aim at making of that place. It would be easy to continue mathematical researches, and possibly

in time to build up a reasonable biological outlook. Is that the whole programme?

Do you remember the help you gave in getting my first Edinburgh paper [CP 9] accepted, and introducing me to Horace Brown?⁶⁰

[P.S.] I enclose a good one from MacBride; he has just refrained from underlining *Mathematics*.

Darwin to Fisher: 1 March 1929

You give me rather a difficult conundrum to solve about the professorship. I told you that you were unlikely to get into the R.S., especially first shot, and if I now strongly advise you not to begin to count your chickens, I really hope that I am equally at fault. Even if you are to get it, I am inclined to think that the first effort should be to keep on on the old lines to a considerable extent, whilst making the value of the results bear some relationship to the labour involved. From this and other points of view, it is well to realize that those of the staff who hold regular University appointments—I do not know how many do—cannot be got rid of, even if you should desire to do so. ... Nothing short of murder is now a sufficient excuse for sacking a reader or other high official. This will make them more independent, and difficult to turn on to new lines. They will, moreover, all be more or less prejudiced, I suppose, against anyone who is connected with the Eugenics Society, and taking over such a staff may make the job rather far from a bed of roses. Those who could stay with Pearson became, as far as I could see, his willing slaves, and that spirit won't wear off quickly. It will also probably affect the chances of your appointment. The Board consists of (1) outsiders of highest standing, (2) members of the University, (3) members of the College. If Pearson is alive, he will pull his hardest to get the last two lots appointed so as to back his nominee. And you must admit that you have not always dealt with him in the gentlest way. And as to the outsiders, they must and are right to be a good deal influenced by what the University people say as to the probabilities of smooth running. I am writing exactly what I think, even at the risk of saying what is disagreeable, and showing myself a false prophet. But that is, I am sure you will recognize, what should be done by a true friend.

Darwin to Fisher: 4 March 1929

Perhaps I did not express myself clearly also. If I knew I was going to get the job I should look on it to a large extent as a running machine, with a good deal of momentum. I should consider that it could not be stopped and directed in any new direction quite at pleasure. I should feel that my task would be rather to guide it gradually into better paths. And that I could hardly form any sound idea of what these lines should be in detail till I was in the saddle. Fixed ideas would be little use. This would be my idea of what

I should do myself, and it may have made me lazy in not thinking out the lines I should adopt if I had to decide in advance. I have no fear of your not having sound ideas enough. If you got the job tomorrow, I should hope that the finishing up of your book would be a main task, together with some new investigations to confirm your theories. For instance, get land shells from an island, sufficiently different from the mainland form to prove long separation, and sufficiently alike to be comparable; and then measure their variance. Your work on natural selection will confirm the theories on heredity which you hold, and I am sure that Galton would have felt that anything which made hereditary theory stand on more sure foundations would be a valuable help to eugenics. Broadly to bring about that result by statistical enquiry would, I hope, be your broad aim.

I have dipped into a few pages of Chap. IV-V, not more as I have had a job on hand. I wonder if I understand rightly the increase of variance with numbers. With a 'population' of a single couple, the result would be a pure line, and no variance. That I see fairly well. But it never occurred to me that the more you depart from 2 as a population, the greater must become the variance. I wonder if this is thinking on right lines. It seems to me very important from the species-making point of view. A species in a big area will be divided into groups of different sizes, and not breeding quite freely together; and they will come to have different variances, and different rates of progress. They will also advance on different lines somewhat, and the bigger will kill out the smaller, and so a split will take place. I wonder if you will touch on these problems.

Darwin to Fisher: 8 March 1929

The impression I get from this chapter [IV] is that you have been digging in virgin soil, and that if you have not covered the whole surface, it is because the ground is very very stiff. In pioneer work of this kind, no one can be expected to solve all the problems.

I have the satisfaction—perhaps not wholly unalloyed—of finding that my father's view as to big species ... [*Origin*, Chap. VI] are right and that my criticisms on p. 19 of my *Organic Evolution* are wrong!⁶¹ At least, that is how I read your conclusions. ...

I give on separate pages a few notes. They are of little value, but I wrote them down as I thought about it.

Don't bother to discuss any point.

It is a big work, but you will win through.

Fisher to Darwin: 19 March 1929

Many thanks for the little copy of the *Origin*. I hope some time to compare it carefully with the 6th Edition, though it is not easy always to appreciate

whether the changes are intended only to improve the form of the sentence, or to modify its sense.

I forgot whether I have ever broached to you quite an old intention of mine to dedicate my book to you, with some such remark as that I have discussed with you some of the problems during 15 years. I cannot help it that this will be read as an overstatement and as implying that you agree with more than you do. I imagine that such an implication in so far as it is misleading will apply chiefly to the human chapters.

I enclose the introductory chapter on Man, which is necessarily rather diffuse, but is aimed at preparing the reader for what follows. Also Chapter VII in case you care to look at it. I do not expect you to agree that I am necessarily right about Man, but only that I am approaching the subject in a rational spirit.

Darwin to Fisher: 20 March 1929

Thank you for the two chapters safely come to hand. I hope to tackle them before long. ...

I shall be proud to have your book dedicated to me, and it will greatly enhance the pleasure with which I shall see it in print. I am not the least afraid of being tarred with the same brush as yourself, especially as a dedication never implies complete agreement. I am only afraid that you will imply that I have been of more use to you than has really been the case. ...

Darwin to Fisher: 26 March 1929

I was glad to see Mimicry again. It seems to me a good bit of work, and we hope it may make wiser biologists see that some of their problems can only be attacked mathematically or numerically.

Your Chap. VIII seems to me to be one of the most interesting in the book, and very well written. I have made a few notes in the margin where it seems to me improvements might be made. One idea one sentence is, I think, a good rule. All about ants interested me *much*.

In all essentials I see no reason to differ from you. A few minor points we don't see eye to eye. I cannot agree with what you say ... [*GTNS*, p. 190] about the elephant's trunk. The brain seems to me a *far* more complicated affair. All that I could say would be that the trunk is, like the brain, such a complicated affair that we are quite incapable of fully understanding its action.

I don't agree with what you say as to my father's views—see the first pages of my *Organic Evolution*.⁶² He thought evolution, I believe, of enormous importance in itself as helping to co-ordinate many facts—in geology, embryology, etc. He felt that Lamarck had not opened his eyes, and without a real cause he could not open the eyes of others. Then ...

[GTNS p. 192] you seem to me to ride far too easily over the greatest difficulty in human thought—uniformity and free will. ...

I find myself bound to believe I have free will, and also bound to believe I have inherited conscience—and the two ideas seem to me contradictory. It is the mystery of mysteries, which I do not happen to have solved! ...

Fisher to Darwin: 28 March 1929

Many thanks for your letter on Chapters VII and VIII.

About free will, modern physical views do seem to be beginning to make a little difference to the problem. If you consider the two alternative dogmas—(i) the exact laws of physics can be expressed as differential equations, (ii) the exact laws of physics are statements of probability, I doubt if any of the wave mechanics people would say now that (i) is more probably true than (ii).

If (ii) were true, interest centres on the ultimate independent units, independent being now defined purely by the law of compounding independent probabilities. Such units are like monads, there is no going behind them, and though the behaviour of a large aggregate can be predicted, that of an individual cannot be. Monads need not be permanent entities in time.

The question arises 'What determines which possible course a monad will take?' and the answer on this system is definitely *NOTHING* external to the monad. We may, if we like, say the monad chooses, but not that its choice, like that of man, in my use of the term, is influenced by outward circumstances.

There is no contradiction to rational thought in all this, though it certainly leaves unsolved the question of undetermined choice in the animal brain. It is not easy to imagine a system of considerable physical size the behaviour of which is appreciably arbitrary, but, though not easy, it is not impossible.

I doubt if all this affects my actual argument, which only requires that different men should behave differently, and would, I think, apply quite well to automata if they had an illusion of free choice.

I am particularly anxious to avoid misrepresenting your father's views; though I do not agree in emphasis with the earlier pages of *Organic Evolution*. If Lamarckism had seemed acceptable I think it would have done all that your father said about Natural Selection and would therefore have been as important as Natural Selection really is. To me it all hangs on the if. I believe your father jibbed before 1837 at putting forward the historical evidence without an effective working cause, and that this attitude he would feel to be his duty as a follower of Lyell in geology.

In order to give a better form to the sentence, I have amended [it] to:

With a clear grasp of scientific principle which is not always sufficiently appreciated, it is evident that they felt that the mere historical fact of descent with modification,

however great is popular interest, could not be usefully discussed prior to [or] (was of minor importance compared with) the establishment of the means by which such modification is being brought about.⁶³

Let me know if this seems to you a true statement of the state of opinion which made the reading of Malthus the turning point in the development of Evolutionary theory. This is not quite the same as asking you to agree with me in the matter of emphasis, which I do not altogether expect.

There is a sense in which an elephant's trunk is more different from a pig's snout than a man's brain from a dog's. I will even claim than a man's mind than a dog's, which is more than I can say. However, the example is not the best I could have chosen and perhaps I ought to suppress it. [P.S.] *I have just received a third daughter*. All well.

Fisher to Darwin: 2 April 1929

I have the chapters back and wrote a reply which I find is still waiting to be typed, so this will go with it. ...

I have considered but not written about selection of mutation rates, and I am convinced that they are too small to make any difference. The only exception I should make is that deleterious mutations which have perhaps been occurring for millions of generations might in the course of time become very frequent, and this could be checked by Natural Selection. It is interesting that actually they do not seem to get beyond about 1 in 10^5 , which seems to me a marvellously high level for Natural Selection to check them at. This is in *Drosophila*; plants are certainly different and we need to know more about them.

If I am right, beneficial mutations when they are being selected must have rates of about 10^{-9} or 10^{-12} , and a strain with double the average mutation rate would have no time to increase before the whole population has adopted the new mutation. ...

Darwin to Fisher: 5 April 1929

I am a bit hunted, as I am going away for a few days, and want to wipe things up first. Hence I do not expect to answer your letters for a week or more, except to thank you for telling me your news. I hope that mother and daughter both go well. You are answering, 'What is Eugenics?' in the most practical manner.

Darwin to Fisher: 12 April 1929

I can now answer your two letters ...

[As] to my old friend free will, I am afraid you don't help me. Eddington says we can foretell an average because it is an average. I don't agree. ... You do not seem to me to get over the fact that determinism is a necessary

postulate of science, or to help me in believing this at the same time as free will. ...

Then as to my father's views, I daresay I did overstress what I said in *Organic Evolution*, for I did not then realize the effect of the pressure to make him minimize natural selection. What you propose now to say seems to me quite correct, i.e. 'could not be usefully discussed ...'. You leave out the word 'importance', which I believe constantly leads us into trouble, not being defined. Looking to the future we might say that the discovery of the methods of evolution are of far greater importance now than the fact of evolution. But it would be hard to say exactly what was meant. Anyhow it would imply that the fact of evolution was firmly established. If that is *not* admitted, then we should say that the loss of a belief in evolution would be a more important catastrophe than the loss of a belief in natural selection, the whole being greater than the part. ...

Fisher to Darwin: 18 April 1929

I think you have answered Eddington rather than myself, about free will. What I mean will be clearer from a related point. On a purely deterministic scheme, causation itself would be an illusion, [since] all things being already assigned their appropriate places in space-time, it would be very arbitrary to take two items of the *nexus* and call one cause and the other effect. This would be so even if subsequent and antecedent in time were unambiguous terms, for, as Eddington emphasizes, one might reverse these terms. Introduce arbitrary elements and causation takes quite a definite meaning, that if A had happened otherwise (as it might at that instant quite well have done), then B would have been modified. Now I feel that the reality of causation originating in self is all we have a right intuitively to claim; put in this way, one abstracts the essential element in the psychology of choice from all its less relevant connections. I admit that one ought still to hesitate about saying 'it is I that choose', because it is not clear that the 'I' can be identified with any particular element of the activity with which we identify ourselves. But physical arbitrariness does seem to have the great merit of reinstating causation.

Fisher to Darwin: 11 May 1929

On paying my sub. to the Royal [Society], I received a number of forms to fill in, among them a very meagre one designed for statistical information, asking I think nothing but my age.

It struck me that the body of Fellows is itself an interesting body Eugenically, and that whatever the scientific value of the data ultimately accumulated, it would be a good thing if the Secretaries could be induced to authorize a much fuller form, especially about reproduction, if only to call the attention of new Fellows to an important question.

I tried to draw out a form, but it is shockingly difficult to frame anything useful but not inquisitive. Would you care to help me frame a questionnaire, which I shall send in? ...

Darwin to Fisher: 15 May 1929

I, like you, do not see my way clearly to frame a good set of questions. ... My experience is that scientific men are, outside their own narrow sphere of work, just as narrow, conservative, and touchy as any other class. How would it be, first of all, to discuss with the biological secretary [of the Royal Society] your idea in the vague, and see what he says? If nothing would come of it, it would only be a regrettable waste of your time. ...

Darwin to Fisher: 15 May 1929

... Do not you think you ought to rejoin the Stats.?⁶⁴ May I set the ball rolling? Mallet, I, and who else for sponsors? Udney Yule? If the latter, give me his address. I am sure now you are F.R.S. you should be F.R.S.S.

Darwin to Fisher: 25 June 1929

You remember no doubt that I spoke to you about rejoining the R.S.S. After doing so, I wrote to both Yule and Mallet, and the enclosed signed forms are their practical replies, willingly sent. I should add, however, that they both sign of the supposition that you really *wish* to rejoin, Yule considering that it would be doubly unfortunate if anything, even financial considerations, were to lead you to resign for a second time. You know that it was I who suggested to you that it would be well that you should rejoin, and that being the case, perhaps I may be allowed to explain very clearly what was in my mind in so doing. To take what was really a secondary consideration first, I knew that there had been some friction before your resignation, and I wanted to see that episode entirely forgotten by all, which would best be brought about by your quietly rejoining the Society. What was more in my mind was that it would be useful to you to be a member, and that you would be useful as a member. On that last point, it was not your taking part in the management of the Society I had in mind. That might come, but I myself think that the leading men of science are apt to take up too much of their valuable time in routine work needing only patience and perseverance. My father could not have done the work he did, but for his ill health keeping him free of routine work. You have one troublesome society⁶⁵ on hand, and there more is needed, because it necessitates decisions in regard to policy. I don't want to be the cause of more of your time being frittered away, though I feel you could play a useful part in discussions or on committees in regard to questions especially interesting to you.

Now if you do decide that you yourself do really wish to rejoin the Society, I want you ~~to~~ *do me* a favour, and accept a life membership as a birthday present from me; then each time the journal reaches you after I have departed, you will look on it as a little gift from me, and that thought would now give me real pleasure. I look on my money to some extent as a trust, and this is, I believe, a good way of fulfilling my trust. If you will do me this favour, send the enclosed *at once* to *your* bank, and also the enclosed letter and form (filling in your name) to the R.S.S. When the election is completed—which I gather will not be for some months, because there will be no meetings—send them a cheque for £21 *drawn by yourself*. No one but you, I, and your wife should know of this. Mallet and Yule neither have, nor will have, any idea of what I am suggesting. Now *do* accept this gift in the spirit in which it is made.

Fisher to Darwin: 27 June 1929

Let me thank you at once for the very great kindness of your idea respecting the R.S.S., and the thoughtfulness with which you have carried it out. I can have no hesitation in accepting your offer, put as you put it, and will do my very best to see that the result is all that you desire. The journal as it comes out will be a perpetual reminder of your kindness and goodwill.

For the moment I have mislaid your letter on longevity, which I had meant to return with this. I certainly hope to find it soon. Only one or two points which might interest you have occurred to me.

In man, the death-rate increases and the expectation of life decreases with increasing age. Death might be just as inevitable without this being so. For example, if the expectation of life were 20 years at all ages, we should have a half chance of dying within about 14 years, only one in a thousand would live to be 140, and one in a million to 280. We should all die sooner or later as we do now, only—if fertility continued—even the oldest would have the same expectation of further posterity as the youngest, and would be as much affected by selection, and consequently there would be no tendency for their death-rate to become higher than at early maturity, where in man it is least. In fact, the incidence of death or cessation of reproduction (or at least of reproductive usefulness) determines the action of natural selection, which in turn reacts on the death-rate. In an oak in a forest, I suppose an old tree has a greater expectation of posterity than a young one, so that it would be a bad bargain for the father oak to benefit his offspring unless he could do so by losing considerably less than the offspring gains.

The reproductive value at different ages must determine the extent to which parental care pays. If all ages were of equal reproductive value, a species would tend to benefit its offspring up to the point at which the offspring gains double the advantage which the parent loses, but no further. Of course immature offspring are usually worth much less, and so should be

cared for only at a cheaper rate still. But if crocodiles were able to recognize their mature offspring, I suppose they would co-operate with them not only on terms of mutual advantage, but on terms of joint advantage so long as the loss of either did not exceed half the gain of the other. Hence society starts with the family.

Fisher to Darwin: 29 June 1929

I have just finished correcting duplicate copies up to the last Chapter, and enclose five chapters [VIII-XII] on man, including the one you had before, so that you can see what I was driving at in writing in it what I did. The other copy is going straight to the publisher who has been hurrying me a little. I am afraid he will have a shock when he reads the human chapters and I only hope you won't. I feel on a knife-edge between timidity and audacity and need all the wisdom I can collect if I am to keep my balance.

Darwin to Fisher: 2 July 1929

I like your dedication, & I still more like the thought that you want to insert it. *Whatever* wording you select I shall be pleased with. If it is to indicate what I have *wished* to do, it is certainly true to speak of the 'encouragement given to the author during the last fifteen years by discussing many of the problems dealt with in this book'.

The big pile of MS. has come to hand, and what a pile! If you want it back by any particular date, let me know. ...

Darwin to Fisher: 18 July 1929

I have begun by again reading Chap. VIII with great interest. ...

Chap. IX ..., Chap. XI. These chapters are so interesting that I wish they could have come earlier in the book. It takes a lot of thinking, and I feel I am no longer able in one reading—if at all—to criticize effectively. It seems all sound, as far as I can judge. But it is stiff.

Chap. XII. My feeling on reading these chapters is that you have written a very important book, and one which will slowly—though slowly—influence public opinion. I am so much inclined to agree with your views that I don't feel it startling or alarming. I think you should look forward to the issue of a second edition, say ten years hence, and with that in view keep keeping it up to date. ...

You must be glad that your last [chapter] is finishing, and you have my congratulations.

[P.S.] You must not be disappointed at a small sale. It is the kind of book to work through others. I shall read it all again when published, more slowly, and shall take more in.

Darwin to Fisher: 2 October 1929

I have not yet read your food paper [CP 82], but intend to do so when I can give it quiet thought, which I see it will need. Now I want to *amuse* myself with another evolution letter, this time to consider when evolution may, not must, be slow. But I want to begin irrelevantly about butterflies.

The Meadow Brown, and two closely allied species, have black spots—eyes—on the *undersides* of their wings, with little white marks on them. Look at any picture of an eye, and you will generally see it as a black disk with a white splash on it, the reflection of some light. Is it fanciful to think the white spot on the Meadow Brown's eye is to make it more protective? It may be. My point, however, is that being found in 3 allied species, it is probable, but not certain, that it was evolved before these 3 bifurcated; and this may have been a very long time ago, considering the place in evolution occupied by insects. Being so long in existence, it hardly can be at all harmful. Here then is a case where, I suggest, evolution can have acted with extraordinary slowness. If two butterflies were on the same flower, and some insect went to eat them, and ate the one without white marks in his eyes, because they were less like eyes, that might cause a permanent change of minute proportions in the proportion of genes in the species. In fact, when a selective process does a very little good and no harm whatever, it may proceed with any degree of slowness. ...

What puzzles me about butterflies is this—there is no mimicry in England, I think, and to say that birds don't eat butterflies here often is not to the point. But nearly all are duller coloured on the underside, surely for protection. This, I guess, must be some disadvantage, as making them less conspicuous in the mating season. ... Hence there must be active selection still going on to preserve the dull colours on the underside. Butterflies do not seem to mind showing off, as it were, on the ground or on flowers in the day time. They show little sign of fear, and I have never heard of a bird going at them when sitting. From all this I guess that this underside protection is entirely for night use. I have seen an account of a white butterfly carefully selecting a white flower for its perch for the night. But what creatures attack sitting butterflies *at night*? I cannot think, unless it is bats. Has anyone examined the insides of bats to settle the question? If you ever come across a wise bugologist, ask him the question. ...

Fisher to Darwin: 4 October 1929

... I know the circular spots on the undersides of the Ringlet, Meadow Brown, Gatekeeper and Scotch Argus, but are they eyes? There are two points which might give a clue to their interpretation, one that they occur in series, about 7 in the Ringlet, and secondly, that the Meadow Brown and, I think, the Scotch Argus, have one of them doubled. Is it possible that in

twilight they look like dewdrops, a dark disc with a bright point? All these species haunt grasses, but I do not know if they roost on them. If so, perhaps amphibia and reptiles are the enemies. I wish I were a naturalist. ...

Darwin to Fisher: 4 October 1929

I have just been reading Haldane in *Nature*.⁶⁶ I am glad to see that he mentions your work, and appears to see its importance. I do not see anything in the whole article which necessarily runs counter to your arguments. Things would work out more neatly from a mathematical point of view if all heredity was dependent on genes and small mutations. But we do know that sudden changes in chromosome number do take place, and that must be allowed for. ...

Fisher to Darwin: 7 October 1929

Yes, I agree with Haldane, on selection in general; it is only on my dominance theory that at first sight he was inclined to attack me. Perhaps he will.

I regard the grosser types of mutations as chiefly of use in producing physiological isolation, and for this reason as frequently found as between nearly related species [*sic*].

I am rewriting most of Chapter IV; it is a burden.

Darwin to Fisher: 20 October 1929

I read your *Realist* article [CP 82] yesterday with some care, though I have not yet fully absorbed it. You know that I agree heartily with all the family allowances part, and the whole of it made me think hard. At first I decided not to write to you because I feel my views are *not fixed*, but on second thoughts I decided to do so, as it is probably now or never.

I have been in the habit of regarding things as follows. The use of machinery, etc., has enabled one man to produce more food. Hence men had to leave the country, this movement being increased by the manufacture of agricultural tools in towns. Conservatism resulted in wages being lower in the country, a difference compared to town wages being produced, which is slowly lessening. Everything became cheaper in like manner, but all men sought and generally got employment. The number of men employed in agriculture as compared with the numbers in other callings is an index of the expenditure on other things besides food, and therefore of the standard of living. If you turn your diagram on p. 48 upside down, it seems to me to give a rough measure of the rise in the standard of living. Looked at thus, it does not seem as 'serious' as you make it out to be. Looking to the future, it will go on. The advantage of cheap nitrogen will be, besides more production, less labour for what is produced. I forgot to say above that I see *far more* desire to go from country to town than vice versa.

On p. 54, you say that it might be wise, in the interests of existing cultivators, to restrict the area of growth—as was attempted with india rubber. This may be true, but it is protection, and like all protection, it injures others. It may not be true as regards *labour*, which I think becomes apparent if *rent* is taken into account. The men thrown out of work by the restriction of area, or not getting work, would tend to keep down wages. The rise in prices would raise rents. The net result *might* be no rise in the standard of living of labour and a greater differentiation of wealth. I don't know what it *would* be.

We have to face the fact that town life is going to predominate, and to try to make it everywhere as healthy and cheerful as it is in our best towns.

I have been trying to think what meaning I should attach to certain expressions. The over-production of goods would generally mean, I think, the production of goods which had to be sold at a loss. This would always be due to a mistake in estimates. It would never be a permanent situation. It would be the same in regard to food, if over-production is used in this sense. If the phrase means production such as tends to lower prices, I see nothing to say where it begins or ends.

To over-population I can give a certain not too definite meaning. If we imagine a population increasing from zero, I suppose at first, on the principle of increasing returns as explained in text books, prices would fall. They would go on falling up to a point, and then begin to rise; and the standard of living would rise and fall similarly. Where the change took place would be the optimum population. I assume knowledge not to change. But with a change in knowledge it is probable that the optimum for today would not tend to produce the optimum in the future. How to take the future into account theoretically, I do not see.

You speak of the development of the British Empire, and I think some of my father's words in (?) *The Descent of Man* could be quoted in support of this view. I cannot make up my mind how much I would sacrifice our present standard of living for this object. I would go some way. But, if we do so, let us be open, and declare plainly that *over-population* is what we want, so as to have numbers ready to go abroad.

On a few minor points. White men have known and inhabited tropical West Africa for ? 400 years. Why has not this potential food-supply area been developed? I think there must be some solid reason. Chinese and Indian civilizations have, for far longer, been close to undeveloped tropical areas. Do coffee, cotton and tobacco flourish where tropical forest is thickest? I thought not.

My manufacturing firm did not speculate beyond what was well in sight when considering capital expenditure. I think few firms are built up on longer expectations.

As to p. 56, I regard the fall in the death-rate as the most potent cause of

the fall in the birth-rate, contraception having made the coincidence take place much more rapidly, and done a little more in addition. I guess you would agree.

With regard to over-population, it seems to me that, accepting my definition, all Europe is probably much over-populated. By cutting off the industries producing lowest returns and throwing the worst land out of cultivation, would not the standard of living rise?

When the coal gives out, then we shall certainly be over-populated. How will this begin to show itself? Will it not be by unemployment? That seems to me the best rough test we can get for over-population, and I am sorry to see it discredited. Waves of unemployment will occur always, but how can we tell it is only a wave? Is it not best to keep this practical test well before our eyes?

No more boring you today. This at all events shows that what you say has set me thinking very hard.

[P.S.] *No answer needed.*

Fisher to Darwin: 25 October 1929

Supposed a fixed population with two needs only, Food and Bricks, say. They work at these two industries until an extra expenditure of a unit of labour upon either is just balanced by the additional satisfaction due to greater quantity or better quality of the product. Let them make an invention which enables them to produce more or better bricks with the same labour. Bricks will become cheaper relative to food, and they will direct some of the labour previously given to brick-making towards food production, the standard of living in both respects being raised, and maintained equal as between brick-makers and food-producers. If the invention applies only to food production, the reverse should take place, and if the progress of knowledge applies equally successfully to the two industries, the standard of living will rise, without diversion of labour.

I do not think that we can argue that mechanical improvements have aided food production more than industry, but rather far less, except in the important item of opening out new lands. The facts that such lands are available, that it is politically important to civilize them, and that there is little else other than agriculture that we can do with them, are those which I am inclined to emphasize as the causes of the lowering prices of foods, and the diversion of labour to other occupations. I think this is only another way of saying *World under-population*.

As to local over or under-population, I have had great difficulty in understanding how the state of employment is in any sense an index of it. No one believes that the number of jobs is fixed, without reference to the demand for services, and this demand turns everywhere on the population to be served. Of course certain jobs, such as police supervision, will not increase

proportionately to the population, but this only shows that a denser population can devote a large proportion of its man-power to productive work. If I wanted over-population I should be open enough in saying so, but I cannot see the evidence that 40 millions, or 90 millions, *is* over-population for this country.

Unemployment means, I think, supporting a number of men capable of doing useful work, without giving them an opportunity of doing it. Why should this maladjustment be associated with the condition in which an increase of population lowers the general standard of living (over-population) rather than one in which an increase of population raises the general standard of living? I cannot find any logical connection.

It is probable that I differ from you essentially about Free Trade and Protection, for I have never understood why Free Traders, however right they may be as to the advantages of Free Trade when full employment is available, do not accept Protection at least as a means of guaranteeing full employment for the available man-power. Any useful work seems better than none. I leave aside the advantage which I believe Protection gives of choosing among different industries which shall be fostered.

I am rather surprised that you do not think the confident expectation of world settlement has influenced our commercial as well as our political development. The unquestioned confidence with which men speak, even in Australia, of 'when the interior is opened up', has certainly led many men to make their homes in the wilderness to their ultimate ruin. Has the financial loss been borne only by a few wild enthusiasts, or is it shared in less proportion by others who use the same phrases?

I have simply picked out the points in your letter I disagree with, or on which I think your opinion might be modified by what could be said on the other side. So I am very argumentative. About tropical forest, do you know any physiographical reason why the valleys of the Ganges and the Yangtse should not revert to dense forest, if the cultivators were removed?

Darwin to Fisher: 1 November 1929

I did not answer your 'argumentative' letter, as time did not permit—or I was lazy. I wish we could have a real good jaw over some of these points. I don't hold out strongly about tropical forests, and would only make two points. Do not both the Ganges and Yangtse valleys have cool seasons? Then it seems to me that the very luxuriance of growth in the all round hot and damp climates seems to increase the difficulty of cultivation, and would make it only possible at a low standard of living. But I don't feel sure. As to your bricks and food, the difference seems to me to lie in the fact that the amount of food wanted per head is strictly limited, whilst the amount of goods which might help to raise the standard of living, including leisure, is quite unlimited. Calculate the percentage of exertion a naked savage

expends on his food and on other things, and the same with civilized persons, and my point would stand out. Each item of food may not have been helped more than separate items of other things. I am looking at food as a whole versus other things as a whole. The question seems to me to be to what extent the population can be increased whilst maintaining our standard of living. The land at the margin of cultivation must be one important factor. I have no doubt a considerable increase can *slowly* be made by more colonial land being made available, but am inclined to think that the possibilities have been much exaggerated. As to unemployment, as far as I can see no one would be unemployed if all would take the best pay they could get. It is all a question of keeping up the standard of living. Surely, if bad land is cultivated and bad trades carried on, it absorbs the unemployed in a useful manner, but it does not allow unemployment to act as a regulator to prevent a fall in the standard. Now I had intended to have said nothing, and now I have jotted down some half-baked thoughts. As to free trade, we should have a fine fight, for some of your reasons for are my reasons against! There I admit, however, that free traders generally over-state their case. Some of the indirect results would be the worst, e.g. political corruption. Better burn this letter!

Fisher to Darwin: 12 November 1929

I have left yours of the 1st inst., unanswered unduly long, and I doubt if I know enough of economics to answer it properly.

My feeling about the valley lands of the equatorial rain belt is that the vigour of native vegetation has imposed a serious obstacle to cultivation by tribes at a low level of social organization, and that they have never been subjugated by natives for this reason, but that they possess immense natural resources not only for timber but for food production, if reclaimed on a large scale with great resources and determination. Whether the Asiatic valleys were easier to control, or have happened to be attacked by better organized or more persevering peoples, I cannot easily guess.

I quite agree that the increased real value consumed will be greater (when the standard of living rises) in goods other than in food, the demand for which is relatively inelastic, but this will not explain an increase in the price of any one particular item, such as pig-iron, as compared with a bushel of wheat. Our daily budget ought in fact to comprise more pig-iron in various forms, and not so much more wheat, but not dearer pig-iron relative to wheat.

If a population were too great for its natural resources, would it not tend (if well organized) to lower its standard of living by putting in more work, at the expense of longer hours, later pensioning, shorter, more intensive industrial schooling, etc., in fact more employment and less leisure? If this were becoming burdensome, there would be a case for diminishing

population, supposing there were really a decreasing return from the natural resources for the labour being expended. But unemployment, as we know it, is a kind of wasted leisure. Men, women, and children are supported without adequate economic contribution, but also without being able to make the indirect cultural contribution of a leisure class. I doubt altogether if the standard of living in the working class (or the country) generally would fall, if the unemployed were taken at lower wages, provided there were adequate wage differentiation for skill and output, which should not be beyond intelligent social organization, however difficult in the prevailing state of opinion.

How would a small compulsory automatic wage increase with length of service work in practice, in conjunction with unrestrictedly low initial pay? It is not obvious to me that frequent dismissals would be profitable to the employer in most industries.

Darwin to Fisher: 16 November 1929

I always like getting yours, because they make me think. I guess inventions have lowered the difficulty of production of both iron and wheat, though iron more than wheat. A man does not now get or want much more bread, but he gets a totality of other things than food much greater than before, and that means a rise in his standard. Then you mean that if there is now increasing over-population, it ought to show itself in a decrease in the standard. There would be that tendency; but if increasing knowledge is making a rise in the standard a possibility, then the standard may be rising, and yet the increase in the population may be lowering the *possible* but not the actual standard; and it is the possible that I am inclined to look to. I believe with you that the standard of living would rise with the employment of the unemployed. The difficulty is a practical one of employing them, about which I don't see my way clearly in this imperfect world. I think I am rather more accepting human imperfection and folly as a necessary ingredient, whilst you are considering more ideal pictures. A compulsory rise of pay is a plan I have never thought of, and now do not, probably, see its full merits, though I see some. But it seems too far outside practical politics to me. But I won't write more, because I am wandering and must keep my brains for my next job. ...

Fisher to Darwin: 28 November 1929

Mrs. Hodson called my attention to the advertisement of this Chair⁶⁷ and the subjects in view, in the marked paragraph, do seem rather attractive.

Would you advise me putting in for it? ...

Darwin to Fisher: 29 November 1929

I am certainly of opinion that you should have a shot at the enclosed. I see

no argument against it. You must not mind failure. They are, I think, a cranky body, and one cannot guess what line they will take. ...

Fisher to Darwin: 3 December 1929

... I suppose I ought to raise the question of subordinate appointments if things go any further. I should like to get a geneticist, and an experimental psychologist, if they will go so far. ...

Darwin to Fisher: [4 December 1929?]

... As to the staff under you, would it not be wise to catch your hare before trying to fatten it? ... Seriously, I advise beginning slowly. And I am not so sorry as you will be. I believe you still have a lot in your head which merely needs leisure and opportunity to bring it out. It is in such lines that you will continue to enhance your reputation. My father only had one old and inefficient gardener for his 'staff' for many years, and I believe his work was in some ways all the better in consequence. It made it more original. And I want to get emptied out of your head all that is original in it, and I believe that means a lot. ...

Fisher to Darwin: 6 December 1929

I take your letter as a salutary dose of medicine, and by way of giving their proper weight to your points should like to discuss them.

The value to me of the hare—unfattened—consists of two items; (i) £170 per annum increased salary, with a prospect of £250 more in 5 years' time (both less tax), (ii) the possibility that my work in mathematical statistics will be more valuable if applied to researches on Man. I do not really now lack opportunity to say anything I have to say about Man, but could perhaps reduce our present ignorance somewhat by designing and directing specific enquiries and studies in the subject.

My department here⁶⁸ now has two research assistants of the status and pay of University lecturers, four laboratory assistants for routine computations and clerical work, and a variable number (at the moment four) [of] voluntary workers, three of whom from Australia, Denmark and India, correspond to advanced students doing research, while the fourth is an American Professor writing a text book on Statistics. I have to consider whether a smaller organization would make any useful headway in the problems proposed for the new research Professor. Do you not think this should be considered early, if not before applying for, at least before accepting such an appointment?

Would you agree with me that, at about 50, your father had decided that there was little more to be done for the subject out of his own head, but that as a good theorist makes a good observer, so still more in experimentation, that there was a great need for well directed experimentation which

should answer the problems, and consolidate the conclusions, at which he had arrived?

If this is so, he was several generations in advance of his time, and in the absence of a ready supply of trained assistants, and under the restriction of working at his private expense, he was unable, without being unwilling, to set a much needed example of what a director of research should be. Were his experiments really any better, I mean more useful to himself and others thinking of the subject, than they would have been had he been in Sir Daniel Hall's place at Merton?⁶⁹ I doubt it. The contribution of the inefficient gardener must chiefly be to destroy or mix batches of experimental plants, and if one picks up scraps of observational information from his mistakes, are not the experiments of others, usually carefully published and open to inspection, a sufficient source of enlightenment of this sort? ...

Darwin to Fisher: 7 December 1929

... I have had little experience myself of team work, and my judgement is of no value on its merits as a whole. My father wrote *Domesticated Animals*⁷⁰ when he was 59 years of age. He had half prepared a manuscript on Variation under Nature (no one seems to know what has become of it!!) and we hoped he would go on with it. But he said he was physically incapable of attacking another big job, and he took to his botanical work as being much easier. Certainly, team work does a lot, and it wants a good man to shove it along. But I still think that the highest and most original work is done by the nearly unaided individual. Here I am no doubt getting into very debatable ground. Directing a lot of underlings must take a lot of time, and the question is whether, with each individual, that time could be better spent in some other way. Anyhow, I want you to have time enough to empty your head of all that is original in it.

Darwin to Fisher: 16 December 1929

I have been pouring some cold water on your back lately, and I want you, if need be, to pour some on mine—though it is a disagreeable job. I wrote enclosed as a possible letter to *Nature*.⁷¹ It has turned out longer than I expected. My question is, should it go to *Nature* or the waste paper basket? Or elsewhere? ...

I have been turning over the pages of a big book, Wheeler's *Social Insects*, 1928. For me it is an aggravating book. He simply loves new scientific terms, and as I find them difficult now to remember, especially as I have no classics to help me, I was constantly swearing at him. My father used to say that everyone inventing a new term should be fined. ... We see the usual phrase—'natural selection has lost its value as an explanation of the origin of adaptive variations'—though I did not see why he thought so. He ad-

vocates instead of 'forever croaking "natural selection"', to say nothing but ignoramus. That seems very sound advice to himself! But the book contains a lot of facts.

This is a muddled letter, but I guess you will see its drift.

Now don't be afraid of applying the cold water cure. ...

Fisher to Darwin: 17 December 1929

I am all for publishing the letter, except the last sentence, which does not, I fancy, add to what you have said, and might be taken to mean more than you do.

I have added a few trifling suggestions, all mistypings I think, save one, where the sentence is twice reversed by 'against'. You know what I mean, like: 'We cannot avoid repudiating the opinion that there is no substantial evidence against the view that countersuggestion has in no case inhibited the negative attitude of the subject.' I am not sure now whether this is nonsense or not.

Notes

1. The earliest dated letter from Darwin to Fisher which we have is that of 3 September 1915. As that letter shows, Darwin had, before this, been sending various problems to Fisher for him to solve. The first two letters represented in this collection, though undated, were evidently written before September 1915. They are of interest in revealing not only the problems Darwin was submitting to Fisher but also the manner in which he expressed them.
2. Though his model was a simple one, Darwin was asking, in effect, if the law of ancestral heredity could be explained in Mendelian terms. See CP 9, p. 421.
3. See Fisher, R.A. (1915). The evolution of sexual preference. *Eugenics Rev.* 7, 184-92 (CP 6).
4. See Darwin, L. (1913). Heredity and environment. *Eugenics Rev.* 5, 152-3. Darwin questioned the use of the phrase, 'the relative influence of heredity and environment', and suggested that it should be avoided because of the difficulty in giving a general meaning to environmental variation. He illustrated his argument by referring to an 'ideal republic', where 'not only were all the children removed from their parents, but where they were all treated exactly alike'. He wrote that, 'in these circumstances none of the differences between the adults could have anything to do with the differences of environments and all must be due to some differences in inherent factors. In fact the environment correlation coefficient would be nil, whilst the heredity correlation coefficient might be high.' Shortly afterwards, Karl Pearson published a paper criticizing Darwin's argument (see Pearson, K. (1914). On certain errors with regard to multiple correlation occasionally made by those who have not adequately studied this subject. *Biometrika* 10, 181-7). Pearson wrote, 'The coefficient of correlation for the environment might be anything from -1 to +1; the only obvious fact would be that you could not find its value, except in the form 0/0, from an environment which precluded any measure of variation. How again Sir Francis [Galton] would have smiled at the notion that the

coefficient of correlation for a constant environment must be nil. Why should we follow such advice as that given by the President of the [Eugenics Education] Society to avoid as far as possible 'such phrases as the relative influence of heredity and environment' when on his own showing he does not in the least appreciate the methods by which this relative influence is measured?' Pearson had earlier written to Darwin pointing out his 'error' and in October 1913 in the *Eugenics Review*, Darwin had published a note saying that it had been pointed out to him that he had made a blunder.

Darwin's letter of 3 September 1915 shows that Fisher must have written supporting Darwin's position and urging that Pearson's judgement should be challenged. Referring to this correspondence, Joan Fisher Box has written in *FLS* (p. 52) that it 'showed each the quality of the other: Fisher appreciated Darwin's scientific perception and his lack of self-seeking, and Darwin appreciated Fisher's scientific understanding and his immediate impulse to correct what he felt to be an abuse of science and of justice.'

At about this time, Fisher began detailed work on his analysis of the correlations between relatives. In his major paper on the subject (*CP 9*), completed by mid-1916, he showed how the variance of biological measurements could be partitioned into environmental and genetical components. This analysis was later used by others to define a coefficient of heritability measuring the relative influence of heredity and environment. Fisher never used this coefficient which he regarded as 'one of those unfortunate short cuts, which have often emerged in biometry for lack of a more thorough analysis of the data' (*CP 245*).

5. Mr G.U. Yule, Lecturer (late Reader) in Statistics, University of Cambridge.
6. Schuster, E. (1913). Heredity and environment. *Eugenics Rev.* 5, 260-1.
7. This letter shows one of Darwin's attempts to clarify the usage of different terms for describing biological variation. The definition which he gives here for fluctuations is unusual, even for Darwin. Elsewhere, he uses fluctuations to describe variation due to differences in the environment.
8. Presumably Pearson's article cited in Note 4.
9. Darwin's paper was published in 1916 in *J. R. Stat. Soc.* 79, 159-75.
10. Snow, E.C. (1912). The influence of selection and assortative mating on the ancestral and fraternal correlations of a Mendelian population. *Proc. R. Soc. B* 85, 195-6.
11. Yule, G.U. (1906). On the theory of inheritance of quantitative compound characters on the basis of Mendel's laws. A preliminary note. *Rep. 3rd Int. Con. Genetics*, pp. 140-2.
12. These letters throw light on some of the problems concerning publication of Fisher's paper on the correlation between relatives on the supposition of Mendelian inheritance (*CP 9*). This was submitted originally to the Royal Society of London in mid-1916. The reports of the Society's referees, K. Pearson and R.C. Punnett, have been published in full in *Notes and Records of the Royal Society of London*, 31, 153-5, (1976). Pearson emphasized that the author had adopted 'a special hypothesis for determining the somatic characters of an individual dropping the Mendelian phenomenon of dominance'. He reported that the paper was not of much interest from the biometric standpoint and said that whether it be published or not should depend on Mendelian opinion. Punnett for his part, said that whatever the paper's value from a biometric standpoint it was not of much interest to biologists, though he did add, 'frankly I do not follow it owing to my ignorance of mathematics'.

In 1917 the paper was submitted to the Royal Society of Edinburgh through J. Arthur Thomson. The Secretary of the Royal Society of Edinburgh has kindly informed me that the Society's records show that Fisher's paper was examined by three referees, J. Brownlee, J.F. Tocher, and E. Whittaker; on the basis of their reports, the Society's Council decided on 5 November 1917 that the paper could not be accepted as it was on account of its great length. The author was advised that an abstract of 10 pages could be published in the Society's *Proceedings*.

The letters reproduced here reveal Leonard Darwin's central role in making it possible for Fisher's paper to be published in full. Having sought and obtained advice from Edinburgh that a donation of between £25 and £30 would allow publication of the entire paper, Darwin promptly said that the Eugenics Education Society would provide this. When the Council of the Royal Society of Edinburgh was told about an offer of financial support at its meeting on 11 January 1918, it agreed that the paper could be published in full in the *Transactions*—but only if £43 were donated to supplement the £12 which was all that the Society could provide. This increase in the estimated cost led to further difficulties, but again Leonard Darwin was ready to assist; the Society's Council, meeting on 3 June 1918, was advised that Darwin had offered to underwrite the balance required for publication of the paper in full. Darwin's letter to Fisher of 6 May 1918 suggests, perhaps, that as difficulties developed over publication in Edinburgh, arrangements were being made to publish the paper in the *Eugenics Review*. When publication went ahead in Edinburgh, Fisher published a short general article on the causes of human variability in the *Eugenics Review* (*CP 10*), with a reference to the big paper.

13. Professor K. Pearson of the Galton Laboratory, University College, London, had written to Fisher in rather guarded terms about a post in the Laboratory.

Old Schoolhouse,
Coldharbour,
Near Dorking.

Dear Mr. Fisher,

August 2, 1919

Your name has been mentioned to me as a possible man for a post I have to fill at the Galton Laboratory, namely that of a senior assistant at £350 per annum. I do not know whether the post would have inducements for you, and I fully realize that there would be difficulties in the way. I want a man who will throw himself wholeheartedly into the work at the Laboratory as it is at present organized, not a research worker who would follow his own individual lines regardless of the general scheme of work. A real taste for and patience in the somewhat laborious work of computing tabulating and reduction is essential. Mathematical knowledge is very essential, but it is in a sense secondary, i.e. we do not seek mathematical problems, we have quite enough as they arise in the ordinary course of our work. At the same time I, of course, endeavour to encourage all research tending to extend theory so far as it is of importance to our own subject. At the same time I like also primarily a man who has had experience of observations or measurements, and if possible has been through our special training in computing and statistics. I find as a rule that a high Cambridge wrangler usually takes two years to become an efficient practical statistician and computer, and that by this time or before he wants a more highly paid post than we can give. I want somebody who will stick loyally by the Laboratory for a number of years especially during the present critical time, when we are going into a new building with very considerable extension of our work and possibilities, but with inadequate funds owing to the war-conditions. I have one or two men in view, but as you have been specially mentioned from Cambridge

I feel I must write to you among them and find out what your views may be. I may be in London for a day during August, if you cared for a talk, or this is not inaccessible via Reading and Dorking.

I am,

Yours very sincerely,

Karl Pearson

14. Darwin was Chairman of Bedford College, 1913-20.
15. This was Fisher's draft manuscript of three chapters for a book (never published) dealing with 1. variation in human family size, 2. the effects of birth limitation, and 3. the role of selection in human society.
16. Brentano, L. (1910). The doctrine of Malthus and the increase of population during the last decades. *Econ. J.* 20, 371-93. A.C. Pigou (1912), in his book *Wealth and welfare*, wrote that Brentano's investigations 'suggest that, at the present time, increased prosperity in any class in the modern world is likely to work, not for any increase, but actually for a contraction in the number of births.'
17. Fisher had been appointed as statistician at Rothamsted Experimental Station.
18. i.e. birth limitation.
19. Darwin's paper on the postulates needed for evolution—see his letter of 5 April 1919.
20. Fisher had sent Pearson his paper on the probable error of the correlation coefficient for publication in *Biometrika*. Pearson replied that he could not give it his full attention and asked Fisher to publish it elsewhere, saying he was 'compelled to exclude all that I think is erroneous on my own judgement, because I cannot afford controversy'. The paper (CP 14) was later published in the new journal *Metron*. See *FLS*, p. 83.
21. Dr M. Greenwood, an Honorary Secretary of the Royal Statistical Society.
22. Fisher had perhaps enquired about the studies which had led Charles Darwin to conclude in Chapter II of the *Origin* that 'wide-ranging, much diffused and common species vary most'. See CP 24 (p. 324) and CP 52.
23. Hagedoorn, A.L. and Hagedoorn, A.C. (1921). *The relative value of the processes causing evolution*. Martinus Nijhoff, The Hague.
24. Darwin's notes conclude with the following passage.
The most novel and interesting arguments in the book relate to a unifying process which has without doubt been inadequately explored hitherto. This process depends on the fact that chance is continually weeding out some of the rarer types, with the inevitable result that as time goes on a freely interbreeding group must become more and more uniform in character. This theme is developed in many directions with great ability; but we feel that it tends to run away with the author. When a horse runs away with a rider it proves that the horse is not lame and that the rider at all events has courage enough to attempt to ride such a horse. Unquestionably this influence must be taken into account, but we feel that it will have far less effect than is here depicted.
25. Fisher's review of the Hagedoorns' book (CP 17) is his first published discussion of the roles of selection, mutation, and drift in evolutionary change and points the way to several of his later papers.
26. Darwin's pamphlet was published in 1921 under the title *Organic evolution: outstanding difficulties and possible explanations*. Cambridge University Press.
27. See Darwin's letter of 22 August 1919.
28. The Royal Statistical Society had refused, without explanation, to publish an article by Fisher on χ^2 . See *FLS*, p. 87.

29. Mr A.W. (later Sir Alfred) Flux, an Honorary Secretary of the Royal Statistical Society.
30. Sir Bernard Mallet, an Honorary Vice-President of the Royal Statistical Society.
31. Probably C. Tate Regan who gave the Presidential Address on Organic Evolution to Section D (Zoology) of the British Association for the Advancement of Science in 1925.
32. The proofs of Darwin's book, *The need for eugenic reform*.
33. Following the Galton Lecture by the Bishop of Birmingham, the Dean of St Paul's Cathedral, London, the Very Rev. W.R. Inge, wrote an article on Eugenics and Religion which was published in the *Morning Post* on 5 March 1926. In this article the Dean expressed his opposition to eugenical sterilization which he described as 'mutilation'.
34. 'Old Elijah' presumably refers to K. Pearson.
35. Fisher's review of *The need for eugenic reform* (L. Darwin). See CP 54.
36. This quotation presumably comes from an early draft of Fisher's review. In the printed version, 'not' has been replaced by 'much more than'.
37. Berg, L.S. (1926). *Nomogenesis or evolution determined by law*. Constable and Co., London.
38. A reprint of the first edition of *The origin of species* was published by Watts, London in 1950. In 1959, *The origin of species—a variorum text*, edited by Morse Peckham, was published by the University of Pennsylvania Press. This contains a record of every change, addition, or omission that Charles Darwin made in the five revisions of the *Origin*.
39. See Darwin, L. (1927). Natural selection. *Eugenics Rev.* 18, 285-93.
40. Fisher's proposal was evidently accepted for a review article on Berg's *Nomogenesis* by Professor E.W. MacBride appeared in *Eugenics Rev.* 19, 32-7, (1927). According to MacBride, 'Berg's destructive criticism of the theory that the natural selection of fortuitous variations is the cause of evolution is excellent and convincing, but his attempt to institute in its place a constructive idea of orthogenesis is exceedingly weak.'
41. Tredgold, A.F. (1927). Mental disease in relation to eugenics. The Galton Lecture. *Eugenics Rev.* 19, 1-11.
42. This presumably refers to Fisher's correspondence with C. Tate Regan (p. 252).
43. See Fisher's letter of 7 February 1927 to Regan.
44. See Darwin, L. (1928). Natural selection—a correction. *Eugenics Rev.* 20, 142-3.
45. i.e. Fisher's suggested theory of the evolution of dominance.
46. This is presumably the quotation from Charles Darwin included on page 3 of *GTNS*.
47. See *GTNS*, p. 1.
48. i.e. the evolution of dominance. See *GTNS*, Chap. III.
49. See Notes 39 and 44.
50. Darwin's letter with his numbered comments on Fisher's Chapter III on the evolution of dominance has not been preserved.
51. Presumably Darwin had suggested that Fisher should include something on the creation of new loci in Chapter III and Fisher, apparently, meant to suggest that he saw no way of including this in his theory.
52. See *GTNS*, p. 55.
53. See Darwin's letter of 2 April 1921.
54. i.e. Chapter VI of *GTNS*. See Fisher's letter of 18 February 1929.

55. The copy of Galton's letter to Darwin dated 5 October 1910, begins as follows.
- I can't help in solving your question. The answer must greatly depend on *where* the people live and *how*. In many villages, notably Scotch sea-shore ones, the Fisher folk never marry outside their immediate neighbourhood. In such an extreme case the number of their forefathers, any number of generations back, would hardly exceed that of the present villagers. On the other hand, a migratory population might have greatly intermarried with outsiders.
56. See the fifth paragraph of the Preface to *GTNS*.
57. i.e. the list of new Fellows of the Royal Society.
58. On leaving Cambridge in 1913, Fisher had worked first as a statistician with the Mercantile and General Investment Company in London, and then as a schoolmaster, teaching mathematics and physics for five years until 1919, when he was appointed as statistician at Rothamsted Experimental Station.
59. i.e. the Galton Professorship of Eugenics, University College, London.
60. See *FLS*, p. 61.
61. In Chapter VI of the *Origin*, Charles Darwin wrote, 'forms existing in larger numbers will have a better chance, within a given period, of presenting further favourable variations for natural selection to seize on, than will the rarer forms which exist in lesser numbers' and 'the most common forms, in the race for life, will tend to beat and supplant the less common forms, for these will be more slowly modified and improved.'
- On the other hand, in *Organic evolution* Leonard Darwin wrote (p. 19), 'Once a beneficial mutation has survived for a few generations, the chances of its extinction become very small; and when this is the case, it matters little whether the surrounding population be large or small.'
62. In the first pages of *Organic evolution*, Leonard Darwin suggested that his father regarded 'the establishment of a belief in descent with modification' as his primary object and that the question of the method by which evolution occurred had been seen as less important.
63. See *GTNS*, p. 198.
64. i.e. the Royal Statistical Society. See Darwin's letter of 12 March 1923 (p. 76).
65. i.e. the Eugenics Society.
66. Haldane, J.B.S. (1929). The species problem in the light of genetics. *Nature* 124, 514-16.
67. The Chair of Social Biology, London School of Economics—to which Lancelot Hogben was ultimately appointed.
68. The Statistical Department, Rothamsted Experimental Station.
69. Sir Daniel Hall was Director of the John Innes Horticultural Institution, 1926-39.
70. Darwin, C. (1868). *The variation of animals and plants under domestication*. J. Murray, London.
71. Darwin, L. (1930). Evolution and evidence. *Nature* 125, 126-7.