for successful followers of science, it is to be hoped that in addition to the many new openings in industrial pursuits, the gradual but sure development of sanitary administration and statistical inquiry may in time afford the needed profession. These and adequately paid professorships may, as I sincerely hope they will, even in our day, give rise to the establishment of a sort of scientific priesthood throughout the kingdom, whose high duties would have reference to the health and well-being of the nation in its broadest sense, and whose emoluments and social position would be made commensurate with the importance and variety of their functions." (pp. 258–60.)

Much of what Galton wished in 1874 to see achieved has since been done, although plenty remains to occupy fully the attention of educational reformers. It is singular, however, to note how little Galton's services to educational reform have been recognised, and yet in this book he is voicing the opinions of a very large section of the scientific men of that day; and these views filtered down through the press until they ultimately reached the politician. The last sentence but one appealing for development of sanitary administration and *statistical* inquiry finds Galton on common ground with Florence Nightingale—a link to which we shall return later. But alas! their dreams are still far from realisation; it is still held laughable to suggest that the statistician is a fundamental need, if we are to understand what makes for or mars the health and well-being of our nation in its broadest sense.

B. DARWIN AND THE PANGENESIS EXPERIMENTS

As Galton's views on heredity brought him to a certain extent into conflict with De Candolle, so also they brought him at an even earlier date into a disagreement with Charles Darwin. At the end of 1869 as a result of his discussion of pangenesis in the Chapter entitled 'General Considerations' of *Hereditary Genius*, Galton determined to test experimentally Darwin's 'provisional' hypothesis. In that discussion Galton directly speaks of gemmules circulating in the blood (see our p. 113). Although Darwin read this book, I can find no trace of a letter at that date repudiating the idea of circulation in the blood being the essential method of transfer of gemmules. From December 1869 to June 1870 I find twelve letters of Galton to Darwin about the experiments on transfusion of blood. That Darwin answered some, perhaps all, of these letters is clear, but I have not succeeded in finding any replies. It is possible that after the letters to *Nature* of 1871, Galton destroyed them. At any rate in the list of Darwin letters prepared in 1896 by Galton himself none of these letters are referred to. All the Darwin rabbit letters that have survived are those which *followed* the publication of Galton's paper "Experiments in Pangenesis by Breeding from Rabbits of a pure variety, into whose circulation blood taken from other varieties had previously been largely transfused." This was read at the Royal Society on March 30, 1871[1]. These letters refer to a continuation of the experiments,

been self-taught and was due to his following up of an innate taste for science, and Galton expressed himself in much the same language: see our Vol. I, p. 12.

[1] *Royal Soc. Proc.* Vol. XIX, pp. 394–410, 1871.

also with negative conclusions, which results confirmatory of the thesis of his memoir Galton never to my knowledge published in detail. Those who read the letters below cannot doubt that Darwin knew the nature of the experiments, and knew that Galton was assuming that the 'gemmules' circulated in the blood. The whole point was to determine whether the hereditary units of a breed A could be transferred by transfusion of blood to members of a breed B and would 'mongrelise' the offspring conceived later by B. Was the 'blood' indeed as supposed in folk-language all over the world a true bearer of hereditary characters? That question is itself of importance, even apart from the question of Darwin's theory of heredity. But the publication of these letters has in this particular instance a deeper significance. It is a biographer's duty to illustrate the real strength of his subject's character, not merely to call it great. I know of no case in which a disciple's reverence for his master has exceeded that shown by Galton for Darwin in this matter. I doubt if any natures the least smaller than those of Darwin and Galton would have sustained their friendship unbroken, even for a day, after April 24th, 1871. I feel that the self-effacement of Galton in this instance is one of the most characteristic actions of his life; but it is not one that a biographer can disregard, however great his reverence for Darwin. Here are the letters extending from the start of the pangenesis experiments to nearly the time when Galton began to write his paper.

(1) 42, Rutland Gate, S.W. *Dec.* 11, 69.

My dear Darwin, I wonder if you could help me. I want to make some peculiar experiments that have occurred to me in breeding animals and want to procure a few couples of rabbits of marked and assured breeds, viz: *Lop-ear* with as little tendency to Albinism as possible. *Common Rabbits*, ditto. *Angora* albinos. And I find myself wholly unable to get them, though I have asked many people. Do you know anybody who has such things? I write without your book in reach, but you there especially mention a breeder of Angoras. Also you quote with approbation from Delaney's little book. Are either or both of those men accessible and likely to help? Pray excuse my troubling you; the interest of the proposed experiment—for it is really a curious one—must be my justification. Very sincerely yours, Francis Galton.

(2) 42, Rutland Gate, S.W. *March* 15, 70.

My dear Darwin, Very many thanks for the information and books. When I have got up the subject, I will write again, and will in the meantime take all care of the books.

I shall hope in a week from now to give you some news and by Saturday week definite facts about the rabbits. One litter [?doe] has littered to-day and all looks well with her. Two others towards the end of the week, viz: Wednesday and Saturday. I grieve to say that my most hopeful one was confined prematurely by 3 days having made no nest and all we knew of the matter was finding blood about the cage and the *head* of one of the litter. She was transfused from yellow and the buck also from yellow. Well the head was certainly much lighter than the head of another abortion I had seen, and was certainly *irregularly* coloured, being especially darker about the muzzle, but I did not and do not care to build anything about such vague facts and have not even kept the head. As soon as I know *anything* I will write instantly and first to you. For my part, I am quite sick with expected hope and doubt.

Ever very sincerely, F. Galton.

It will be seen from Letters (1) and (2) that between Dec. 11, 1869 and March 15, 1870, Galton must by letter or verbally have communicated the purpose of his experiments to Darwin. He now speaks quite openly of the transfusion and its possible effect on the nature of the offspring.

(3) 42 Rutland Gate, S.W. *March* 17, 1870.

My dear Darwin, No good news. Bartlett assured me this morning that it was a popular prejudice that young rabbits might not be looked at, reasonable care being taken, so we opened 2 boxes and examined the litters. The first contained four dead young ones all true silver greys. One, however, has a largish light-coloured patch on its nose, but Bartlett tells me that this is not unusual with silver greys as the very tips of their noses are often white. However this patch is somewhat larger and there are faint hopes, I think, that it may prove more considerable than Bartlett believes. I have one more litter yet to come and hope to send you the result by Monday evening post. I have coupled a new pair and re-coupled the 2 does whose litters have failed, one of them with a more suitable mate, and expect the following results:

Date of expected litter	Buck transfused from rabbit coloured as below	Doe transfused from
April 14	Hare-coloured	Hare-coloured
April 16	Yellow	Yellow
April 16	Black and white	Black and white

The quantity of blood transfused was only 1·25 per cent. of the weight of the rabbits which is only the same thing as 30 oz. of blood to an ordinary man. I know this is a very small proportion of the whole amount of blood, but hope by a second operation on the old bucks and by improved operations on all the young ones to get a great deal more of alien qualities into their veins. Very sincerely yours, Francis Galton.

In a letter of Mrs Darwin's to her daughter Henrietta dated Down, Sat., Mar. 19 [1870] we read:

"F. [Father] is wonderfully set up by London, but so absorbed about work, etc. and all sorts of things that I shall force him off somewhere before very long. F. Galton's experiments about rabbits (viz. injecting black rabbit's blood into grey and *vice versa*) are failing, which is a dreadful disappointment to them both. F. Galton said he was quite sick with anxiety till the rabbits *accouchements* were over, and now one naughty creature ate up her infants and the other has perfectly commonplace ones. He wishes this exper[t] to be kept quite secret as he means to go on, and he thinks he shall be so laughed at, so don't mention......." *A Century of Letters*, Vol. II, p. 230.

(4) 42, Rutland Gate, S.W. *March* 22, 1870.

My dear Darwin. Another litter—this time of 4—and all of them are true silver greys.—Also, one of the does (mentioned in my last letter as transfused from a black and white) is dead.

My stud now stands as overleaf[1]. I call each silver grey by the name of the colour of the rabbit from which it has been infused. I also give the particulars of my first batch. You will see that there was much less variety in my pairs then, than there is now. I hope to try a new mode of transfusion upon a wholly new stock, taking younger rabbits and putting much more alien blood into them. Ever very sincerely yours, F. Galton.

[There follows a list of transfusions into bucks and does of first and second batches.]

(5) 42, Rutland Gate, S.W. *March* 31, 1870.

My dear Darwin, Better news—decidedly better. I opened the hutches where the young rabbits are, this morning, and found now that the white patch on the nose of which I spoke had become markedly conspicuous and larger, but also that a white vertical bar had begun to

[1] I have not thought it needful to reproduce this table, as the details of the experiments are given in the paper as finally published.

appear in the forehead[1]. On going to the other litter, which I had never before got a proper view of, I found another young one with precisely similar marks. (The male parent was the same in both cases.) I have spent a most unsuccessful morning with new apparatus trying to inject more completely; but I have yet hopes of success by making some alterations.

I will return to you Naudin and the 2 pamphlets by *to-morrow's* book post. Very many thanks for them and for all the references. With great reluctance, I feel it would be too much for me to undertake the experiments. I am too ignorant of gardening, and, living in London with a summer tour in prospect, I don't see my way to a successful issue; but I hope to practise my eye and get some experience this year which may be of service next year or hereafter. I congratulate you about the Quagga taint. Once more about the rabbits, very many thanks for your hints, I will try more grey blood. Bartlett takes great interest and gives much care. Murie's assistant looks after the rabbits. Murie himself looks in now and then,

Very sincerely, F. GALTON.

Owing to the failure of Darwin's parallel letters we have no knowledge of what his hints were. The nature of the proposed plant-rearing experiments is equally unknown to us, but the suggestion may have remained in Galton's mind and have borne fruit in the sweet-pea experiments of a few years later. The Quagga taint[2] has close bearing on the present subject, for if a mother of breed *A* bore a child to a father of breed *B*, it seems likely that the 'gemmules' in the 'circulation' of the unborn child might pass into the mother's circulation and possibly affect a child born later to a father of her own breed *A*. The Quagga case, as indeed all instances up-to-date, of so-called telegony can now be dismissed from consideration. They depend essentially on (i) observation of variation within the pure breed not being sufficiently wide, or (ii) the assertions of kennel-men and others endeavouring to screen their responsibility for unplanned matings.

It is clear from this fifth letter that Galton was still hoping against the weight of accumulating facts for evidence that foreign 'gemmules' had been transfused with the blood.

(6) 5, BERTIE TERRACE, LEAMINGTON. *April* 8, 1870.

MY DEAR DARWIN, The white nose and vertical bar is, I find, of no importance. Bartlett was not accessible the day I found them out, but he has since told me they are common varieties, and I hear the same from Mr Royds, the rabbit-fancier and judge of poultry shows, from whom I bought them. Before leaving London last week I succeeded in infusing 2 per cent. of the rabbit's weight in alien blood, before I had only achieved 1·25 or 1/80th part which (on the supposition of Huxley that blood constitutes 1/10th of the whole weight of the body) is only 1/8th of the blood. In other words my transfusion, hitherto, has given only 1 great-grandparent of mongrel blood to the otherwise pure silver greys, and this is a very small matter. I do not like to risk another operation on the other jugular of my rabbits till after the forthcoming 3 litters, not till after I have had more success in the system of more abundant transfusion. I can do nothing with the blood in its natural state, it coagulates so quickly, so I defibrinise it. If I cannot ever succeed in transfusing as much into the rabbits as is necessary to make a fair experiment, I must go to larger animals, and try cross-circulation with big dogs.

[1] Pencil note against this word: 'white star'; Galton does not use the now common word 'flare.'

[2] See *Animals and Plants under Domestication*, Vol. I, pp. 403–4, 1st Edn. Vol. I, p. 345, Ed. 1875. Darwin believed absolutely in telegony and attributes it to the "diffusion, retention and action of the gemmules included within the spermatozoa of the previous male." *Animals and Plants*, 1st Edn. Vol. II, p. 388. Darwin's words seem to indicate that mere coition as apart from bearing offspring might produce telegony. The theory of telegony suggests that later born offspring should be more like the father than earlier born, but I have found no trace of this; see *R. S. Proc.* Vol. LX, pp. 273–83, 1896.

You are very kind in giving me so much valuable advice and so much encouragement.

Miss Cobbe's review is very characteristic. She has not, however, quite caught what I am driving at in religious matters and which—if the book shall be enough read to make it reasonable for me to do so—I shall express more clearly. Very sincerely yours, FRANCIS GALTON.

The religious views are probably those of the *Hereditary Genius*: see our p. 114. The review, entitled: 'Hereditary Piety[1],' by Frances Power Cobbe, will be found in the *Theological Review*, April, 1870. I do not know whether she was at this time a correspondent of Galton's, but she was so in 1877.

(7) 5, BERTIE TERRACE, LEAMINGTON. *April* 26/70.

MY DEAR DARWIN, Two more litters and no happy results, the young being all true silver greys. There ought to have been a third litter but the doe had not kindled. I shall next give a fresh infusion to every one of my old stock and hope to raise the proportion of alien blood in their bodies to at least 3 per cent. of their entire weight, or, say 30 per cent. of their entire blood.

I am obliged to defer all this for a week or two longer for my mother has been lying at the verge of death for a fortnight and I am wanted by her. She is now a trifle better and her illness—the result of bronchitis—may be less acute for a while and I may be able to get back to London. We have no reasonable hope that she will ever recover even a more moderate degree of health. Very sincerely yours, FRANCIS GALTON.

(8) 42, RUTLAND GATE, S.W. *May* 12, 1870 (written at the *Athenaeum*).

MY DEAR DARWIN, Good rabbit news! One of the latest litters has a white forefoot. It was born April 23rd, but as we did not disturb the young, the forefoot was not observed till to-day. The little things had huddled together showing only their backs and heads, and the foot was never suspected. The mother was injected from a grey and white and the father from a black and white. This, recollect, is from a transfusion of only 1/8th part of alien blood in each parent; now, after many unsuccessful experiments, I have greatly improved the method of operation and am beginning on the other jugulars of my stock. Yesterday I operated on 2 who are doing well to-day, and who now have 1/3rd alien blood in their veins. On Saturday I hope for still greater success, and shall go on...until I get at least one-half alien blood. The experiment is not fair to Pangenesis until I do.

We are for the time relieved from anxiety about my poor dear Mother, who suffered the agonies of death over and over again, but has strangely pulled through, and is now comfortable though very weak and seriously shaken. Very sincerely yours, FRANCIS GALTON.

The appearance of an 'orphan foot,' or even two, in normally whole-coloured animals purely-bred is a common event; but it is interesting to note how Galton seized any feature he could that supported mongrelisation, and thus the demonstration of the truth of 'pangenesis.' He discusses this white foot, pp. 402–3 of his paper, but, I think, might have dismissed it as he did the white noses and flare of some of his first batch of litters.

(9) 42, RUTLAND GATE, S.W. *June* 1st, 1870 (written at the *Athenaeum*).

MY DEAR DARWIN, Though I have no new litter to report, and shall have only one before the end of the month, I do not like to let more time go by, without heartily thanking you for your helpful and encouraging letter. I will not trouble you with details now, but simply say that I feel sure, unless some unexpected disaster to my stock should arise, that I shall have a very complete set of experiments finished before August. My bucks have been heavily re-transfused and I have a doe in the same state. Also I shall have all the combinations, extreme and intermediate, of pure and transfused bucks with pure and transfused does.

I find I cannot manage pigeons for want of a dove-cot, and dare not try dogs lest the Zoological Gardens should be alarmed by the noise and I should be extruded. But notwith-

[1] Reprinted in *Darwinism in Morals, and other Essays*, 1872.

standing this, I can assure you that I have the matter firmly in hand, and will be guided by the results, as to the extent of future work. Defibrinised blood is my salvation. I literally put into my silver greys during one operation as much blood as I can get from two rabbits each of the same size as the patient, and I have three bucks who have undergone two operations (but unluckily the earlier ones were far less successful). Very sincerely yours, FRANCIS GALTON.

(10) 42, RUTLAND GATE, S.W. *June* 25th, 1870.

MY DEAR DARWIN, A curious and, it may be, very interesting result delays my transfusion experiments. It is that 2, and I think all 3, of the does that had been coupled with the largely tranfused bucks prove *sterile*! Of course the sterility may be due to constitutional shock, or other minor matters, but, it *suggests* the idea that the reproductive elements are in the portion of the blood which I did *not* transfuse;—to wit the *fibrine*. In my earlier experiments, the blood was only partially defibrinised,—hence I was able to get a white leg; but in these later ones it was wholly defibrinised. It seems reasonable that the part of the blood which does most in the reparation of injuries should also be most rich in the reproductive elements. Of course I go on with the experiments with modifications of procedure....I wish I had more to tell you. I have transfused into 32 rabbits, in six cases twice over....

Very sincerely yours, FRANCIS GALTON.

The letters now break off, and the Galtons went to Paris on July 15th, intending to go to Switzerland; they did go to Grindelwald, but the declaration of war between France and Prussia led them to return. Here, after a stay at Folkestone, they paid visits to the Gurneys, at Julian Hill, at Leamington and at the Groves, reaching London only on October 17th (*L. G.'s Record*). On September 27th, George Darwin, however, wrote that his father sent his thanks for Galton's rabbit message and said that he was deeply interested in the success of the experiment. The nature of that experiment is clear, although Galton's letter detailing it appears to have perished; it is provided by Galton's paper itself; it was to cease defibrinisation, and it was done by establishing cross-circulation between the carotids, the great arteries of the neck.

"If the results were affirmative to the truth of Pangenesis, then my first experiments would not be thrown away; for (supposing them to be confirmed by larger experience) they would prove that the reproductive elements lay in the fibrine. But if cross-circulation gave a negative reply, it would be clear that the white foot was an accident of no importance to the theory of Pangenesis, and that the sterility need not be ascribed to the loss of hereditary gemmules, but to abnormal health, due to defibrinisation and, perhaps, to other causes also.

My operations of cross-circulation (which I call x) put me in possession of three excellent silver-grey bucks, and four excellent silver-grey does.......There were also three common rabbits, bucks, which were blood mates of silver-greys, and four common rabbits, does, also blood mates of silver-greys. From this large stock I have bred eighty-eight rabbits in thirteen litters, and in no single case has there been any evidence of alteration of breed. There has been one instance of a sandy Himalaya; but the owner of this breed assures me they are liable to throw them, and as a matter of fact, as I have already stated, one of the does he sent me did litter and throw one a few days after she reached me. The conclusion from this large series of experiments is not to be avoided, that the doctrine of Pangenesis, pure and simple, as I have interpreted it, is incorrect." (p. 404, *loc. cit.*)

Galton concludes that the gemmules are not independent residents in the blood; they either reside in the sexual gland itself, the blood merely forming nutriment to the growth, or they are merely temporary inhabitants of the blood and rapidly perish, so that the transfused gemmules perished before the period elapsed when the animals had recovered from their operations. Galton suggests that an experiment might be made—as the animals released from the operating table seemed little dashed in spirits, play, sniff and are ready to fight—to mate them at once.

"It would be exceedingly instructive, supposing the experiment to give affirmative results, to notice the gradually waning powers of producing mongrel offspring."

Galton clearly intended to continue the experiments; for a week after his paper was read he writes to George Darwin thanking him for a letter in which he had stated that his father was willing to take charge of eight of the rabbits[1]. Galton gives particulars about these eight young rabbits, how they should be mated and when the young should be returned to London for further operations.

"My paper will come out in the next number of the *R. Society Proceedings* and I will send your Father a copy with their pedigree marked." The *locus* for experimenting has, however, changed. "Though I shall not have my old excellent assistant Fraser, who sails this day week for Calcutta, I shall have the run of the University College Physiological Laboratory and shall be able, I believe, to conduct all the operations there with convenience greater than hitherto."

Again Darwin's letter is missing, but on April 25 Galton writes:

(11) 42, Rutland Gate, *April* 25, '71.

My dear Darwin, I am grieved beyond measure to learn that I have misrepresented your doctrine, and the only consolation I can feel is that your letter to 'Nature' may place that doctrine in a clearer light and attract more attention to it. I write hurriedly, as time is important to save the morning's post, in order to point out two passages which, I hope, in your letter to 'Nature' you will explain at length, so as to remove the false impression of Pangenesis under which I and probably others labour. In "Domestication of Animals etc." p. 374"......throw off minute granules or atoms, which *circulate* freely throughout the system......" And p. 379"...... the granules must be thoroughly diffused; nor does this seem improbable considering......the steady circulation of fluids throughout the body." (Is there not also a passage in which the words "circulating fluid" are used? I cannot hurriedly lay my hand on it, but believe it to exist.) Believe me—necessarily in great haste—Very sincerely yours, Francis Galton.

(12) 42, Rutland Gate, *May* 2/71.

My dear Darwin, I send a copy of the rabbit paper, in which I have marked the genealogy of the 6 little ones (p. 401).

You will see my reply in next week's 'Nature'. I justify my misunderstanding as well as I can and, I think, reasonably. The half plaintive end to the letter will amuse you. Very sincerely yours, Francis Galton.

I begin an entirely new and different series of experiments to-morrow.

One letter more before we come to the *Nature* correspondence. Darwin's and Galton's letters in *Nature* opened a general correspondence, in part of which Darwin was roughly handled and Galton wrote to him as follows:

(13) 42, Rutland Gate, *May* 12/71.

My dear Darwin, I have just seen ——'s not nicely conceived letter in 'Nature' on Pangenesis, and write at once to you, lest you should imagine that I in any way share the *animus* of the letter. I do not know him; at least, I have, perhaps twice only, had occasion to converse with him,—and what he says, certainly does not express my own opinion as expressed elsewhere and to others. I should not feel easy, if I did not disavow all share in it to you. Ever very sincerely, Francis Galton.

My new experiments are not hopeful—alas! I hope Pangenesis will get well discussed now.

[1] A postcard dated April 14th Down:—"The rabbits arrived safe last night and are lively and pretty this morning C. D."—seems to belong to this date.

Before we turn to the *Nature* letters, we must note one or two points, namely:

(*a*) Galton kept Darwin fully informed of the transfusion of blood experiments, and further stated their bearing on Pangenesis.

(*b*) Darwin clearly made throughout the experiments hints for their modification and extension even to other species.

(*c*) Galton's letters and paper are not compatible with Darwin having at any time warned him that the circulation of the blood was not a necessary factor in his own theory.

(*d*) Galton's words on p. 395 of his memoir cited by Darwin were too sweeping, but at the same time they were actually qualified by what he wrote on p. 404 that "the doctrine of Pangenesis, pure and simple, *as I have interpreted it*[1], is incorrect."

Letter of Charles Darwin in *Nature*, April 27, 1871.

"Pangenesis." In a paper, read March 30, 1871, before the Royal Society, and just published in the Proceedings, Mr Galton gives the results of his interesting experiments on the inter-transfusion of the blood of distinct varieties of rabbits. These experiments were undertaken to test whether there was any truth in my provisional hypothesis of Pangenesis. Mr Galton, in recapitulating "the cardinal points," says that the gemmules are supposed "to swarm in the blood." He enlarges on this head, and remarks, "Under Mr Darwin's theory, the gemmules in each individual must, therefore, be looked upon as entozoa of his blood," etc. Now, in the chapter on Pangenesis in my "Variation of Animals and Plants under Domestication," I have not said one word about the blood, or about any fluid proper to any circulating system. It is, indeed, obvious that the presence of gemmules in the blood can form no necessary part of my hypothesis; for I refer in illustration of it to the lowest animals, such as the Protozoa, which do not possess blood or any vessels; and I refer to plants in which the fluid, when present in the vessels, cannot be considered as true blood[2]. The fundamental laws of growth, reproduction, inheritance, etc., are so closely similar throughout the whole organic kingdom, that the means by which the gemmules (assuming for the moment their existence) are diffused through the body, would probably be the same in all beings; therefore the means can hardly be diffusion through the blood. Nevertheless, when I first heard of Mr Galton's experiments, I did not sufficiently reflect on the subject, and saw not the difficulty of believing in the presence of gemmules in the blood. I have said (Variation, etc., vol. ii, p. 379) that "the gemmules in each organism must be thoroughly diffused; nor does this seem improbable, considering their minuteness, and the steady circulation of fluids throughout the body." But when I used these latter words and other similar ones, I presume that I was thinking of the diffusion of the gemmules through the tissues, or from cell to cell, independently of the presence of vessels,—as in the remarkable experiments by Dr Bence Jones, in which chemical elements absorbed by the stomach were detected in the course of some minutes in the crystalline lens of the eye; or again as in the repeated loss of colour and its recovery after a few days by the hair, in the singular case of a neuralgic lady recorded by Mr Paget. Nor can it be objected that the gemmules could not pass through tissues or cell-walls, for the contents of each pollen grain have to pass through the coats, both of the pollen tube and embryonic sack. I may add, with respect to the passage of fluids through membrane, that they pass from cell to cell in the absorbing hairs of the roots of living plants at a rate, as I have myself observed under the microscope, which is truly surprising.

When, therefore, Mr Galton concludes from the fact that rabbits of one variety, with a large proportion of the blood of another variety in their veins, do not produce mongrelised offspring, that the hypothesis of Pangenesis is false, it seems to me that his conclusion is a little

[1] I have italicised these words to emphasise Galton's attitude.

[2] Note by the biographer. It would seem feasible to test the theory of pangenesis in the case of plants by considering the results obtained from the seeds of grafted and non-grafted plants of the same species.

hasty. His words are, "I have now made experiments of transfusion and cross-circulation on a large scale in rabbits, and have arrived at definite results, negativing, in my opinion, beyond all doubt the truth of the doctrine of Pangenesis." If Mr Galton could have proved that the reproductive elements were contained in the blood of the higher animals, and were merely separated or collected by the reproductive glands, he would have made a most important physiological discovery. As it is, I think every one will admit that his experiments are extremely curious, and that he deserves the highest credit for his ingenuity and perseverance. But it does not appear to me that Pangenesis has, as yet, received its death blow; though, from presenting so many vulnerable points, its life is always in jeopardy; and this is my excuse for having said a few words in its defence. CHARLES DARWIN.

Letter of Francis Galton in *Nature*, May 4th, 1871.

"Pangenesis." It appears from Mr Darwin's letter to you in last week's *Nature*, that the views contradicted by my experiments, published in the recent number of the "Proceedings of the Royal Society," differ from those he entertained. Nevertheless, I think they are what his published account of Pangenesis (Animals, etc., under Domestication, ii, 374, 379) are most likely to convey to the mind of a reader. The ambiguity is due to an inappropriate use of three separate words in the only two sentences which imply (for there are none which tell us anything definite about) the habitat of the Pangenetic gemmules; the words are "circulate," "freely," and "diffused." The proper meaning of circulation is evident enough—it is a re-entering movement. Nothing can justly be said to circulate which does not return, after a while, to a former position. In a circulating library, books return and are re-issued. Coin is said to circulate, because it comes back into the same hands in the interchange of business. A story circulates, when a person hears it repeated over and over again in society. Blood has an undoubted claim to be called a circulating fluid, and when that phrase is used, blood is always meant. I understood Mr Darwin to speak of blood when he used the phrases "circulating freely," and "the steady circulation of fluids," especially as the other words "freely" and "diffusion" encouraged the idea. But it now seems that by circulation he meant "dispersion," which is a totally different conception. Probably he used the word with some allusion to the fact of the dispersion having been carried on by eddying, not necessarily circulating, currents. Next, as to the word "freely." Mr Darwin says in his letter that he supposes the gemmules to pass through the solid walls of the tissues and cells; this is incompatible with the phrase "circulate freely." Freely means "without retardation"; as we might say that small fish can swim freely through the larger meshes of a net; now, it is impossible to suppose gemmules to pass through solid tissue without *any* retardation. "Freely" would be strictly applicable to gemmules drifting along with the stream of the blood, and it was in that sense I interpreted it. Lastly, I find fault with the use of the word "diffused" which applies to movement in or with fluids, and is inappropriate to the action I have just described of solid boring its way through solid. If Mr Darwin had given in his work an additional paragraph or two to a description of the whereabouts of the gemmules which, I must remark, is a cardinal point of his theory, my misapprehension of his meaning could hardly have occurred without more hesitancy than I experienced, but I certainly felt and endeavoured to express in my memoir some shade of doubt; as in the phrase, p. 404, "that the doctrine of Pangenesis, pure and simple, as I have interpreted it, is incorrect."

As I now understand Mr Darwin's meaning, the first passage (ii, 374), which misled me, and which stands: "......minute granules......which circulate freely throughout the system" should be understood as "minute granules......which are dispersed thoroughly and are in continual movement throughout the system"; and the second passage (ii, 379), which now stands: "The gemmules in each organism must be thoroughly diffused; nor does this seem improbable, considering......the steady circulation of fluids throughout the body," should be understood as follows: "The gemmules in each organism must be dispersed all over it, in thorough intermixture[1];

[1] In later editions of his book, Darwin replaced "circulate freely" by "are dispersed throughout the whole system" and he cancelled the words that this diffusion was not "improbable considering the steady circulation of fluids throughout the body." But elements "dispersed throughout the whole system" surely should have appeared in the blood. In a footnote to his later editions (1875, II, p. 350) Darwin admits that he should have expected to find gemmules in the blood "but this is no necessary part of the hypothesis."

nor does this seem improbable, considering......the steady circulation of the blood, the continuous movement, and the ready diffusion of other fluids, and the fact that the contents of each pollen grain have to pass through the coats, both of the pollen tube and of the embryonic sack." (I extract these latter addenda from Mr Darwin's letter.)

I do not much complain of having been sent on a false quest by ambiguous language, for I know how conscientious Mr Darwin is in all he writes, how difficult it is to put thoughts into accurate speech, and, again, how words have conveyed false impressions on the simplest matters from the earliest times. Nay, even in that idyllic scene which Mr Darwin has sketched of the first invention of language, awkward blunders must of necessity have often occurred. I refer to the passage in which he supposes some unusually wise ape-like animal to have first thought of imitating the growl of a beast of prey so as to indicate to his fellow-monkeys the nature of expected danger. For my part, I feel as if I had just been assisting at such a scene. As if, having heard my trusted leader utter a cry, not particularly well articulated, but to my ears more like that of a hyena than any other animal, and seeing none of my companions stir a step, I had, like a loyal member of the flock, dashed down a path of which I had happily caught sight, into the plain below, followed by the approving nods and kindly grunts of my wise and most respected chief. And I now feel, after returning from my hard expedition, full of information that the suspected danger was a mistake, for there was no sign of a hyena anywhere in the neighbourhood. I am given to understand for the first time that my leader's cry had no reference to a hyena down in the plain, but to a leopard somewhere up in the trees; his throat had been a little out of order—that was all. Well, my labour has not been in vain; it is something to have established the fact that there are no hyenas in the plain, and I think I see my way to a good position for a look out for leopards among the branches of the trees. In the meantime, Vive Pangenesis! FRANCIS GALTON.

In view of the previous correspondence lasting for nearly two years—referred to only in words which Darwin alone could appreciate: "followed by the approving nods and kindly grunts of my wise and most respected chief"—I think this letter of Galton's to *Nature* is one of the finest things he ever wrote in his life; it is few men who have such a great opportunity and use it so bravely. Vive Pangenesis!

Darwin may have saved his theory—for a time, but Galton saved by his restraint his own peace of mind. It suggests the spirit of the old Quaker David Barclay, his ancestor[1]:

> Yet with calm and stately mien,
> Up the streets of Aberdeen
> Came he slowly riding...

It is certain that those who reverence Galton will appreciate what he did, and those who reverence both Galton and Darwin will rejoice that their friendship remained unbroken. Nay, not only seemed intensified, but *mirabile dictu* Darwin now took even an emphasised part in the blood transfusion experiments, which went on for another three years at least! The rabbits now passed to and fro between London and Down and several of Darwin's and Galton's letters exist[2]. I cannot help thinking that Darwin still thought some argument for Pangenesis might arise from this further

[1] For some account of this ancestor of Francis Galton, see Vol. I, p. 29.

[2] It is a grave misfortune that Darwin never put the *year* on any of these letters. Galton attempted but not very successfully to date them in 1896. When I wrote my *Francis Galton, A Centenary Appreciation* (University Press, Cambridge), I thought some of Darwin's rabbit letters referred to the first rabbit experiments, but I now feel sure this is not correct. I think I have got them into proper sequence with Galton's, and they all belong to the *second* and unpublished rabbit series.

work, otherwise it is hard to understand why the further work was carried out, especially why in association with Darwin, who had denied its bearing on Pangenesis.

There was evidently a good deal of correspondence, now sadly missing—which would have explained Darwin's views on these renewed experiments—during the summer of 1871. We shall now put before the reader the remainder of this somewhat fragmentary correspondence, using it as a frame for Galton's earlier work on heredity, which we shall discuss as it is referred to. Some few of the letters of Galton have been printed in the preceding chapter; others are omitted as merely referring to the arrangement of meetings in London or at Down.

(14) We are now in Yorkshire. (*Address*) 42, RUTLAND GATE, LONDON, *Sept.* 13/71.

MY DEAR DARWIN, I had proposed writing to you, in a few days' time, about the rabbits when I received your letter. First, let me thank you very much for the kind care you have taken of them. Secondly—I grieve to hear from you, that your holiday has not been so much of a success as you had hoped so far as health is concerned and, thirdly on my own part, I am glad to say, I am and have been particularly well (except only a boil inside the ear, which hurt badly for a few days).

To return to the rabbits:—Will you kindly prevent the bucks having any further access to the does, and make away with all the young except, say, 4 or 5 as a reserve in case of continued accident in the forthcoming series of operations. As soon as I return to town, towards the end of October, I will ask you to send me the old rabbits, and will begin at once to cross-circulate every one of them. My present assistant (a most accomplished young M.B. in medical science) has not the manipulative skill of my old friend and I fear I shall have an undue proportion of corpses, but there *must* be *some* successes out of the 3 does and 3 bucks that you have and the other 3 that I have.

Latterly, my whole heart has been in *rats*; white, old English black, and wild grey, which I have had Siamesed together in pairs, chiefly white and wild grey (for my stock of black is low), in a large number of cases—perhaps 30 or 40 pair. These have been fairly successful operations so far as the well-being and comfort of the animals is concerned, but unexpected, out-of-the-way accidents, are continually occurring. One pair died after 63 (about) days of [union] and injection into the body of the one passed into the other. I hope in this way to test Pangenesis better than by the cross-circulation for if even 1 *drop of blood per hour* passes from rat to rat, a volume equal to the entire contents of the circulation of either will be interchanged in 10 days, and this is equal in its effects to a pretty complete intermingling of the bloods. All crystalloids diffuse readily from rat to rat (as poisons) through the tissues, and as we know that eggs of entozoa are carried through the veins by the blood, it seems that a long continued Siamese union would be a valuable means of experiment.

We look forward with much pleasure to our return to town, to see your daughter in her new house. I do not think that I wrote myself, for my wife was writing to offer you, which I do now, my heartiest congratulations on the event. But, you must miss her.

Ever sincerely yours, FRANCIS GALTON.

(15) 42, RUTLAND GATE, S.W. *Nov.* 9/71.

MY DEAR DARWIN, I had not the least doubt but that I could have sent you before now definite results about my rabbits, but I cannot:—you must have patience with me and wait yet longer. The cold has killed one litter to which I had looked forward, and I have had a series of other mishaps not worth specifying, the result of which is that I have only one silver grey litter to go by—viz:—that of which I told you, which included a yellow one, slate grey on the belly, *with some white on his tail*. I should have thought this a great success but it may be pronounced a 'yellow smut'! Another result is that I have built a good serviceable little house for the rabbits in my own backyard and have all the best of them under my own eye, now. The litter that died from cold, *looked* very hopefully marked—but I think one cannot trust to, apparently, pied markings in very young silver greys. I will write again as soon as I have definite results; and when the little yellow fellow is somewhat older, he is now 6 weeks, I will get opinions about him. Very sincerely yours, FRANCIS GALTON.

If you can easily lay your hands upon Gould's Anthropology of N. America, I should be grateful for it.

(16) 42, RUTLAND GATE, S.W. *November* 21/71.

MY DEAR DARWIN, I am truly ashamed to have trespassed so long on your kindness, in keeping the rabbits, but until now, owing to a variety of causes (including an epidemic where the animals are kept), I could not ask for them back. Now, all is ready to receive them in University College and I should be much obliged if you would instruct your man to send them there. I enclose labels with the address:—Charles H. Carter, Museum, University College, Gower Street, London—to put on them. Mr Carter will receive them when they arrive. Please tell your man to keep the bucks and does separate and to write *bucks* on the hamper which contains them. Will you also let me know what I am indebted to you for their feed and keep, including a judicious 'tip' to your man. I am really most obliged to you, I should have been stranded in this experiment, without the help, because I have only 2 of my lot of rabbits alive and they are both out of condition and I doubt if one will live.

The College *shuts up at 5 in the afternoon* and nothing can be received after that hour. If that is too early for the carrier, what shall I do?—When may I expect them to arrive? My rats have died sadly, but owing to causes foreign to the effects of the operation. My last living pair, after being united nearly 3 months, were killed last week for the purpose of injection. Dr Klein kindly did it for me. One animal was injected with blue and the other with red, and *vascular union is proved*; but the connection was small, however Dr Klein thinks that with a more protracted connection the union would have been more complete. So I shall go on with vigour. Very sincerely yours, FRANCIS GALTON.

(17) 42, RUTLAND GATE, S.W. *November* 24/71.

MY DEAR DARWIN, The results are indeed most curious—You must kindly permit me to run down to you to-morrow (Saturday) for an hour or so, to see them and to fix what to do. I see my train would land me at Orpington at 11.12, so I suppose I should arrive at Down at about half past twelve. If however it should be a really wet day, I would postpone coming till Tuesday. You are indeed most kind to have taken all these pains for me and I sincerely trust the experiment may yet bear some fruit. I happened to be very unlucky with my Angora *transfusions* but there is no reason why they or the cross-circulation should not succeed and I will do my best to try it. Very sincerely yours, FRANCIS GALTON.

(18) 42, RUTLAND GATE, S.W. *Dec.* 2/71. (From *Athenaeum*)

MY DEAR DARWIN, The rabbits arrived quite safely and are in excellent condition. My man's letter to tell me of their arrival did not reach me till after post time last night or I should have written earlier. Once again, most sincere thanks for your kindness in taking care of them. Ever sincerely, FRANCIS GALTON.

Jan. 23rd [?1872] DOWN, BECKENHAM, KENT.

MY DEAR GALTON, The Rabbits have lost their patches and are grey of different tints, so you were right. They are quite mature now and ready to breed. We have put 2 does to a buck, for one more generation. Had you not better have the others soon, as we shall soon want space for the Breeders?

Have you seen Mr Crookes? I hope to Heaven you have, as I for one should feel entire confidence in your conclusion[1]. Ever yours sincerely, CH. DARWIN.

[1] I think this refers to Galton's investigations into spiritualism with Crookes (see our p. 63 *et seq.*). In *More Letters of Charles Darwin*, Vol. II, p. 443, there is a letter of Darwin to Lady Derby which reads: "If you had called here after I had read the article [probably Crookes' 'Researches in the Phenomena of Spiritualism,' *Quarterly Journal of Science*, 1874] you would have found me a much perplexed man. I cannot disbelieve Mr Crookes' statement, nor can I believe in his result. It has removed some of my difficulty that the supposed power is not an

(19) 42, RUTLAND GATE, S.W. *February* 1/72. (At *Athenaeum*)

MY DEAR DARWIN, If you can make it convenient to send, in separate hampers, 1 buck and 1 doe, I should be glad, as then my stock will be large enough to be above risk of accident. As for the others, pray do what you like with them. Would you send the pair, as before, addressed to—Dr Charles Carter, University College, Gower Street, and if you could kindly let a postage card be sent to him, to say when they might be expected, they would be the more sure to be immediately attended to. I grieve to say, that I find I must abandon the rats, as a task above my power to bring to a successful issue. I am most truly obliged for the care you have taken of the rabbits—I heartily wish, for my part, that I could have done more in the way of experiment than I have effected. Very sincerely yours, FRANCIS GALTON.

(20) 42, RUTLAND GATE, S.W. *May* 26/72.

MY DEAR DARWIN, I feel perfectly ashamed to apply again to you in my recurring rabbit difficulty, which is this: I have (after some losses) got three does and a buck of the stock you so kindly took charge of cross-circulated, and so have means of protracting the experiments to another generation, and of breeding from them and seeing if their young show any signs of mongrelism. They do not thrive over well in London, also we could not keep them during summer at our house, because the servants in charge when we leave could not be troubled with them. Is it possible that any of your men could take charge of them and let them breed, seeing if the young show any colour, then killing the litter and breeding afresh, 2 or 3 times over? I would most gladly pay even a large sum—many times the cost of their maintenance—to any man who would really attend to them. Can you help me? Ever sincerely yours, FRANCIS GALTON.

DOWN, BECKENHAM, KENT. *May* 27th. [1872?]

MY DEAR GALTON, We shall be very happy to keep the 4 rabbits and breed from them. I have just spoken to my former groom (now commuted into a footman) and he says he will do his utmost to keep them in good health. I have said that you would give him a present, and make it worth his while; and that of course adds to the expense that you will be put to, and I have thought that you would prefer doing this to letting me do so, as I am most perfectly willing to do.

If you will send an answer by return of post, I will direct our carrier, who leaves here every Wednesday night, to call on next Thursday *morning* at whatever place you may direct. Next week we shall probably be at Southampton for 10 days.

We have now got 2 litters from some of the young ones which you saw here; and my man says that in one litter there are some odd white marks about their heads; but I am not going again to be deluded about their appearance, until they have got their permanent coats.

Yours most sincerely, In haste for post, C. DARWIN.

(21) 42, RUTLAND GATE. *May* 28th, 1872.

MY DEAR DARWIN, You are indeed most kind and helpful and I joyfully will send the rabbits. But really and truly I must bear every expense to the full and will rely on your groom telling me, at the end; in addition to his present. The rabbits are none of them absolutely recovered, at all events the buck and 1 doe are not, but they will want no further attention in respect to what remains unhealed of their wounds. Two of the does are believed to be in kindle, having been left with the buck a fortnight and 10 days ago. I will tell

anomaly, but is common in a lesser degree to various persons. It is also a consolation to reflect that gravity acts at any distance, in some wholly unknown manner, and so may nerve-force. Nothing is so difficult to decide as where to draw a just line between scepticism and credulity. It was a very long time before scientific men would believe in the fall of aerolites; and this was chiefly owing to so much bad evidence, as in the present case, being mixed up with the good. All sorts of objects were said to have been seen falling from the sky. I very much hope that a number of men, such as Professor Stokes, will be induced to witness Mr Crookes' experiments."

It will be clear that at this time—after the Galton investigations but before Huxley's report (see our p. 67)—Darwin was endeavouring to retain an open mind.

Dr Carter to label and send all particulars with them and to mark their backs with big numerals in ink. The carrier should call at University College for them, asking the porter at the gate. I enclose a paper for him. Once again, with sincere thanks,

Ever yours, FRANCIS GALTON.

I have just corrected proofs of a little paper to be shortly read at the Royal Society on "Blood-relationship" in which I try to define what the kinship really is, between parents and their offspring. I will send a copy when I have one; it may interest you.

42, RUTLAND GATE, S.W. *May* 29/72.

MY DEAR DARWIN, May I lunch with you on Thursday and arrange about rabbits? We shall then be staying for 2 days in your neighbourhood at Mrs Brandram, Hayes Common.

Your letter reached me just before we were leaving town for a Saturday and Sunday visit, and I did not reply at once, waiting to be sure about our engagements. If I don't receive a post card at above address to say 'no,' I will come. Ever sincerely yours, FRANCIS GALTON.

The spiritualists have given me up, I fear. I can't get another invite to a séance.

42, RUTLAND GATE, S.W. *June* 4/72.

MY DEAR DARWIN, Thank you very much about the rabbits. I however sincerely trust you did not send your man all the way on purpose for them alone! Anyhow I feel I have put you to much trouble and can only repeat how greatly I am obliged.

Your criticisms on my paper are very gratifying to me, the more so that the question you put is one to which I can at once reply. You ask, why hybrids of the first generation are nearly uniform in character while great diversity appears in the grandchildren and succeeding generations[1]? I answer, that the diagram shows (see next page[2]) that only 4 stages separate the children from the parents, but 20 from their grandparents and therefore, judging from these limited data alone, (ignoring for the moment all considerations of unequal variability in the different stages and of pre-potence of particular qualities etc.,) the increase of the mean deviation of the several grandchildren (from the average hybrid) over that of the several children is as $\sqrt{20}$: or more than twice as great. The omitted considerations would make the deviation (as I am prepared to argue) still greater.

I will add the explanatory foot-note you most justly suggest, and should be very glad if you would let me have your copy back (I will return it) with marks to the obscure passages that I may try to amend them.

I found the [?writing] an uncommonly tough job; having to avoid hypothesis on the one hand and truism on the other and, again, the difficulty of being sufficiently general and yet not too vague. It is very difficult to draw a correct verbal picture in *mezzo-tint*, I mean by burnishing out the broad effects and not by drawing hard outlines.

Ever very sincerely yours, FRANCIS GALTON.

I have knocked every symbol out of my paper and wholly rearranged the diagrams etc., to make it less unintelligible. F. G.

A pleasant journey and rest to you all!

Galton's paper was read at the Royal Society on June 13th of this year, and we now turn to its examination.

We have seen (p. 114) that Galton as early as 1869 propounded a doctrine equivalent to the continuity of the germ-plasm and the non-in-

[1] In *Animals and Plants under Domestication*, 1st Edn. 1868, p. 400, Darwin writes "Crossed Forms are generally at first intermediate in character between their two parents; but in the next generation the offspring generally revert to one or both their grandparents, and occasionally to more remote ancestors." He then proceeds to explain this by latent gemmules, and had he been a statistician could have deduced at once a Mendelian quarter! He points out the triple character of the second generation of hybrids distinctly.

[2] I omit the diagram, as I have failed to interpret it and therefore cannot transcribe it properly.

heritance of acquired characters. This position, which is clear cut and fairly easily defensible, was I hold later obscured in his mind by two influences (*a*) the strong belief of Darwin in the inheritance of acquired characters, and (*b*) Darwin's doctrine of pangenesis. Both may be summed up in the single influence: an intense admiration for Darwin, which enforced an exaggerated respect for the authority of his judgment in individual instances. The doctrines of pangenesis and of the inheritance of acquired characters seem to me to have actually retarded Galton's progress and to have rendered his statement of his own views less clear than they otherwise would have been. I trace this influence particularly in his paper 'On Blood-relationship' of 1872[1]. This memoir would, I think, have given a sharp-cut theory had it not been darkened by the shadow of Darwin's views on heredity.

We will cite in regard to this the opening words of the 'Blood-relationship':

"I propose in this memoir to deduce by fair reasoning from acknowledged facts, a more definite notion than now exists of the meaning of the word 'kinship.' It is my aim to analyse and describe the complicated connection that binds an individual, hereditarily, to his parents and to his brothers and sisters, and therefore, by an extension of similar links, to his more distant kinsfolk. I hope by these means to set forth the doctrines of heredity in a more orderly and explicit manner than is otherwise practicable.

From the well-known circumstance that an individual may transmit to his descendants ancestral qualities that he does not himself possess, we are assured that they could not have been altogether destroyed in him, but must have maintained their existence in latent form. Therefore each individual may properly be conceived as consisting of two parts, one of which is latent and only known to us by its effects on his posterity, while the other is patent and constitutes the person manifest to our senses." (p. 394.)

Galton then proceeds to say that *both* these patent and latent elements in the parent give rise to the 'structureless elements' in the offspring. Now in the above sentences Galton clearly divides the 'structureless elements' of the parent into those which give rise to the somatic characters of the parent, and those which remain latent. At first sight we might suppose from the above definitions that Galton did not include latent elements similar to those which produced the somatic characters, but it appears from his remarks on p. 398 that he really did so, for he attributes on that page special features in the offspring corresponding to special features in the parents, not to the somatic characters in the parents, but to 'latent equivalents.' In other words, he considers that, in the bulk of cases, the correspondence in somatic characters between parent and child is not due to any influence of the somatic characters of the parent, but results from the latent elements of the parent. Thus Galton's 'latent elements' constitute absolutely the gametic elements of more modern notation. Had Galton gone at this time a stage further, and asserted that the somatic characters of the parent were only an index to the latent elements in him, and not directly associated with the bodily characters of the offspring, he would have reached an important principle. I hesitate to call that principle merely the continuity of the germ-plasm, for Galton saw a good deal further than anything contained in the word 'continuity' itself. He believed that both in the case

[1] *Proc. R. Society*, Vol. xx, pp. 394–402.

of the patent and the latent elements selection took place, so that not only are the somatic elements a selection of all possible somatic elements of an individual of the same ancestry, but the latent elements or germ-plasm were themselves a selection. This selection he termed 'class representation.' That the somatic or bodily characters are a selection is, of course, obvious; that the germ-plasm is selected also is extremely probable, but less easily demonstrated. Galton represented to himself the 'structureless elements' as a vast congeries of individual elements—like balls of a great variety of colours in a bag. A selection is made of these ('class representation') for the embryonic elements which by development become the adult elements, the somatic characters; that is the simple explanation of variation in the somatic characters of individuals of the same ancestry and reared under the same environment. Another selection from the same bag gives the germ-plasm of the individual on which his gametic characters depend, i.e. the possibilities of his descendants. Thus the continuity of the 'latent elements' or as we might say of the germ-plasm was in Galton's mind broken by continual selection. The 'class representation' of the somatic characters giving the phenomenon of visible variation, and the 'class representation' of the germ-plasm the variation of stocks or stirps.

Galton did not in this paper, I do not think he ever did, carry out his hypotheses to their legitimate conclusions. In the first place the two selections from our 'bag' cannot be treated as wholly independent; the somatic characters are not perfectly correlated with the gametic characters, but they are correlated with them, and as we descend to highly specialised races highly correlated with them. It would not be unreasonable to suppose that the somatic characters arise from a sub-selection of the gametic group, or from leaving a portion of this drawing 'on the table.' But the selection of the germ-plasm must lead to its simpler and simpler structure, especially in the case of unisexual reproduction. The course of evolution must on this hypothesis start with a highly complex germ-plasm and tend to break this up into simpler and simpler groups as generation by generation more elements are differentiated, i.e. organism differs from organism by having fewer and fewer common latent elements. We should see genera breaking up into species, species into local races, and ultimately races into stirps and possibly stirps into the merely ideal 'pure lines,' or organisms in the case of which it would be impossible to carry germ-plasm selection further for it would have become of one type only; the innumerable balls of immense variety in our bag would have been reduced to a single colour!

Darwin's natural selection acts only on evolution through the definite correlation of somatic and gametic characters. Galton's germinal selection, a random selection at the output of each new individual, must—if there be isolation—tend to produce species, races and sub-races. A pure race could only be one in which all latent elements were so substantially represented that there was little chance of a 'class representation' excluding any of them[1]. This is not the place to discuss at length the bearing of Galton's

[1] Purity of race might also be preserved by much intra-racial crossing.

germinal selection on the origin of species, or his 'class representation' on the origin of somatic variations. He did not press it himself to its legitimate conclusions, and probably did not see its full bearings on evolution. His general scheme from 'structureless elements' of parent to those of offspring is as follows:

$$\begin{array}{c}\text{Structureless}\\ \text{Elements of}\\ \text{Parent}\end{array}\left\{\begin{array}{l} \left\{\begin{array}{c}\text{through `Class}\\ \text{Representation'}\\ \text{afford}\end{array}\right\}\begin{array}{c}\text{Embryonic}\\ \text{Elements}\end{array}\left\{\begin{array}{c}\text{which by a}\\ \text{development}\\ \text{become}\end{array}\right\}\begin{array}{c}\text{Adult}\\ \text{Elements}\end{array}\left\{\begin{array}{c}\text{which by a}\\ \text{Second Selection}\\ \text{contribute to}\end{array}\right\} \\ \\ \left\{\begin{array}{c}\text{through `Class}\\ \text{Representation'}\\ \text{afford}\end{array}\right\}\begin{array}{c}\text{Latent}\\ \text{Elements}\\ \text{in Embryo}\end{array}\left\{\begin{array}{c}\text{which by a}\\ \text{development}\\ \text{become}\end{array}\right\}\begin{array}{c}\text{Latent}\\ \text{Elements}\\ \text{in Adult}\end{array}\left\{\begin{array}{c}\text{which by a}\\ \text{Second Selection}\\ \text{contribute to}\end{array}\right\}\end{array}\right\}\begin{array}{c}\text{Structureless}\\ \text{Elements of}\\ \text{Offspring}\end{array}$$

What I have termed a 'Second Selection' Galton terms 'Family Representation,' I think, on the ground that these selections produce the various somatic and gametic differences to be found in the members of the same family[1]. But it seems to me that it would be best simply to speak of first and second selections instead of 'class' and 'family' representations. Having put forward this scheme Galton now proceeds to express his grave doubts as to the 'adult elements' contributing anything or at least anything substantial to the 'structureless elements' of the offspring. He asserts that where the parents have a patent character that also exists in the latent form, i.e. in their gametic characters,

"I should demur, on precisely the same grounds, to objections based on the transmission of qualities to grandchildren being more frequent through children who possess those qualities than through children who do not; for I maintain that the personal manifestation is, on the average, though it need not be so in every case, a certain proof of the existence of some latent elements." (p. 399.)

In other words Galton is insisting on the somatic characters being only correlated with, or an index to, the gametic characters, and on the absence of complete association. He states that:

"the general and safe conclusion is, that the contribution from the patent elements [somatic characters of parent] is very much less than from the latent elements [gametic characters of parent]." (p. 399.)

And again:

"We see that parents are very indirectly and only partially related to their own children, and that there are two lines of connection between them, the one [adult latent elements] of large and the other [adult somatic elements] of small relative importance. The former is a collateral kinship and very distant, the parent being descended through two stages from a structureless source, and the child (as far as the parent is concerned) through five distinct stages from the same source; the other but unimportant line of connection is direct and connects the child with the parent through two stages." (p. 400.)

Galton even speaks of the 'structureless elements' that go to form the embryonic elements of the parents as going so far as heredity is concerned to "a nearly sterile destination."

Why did not Galton have the confidence at this time to say wholly sterile destination? I think there is not the least doubt that the *l'enfant*

[1] Of course Galton recognised the biparental contributions and in a second diagram shows the increased complexity.

terrible of Darwin still somewhat obscured his view[1]. Instead of 'Vive Pangenesis!' his cry ought to have been 'Pangenesis à la lanterne pour l'amour de la Science!' Galton could not swim absolutely against the current of gemmules flowing from the somatic organs to reinforce the germinal cells! He still thought that Darwin's insistence on the heredity of some acquired characters could not be the fabric of a dream[2]. He saw the light but authority was too great:

"We cannot now fail to be impressed with the fallacy of reckoning inheritance in the usual way, from parents to offspring, using those words in their popular sense of visible personalities. The span of the true hereditary link connects, as I have already insisted upon, not the parent with the offspring, but the primary elements of the two, such as they existed in the newly impregnated ova, whence they were respectively developed. No valid excuse can be offered for not attending to this fact, on the ground of our ignorance of the variety and proportionate values of the primary elements; we do not mend matters in the least, but we gratuitously add confusion to our ignorance, by dealing with hereditary facts on the plan of ordinary pedigrees—namely, from the *persons* of the parents to those of their offspring." (pp. 400–1.)

No Mendelian ever put more strongly than Galton thus did that somatic characters are no measure of gametic possibilities! Nay, Galton knew all about the fact that the second generation of hybrids shows more diversity than the first, but he did not call it, and perhaps rightly did not call it, 'segregation.'

"It is often remarked that the immediate offspring of different races resemble their parents equally, but that great diversities appear in the next and the succeeding generations.... A white parent necessarily contributes white elements to the structureless stage of his offspring and a black, black; but it does not in the least follow that the contributions from a true mulatto must be truly mulatto." (p. 402.)

Yet Galton—and after him the whole Biometric School—have been accused at random of asserting that all characters blend!

[1] The grave danger of Pangenesis was that it could, if by a very artificial mechanism, account for so much—rightly recognised or wrongly interpreted—phenomena; it therefore blocked the way to a simpler theory which, possibly truer to nature, could not account for the latter. Hence arose the controversy as to the inheritance of 'acquired characters' of later days, a slow process of getting rid of wrong interpretations. Lastly many phenomena which Darwin accounts for by the diffused gemmules of pangenesis can be equally well described by aggregated germinal units in the reproductive cells.

[2] Darwin even thought of the inheritance of insanity as that of an acquired character. Habits, mental instincts and even insanity modified the nerve-cells and were transmitted to the offspring by differentiated gemmules (*Animals and Plants...*, 1st Edn. Vol. II, p. 395). "No one who has attended to animals either in a state of nature or domestication will doubt that many special fears, tastes, etc., which must have been acquired at a remote period, are now strictly inherited": wrote Darwin in 1873 (*Nature*, Vol. VII, p. 281). While some instincts may have been developed by long ages of selection, "other instincts may have arisen suddenly in an individual and then been transmitted to its offspring independently both of selection and serviceable experience though subsequently strengthened by habit." Darwin then cites the case of Huggins' dog 'Kepler,' but it seems to me that there was far too little known of the ancestry of 'Kepler' in *all* lines to base any evidence for the inheritance of acquired characters in a certain family of dogs having an antipathy to butchers and their shops.

There is not the slightest doubt that 20 to 30 years hence we shall hear of nervous breakdowns attributed to 'shell-shocked' fathers of the Great War, and probably spoken of as instances of the inheritance of acquired characters. Investigation of the family history of cases of 'shell-shock' shows, however, that the bulk of these cases are associated with mentally anomalous stocks.

Galton concludes as follows, therein re-asserting the difference between somatic and gametic qualities, and at the same time the value of the statistical method:

"One result of this investigation is to show very clearly that large variation in individuals from their parents is not incompatible with the strict doctrine of heredity, but is a consequence of it wherever the breed is impure. I am desirous of applying these considerations to the intellectual and moral gifts of the human race, which is more mongrelised than that of any other domesticated animal. It has been thought by some that the fact of children frequently showing marked individual variation in ability from that of their parents is a proof that intellectual and moral gifts are not strictly transmitted by inheritance. My arguments lead to exactly the opposite result. I show that great individual variation is a necessity under present conditions; and I maintain that results derived from large averages are all that can be required, and all we could expect to obtain, to prove that intellectual and moral gifts are as strictly matters of inheritance as any purely physical qualities." (p. 402.)

It is curious that in the face of such a passage as this, there should still exist writers who have not grasped that the inheritance of the mental and moral qualities was a foundation stone of Galton's creed of life. His whole theory of inheritance was developed to account for supposed difficulties in this principle raised by his critics. And the principle itself—the equal inheritance of the psychical and physical characters—was the basis of his proposal to better the race of man by giving primary weight to his nature, and only secondary importance to his nurture. This paper of Galton's is now half-a-century old; I know of no earlier paper which pointed out so definitely the distinction between the somatic and gametic characters, which emphasised the continuity of the germ-plasm[1], which raised at the very least doubts as to the inheritance of acquired characters, which asserted that the personal or bodily characters of the offspring were not the product of those of the parents, and taught that the resemblance of father and son was really like that of brothers, for all were products of selected elements of a continuous germ-plasm. I feel that adequate credit has rarely been given by biologists to Francis Galton for these results, and there is no excuse for this neglect, for the paper in question was not published in an obscure journal, but in the proceedings of the foremost English learned society.

I can only hope that, however late in the day, this *Life* of Galton may aid in demonstrating the real parentage of certain now widely-current ideas.

We may now return to the rabbit correspondence.

9, ROYAL CRESCENT, MARINE PARADE, BRIGHTON. *August* 11/72.

MY DEAR DARWIN, The buck is quite well—the enclosed note just received explains everything. Now that Dr Carter has returned, he will see that all is rightly done. Will you kindly tell your servant to explain to the carrier? Very sincerely yours, FRANCIS GALTON.

[1] To show how opposed this was to Darwin's views I may cite the *Animals and Plants...*, 1st Edn. Vol. II, p. 383: "The reproductive organs do not actually create the sexual elements; they merely determine or permit the aggregation of the gemmules." "Use or disuse etc. which induced any modification in a structure should at the same time or previously act on the cells... and consequently would act on the gemmules" (p. 382). "Hence, speaking strictly, it is *not* the reproductive elements nor the buds, which generate new organisms, but the cells themselves throughout the body" (p. 374), i.e. by the production of gemmules which aggregate in buds or sexual elements.

42, Rutland Gate, *Nov.* 7/72.

My dear Darwin, Accept very best thanks for *Expression* which I have been devouring; you will, I am sure, receive numberless letters of hints corroborative of the points you make; even I could and will send some. But I write specially to say that if you care to send any more printed circulars of queries, I can dispose of three this very month most excellently for you. One by an expedition up the Congo, another by a man from the Zanzibar side into Africa and a third by a very intelligent German (English speaking) head of a missionary college on his way to my old country in Africa.

Would you have a short note sent me,—pray do not write yourself—about the rabbits.

Ever sincerely yours, Francis Galton.

P.S. You do not I think mention in *Expression* what I thought was universal among blubbering children (when not trying to see if harm or help was coming out of the corner of one eye) of pressing the knuckles against the eyeballs; thereby, reinforcing the orbicularis.

What a curious custom hand-shaking is and how rapidly savages take to it in their intercourse with Europeans.

I have a pamphlet of yours to send back.

Down, Beckenham, Kent. *Nov.* 8th. [1872?]

My dear Galton, I was going in a day or two to have written to you about the rabbits.

Those which you saw when here (the last lot) and which were then in the transition mottled condition have now all got their perfect coats, and *are perfectly true in character*. They are now ready to breed, or soon will be; do you want one more generation? If the next one is as true as all the others, it seems to me quite superfluous to go on trying.

Many thanks for your note and offer to send out the queries; but my career is so nearly closed, that I do not think it worth while. What little more I can do, shall be chiefly new work.

I ought to have thought of crying children rubbing their eyes with their knuckles; but I did not think of it, and cannot explain it. As far as my memory serves, they do not do so whilst roaring, in which case compression would be of use. I think it is at the close of a crying fit, as if they wished to stop their eyes crying, or probably to relieve the irritation from the salt tears. I wish I knew more about the knuckles and crying.

I am rejoiced that your sister is recovering so well: when you next see her pray give her my very kindest remembrances. My dear Galton, Yours very sincerely, Ch. Darwin.

What a tremendous stir-up your excellent article on 'Prayer' has made in England and America.

42, Rutland Gate, *Nov.* 15/72.

My dear Darwin, I have left your kind letter of ten days since unanswered, having some possible rabbit combinations in view which have ended in nothing. The experiments have, I quite agree, been carried on long enough. It would be a crowning point to them if your groom could get a prize at some show for those he has reared up so carefully, as it would attest their purity of breed. There is such a show, I believe, impending at the Crystal Palace. Enclosed is a £2 cheque. Will you kindly tip him with it for me, assuring him how indebted I feel for his attention. I don't know how I can repay *you*!

Would it not be worth while before abandoning the whole affair to get a litter from each of the available does, not with a view of keeping the young, but simply of seeing whether any are born mottled, and if not of then killing them? The reason being, that the mixed breed are so very apt to take wholly after one or the other ancestor, and one might get no other evidence of impure blood than a rare instance of a decidedly mongrel birth.

However I leave this quite in your hands, knowing that it means 5 or 6 weeks more trouble with the rabbits.

I read and re-read your *Expression* with infinite instruction and pleasure, and feel sure that its influence will soon be seen at the Royal Academy. Enclosed is a small addition to the note about the family on p. 34.

My sister Emma, I am rejoiced to say, is now at the seaside steadily mending in perfect quiet and in full hopes of complete restoration to health. I wish most heartily that yours was better. Ever sincerely yours, Francis Galton.

DOWN, BECKENHAM, KENT. *Dec.* 30 [1872].

MY DEAR GALTON, A young Mr Balfour, a friend of my son's, is staying here. He is very clever and full of zeal for [Biology]. He has been transplanting bits of skins between brown and white rats, in relation to Pangenesis! He wants to try for several successive generations the same experiment with rabbits. Hence he wants to know which colours breed truest. I have, of course, recommended silver greys. What other colour breeds true? Can you tell me? I think white or albinos had better be avoided. Do any grey breeds, of nearly the colour of the wild kind, breed true? Will you be so very kind as to let me hear? I much enjoyed my short glimpse of you in London. Ever yours, C. DARWIN.

DOWN, BECKENHAM, KENT. *Jan.* 4th [1873].

MY DEAR GALTON, Very many thanks for Fraser[1]: I have been greatly interested by your article. The idea of castes being spontaneously formed and leading to intermarriage is quite new to me, and I should suppose to others. I am not, however, so hopeful as you. Your proposed Soc[y] would have awfully laborious work, and I doubt whether you could ever get efficient workers. As it is, there is much of insanity and wickedness in families; and there would be more if there was a register. But the greatest difficulty, I think, would be in deciding who deserved to be on the register. How few are above mediocrity in health, strength, morals and intellect; and how difficult to judge on these latter heads. As far as I see within the same large superior family, only a few of the children would deserve to be on the register; and those would naturally stick to their own families, so that the superior children of distinct families would have a good chance of associating most and forming a caste. Though I see so much difficulty, the object seems a grand one; and you have pointed out the sole feasible, yet I fear utopian, plan of procedure in improving the human race. I should be inclined to trust more (and this is part of your plan) to experimenting and insisting on the importance of the all-important principle of Inheritance. I will make one or two minor criticisms. Is it not probable that the inhabitants of malarious countries owe their degraded and miserable appearance to the bad atmosphere, though this does not kill them; rather than to "economy of structure"? I do not see that an orthognathous face would cost more than a prognathous face; or a good morale than a bad one. That is a fine simile (p. 119) about the chip of a statue: but surely nature does not more carefully regard races than individuals, as [I believe I have misunderstood what you mean] evidenced by the multitude of races and species which have become extinct. Would it not be truer to say that nature cares only for the superior individuals and then makes her new and better races. But we ought both to shudder using so freely the word 'Nature' after what De Candolle has said.

Again let me thank you for the interest received in reading your essay.

Yours very sincerely, CH. DARWIN.

Many thanks about the rabbits: your letter has been sent to Balfour: he is a very clever young man, and I believe owes his cleverness to Salisbury blood.

This letter will not be worth your deciphering. I have almost finished Greg's *Enigmas*. It is grand poetry, but too utopian and too full of faith for me; so that I have been rather disappointed. What do you think about it? He must be a delightful man.

I doubt whether you have made clear how the families on the Register are to be kept pure and superior, and how they are in course of time to be still further improved.

I do not know whether Francis Balfour's experiments were ever pushed to their final conclusion, but if so, I have small doubt what that conclusion would be: A change of somatic character would not affect in a highly developed mammal the gametic characters, whatever arguments may be advanced from graft-hybrids[2]: Galton's own blood-transfusion experiments came to an end at this time. There are only two references that I have been able to find to the results of the second series, so much of which was

[1] This is the "Hereditary Improvement"; see our p. 117.

[2] *Animals and Plants under Domestication*, Vol. I, pp. 413—24, Vol. II, p. 360, 2nd Edn. 1875.

actually carried on at Down. The first is by Darwin himself in the footnote p. 350 (1875) of the 2nd edition of his *Animals and Plants...*:

"He [Mr Galton] informs me that subsequently to the publication of his paper he continued the experiments on a still larger scale for two more generations, without any sign of mongrelism showing itself in the very numerous offspring."

The second occurs in Galton's paper "A Theory of Heredity"[1] in a footnote, p. 342:

"I subsequently carried on the experiments with improved apparatus, and on an equally large scale, for two more generations."

Two slight footnote notices of what occupied much of Galton's time and energy for two or more years! But the result was really of value; it demonstrated that the blood was not a primary factor in heredity[2], and it weakened to an extent, perhaps hardly realised by Darwin, the probability of pangenesis. The misfortune was that Galton could not yet dismiss the whole mechanism of gemmules.

Differences, however, between the two men on this subject did not interfere for a moment with their warm friendship, and we next find Darwin giving Galton aid in two additional matters; the first is in answering his *questionnaire* concerning the nature and nurture of English men of science, and the second in growing sweet-peas—the inquiry which led to the conception of measuring correlation.

The answers which Galton received from his correspondents in the men of science inquiry are of extraordinary interest; they form brief auto-characterisations[3] by the leading scientific Victorians—Darwin, Hooker, Huxley, Spencer, Clerk Maxwell, Stokes and many others. The *questionnaire* was accompanied by a letter setting forth the scope of the inquiry. It runs

ANTECEDENTS OF SCIENTIFIC MEN.

42, Rutland Gate, London.

To Charles Darwin, Esq. In the pursuit of an inquiry parallel to that by M. de Candolle, I have been engaged for some time past in collecting information on the Antecedents of Eminent Men. My present object is to set forth the influences through which the dispositions of Original Workers in Science have most commonly been formed, and have afterwards been trained and confirmed. As a ready means of directing attention to the importance and interest of this inquiry, I append, overleaf, a reprint of a short review of the work of M. de Candolle, which I contributed to the 'Fortnightly Review' of March, 1873.

The result of my past efforts has clearly impressed upon me the fact that a sufficiency of data cannot be obtained from biographies without extreme labour, if at all; therefore, instead of imperfectly analysing the past, it seems far preferable to deal with contemporary instances, and none are more likely to appreciate the inquiry or to give correct information than Men of Science.

The number of persons in the United Kingdom who have filled positions of acknowledged rank in the scientific world is quite large enough for statistical treatment. Thus, the Medallists of the chief scientific societies; the Presidents of the same, now and in former years; those who have been elected to serve at various times on the Council of the Royal Society, and similarly,

[1] *Journal of the Anthropological Institute*, Vol. v, pp. 329–48, 1875.

[2] Even Darwin in his use of language was influenced by popular belief as the reader will find if he turns to the postscript of Darwin's letter of Jan. 4, 1873 on p. 176.

[3] Darwin's is reproduced at length in Francis Darwin's *Life and Letters of Charles Darwin*, Vol. III, pp. 177–8.

the Presidents of the several sections of the British Association, form a body of little less than two hundred men, now living, a considerable portion of whom stand in more than one of the above categories. Other methods of selection give fifty or a hundred additional names.

Falling as you do within the range of this inquiry, may I ask of you the favour of furnishing me with information? If you should desire any portions of what you may send to be considered as private, they will be used in no other way than to afford material for general conclusions.

I send herewith a schedule which contains the questions to which I am seeking replies.

FRANCIS GALTON.

It would not I think be indiscreet to give in two notable instances the replies as to "special talents, as for mechanism, practical business habits, music, mathematics, etc.," also those on hereditary characteristics.

DARWIN.

Special talents, none, except for business, as evinced by keeping accounts, being regular in correspondence, and investing money very well; very methodical in my habits. Steadiness; great curiosity about facts, and their meaning; some love of the new and marvellous.

Somewhat nervous temperament, energy of body shown by much activity, and whilst I had health, power of resisting fatigue. An early riser in the morning. Energy of mind shown by vigorous and long-continued work on the same subject, as 20 years on the *Origin of Species* and 9 years on *Cirripedia*. Memory bad for dates or learning by rote; but good in retaining a general or vague recollection of many facts. Very studious, but not large acquirements. I think fairly independently, but I can give no instances. I gave up common religious belief almost independently from my own reflections. I suppose that I have shown originality in science, as I have made discoveries with regard to common objects. Liberal or radical in politics. Health good when young—bad for last 33 years.

Father. Practical business habits; made a large fortune and incurred no losses. Strong social affection and great sympathy with the pleasures of others; sceptical as to new things; curious as to facts; great foresight; not much public spirit; great generosity in giving away money and assistance. Freethinker in religious matters, great power of endurance.

Mother. Said to have been very agreeable in conversation.

HUXLEY.

Strong natural talent for mechanism, music and art in general, but all wasted and uncultivated. Believe I am reckoned a good chairman of a meeting. I always find that I acquire influence, generally more than I want, in bodies of men and that administrative and other work gravitates to my hands. Impulsive and apt to rush into all sorts of undertakings without counting cost or responsibility. Love my friends and hate my enemies cordially. Entire confidence in those whom I trust at all and much indifference towards the rest of the world. A profound religious tendency capable of fanaticism, but tempered by no less profound theological scepticism. No love of the marvellous as such, intense desire to know facts; no very intense love of my pursuits at present, but very strong affection for philosophical and social problems; strong constructive imagination; small foresight; no particular public spirit; disinterestedness arising from an entire want of care for the rewards and honours most men seek, vanity too big to be satisfied by them.

Father. A good musician and possessed a curious talent for drawing heads with pen and ink[1]. Impulsive but kindly; nothing otherwise remarkable.

Mother. Very impulsive and strong partizan; strong affections, marked religiosity and a constructive imagination worthy of a novelist. Physically and mentally I am far more like my mother than my father. Family generally, hot temper and tenacity of purpose; considerable power of expression in writing and speaking.

DOWN, BECKENHAM, KENT. *May* 28th, 1873.

MY DEAR GALTON, I have filled up the answers as well as I could; but it is simply impossible for me to estimate the degrees.

My mother died during my infancy and I can say hardly anything about her. It is so impossible for anyone to judge about his own character that George first wrote several of the answers about myself, but I have adopted only those which seem to me true.

[1] Inherited by his son: see *Life*, Vol. I, p. 4. The writer possesses a number of sketches by T. H. Huxley drawn on blotting paper and scraps of paper, probably at a committee meeting.

Now you may perhaps like to hear a few additional particulars about myself. I cannot remember the time when I had not a passion for collecting,—first seals, franks, then minerals, shells etc. As far as I am conscious, the one compulsory exercise during my school life which improved my intellect was doing Euclid, and this was *partly voluntary*.

At Edinburgh I do not think the lectures were of any service to me; but I profited as a naturalist by observing for myself marine animals.

At Cambridge getting up Paley's Evidences and Moral Phil. thoroughly well as I did, I felt was an admirable training, and everything else bosh.

My education really began on board the "Beagle."

I must add that my son Frank said he could safely give as my character, "sober, honest and industrious."

And now I want to ask you a question: if I had 50 men of 2 different nations, and for some reason could not measure all, if I picked out the 10 tallest of each nation, would their mean heights probably give an approximate mean between all 50 of each nation?

I hope you will get full answers to your queries, as I dare say the results will be interesting.

My dear Galton, Yours sincerely, CH. DARWIN.

42, RUTLAND GATE, S.W. *May* 30/73.

MY DEAR DARWIN, I am truly obliged by the Schedule. A few others are sent, many are promised and I have much hopes of useful statistical result in many ways. All I have thus far got confirms the belief that the families will be on the average very small. As for what the usual education will have been, I cannot yet guess.

In reply to your query about the 50, there seems—or it may be that I am stupid—that a word is omitted, displaced or somehow wrong, because the sense is not clear and I don't know how to interpret the meaning of the phrase "......would their mean heights probably give an approximate mean between all 50 of each nation," but the following will probably include what you want.

If nothing else could be assumed about the two nations than that the 10 tallest out of 50 taken at haphazard from A had a mean height of α', and those from B of β', it would be impossible therefrom to deduce either:—

(1) α and β, the respective mean heights of the 50 A and the 50 B or

(2) the ratio of α to β.

But if you grouped the 10 tallest in either case according to their heights, that is, so many between 5′ 10″ and 5′ 11″, so many between 5′ 11″ and 6′ 0″ etc., it would be possible by comparing the run of these numbers with those of an ordinary Table of the Law of Error, to estimate approximately both (1) and (2).

10 is too small a number to be serviceable I should fear in this way;—100 ought to give excellent results; in any case the degree of regularity with which the numbers happened to run would be the measure of the probability of the accuracy of the results.

If you have any case you want worked out and would send me the figures I will gladly do it. Ever sincerely yours, FRANCIS GALTON.

For the year 1874 there are no letters. Darwin was ill in September 1873, Mrs Tertius Galton (Violetta Darwin) died in February 1874, Mrs Francis Galton was very ill in September and Galton himself at Christmas with "irregular gout and influenza." Darwin's eldest son George (later Professor Sir George Darwin) takes up the correspondence.

We return to-morrow to 42, RUTLAND GATE. *Nov* 16/74.

MY DEAR GEORGE, Thank you kindly for your letter. My wife was alarmingly ill with a sudden vomiting of arterial blood, repeated during the night but fortunately never afterwards recurring. She was extremely weakened and unable to move out of bed for days, or out of the house where we were staying for weeks, but she has steadily mended and now 9 weeks have passed and she *is* almost and *looks* quite herself again[1]. We were staying with Judge Grove at the time, in a house he had taken in Dorsetshire for the shooting,—and his extreme kindness and that of all the family we can never forget.

I am rejoiced at the very good account you give of your health, and the good news of your Father. Somebody ought to make a fortune by "Drosera pills"—vegetable pepsin! The name would be capital. Poor Hooker,—what a frightful blow,—and a young family of girls wanting a mother.

We have been at Leamington for a fortnight and return home to-morrow. Previously we were at Bournemouth, when I renewed an acquaintance with H. Venn of Caius, who is great on "Chaucer." I wonder what your work now is. I saw your rejoinder in the *Quarterly* but not the original attack. I have alluded to your article on "Restrictions etc." in my book, which ought to be out soon. Ever yours, FRANCIS GALTON

GEORGE DARWIN, Esq.

42, RUTLAND GATE, S.W. *Jan.* 8/75.

MY DEAR GEORGE, Thanks for Lady R——'s letter, though her correspondent says little, and many thanks for your letter 3 or 4 days since.

That "curve of double curvature" was a sad slip for "curve of contrary flexure." The other point, I unluckily cannot answer, for I cannot get from the printer my copies of the paper and do not recall the passage or context. When we next meet I will tell you. Thank you much for the equation to the ogive.

Dr A. Clarke and nature have done me a world of good; my heart is set a going again and he quite withdraws a somewhat dispiriting diagnosis which he made when he first saw me. He told me of your diagram, on the facts of which I *most heartily* congratulate you.

On Thursday, Jan. 11th, there will be a Statistical Council when the papers will probably be arranged. If I get there, I will send a postcard to tell when your paper is to come in.

My twin papers come in and some are very interesting. J. Wilson of Rugby is a twin and sends me lots of addresses. I got a most curious letter from Lady E——, whose family abounds with twins, besides one treble and one quadruple birth. I feel saturated with midwifery and am haunted with imaginary odours of pap and caudle! You have real odours of pitch and tar.

Ever sincerely, FRANCIS GALTON.

GEORGE DARWIN, Esq.

42, RUTLAND GATE, LONDON. *April* 14/75.

MY DEAR DARWIN, George told me that you would very kindly have some sweet-peas planted for me, and save me the produce. I send them in a separate envelope with marked bags to put the produce in, and full instructions which I think your gardener will easily understand. I am most anxious to repair the disaster of last year by which I lost the produce of all my sweet-peas at Kew. With very many thanks, Yours very faithfully, FRANCIS GALTON.

June 2nd, 1875. (FONTAINEBLEAU, at present only.)

MY DEAR DARWIN, Thank you very much for your kind letter and information. It delights me that (notwithstanding the Frenchman's assertion) the large peas do really produce large plants, and that the extreme sizes sown (except Q) are coming up. I could not and did not hope for complete success in rearing all the seedlings, but have little doubt that the sizes that have failed may be supplemented by partial success elsewhere.

We have found Fontainebleau very pleasant and are now moving on via Neuchâtel, with some hope that George may, as he was inclined to do, hereafter fall in with us. He knows how to learn our address from time to time. My wife is already markedly better. With our united kindest remembrances to you all, Ever yours, FRANCIS GALTON.

It seems absurd to *congratulate* you on your election to the Vienna Academy, because you are a long way above such honours, but I am glad *they* have so strengthened their list by adding your name to it.

[1] The grave anxiety of a recurrence hung like a sword above the heads of the Galtons for many years. Mrs Galton's *Record* shows that from this time onward, till her death, she was more or less an invalid, in frequent pain, which limited largely her social activities.

42, Rutland Gate. *Sept.* 22/75.

My dear Darwin, In "Domestication," ii, 253, you quote as a striking instance of *variation* a case communicated by Dr Ogle of 2 girl twins who had a crooked finger, no relative having the same. It happened, in my twin inquiries, that a case was sent me which is possibly or probably the same as your's—but which is a case of *reversion*. I send the particulars of this over leaf. You might think it worth while in the view of your 2nd Edition to ask Dr Ogle if his case *was* that of the Misses M——. I am not acquainted myself either with the Misses M—— or with Dr M——. Dr Gilchrist of the Crichton Institution, Dumfries, sent me Dr M——'s communication. We are only lately back in England and are not even yet settled in town. Will Frank kindly send me a line about the *sweet-peas*? With united kind remembrances to you all, Ever sincerely, Francis Galton.

I have been delighting in your "Insectivorous Plants."

Extract from a private letter to me, written by Dr F—— M——. (No address on this letter, but it is from Scotland and was enclosed by Dr Gilchrist of Dumfries.)

The Misses M—— (twins Oct. 16, *in* 1875)

"There is a congenital flexion at the second phalangeal joint of the little finger in each case, but the flexion is not so marked as to cause unsightliness or discomfort. I have ascertained that they inherited this peculiarity from their grandmother on the mother's side. The parents had no trace of it, nor any one of four brothers and three sisters!"

Down, Beckenham, Kent. *Sept.* 22nd, '75.

My dear Galton, I am particularly obliged for your letter, and will write to Dr Ogle. I think his case is different, and if you do *not* hear from me again, you will understand this to be the case.

I enclose a letter which when read kindly return to me.

With respect to the sweet-peas if you have time I think you had better come down and sleep here and see them. They are grown to a tremendous height and will be very difficult to separate. They ought to have been planted much further apart. They are covered with innumerable pods. The middle rows are now the tallest. Three of the plants are very sickly and one is dead. The row from the smallest peas are still the smallest plants. See what I say in "Var. under Dom." Vol. ii, p. 347, about the peculiar properties of plants raised from the *small* terminal peas of the pods.

I am surprised and very much pleased at your liking my "Insectivorous Plants." I hope that your tour has done you much good. My dear Galton, Yours very sincerely, Ch. Darwin.

42, Rutland Gate. *Sept.* 24th, 1875.

My dear Darwin, We have stayed on in town another day so I have got from the Royal Society and send herewith Parts XIV and XV of the *Revue Scientifique* which contain the part of Claude Bernard's lectures which you wished to see. I have put pencil X at pages 324, 325, 327, 352 (in each case on the 2nd column of the page)[1]. These are the principal passages. Please send the pamphlets back when done with, to the Royal Society, *as returned by me*. Also I return the slips from *Nature* (Romanes) with many thanks.

Overleaf I send a note about the continuation of my Pangenesis experiments. I see I made a great mistake about the number of generations when we spoke yesterday. There were only 3 generations operated on, on *both* sides. I don't care to claim cases in which a great grand-*son* was matched with a grand-*daughter* as an additional generation. Besides, the cases were few.

Very sincerely yours, Francis Galton.

Nov. 2nd [1875]. Down, Beckenham, Kent.
Railway Station, Orpington on S.E.R.

My dear Galton, I hear from George that you are going to write on inheritance and therefore I think it worth telling you that Huxley does not at all believe in Balbiani's views and statements. He says he published some years ago some strange facts and then went right round and gave them all up. I send you Wedderburn's note and a pamphlet by him which will amuse you and which need not be returned. Yours very sincerely, Ch. Darwin.

[1] The pencil crosses may still be traced in the Royal Society copy of the *Revue Scientifique* 1874, witness to the fact that great men are not always great enough to obey library regulations!

42, RUTLAND GATE, S.W. *Nov.* 3/75.

MY DEAR DARWIN, It was truly kind of you, to write me with your own hand, a note of warning about Balbiani; but I do not use his statements in any way, in my forthcoming memoir which is to be read next Tuesday at the Anthropological Society.

The general line of it is this:

First I start with the 4 postulates, in favour of which you have so strongly argued, and which may reasonably be now taken for granted:—

1. Organic units in great number.
2. Germs of such units in still greater number and variety (existing *somewhere*).
3. That undeveloped germs do not perish; but multiply and are transmissible.
4. Organisation wholly depends on mutual affinities.

From these 4 postulates, I logically deduce several results, one of which is the importance and almost the necessity of double parentage in all complex organisations, and consequently of sex.

Then I argue that we must not look upon those germs that achieve development as the main sources of fertility; on the contrary, considering the far greater number of germs in the latent state, the influence of the former, i.e. of the personal structure, is relatively insignificant. Nay further, it is comparatively sterile, as the germ once fairly developed is passive; while that which remains latent continues to multiply. From this follows:—

(1) The extremely small transmissibility of acquired modifications (to which I recur).
(2) The fact that exceptional gifts are sometimes barely transmissible (here the sample was over rich and drained the more fecund residue).
(3) The fact of some diseases skipping one or more generations; (here the supposition is made of the germs of those diseases being peculiarly gregarious, hence the general outbreak of them leaves but a small residuum which has not strength to break out in the next generation, but being husbanded in a latent form, there multiplies and recovers strength to break out in the next or in a succeeding generation).

Next, I go into the question of affinities and repulsions, which I put as necessarily numerous and many-sided (while professing entire ignorance of their character) and I argue thence, a long period of restless unsettlement in the newly fertilised ovum, accompanied as we know it to be, with numerous segregations and segmentations in each of which the dominant germs achieve development, while the residue is segregated to form the sexual elements. But I argue, that as our experience of political and other segregations shows that they are never perfect, we are justified in expecting that numerous alien germs will be lodged in every structure and that specimens of all of them will be found in almost all parts of the body. In this way, I account for the reproduction of lost parts, etc., as well as for the inheritance of all peculiarities that had been congenital in an ancestor.

I then consider the cases of inheritance of what had been non-congenital in an ancestor, but acquired by him. I show that the deduction usually made, that the structure reacts on the sexual elements, is not justified by the evidence of adaptivity of race, *when* this depends on conditions which *act equally* on all parts of the body. My reason is, that since the same agents (viz. the germs) are concerned both in growth and in reproduction, the conditions that would modify the one, would simultaneously modify the other; hence they would be collaterally affected and the apparent inheritance is not a case of inheritance at all, in the strict sense of the word. Nay the progress may begin to vary under changed conditions *sooner* than the parent (as in the hair or fleece of the young of dogs and sheep, transported to the tropics).

As regards Brown-Séquard's guinea-pigs;—if I rightly understand and am informed of his experiment, it is open to fatal objection. The guinea-pigs that were operated on appear to have been kept separate from the rest. If so, we should expect the young sometimes to have convulsive attacks from mere imitation, just as we should expect of children brought up in a ward of epileptic patients, or among hysterical people (revivals, dancing mania etc.). Besides, there is not the least evidence that the mutilation of the spinal marrow, on which the parental epilepsy primarily depended, was inherited. I also disparage much other evidence of the inheritance of acquired modifications, leaving but a very small residue to accept. For this residue, I account by supposing the germs thrown off by the structure during its regular reparation, to frequently find their way into the circulation and some of these occasionally to reach the sexual elements and to become lodged and naturalised there, either by finding an unoccupied place or by dislodging others, like immigrants into an organised society, coming from a foreign country. Thus I account both for the fact, and for the great rarity and slowness of the inheritance of acquired modifications.

In conclusion, I restate a former definition, that I gave of the character of the relationship between parent and child, which I make out to be, not like that which connects a parent nation and its colonists, but like that which connects the *representative government* of the parent nation with the representative government of the colonists; with the further supposition, that the government of the parent country is empowered to nominate a small proportion of the colonists.

I have now, so far as the limits of a letter admit, made a clean breast of my audacity in theoretically differing from Pangenesis:—

(1) In supposing the sexual elements to be of as early an origin as any part of the body (it was the emphatic declarations of Balbiani on this point that chiefly attracted my interest) and that they are not formed by aggregation of germs, floating loose and freely circulating in the system, and

(2) In supposing the personal structure to be of very secondary importance in Heredity, being, as I take it, a *sample* of that which is of primary importance, but not the thing itself.

If I could help, even in accustoming people to the idea that the notion of Organic Germs is certainly that on which the true theory of Heredity must rest, and that the question now is upon details and not on first principles, I should be very happy. Ever yours, FRANCIS GALTON.

Thanks for the letter on the Hindoo family, which I will keep, and for the pamphlet on the wholesale execution of weakly people, which I return by book post.

Nov. 4th [1875]. DOWN, BECKENHAM, KENT.
RAILWAY STATION, ORPINGTON, S.E.R.

MY DEAR GALTON, I have just returned from London where I was forced to go yesterday for Vivisection Commission.

I have read your interesting note and am delighted that you stick up for germs. I can hardly form any opinion until I read your paper *in extenso*. I have modified parts of the Chapter on Pangenesis which is now printing, and have allowed that the gemmules may, or perhaps do, multiply in the reproductive organs. I write now as I fancy that you have not read B.-Séquard's last paper, in which he gives 17 or 13 (I forget which) instances of deficient toes on the *same* foot in the offspring of parents, which had gnawed off their own gangrenous toes owing to the sciatic nerve having been divided.

You speak of "almost the necessity of double parentage in all complex organisations." I suppose you have thought well on the many cases of parthenogenesis in Lepidoptera and Hymenoptera and surely these are complex enough.

I am very glad indeed of your work, though I cannot yet follow all your reasoning.

In haste, Most sincerely yours, C. DARWIN.

DOWN, BECKENHAM, KENT. [? *Nov* 4, 1875[1].]
RAILWAY STATION, ORPINGTON, S.E.R.

MY DEAR MR GALTON, My father thought you might care to have the reference to Brown-Séquard's paper. There is a good résumé of all his observations in the 'Lancet,' Jan. 1875, p. 7.

Yours very sincerely, FRANCIS DARWIN.

[1] The reader will note with amusement the *complete* omission of date—the inheritance in an intensified form of a habit peculiar not only to Charles Darwin but also to Mrs Darwin. I only know one letter to which Darwin did put a date, it is the following written to his aunt Violetta Galton, Francis Galton's mother.

July 12, 1871. DOWN, BECKENHAM, KENT.

MY DEAR AUNT, I am very much obliged to you for your great kindness in writing to me with your own hand. My sons were no doubt deceived, and the picture-seller affixed the name of a celebrated man to the picture for the sake of getting his price. Your note is a wonderful proof how well some few people in this world can write and express themselves at an advanced age. It is enough to make one not fear so much the advance of age, as I often do, though you must think me quite a youth! With my best thanks, pray believe me with much respect, Your affectionate nephew, CHARLES DARWIN.

This letter so gracefully suggestive of both Violetta Darwin and Charles Darwin deserves to be put on record.

42, RUTLAND GATE. *Nov.* 5/75.

MY DEAR DARWIN, Three proofs reached me from the *Contemporary Review* of my 'Theory of Heredity,' so I can spare one, and as I know you like to mark what you read, do not care to return it. I hope it will make my meaning more clear. The remarks printed as a note on p. 5, but which I ought to have put in the text, will meet what you wrote about the Hymenoptera.

I am most obliged for what you tell me about Brown-Séquard; I did not know of it, and will hunt up the passage to-day. (Thanks for the reference, received this morning.)

I should be truly grateful for criticisms which might enable me to modify or make clear before it is too late. Ever yours, FRANCIS GALTON.

What a nuisance this modern plan is, of sending proofs in sheet, and not in strip. One can't amend freely.

The paper which Galton sent Darwin is entitled 'A Theory of Heredity.' This memoir was in type for the *Contemporary Review* in November 1875[1], and was read before the Anthropological Institute in the same month. It was revised and printed in the *Journal of the Anthropological Institute* (Vol. v, pp. 329–48), and it is to this issue that we shall refer. The paper follows generally the lines of the 'Blood-relationship' of 1872, except that it still more definitely discards 'Pangenesis' and casts still further doubt on the heredity of acquired characters, and modification of offspring characters by the use or disuse of the same characters in the parent. The paper therefore marks a further stage in Galton's dissent from Darwin's theory and Darwin's views. Galton writes as follows:

"The facts for which a complete theory of heredity must account may conveniently be divided into two groups; the one refers to those inborn or congenital peculiarities that were also congenital in one or more ancestors, the other to those that were not congenital in the ancestors, but were acquired for the first time by one or more of them during their lifetime, owing to some change in their conditions of life.

The first of these two groups is of predominant importance, in respect to the number of well-ascertained facts that it contains, many of which it is possible to explain, in a broad and general way, by more than one theory based on the hypothesis of organic units. The second group includes much of which the evidence is questionable or difficult of verification, and which, as I shall endeavour to show, does not, for the most part, justify the conclusion commonly derived from it. In this memoir I divide the general theory of heredity into two parts, corresponding respectively to these two groups. The first stands by itself, the second is supplementary and subordinate to it." (pp. 329–30.)

After noting that Darwin, in the chapter on Pangenesis in the *Animals and Plants...*, had given the most elaborate epitome then extant of the many varieties of facts which a complete theory of heredity must account for, Galton states that his conclusions will differ essentially from Darwin's, and continues:

"Pangenesis appears more especially framed to account for the cases which fall in the second of the above-mentioned groups[2], which are of a less striking and assured character than those in the first group, and it will be seen that I accept the theory of Pangenesis with considerable modification, as a supplementary and subordinate part of a complete theory of heredity, but by no means for the primary and more important part." (p. 330.)

[1] It appeared in that *Review* in the following month. It was published also in the *Revue Scientifique*, T. x, pp. 198–205, 1876.

[2] Later on p. 347 Galton says that Pangenesis over-accounts for the facts of acquired modifications and reparations.

Galton next defines the word 'stirp' to

"express the sum total of the germs, gemmules or whatever they may be called, which are to be found, according to every theory of organic units, in the newly fertilised ovum—that is in the early pre-embryonic stage—from which time it receives nothing further from its parents, not even from its mother, than mere nutriment[1]...... This word 'stirp,' which I shall venture to use, is equally applicable to the contents of buds, and will, I think, be found very convenient, and cannot apparently lead to misapprehension."

We now pass to the essential features of Galton's theory, which corresponds far more closely than Darwin's to modern ideas, indeed it is often difficult to say how much modern ideas have taken from Galton—without acknowledgment of the source.

The stirp is the organised aggregate of organic units, or germs. The personal structure develops by selection out of a small portion of these units, and the sexual elements of the new individual are generated by the residuum of the stirp. There is no free circulation of gemmules from the cells to be aggregated in the sexual organs. When the somatic elements are being formed from the stirp any segmentation may contain 'stray and alien gemmules,' and many of these may become entangled and find lodgment in the tissue. When these gemmules are lodged in great variety, the somatic cells are really reproductive cells and thus Galton would account for the replacement of a lost limb in the lower animals, or the reparation of simple tissues in the higher ones. The *selection* of organic units to form the somatic characters of the individual from the whole host in his stirp Galton looks upon as of the highest importance. He considers that a sort of struggle for place goes on among the innumerable germs of the stirp, and those germs which are most frequent or have certain intrinsic qualities[2] will be most successful. He considers that this continual selection leads ultimately in unisexual reproduction to the elimination of necessary units and so to degeneration; sex, he argues, is not primary, but a result of the advantage of a more primary double parentage, which lessens the chance of one or more of the needful species of germs in the stirp disappearing by selection[3]. Galton even goes so far as to suggest that where an excess of germs has been withdrawn from the stirp to form a marked character, for example, great ability or even a pathological state, there will be an absence of these germs in the residue, which goes to form the new sexual element, and he accordingly accounts in this way for the offspring of a man of genius having small ability, or again

[1] Galton (p. 341) very aptly remarks that if pangenetic gemmules circulated freely through the system, there can be little doubt that they would reach the body of an unborn child. Thus the paternal gemmules in that body would be dominated by an invasion of maternal gemmules with the final result that an individual would transmit maternal peculiarities far more than paternal ones; "in other words people would resemble their maternal grandmothers very much more than other grandparents, which is not at all the case."

[2] The "dominant germs" are "those that achieve development." (p. 341.)

[3] "There is yet another advantage in double parentage, namely that as the stirp whence the child sprang is only half the size of the combined stirps of his two parents, it follows that one-half of his possible heritage must have been suppressed. This implies a sharp struggle for place among the competing germs, and the success, as we may infer, of the fitter half of their numerous varieties." (p. 334.)

for diseases skipping a generation. This selecting out of germs will not occur in animals of pure breeds for their stirp contains only one or a very few varieties of each species of germ, so that the selection will contain all, and thus the offspring resemble their parents and one another.

"The more mongrel the breed, the greater is the variety of the offspring." (p. 336.)

To this principle, however, Galton adds a limitation, the stirp cannot be indefinitely increased in complexity, because there is a limit to the space it occupies. There is a finite, if great, number of varieties of germs, and of the individual germs in each variety.

"Thus in the gradual breeding out of negro blood, we may find the colour of a mulatto to be the half, and that of a quadroon to be the quarter of that of his black ancestors; but as we proceed further, the sub-division becomes very irregular; it does not continue indefinitely in the geometrical series of one-eighth, one-sixteenth, and so on, but is usually present very obviously or not at all, until it entirely disappears." (p. 335.)

Turning now to the germ which has developed into a somatic cell, Galton questions whether it does produce gemmules at all—at any rate its fertility is far less than that of the latent germ. Influences acting on the somatic cells of the parent are only slightly or not at all represented in the like somatic cells of the offspring. He considers at some length instances of inherited mutilations and of acquired characters, and thinks they may be reasonably looked upon as a 'collection of coincidences.' Even if there are real cases of changes in the somatic cells of the parents influencing the somatic characters of the offspring, Galton would but admit that occasionally gemmules are thrown off by somatic cells, which find their way into the circulation and ultimately obtain a lodgment in the already constituted sexual elements. Such a process is, however, independent of and subordinate to the causes which mainly govern heredity (pp. 347–88). Even to the last Galton did not wholly give up Pangenesis, for Darwin had accepted Brown-Séquard's epileptic guinea-pigs, yet as Galton remarked:

"It is indeed hard to find evidence of the power of the personal structure to react upon the sexual elements that is not open to serious objection." (p. 345.)

Finally I may cite:

"The hypothesis of organic units enables us to specify with much clearness the curiously circuitous relation which connects the offspring with its parents. The idea of its being one of direct descent, in the common acceptation of that vague phrase, is wholly untenable, and is the chief cause why most persons seem perplexed at the appearance of capriciousness in hereditary transmission. The stirp of the child may be considered to have descended directly from a part of the stirps of each of its parents, but then the personal structure of the child is no more than an imperfect representation of his own stirp, and the personal structure of each of the parents is no more than an imperfect representation of each of their own stirps." (p. 346[1].)

Such a modern idea as that parents are only conduit-pipes for the germ-plasm of their stocks is fully expressed by Galton with better limitation, and with fuller suggestiveness, both in this paper and in that on Blood-

[1] From the modern biometric standpoint the association is 'correlational' not causal.

relationship, than in much current literature. It is only the terminology and the fact that Galton was not a professional biologist which have deprived him of the credit due to him as the discoverer or inventor of what we now term the 'continuity of the germ-plasm.' Might not that theory, Galton modestly suggests, be substituted with advantage for that of pangenesis?

DOWN, *Nov.* 7th [1875].

MY DEAR GALTON, I have read your essay with much curiosity and interest, but you probably have no idea how excessively difficult it is to understand. I cannot fully grasp, only here and there conjecture, what are the points on which we differ—I daresay this is chiefly due to muddle-headiness on my part, but I do not think wholly so. Your many terms, not defined "developed germs"—"fertile" and "sterile" germs (the word 'germ' itself from association misleading to me), "stirp,"—"sept," "residue" etc. etc., quite confounded me. If I ask myself how you derive and where you place the innumerable gemmules contained within the spermatozoa formed by a male animal during its whole life I cannot answer myself. Unless you can make several parts clearer, I believe (though I hope I am altogether wrong) that very few will endeavour or succeed in fathoming your meaning. I have marked a few passages with numbers, and here make a few remarks and express my opinion, as you desire it, not that I suppose it will be of any use to you.

(1) If this implies that many parts are not modified by use and disuse during the life of the individual, I differ from you, as every year I come to attribute more and more to such agency.

(2) This seems rather bold, as sexuality has not been detected in some of the lowest forms, though I daresay it may hereafter be.

(3) If gemmules (to use your own term) were often deficient in buds I could but think the bud-variations would be commoner than they are in a state of nature; nor does it seem that bud-variations often exhibit deficiencies which might be accounted for by absence of the proper gemmules. I take a very different view of the meaning or cause of sexuality.

(4) I have ordered Fraser's Mag. and am curious to learn how twins from a single ovum are distinguished from twins from 2 ova. Nothing seems to me more curious than the similarity and dis-similarity of twins.

(5) Awfully difficult to understand.

(6) I have given almost the same notion.

(7) I hope that all this will be altered. I have received new and additional cases, so that I have now not a shadow of doubt.

(8) Such cases can hardly be spoken of as very rare, as you would say if you had received half the number of cases which I have.

I am very sorry to differ so much from you but I have thought that you would desire my open opinion. Frank is away; otherwise he should have copied my scrawl.

I have got a good stock of pods of Sweet Peas, but the autumn has been frightfully bad; perhaps we may still get a few more to ripen.

My dear Galton, Yours very sincerely, CH. DARWIN.

A. R. Wallace took a different view as to what Galton had achieved in a letter of the following spring.

THE DELL, GRAYS, ESSEX. *March* 3rd, 1876.

DEAR MR GALTON, I return your paper signed. It is an excellent proposal. I must take the opportunity of mentioning how immensely I was pleased and interested with your last papers in the *Anthrop. Journal*. Your 'Theory of Heredity' seems to me most ingenious and a decided improvement on Darwin's, as it gets over some of the great difficulties of the cumbrousness of his Pangenesis. Your paper on Twins is also wondrously suggestive.

Believe me, Yours very faithfully, ALFRED R. WALLACE.

F. GALTON, Esq.

42, Rutland Gate. *Nov.* 8/75.

My dear Darwin, Alas! Alas!—and I had taken such pains to express myself clearly, and I see what I mean, so clearly!

I was most obliged for the Brown-Séquard reference in the *Lancet*, and will certainly alter the paragraph. His non-publication of the papers, even in abstract, read by him at the British Association in 1870, had given me additional fear that there was something wrong.

All the other points you refer to in your letter, I will do what I can about: i.e., make clearer, answer, or amend; but it is too late to make more than small alterations in the proof.

Thank you for reference and offer to send Panum, but I have a description of his results, so far as I want them, in C. Dareste (*Ann. Sc. Naturelles* [*Zoologie*, T. XVII], 1862, 'Sur les œufs à double germe,' p. 34).

In my 'Fraser' article there is a most unlucky and absurd collocation of words, which I heartily hope no critic will seize upon, for which I simply can't account except in the supposition of badly scratching out in the MS., and variously altering some passage. It is about 'double yolked eggs' and 'simple germs'. I ought never to have passed it in proof; but there it is.

The twins born in one chorion,—never mind whether 2 amnions or not,—is Kleinwächter's dictum which he fortifies by numerous modern German authorities; Kiwisch being the only one who, it appears, still talks of fusion of membranes. I also noted the remark in the Catalogue of the Museum Coll. Surgeons "Teratology" that twins in one chorion are *probably* (I think that was the word) derived from 2 germinal spots on one ovum.

If you care to see Kleinwächter, I could send it you.

Very sincerely yours, Francis Galton.

42, Rutland Gate, S.W. *Nov.* 10/75.

My dear George, I got my back Statistical Society publications last night and have read your cousin-paper with very great interest[1]. You certainly have exploded most effectually a popular scare. Would it be profitable to make any "probable error" sort of estimate of your results, which should eventuate in some such form as this: "The injurious effects of first-cousin marriages, measured in such and such ways, cannot exceed so and so, and probably do not exceed so and so"?

You ought to found a fortune upon your discovery,—Thus: there are, say, 200,000 annual marriages in the kingdom, of which 2,000 and more are between first cousins. You have only to print in proportion, and in various appropriate scales of cheapness or luxury:

"WORDS of Scientific COMFORT
and ENCOURAGEMENT
To COUSINS who are LOVERS"

then each lover and each of the two sets of parents would be sure to buy a copy; i.e. an annual sale of 8,000 copies!! (Cousins who fall in love and don't marry would also buy copies, as well as those who think that they *might* fall in love.)

I read my "Theory of Heredity" at the Anthropological last night, when up got a mad spiritualist who orated, and then offered to address the meeting on the subject as a medium; the spirit speaking through his lips. (This was not accepted.)

Ever sincerely yours, Francis Galton.

George Darwin, Esq.

Nov. 10th. Night. [1875] Down, Beckenham, Kent.
Railway Station. Orpington. S.E.R.

My dear Galton, I have this minute finished your article in Fraser and I do not think I have read anything more curious in my life. It is enough to make one a Fatalist, I am in a passion with the *Spectator* who always muddles if it is possible to muddle. But after all he does not write so odiously as I did in my letter, which you received so beautifully. I should be glad

[1] *Journal of the Royal Statistical Society*, Vol. XXXVIII, pp. 153–82. I may perhaps be permitted to add the word of warning that the danger of cousin marriage is *not* a popular scare. Any patent or latent defect is certain to be emphasised by cousin marriage as of course any good characteristic.

to be convinced that the obscurity was *all* in my head, but I cannot think so, for a clear-headed (clearer than I am) member of my family read the article and was as much puzzled as I was. To this minute I cannot define what are "developed," "sterile" and "fertile" germs. You are a real Christian if you do not hate me for ever and ever.

I shall try you when we come to London in a month or six weeks time, as I want to ask a question about averages, which can be asked in a minute or two, but would fill a long letter.

Yours very sincerely, CH. DARWIN.

P.S. As soon as I am sure that no more pods of Sweet Peas will ripen, I will send all the bags in a box per Railway to you.

42, RUTLAND GATE, S.W. *Nov.* 26/75.

MY DEAR DARWIN, How can I thank you sufficiently for the trouble you have taken with the peas, which arrived last night in beautiful order. You must let me know, when we next meet, if there is anything I owe you for payments of any kind connected with them; Will you, in the meantime, give the enclosed 10/- (I send an order made out in *your* name) to the gardener from me? and tell him that I am much obliged for his care.

Ever yours, FRANCIS GALTON.

Romanes has told me much of his wonderfully interesting results with the Medusae.

Dec. 18th [1875] (Home on Monday).

MY DEAR GALTON, George has been explaining our differences. I have admitted in new Edit. (before seeing your essay) that perhaps the gemmules are largely multiplied in the reproductive organs; but this does not make me doubt that each unit of the whole system also sends forth its gemmules. You will no doubt have thought of the following objection to your view, and I should like to hear what your answer is. If 2 plants are crossed, it often or rather generally happens that every part of stem, leaf—even to the hairs—and flowers of the hybrid are intermediate in character; and this hybrid will produce by buds millions on millions of other buds all exactly reproducing the intermediate character. I cannot doubt that every unit of the hybrid is hybridised and sends forth hybridised gemmules. Here we have nothing to do with the reproductive organs. There can hardly be a doubt, from what we know, that the same thing would occur with all those animals which are capable of budding and some of those (as the compound Ascidians) are sufficiently complex and highly organised.

Yours very sincerely, CH. DARWIN.

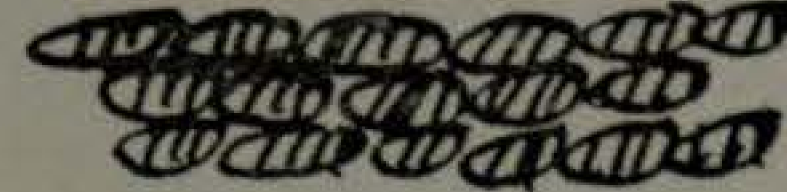
42, RUTLAND GATE. *Dec.* 19/75.

MY DEAR DARWIN, The explanation of what you propose does not seem to me in any way different on my theory, to what it would be in any theory of organic units. It would be this:

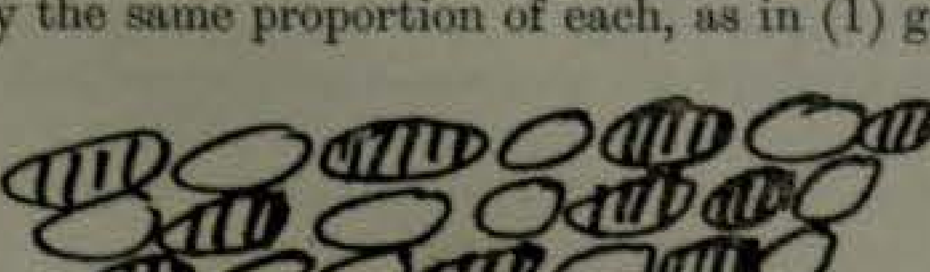
Let us deal with a single quality, for clearness of explanation, and suppose that in some particular plant or animal and in some particular structure, the hybrid between white and black forms was exactly intermediate, viz: grey—thenceforward for ever. Then a bit of the tinted structure under the microscope would have a form which might be drawn as in a diagram, as follows:—

White Form.

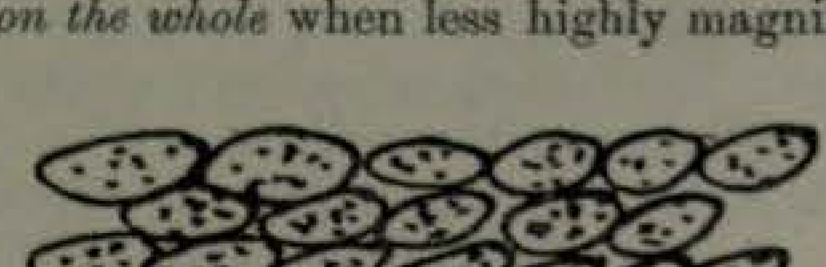
Black Form.



whereas in the hybrid it would be either that some cells were white and others black, and nearly the same proportion of each, as in (1) giving *on the whole* when less highly magnified a



(1)



(2)

uniform grey tint,—or else as in (2) in which *each cell* had a uniform grey tint.

In (1) we see that each cell had been an organic unit (quoad colour). In other words, the structural unit is identical with the organic unit.

In (2) the structural unit would not be an organic unit but it would be an organic *molecule*. It would have been due to the development, not of one gemmule but of a group of gemmules, in which the black and white species would, on statistical grounds, be equally numerous (as by the hypothesis, they were equipotent).

The larger the number of gemmules in each organic molecule, the more *uniform* will the tint of greyish be in the different units of structure. It has been an old idea of mine, not yet discarded and not yet worked out, that the number of units in each molecule may admit of being discovered by noting the relative number of cases of each grade of deviation from the mean greyness. If there were 2 gemmules only, each of which might be either white or black, then in a large number of cases one-quarter would always be quite white, one-quarter quite black, and one-half would be grey. If there were 3 molecules, we should have 4 grades of colour (1 quite white, 3 light grey, 3 dark grey, 1 quite black and so on according to the successive lines of "Pascal's triangle"). This way of looking at the matter would perhaps show (*a*) whether the number in each given species of molecule was constant, and (*b*), if so, what those numbers were[1]. Ever very faithfully yours, FRANCIS GALTON.

42, RUTLAND GATE, *Dec.* 22/75.

MY DEAR GEORGE, I have never supposed otherwise than that the gemmules *breed abundantly* all over the body, though I look upon them merely as *local parasites*, so to speak, that live, multiply and die in great multitudes in the places where they are lodged, though occasionally some of them may be detached and drifted along with the circulation, and so find their way to the sexual elements—as was explained in the second part of the paper.

It is by the *abundance* of all sorts of them, in every part of the body, that I accounted in my paper for the reproduction of mutilated parts, and other specified phenomena, adding: "It would much transcend my limits if I were to enter into these and kindred questions, but it is not necessary to do so, for it is sufficient to refer to Mr Darwin's work, where they are most fully and carefully discussed, and to consider while reading it whether the theory I have proposed could not, as I think it might be, substituted with advantage for Pangenesis."

[I have not the *Contemporary Review* by me and cannot give the page of the extract. My copy is merely a revise, paged from 1 onwards. It is in the 12th page of the revise.]

In this passage, I meant to include propagation by buds. You will see in the preceding page an allusion to the way in which the scattered alien germs "thrive and multiply."

Now for the application of all this: wherever in a plant developed out of a bud or seedling, (no matter which, for the 'stirp' is similar in both cases) the alien, localised germs happen to be congregated in sufficient number and varieties to form material for a fresh stirp, there will be a tendency to produce a bud. Structural conditions, such as those found at the parts where buds usually shoot, must of course be helpful in forwarding this tendency.

The advantage of my theory appears to be this:—

By Pangenesis, we should expect *all* animals, however highly organised, to throw out buds.

By my theory, I argue that where the animals are complex, the variety of germs concerned in the making of them must be proportionately great, and consequently the probability of a complete set of them being anywhere in existence, in the same immediate neighbourhood, is diminished. Hence, the lower the organisation, the more freely does it bud and the higher ones do not bud, which is in accordance with fact.

The budding, even of the highest animals in the embryonic stage, is intelligible by the joint action of 3 causes special to that period:

(1) The differentiation is less complete, and germs destined to be separated are then together.

(2) The embryo being small, the alien germs in separate structures are nearer than they become afterwards.

(3) The tissues are softer and afford less obstacle to the approach and aggregation of the germs under their mutual affinities.

[1] This letter shows how very closely Galton's thought at this time ran on Mendelian lines. The passage should be taken in conjunction with that on p. 402 of the memoir on Blood-relationship. See our pp. 170–4 and compare p. 84.

I hope I have answered fully enough, and much regret that I misunderstood the question, as put in your Father's letter, and have given you both unnecessary trouble. I am eager to receive criticisms—even adverse ones. Ever yours, FRANCIS GALTON.

About your Father's plants and the statistics of growth:—In cases where not only the *one* biggest of each sort, but the two or three biggest were measured, the uncertainty of the relative values of the moduli of variability of the two sorts would be materially diminished.

42 RUTLAND GATE, LONDON. *Jan.* 30th, 1876.

MY DEAR GEORGE, I was very glad to hear good news of you from Litchfield, who dined with us a few days back; (but not with your sister, I am sorry to say, as she was not then well). Strachey was nearly going yesterday to look after your map frame, possibly he did after all (he asked me to join him but I was engaged). He thought of taking it bodily away. Never did a thing hang so long in hand as this, but I am powerless to help. I can't understand it, as Strachey is so energetic in much that he undertakes and does it so well.

I got a letter from Glaisher a short time back about my "exponential ogive" whereof he much approves, name and all, and he gives me a compact expression for it, in terms of his "error function." I enclose a copy of part of what he says. In working out your Father's plant statistics, it occurred to me that it would be uncommonly convenient to calculate an exponential ogive table, which I did, and since receiving Glaisher's letter I sent it to him to see if he could get it properly recalculated for me directly from his formula. You see,—by knowing *any* two ordinates, you know the whole curve and can at once get the value of any other ordinates in it. I need not bother you with particulars about the table, further than that it gives ordinates from 1 to 50 in an ogive of 100 places, from 1 to 50 in an ogive of 1,000 places, ditto 10,000, 100,000 and a million. So that all goes into a page.

But I could not make out anything by its means about those data concerning your Father's self- and crop-fertilised plants in which only the biggest were measured. Their "run" was too irregular. I could get no two trustworthy ordinates. The ignorance of the number of plants in the row did not so much matter, because one knew it within limits and could find what the result would be for those limits; between which the real result must lie, and these were not extravagantly far apart.

We have had astonishing fogs in this part of London, that is going up from here to Hyde Park corner. I never saw one thicker than yesterday. Your friend Cookson, whom I met walking this morning, told me that in one place he could only see three flagstones off. I suppose you have glorious sunshine in Malta.

Tyndall's lecture about Bacteria was a great success and seems to have utterly smashed the adherents of Bastian. I conclude from the theory that the physiological reason of immortality in the next life is that there are no Bacteria in the pure air of heaven!—nothing to cause corruption.

I send reprints of my twins and theory of heredity (revised); one of the twin papers is new and so is the last paragraph about the *cuckoo* in the one that was in Fraser, which if you care to look at may interest you. Romanes' paper has been selected as the Croonian Lecture of the Royal Society for the year; a well-deserved honour. There seems to be an epidemic in the learned societies. Not only the Linnean, but now the Anthropological has got into such a state, and the respectable Athenaeum is all in a boggle about its future trustee to replace Lord Stanhope. Pray remember me very kindly to your brother and with my wife's best regards. Ever yours, FRANCIS GALTON.

GEORGE DARWIN, Esq.

2, BRYANSTON ST. [1877]

MY DEAR G[ALTON]. I have just bethought me, that I received a French essay a few months ago on the effects of the conscription on the height of the men of France and on their liability to various diseases which rendered them unfit for the army, due to the weaker men left at home propagating the race. He shows, I think rightly, that no one hitherto had considered the problem in the proper light. I forget author's name,—and where published.

Do you know this essay? and would you care to see it. I suppose that I could find it, but I think I have not yet catalogued it. It seemed to me a striking essay.

Ever yours sincerely, CH. DARWIN.

42 Rutland Gate, *Jan.* 12/77.

My dear Darwin, Thanks very many: When you come across the essay I should be very glad to see it. I know of a curious *Swiss* memoir, something apparently to the same effect, in which the author says that the Swiss yeomen are very apt to leave their homestead to a sickly son, knowing that he will not be called out on service, nor tempted to take service abroad in any form, but will stay at home and look after the property. Consequently the Swiss landed population tend to deteriorate.

I will try hard to put in practice your valuable hints about making my lecture as little unintelligible and dull as may be and have hopes of succeeding somewhat. George has *most kindly* taken infinite pains to the same end. Ever sincerely yours, Francis Galton.

Charles Darwin, Esq.

Down, *Feb.* 11th. [1877?]

My dear G. The enclosed is worth your looking at. It was sent me from N. Zealand as the writer thought we should not in England see Tickner's Life! I should think T. was to be trusted, and if so case very curious. It makes me believe statement about inherited handwriting. I shall never work on inheritance again. The extract need not be returned.

Ever yours, C. Darwin.

I do hope Mrs Galton is pretty well again.

42, Rutland Gate, *Feb.* 22/77.

My dear Darwin, By this book post I return Tickner's book with many thanks (after keeping it an unconscionable time, but I knew you did not want it and it was useful to refer to to me).

About the deaf and dumb men speaking with Castilian etc. accent, according to their teachers, I cannot help thinking it sufficiently explained by their imitation of the actions of the lips etc. of the teachers. I have tried in a looking-glass, and it seems that I mouth quite differently when I speak broad Scotch; again, last year, I was trying some experiments with Barlow's 'logograph' and the traces were greatly modified under different conditions of cadence.

Let me, before ending, heartily congratulate you on the German and Dutch testimonial of which I see a notice in to-day's *Times*, and take the opportunity of wishing you many, very many happy returns of the birthday. Ever sincerely yours, Francis Galton.

My wife is convalescent and already walks out a little.

42, Rutland Gate, *May* 24/78.

My dear Darwin, The enclosed "Composite Portraits" will perhaps interest you. The description of them is in this week's *Nature* (p. 97). You will see that I have there published the letter you kindly forwarded to me from Mr Austin of New Zealand (to whom I am now about to write a second time). Together with the villain's (absit omen!) I send 3 of our own family ancestors which I have had made, and for which you may care to find some place somewhere. The original portraits are in the possession of Reginald Darwin and are those of our uncle Sir Francis Darwin and of our great-grandfather and of our great-great-grandfather respectively[1] (as you will find written on their backs). These take the Darwin family back for $2\frac{1}{2}$ centuries. There seems to be a great deal of the Darwin type in William Darwin b. 1655.

I hear vague rumours of your wonderful investigations in the growth etc. of plants, and am eager for the time when they shall be published. Ever sincerely yours, Francis Galton.

Down, Beckenham, Kent. *March* 22/79.

My dear Galton, Dr Krause has published in Germany a little life of Dr Eras. Darwin, chiefly in relation to his scientific views; and to do our grandfather honour, my brother Eras. and myself intend to have it published in English. I intend to write a short preface to it, chiefly for the sake of contradicting the chief of Miss Seward's calumnies; and this I can do from having a letter from your aunts written at the time, and from my father's correspondence with Miss Seward. But I further intend to add a few remarks about our grandfather. Can you aid me with any information or documents?

[1] See Plates VI and LXII in Vol. I and remarks on p. 243.

I have one nice and curious letter to Miss Howard which I will publish. Also many letters to Josiah Wedgwood and to the famous Reimarus, but I doubt whether any of these will be worth publishing. Do you know whether there are any letters in the possession of any members of the family which might be worth publishing; and could you take the trouble to assist me by getting the loan or copies of them?

Several years ago I read the memoirs of your Aunt Mrs Schimmelpenninck and so far as I can remember many of the stories about Dr Darwin seemed very improbable. Did you ever hear your mother speak of this book, and can you authorise me to contradict any which are injurious to his good name? I am sure you will forgive me for troubling you on this head as we have a common interest in our grandfather's fame. Yours very sincerely, CHARLES DARWIN.

Saturday, 6, QUEEN ANNE STREET.

MY DEAR GALTON. If it would not bore you, can you come to luncheon here on Monday at 1 o'clock; as it will be my best chance of seeing you. I have been extremely sorry to hear that you have not been well of late and that you are soon going abroad.

Yours very sincerely, CH. DARWIN.

April 30 [1879]. DOWN, BECKENHAM.

Many thanks. The extract will come in capitally. You are vy. good to take so much trouble. Mrs Sch.[1] received all safe, and shall soon be returned. I much enjoyed my talks with you. C. D.

The following letter probably has reference to Elizabeth Collier's birth[2], and may possibly aid in the final solution of the difficulty as to her origin.

DOWN, BECKENHAM, KENT. *June* 8 [1879].

MY DEAR GALTON, Many thanks for your note. I have lately been staying with my sister, Caroline, and she says my memory is in error about the mysterious visitor. She believes his name was Brand, and that it was in the time of Colonel Pole; I cannot but doubt about the latter point. My sister feels pretty positive that the gentleman stayed at the house of a neighbour (name forgotten) and never visited Mrs Pole or Mrs Darwin, but sent her respectful and very friendly messages. Nevertheless she was never at ease till he had left the country. Thanks for all your help. I have fixed our photograph of Dr D. Ever yours, C. DARWIN.

P.S. If you should come across Dr Lauder Brunton, see if he has anything more to communicate about Dr D. for I shall soon go to press.

42, RUTLAND GATE, *Nov.* 12/79.

MY DEAR DARWIN, It was with the greatest pleasure that I received and read your biography of Dr Darwin.

What a marvel of condensation it is, and how firmly you lay hold of facts that had long been distorted and ram them home into their right places.

The biography seems to me quite a new order of writing, so scientifically accurate in its treatment. The many passages you quote are curiously modern in their conception and—(Excuse this horrid paper which folds the wrong way) simple in expression (considering his average style). I still can't quite appreciate the flaw in his mind which made it possible for him to write so very hypothetically for the most part, while at the same time his strictly scientific gifts were of so high an order. There seems to be an unexplained residuum, even after what you quote from him, about the value of hypotheses. I see you have mentioned me twice, very kindly—but too flatteringly for my deserts. How you are *down* upon Mrs Schimmelpenninck and Miss Seward[3]!

[1] Mrs Schimmelpenninck, Galton's aunt: see Vol. I, p. 54.

[2] See Vol. I, p. 21.

[3] I think Galton had a truer appreciation of Erasmus Darwin than possibly his cousin had,—a better historical perspective,—and with all their faults of exaggeration the ladies in question did give something of the 'atmosphere,' which Charles Darwin's portrait lacks. That portrait is wanting, in the full characterisation of a many-sided figure; we can only give reality to it by a study of Erasmus Darwin's own works, local gossip about him and the public opinion of his day—

I now, with fear and trembling lest you should finally vote me a continued bore, venture to enclose copies of some queries I have just had printed and am circulating, after having obtained by personal inquiries a good deal of very curious information on the points in question. I venture to ask you more particularly, because the 'visualising' faculty of Dr Darwin appears to have been remarkable and of a peculiar order and it is possible that yours, through inheritance, may also be similarly peculiar. It is perfectly marvellous how the faculty varies, and moreover some very able men intellectually do not possess it. They do their work *by words*, I am in correspondence with Max Müller about this, who is an *outré* "nominalist."

Very sincerely yours, FRANCIS GALTON.

Thanks for Bowditch (children's growth) which you kindly sent me.

Nov. 14th [1879].

DOWN, BECKENHAM, KENT.
RAILWAY STATION, ORPINGTON. S.E.R.

MY DEAR GALTON, I have answered the questions, as well as I could, but they are miserably answered, for I have never tried looking into my own mind. Unless others answer very much better than I can do, you will get no good from your queries. Do you not think that you ought to have age of the answers? I think so, because I can call up faces of many schoolboys, not seen for 60 years, with *much distinctness*, but now-a-days I may talk with a man for an hour, and see him several times consecutively, and after a month I am utterly unable to recollect what he is at all like. The picture is quite washed out.

I am *extremely* glad that you approve of the little life of our grandfather; for I have been repenting that I ever undertook it as work quite beyond my tether. The first set of proof-sheets was a good deal fuller, but I followed my family's advice and struck out much.

Ever yours very sincerely, CHARLES DARWIN.

QUESTIONS ON THE FACULTY OF VISUALISING[1].

For explanations see the other side of this paper.

The replies will be used for *statistical purposes only* and should be addressed to:—
FRANCIS GALTON, 42, RUTLAND GATE, LONDON.

Questions.	*Replies.*
1. Illumination.	Moderate, but my solitary breakfast was early and morning dark.
2. Definition.	Some objects quite defined, a slice of cold beef, some grapes and a pear, the state of my plate when I had finished and a few other objects are as distinct as if I had photos before me.
3. Completeness.	Very moderately so.
4. Colouring.	The objects above-named perfectly coloured.
5. Extent of field of view.	Rather small.
Different kinds of Imagery.	
6. Printed pages.	I cannot remember a single sentence, but I remember the place of the sentences and the kind of type.
7. Furniture.	I have never attended to it.
8. Persons.	I remember the faces of persons formerly well-known vividly, and can make them do anything I like.
9. Scenery.	Remembrance vivid and distinct and gives me pleasure.
10. Geography.	No.
11. Military Movements.	No.
12. Mechanism.	Never tried.

and I would add, an examination of the innumerable paintings of him from various aspects. He was in no sense a bloodless man, but clearly a man of many crotchets and peculiarities of temperament. I have had the privilege of examining a considerable number of Erasmus Darwin's letters and papers, and feel that his true characterisation remains to be drawn. The final portrait will not be that of Schimmelpenninck, but again not that of Charles Darwin. Meanwhile I find my imagination persists in coupling the supposed extremes: Samuel Johnson and Erasmus Darwin!

[1] For the nature and occasion of these questions the reader must consult Chapter XII.

Questions.	*Replies.*
13. Geometry.	I do not think I have any power of the kind.
14. Numerals.	When I think of any number, printed figures rise before my mind; I can't remember for an hour 4 consecutive figures.
15. Card-playing.	Have not played for many years, but I am sure should not remember.
16. Chess.	Never played.
Other senses.	
17. Tones of voices.	Recollection indistinct, not comparable with vision.
18. Music.	Extremely hazy.
19. Smells.	No power of vivid recollection, yet sometimes call up associated ideas.
20. Tastes.	No vivid power of recalling.

Signature of Sender and Address. CHARLES DARWIN, Down, Beckenham. (Born *Feb.* 12th, 1809.)

April 7, 1880. DOWN, BECKENHAM, KENT.

MY DEAR GALTON, The enclosed letter and circular may perhaps interest you, as it relates to a queer subject. You will perhaps say: hang his impudence. But seriously the letter might possibly be worth taking some day to the Anthropolog. Inst. for the chance of some one caring about it. I have written to Mr Faulds telling him I could give no help, but had forwarded the letter to you on the chance of its interesting you.

My dear Galton, Yours very sincerely, CH. DARWIN.

P.S. The more I think of your visualising inquiries, the more interesting they seem to me.

42, RUTLAND GATE, *April* 8/80.

MY DEAR DARWIN, I will take Faulds' letter to the Anthro. and see what can be done; indeed, I myself got several thumb impressions a couple of years ago, having heard of the Chinese plan with criminals, but failed, perhaps from want of sufficiently minute observation, to make out any *large* number of differences. It would I think be feasible in one or two public schools where the system is established of annually taking heights, weights etc., also to take thumb marks, by which one would in time learn if the markings were as persistent as is said. Anyhow I will do what I can to help Mr Faulds in getting these sort of facts and in having an extract from his letter printed. I am so glad that my 'visualising' inquiries seem interesting to you. I get letters from all directions and the metaphysicians and mad-doctors have been very helpful.

Very sincerely yours, FRANCIS GALTON.

Our united kindest remembrances to you all.

Galton communicated Dr Faulds' letter to the Anthropological Institute; the original is now before me, and it is inscribed, "Addressed to Charles Darwin, Esq. and communicated by F. Galton." Apparently that body did not publish it as they certainly ought to have done. Many years afterwards it was discovered in their archives. Its non-publication, however, was not of such importance as it might have been, for on Oct. 28, 1880, a very full letter from Dr Faulds appeared in *Nature* covering the same ground. To this matter we shall return later.

42, RUTLAND GATE, *July* 5/80.

MY DEAR DARWIN, Best thanks for sending me *Revue Scientifique* with Vogt's curious paper, which I return with many thanks. The passage you marked for me makes me sure that he would give help of the kind I now want and I will write to him. (De Candolle and another Genevese, Achard by name, have already kindly done much.)

I send an advance copy of those "Visualised Numerals" of mine, not to trouble you to re-read what you know the pith of already, but because of the illustrations at the end and also for the chance of your caring to see there the confirmation from other sources (I find that the editor has cut out all Bidder's remarks on this point—which I much regret) of what Vogt says about the left hand executing with facility *in reverse* what is done by the right hand. I made

Bidder scribble flourishes with pencils held in both hands simultaneously and the reflexion of the one scrawl in a mirror was just like the other picture seen directly.

I have just published in *Mind* something more about mental imagery, and when I get my reprints I will send one in case you care to glance at it.

Enclosed is a reference that might be put among your Dr Erasmus Darwin papers, in the event of having again to revise the 'Life'. I had not a notion, until I began to hunt up for the reference, how much he had considered the subject of mental imagery, or the very striking experiment in Part I, Section XVIII. 6 (which in my edition of 1801 is in Vol. I, p. 291), which shows that he himself possessed the faculty in a very marked manner.

We came back after a very successful Vichy visit[1]; my Wife improved at once on getting there, but for my part I have since been unlucky, and am only just out of bed after a week's illness of the same kind as Litchfield's long affair—this partly accounts for bad handwriting. With kindest remembrances to you all from us both and from my sister Emma who is now with us for a few days, Ever sincerely yours, FRANCIS GALTON.

42, RUTLAND GATE, Monday morning, *March* 7/81.

DEAR DARWIN, About Worms[2]:—I have waited for an opportunity of verifying what I told you about the effect of heavy soaking rain, *when it suddenly succeeds moderate weather*, in driving the worms from their holes to the gravel walks, where they crawl for long distances in tortuous courses, and where they die. It has been very frequently observed by me in Hyde Park, and this morning I have again witnessed it in a sufficiently well-marked degree to be worth recording.

It rained heavily on Saturday night last, after a spell of moderate weather. Unluckily I was not in the Park on Sunday till near 1 h. by which hour the birds had had abundant time to pick up the worms. Still, dead worms were about and their tracks were most numerous. On Sunday (last night) it again rained heavily and I was in the Park at 10 h. The tracks were not nearly so numerous as they had been on Sunday morning, but more dead worms were about. I began counting, and found they averaged 1 to every $2\frac{1}{2}$ paces (in length) of the walk, the walk being 4 paces and a trifle more in width.

Walking on, I came to a place where the grass was swamped with rain-water on either side of the raised gravel path, for a distance of 16 paces. In those 16 pace-lengths I counted 45 dead worms.

On not a few previous occasions when I have been out before breakfast, I have *under the conditions already mentioned* seen the whole of the walks strewn with worms almost as thickly as were the 16 pace-lengths just described. The worms are usually very large. I rarely notice dead worms on the paths at other times. Ever sincerely yours, FRANCIS GALTON.

I shall be very curious to learn about the effects of the red light as against those of a strongly actinic colour.

DOWN, BECKENHAM, KENT. *March* 8th [1881].

MY DEAR GALTON, Very many thanks for your note. I have been observing the *innumerable* tracks on my walks for several months, and they occur (or can be seen) only after heavy rain. As I know that worms which are going to die (generally from the parasitic larva of a fly) always come out of their burrows, I have looked out during these months, and have usually found in the morning only from 1 to 3 or 4 along the whole length of my walks. On the other

[1] Both the Galtons enjoyed Vichy and visited it yearly from 1878 to 1881.

[2] Miss Margaret Shaen tells me that she first met Francis Galton at Down, when Darwin was studying earthworms. "They had much talk together on the subject, Mr Galton getting most eager in trying to picture to himself exactly how the worms drew things into their holes to close them up. Mr Darwin was then experimenting with little bits of paper like this , laying them near the worm holes, and finding them drawn down by the point. I remember Mr Galton trying to do the like with his pocket pencil, i.e. to draw the paper down inside his pencil case. I am pretty sure he was keen to test the worms perception of angles by altering the sides of the triangle, getting them more equal to see if the worms would still detect the smallest of the angles and draw that one in. I don't know if Mr Darwin did try any such experiments." See *The Formation of Vegetable Mould through the Action of Earthworms*, pp. 14, 85–95.

hand I remember having in former years seen scores or hundreds of dead worms after heavy rain. I cannot possibly believe that worms are drowned in the course of even 3 or 4 days immersion; and I am inclined to conclude that the death of sickly (perhaps with parasites) worms is thus hastened. I will add a few words to what I have said about their tracks, after stating that I found only a very few dead ones. Occasionally worms suffer from epidemics (of what nature I know not) and die by the million on the surface of the ground.

Your ruby paper answers capitally, but I suspect that it is only by dimming the light, and I know not how to illuminate worms by the same intensity of light, and yet of a colour which permits the actinic rays to pass. I have tried drawing the angle of damp paper through a small cylindrical hole, as you suggested, and I can discover no source of error. Nevertheless I am becoming more doubtful about the intelligence of worms. The worst job is that they will do their work in a slovenly manner when kept in pots, and I am beyond means perplexed to judge how far such observations are trustworthy.

Ever my dear Galton, Yours most sincerely, CH. DARWIN.

42, RUTLAND GATE, *Oct.* 9/81.

MY DEAR DARWIN, Pray accept my best thanks for the worm book, which I have read, as I read all your works, with the greatest interest and instruction. I wish the worms were not such disagreeable creatures to handle and keep by one, otherwise they would become popular pets, owing to your book, and many persons would try and make out more concerning their strange intelligence. Once again very best thanks and believe me,

Ever sincerely yours, FRANCIS GALTON.

DOWN, BECKENHAM. *March* 22nd [1882]

MY DEAR GALTON,—I have thought that you might possibly like to read enclosed which has interested me somewhat, and which you can burn.—I have been on the sick-list, but am improving. Ever my dear Galton, yours very sincerely, CH. DARWIN.

Such, a month before his death, was the last letter of Darwin to Galton.

42, RUTLAND GATE, *March* 23/82.

MY DEAR DARWIN, Best thanks for the American article, which is certainly suggestive, where paradoxical. It is delightful to find that virtue mainly resides in large and business-like families, fond of science and of arithmetic! It eminently hits off the character of your own family and in some fainter degree of my brothers and sisters, and of all Quakerism.

I hope you are quite well again. With our kindest remembrances,

Ever yours, FRANCIS GALTON.

DOWN, Thursday, 20th *April* 1882.

DEAR MR GALTON, My mother asks me to write to you and tell you of my dear father's death. He died yesterday afternoon about 4. He was taken ill in the middle of Tuesday night and remained in a great state of faintness, suffering terribly from deep nausea and a most distressing sense of weakness. He was conscious till within a ¼ hr. of his death. He gradually became more and more pallorless and at last became suddenly worse. I cannot help saying how often I have heard him speak with affection of you[1], Yours affectionately, FRANCIS DARWIN.

I forgot to say what I especially meant to, that my mother bears it wonderfully, she is very quiet and calm.

[1] Mrs Litchfield, Darwin's daughter, tells me that her Father had a great admiration for Galton's acuteness and she has also a memory of her Father saying what fun Galton was. Miss Elizabeth Darwin recalls a visit of Galton when they were all children, and his talking of mesmerising them, but it was not attempted in case it should frighten them. After Miss Henrietta Darwin's marriage, Galton told her he was sure he could mesmerise her, but that it would not be good for her. In his *Memories*, p. 80, Galton tells us that he learnt the art in Austria during his undergraduate days, and mesmerised some 80 persons, but "it is an unwholesome procedure, and I have never attempted it since." By experiment, however, he demonstrated that the exercise of will power by the operator is unnecessary, it is a purely subjective operation.

The following letter to his sister, Miss Emma Galton, is not only of historical interest, but portrays the intense reverence Galton felt for his cousin:

42, RUTLAND GATE, *April* 22/82.

DEAREST EMMA, I feel at times quite sickened at the loss of Charles Darwin. I owed more to him than to any man living or dead; and I never entered his presence without feeling as a man in the presence of a beloved sovereign. He was so wholly free of petty faults, so royally minded, so helpful and sympathetic. It is a rare privilege to have known such a man, who stands head and shoulders above his contemporaries in the science of observation. When the news came on Thursday I went to the Royal Society which met that day and arranged that a request should be telegraphed to the family by the President in the name of the Royal Society asking if they would consent to an interment in Westminster Abbey, to which I have some reason to believe the Dean (who is abroad) would in no way object. If so the funeral would be attended by deputations from all the learned societies. I wrote to Lord Aberdeen, who fully consents on behalf of the Geographical, and who has written accordingly. I was absolutely engaged all yesterday (till after dinner hour even), and could not learn progress. I hope the first wishes of the family may yield and that Charles Darwin may be laid by the side of Newton as the two greatest Englishmen of Science. I had a brief letter from Frank Darwin on Thursday with nothing however in it that was not in the next day newspapers. It was evidently angina. The world seems so blank to me now Charles Darwin is gone. I reverenced and loved him thoroughly. Ever affectionately, FRANCIS GALTON.

On April 26th Darwin was buried in Westminster Abbey[1]; the funeral card runs, "Wednesday April 26th 1882, at 12 o'clock precisely. Admit the Bearer at eleven o'clock to the Jerusalem Chamber." Galton walked in the procession[2], and on the same evening wrote to his sister, Miss Emma Galton, as follows:

42, RUTLAND GATE, *April* 26/82.

DEAREST EMMA, The great ceremony in the Abbey is over. The whole "family" of scientific men were there, a great and imposing gathering. No ostentation but great from its intrinsic worth. The Duke of Argyll and Wallace were the two end pall-bearers, Huxley and Canon Farrar were together, thus all shades of opinion and station were merged. It was touching to see the blind Postmaster-General [Fawcett] led past the coffin. Several past Cabinet Ministers were also present. They had asked me to find out Canon Farrar's views, wishing to have some prominent ecclesiastic, especially one connected with Westminster Abbey, as a

[1] It is noteworthy, perhaps, that Galton on Dec. 27, 1881 had sent a note to the *Pall Mall Gazette* urging the stringent enforcement of rigid sanitary conditions of burial in the case of interments in the Abbey.

[2] Galton was also at Lord Tennyson's funeral and these ceremonies in the Abbey impressed him with the existence of a great failure on such occasions. The solemn procession up the nave to the chancel was not visible to the bulk of the congregation in the transepts. Galton in a letter to *The Times* May 25, 1898 writes: "My own seat was in a good position, but I saw nothing of the distinguished persons who formed the procession except the foreheads of two of the pall-bearers who were of exceptional stature, whose well-known names I need not specify. All the others were sunk wholly out of sight in a trough of crowded humanity. It is a sad waste of effort and opportunity to so mal-organise a great spectacle that its most imposing feature proves to be invisible to the great majority of those who come to see it." Galton's solution was a slightly raised causeway from choir to chancel. It may be objected that we go to honour the dead and not to see a spectacle. But this is not wholly true; it is the spectacle which impresses itself on the multitude and makes them realise, perhaps for the first time, the national value of the great dead. They go to hear and they go to see that their memories and their imaginations may be indelibly impressed. A solemn national funeral repercusses in wider circles than are ever reached by the acts or words of a national hero during life. It sets even the inert inquiring.

pall-bearer and he (Farrar) entered most cordially into the wishes of the family. He offered to act as a pall-bearer either in or without his robes, as desired. He is to preach next Sunday on Darwin at the Abbey and tells me that he wishes to make such amends as he can for the reception formerly given by the Church party to Darwin's works, and we have talked over some points for the sermon.

Reginald Darwin was there and Emma Wilmot and Cameron Galton and H. Bristowe. The family party was so large that most of the ladies (including Louisa) and about half of the men were placed in the seats by the altar rails else the procession would have been too long. H. Bristowe and I walked together. Louisa will write more details. The newspapers will give a much fuller account. The service was not particularly touching; it never is in the Abbey; it is more like the ceremonial of *giving a University Degree*.

I got a card for Erasmus to attend with the family and telegraphed to him to Loxton thinking it *possible* that owing to his admiration of Darwin's works he might like to come, but he declined.

Mrs Darwin is very composed now.

I feel this is a worthless and heartless sounding letter, but as I said the feeling promoted by the ceremony is *not* a solemn one but rather the sense of a national honour and glory.

Ever affectly, FRANCIS GALTON.

The words of the anthem, taken from Proverbs iii. 13, 15, 16 and 17 "Happy is the man that findeth wisdom, and the man that getteth understanding.... Her ways are ways of pleasantness, and all her paths are peace," were aptly chosen, as also the anthem of Handel at the grave-side: "His body is buried in peace, but his name liveth evermore." The ceremony did not strongly appeal, however, to Darwin's Quaker-minded cousin; for him the restful burial in the little churchyard of Claverdon Leys thirty years later seemed indeed appropriate. The next day, April 27th, Darwin's daughter, Miss Elizabeth Darwin, wrote to Galton's sister Emma:

"We have had a great deal of sympathy and it is soothing to feel how many appreciated our dear Father's goodness. He always had a very real affection for your brother and took great pleasure in his company."

On the same day appeared a letter by Galton in the *Pall Mall Gazette*:

The Late Mr Darwin: A suggestion

SIR,—Next Sunday numerous congregations will expect some honourable recognition of the character and works of Charles Darwin. Let me suggest to clergymen generally that they should substitute on that day the 'Benedicite' for the more usual 'Te Deum,' as many of its noble verses are pointedly appropriate to what they would probably wish to say afterwards from the pulpit:—

O all ye Works of the Lord, bless ye the Lord : praise him, and magnify him for ever.
O all ye Green Things upon the Earth, bless ye the Lord : praise him, and magnify him for ever.
O ye Whales, and all that move in the Waters, bless ye the Lord : praise him, and magnify him for ever.
O all ye Fowls of the Air, bless ye the Lord : praise him, and magnify him for ever.
O all ye Beasts, and Cattle, bless ye the Lord : praise him, and magnify him for ever.
O ye holy and humble Men of heart, bless ye the Lord : praise him, and magnify him for ever.

In pursuance of the same idea, let me add that a stained glass window in Westminster Abbey, symbolising these and other verses of the same canticle in its several panels, would be a beautiful monument to the memory of Charles Darwin, and quite in harmony with the surroundings. It would afford a desired opportunity for other countries to share in the erection of a memorial without merging their several contributions indistinguishably into one, as each country might contribute a separate panel. I suggest this window in addition to, and not in substitution of, any bust or tablet that may hereafter be decided upon, and towards all of which I, for one, am prepared to subscribe liberally. I am, Sir, Your obedient servant, F. G.

It was, perhaps, too generous an idea to expect in 1882 that an 'evolution' window could, even in Westminster Abbey, replace the old 'creation' window based upon its neolithic myth. But the time may yet come when the national mausoleum shall contain not only the ashes of the nation's great dead, but some appropriate witness to those living embers of the mind which entitled them to their final resting-place. Galton strongly believed in and generously supported all projects of perpetuating the memory of the worthy dead. It was exhibited not only in the case of Darwin, but in several other instances. Thus in the monument he put up to Erasmus Darwin in Lichfield Cathedral[1], in his support of the Speke memorial and his desire to see it extended to embrace other African pioneers (see our p. 25 *ftn.*), and again in the substantial aid he gave to the Oxford Weldon memorial. I have no doubt fuller investigation would lead to the discovery of other instances[2].

But for Darwin, Galton's affection and reverence were unlimited. Within three weeks of the former's death he wrote to Darwin's son George as follows:

42, Rutland Gate, *May* 16th, 1882.

My dear George, You may be glad to hear that the memorial to your father was fairly started this afternoon and very shortly the letters to foreigners will be sent and notices in the papers will appear. A Sub-Cmte. of the executive Cmte. has only now to fix a few details. I was very sorry to have missed you when you called, as there is much I should like to have heard about you all. I am very glad that your Mother bears up so well.

I wanted too, to speak to you (as I have to Spottiswoode) about getting together available illustrations and memorial scraps of all kinds for a book of mementos for the Royal Society (like those of Priestley—do you know them?). There ought to be a picture of the 'Beagle' if one is procurable and copies (small) of *all* the pictures and photographs. You are no doubt collecting all available information of his early life before his contemporaries and seniors shall have passed away. Every month is precious. I do wish somebody had done this many years ago for Dr Erasmus Darwin. If omitted, this want is soon irrevocable. When you are next in Town pray come to us. Ever yours, Francis Galton.

Talking once to the husband of one of the greatest of Victorian women, about the loss of a great friend—to whose learning and scholarship I owe whatever love I may possess for accurate investigation—he remarked:

"It is difficult to measure what the mental development of an individual loses and what it gains by the death of a friend of dominant personality."

The words seemed to me then harsh and unsympathetic, but I have learnt with the years the element of truth in the experience expressed by them. That truth is not wholly appropriate to the friendship of Galton with Darwin; the latter was only thirteen years Galton's senior, but those years, and Galton's unlimited reverence for intellectual power did, as in the

[1] See Note at the end of this Chapter.

[2] One other instance I can indeed refer to from letters in my possession. He was the prime mover in the scheme for obtaining a portrait of Sir Joseph Hooker. There are numerous letters to Galton approving and enclosing subscriptions, and the letter of Hooker to Galton is worthy of being preserved elsewhere than in an autograph book where I found it:

Royal Gardens, Kew, *May* 15/80.

My dear Galton, Your kind letter announces a most unexpected honour, and a crowning one. I only wish I could feel that I was worthy of it. I am quite at Mr Collier's disposal and very pleased to find that he is the selected artist. Very sincerely Yours, Jos. D. Hooker.

PLATE XVIII

42. Rutland Gate
London S.W.
Dec 9. 1859.

My dear Darwin

Pray let me add a word of congratulation on the completion of your wonderful volume, — to those which I am sure you will have received from every side. I have laid it down in the full enjoyment of a feeling that one rarely experiences after boyish days, of having been initiated into an entirely new province of knowledge which, nevertheless, connects itself with other things in a thousand ways.

I hear you are engaged on a second Edition. There is a trivial error in p. 68. about Rhinoceroses, which I thought I might as well point out. & have taken advantage of the same opportunity to scrawl down ½ a dozen other notes, which may or may not be worthless to you.

With our united kind regards to yourself & Mrs Darwin

Believe me very sincerely yours

Francis Galton

PLATE XIX

The first study at Down, the room in which *The Origin of Species* was written. Photographed in his Father's lifetime by Major Leonard Darwin.

case of Pangenesis, unconsciously shackle the free development of Galton's own ideas. Galton would never have admitted such an aspect of the friendship. To him Darwin was the man who freed him from superstition and directed his life-work into new channels[1]; but nevertheless the onlooker may note, what individual actors cannot apprehend, for he like the dramatist sees the play as a whole. Be this as it may, undoubtedly the year 1882 marked an epoch in Galton's career[2]. As Mrs Galton records, Darwin's death "cast

[1] Galton did not only acknowledge this in the memorable letter to Darwin himself in 1869 (see Vol. I, Plate II) but most gracefully in the speech he made at the Royal Society dinner after receiving the gold medal in 1886. I will cite a portion of it:

"The ethnological aspects of geography now [1860] began to attract me more than the physical ones. It was about this time that the fact dawned on scientific men that the key to the origin of society among civilised nations and to many of their unexplained customs was to be found in the habits of contemporary barbarians. I can assure you, as a specialist in heredity, that I am not speaking without reason when I say that qualities which I seem to have inherited through two of my grandparents gradually yielded precedence to those that I certainly inherited from the other two. Recollect, please, that this medal is awarded to me for 'statistical inquiries into biological phenomena.' I can account fully both for the statistics and the biology. You must please allow me the pleasure of dissecting myself. On my father's side, I know of many most striking, some truly comic, instances of statistical proclivity. I have in my possession many pounds weight of ruled memorandum books severally allotted to almost every conceivable household purpose, which belonged to an aged female relative who died years ago. I also reckon at least five other remarkable instances of a love of tabulation within two degrees of kinship of myself. Again, as regards biology, I am sure there is a similarity between the form of the bent of my mind and that of my mother's father, Dr Erasmus Darwin. The resemblance chiefly lies in a strong disposition to generalise upon every-day matters that commonly pass unnoticed. I have myself attempted some of the very inquiries to which he had drawn attention, in complete unconsciousness that he had done so. It was owing to this hereditary bent of mind that I was well prepared to assimilate the theories of Charles Darwin when they first appeared in his 'Origin of Species.' Few can have been more profoundly influenced than I was by his publications. They enlarged the horizon of my ideas. I drew from them the breath of a fuller scientific life, and I owe more of my later scientific impulses to the influences of Charles Darwin than I can easily express. I rarely approached his genial presence without an almost overwhelming sense of devotion and reverence, and I valued his encouragement and approbation more, perhaps, than that of the whole world besides. This is the simple outline of my scientific history." (*The Times*, Dec. 1, 1886.)

[2] Galton's last tribute probably to Darwin was paid at the Darwin-Wallace celebration of the Linnean Society on July 1st, 1908. The present writer saw him to and from the meeting and knew that he was feeling unwell; his few words were a great effort. After thanking the President for his kind remarks, Galton turned to the main point on which he felt our generation's gratitude to Darwin should be keenest—the freedom Darwin gave us from theological bondage: "You have listened to-day to many speakers and I have little new to say, little indeed that would not be a repetition, but I may add that this occasion has called forth vividly my recollection of the feelings of gratitude that I had towards the originators of the then new doctrine which burst the enthraldom of the intellect which the advocates of the argument from design had woven round us. It gave a sense of freedom to all the people who were thinking of these matters, and that sense of freedom was very real and very vivid at the time. If a future Auguste Comte arises who makes a calendar in which the days are devoted to the memory of those who have been the beneficent intellects of mankind, I feel sure that this day, the 1st of July, will not be the least brilliant." *The Darwin-Wallace Celebration...by the Linnean Society of London*, 1908, pp. 25–6.

It is characteristic of Francis Galton that it was not the enormous influence of Darwin on the biological sciences that he thought of in the first place, but the emancipation of the human intellect from its centuries-old neolithic traditions—the common gain of the average man, only indirectly affected by the spread of scientific knowledge—that he wished to see emphasised.

a deep gloom" over her husband; but it was followed by his most productive decade. Interests in psychological and in statistical investigations had originated well before this date, but as our following chapters will show they now became predominant and displaced to a large extent the more biological aspect of the inquiries which we have associated in the second half of this chapter with Darwin. The philosopher of Down was no longer there either to check error or to restrain imagination. The miniature of Darwin remained on the writing-table, but rather as a symbol of method, than to suggest the warning voice of the revered master:

Ignoramus, in hoc signo laboremus!

NOTE I. ON THE MONUMENT TO ERASMUS DARWIN ERECTED BY FRANCIS GALTON IN LICHFIELD CATHEDRAL, 1886.

About the time when the question of a monument to Charles Darwin in Westminster Abbey was being raised, Galton determined to commemorate the grandfather of both in Lichfield Cathedral, and obtained the permission of the Dean and Chapter for the erection of a memorial medallion. This was executed by E. Onslow Ford. See our Plate XIX. The work of Krause and Charles Darwin on the life and ideas of Erasmus Darwin had drawn the attention of Galton again to his grandfather, and he was more than inclined to revise the opinion he had expressed to de Candolle in 1882 (see our Vol. I, p. 13). Perhaps what weighed much with Galton were the lines from the preface to the *Zoonomia*.

The great Creator of all things has infinitely diversified the works of his hands, but has at the same time stamped a certain similitude on the features of nature that demonstrate to us that the whole is one family of one parent.

There is not a doubt, I think, that Erasmus Darwin anticipated Lamarck in propounding a doctrine of evolution based upon the inheritance of acquired characters, and that he recognised a unity of origin for all forms of life. It was with this impression strong upon him that Galton made his first draft for the Lichfield inscription. It ran as follows:

In memory of Erasmus Darwin, M.D., F.R.S., Physician, Philosopher, and Poet; Author of *Zoonomia*, *Botanic Garden*, &c.; Earliest propounder of the Theory greatly elaborated by his more distinguished grandson, Charles Darwin, which ascribes to the operations of animals and plants, prompted in the first instance by their individual needs, the secondary and higher function of modifying through inheritance by various indirect and slow though certain methods, the forms and instincts of their respective races, in increasing adaptation to the habits of each and to their physical surroundings and thus of furthering the development of organic nature as a whole.

This inscription certainly accords with Erasmus Darwin's view, if it does not lay as much stress on the element of 'will' as Erasmus did. It was, perhaps, not incompatible with Charles Darwin's opinion that at least some acquired characters are inherited. Galton sent it to Huxley for criticism and Huxley replied with the following characteristic note: