of getting fishing. At Matlock, "where the fishing is free there being no fish!" he made up for the want of his favourite pastime by attacking the theory of the vertebrate skeleton. His dissatisfaction with the Archetype theory dated from 1851, when he attended Owen's lectures. A lecture by Professor Huxley, showing the inadequacy of Owen's doctrine in so far as it concerns the skull, encouraged him to express his disbelief in the theory as a whole. "I am busy," says a letter (9 July) "with the onslaught on Owen. I find on reading, the 'Archetype and Homologies' is terrible bosh—far worse than I had thought. I shall make a tremendous smash of it, and lay the foundations of a true theory on its ruins." The month after the article appeared he writes to his father: "Huxley tells me that the article on Owen has created a sensation. He has had many questions put to him respecting the authorship—being himself suspected by some. The general opinion was that it was a settler."

On return to town in October he set about writing a promised article on "The Laws of Organic Forms." In view of another article he mentions that he was to "dine with Mr. Cross, of the great firm of Dennistoun, Cross and Co. He is to give me some information bearing on the morals of trade." An article on "Physical Training," declined for the *Quarterly*, had been accepted for the *British Quarterly*. He had been distributing a few volumes of the *Essays*. Two of the letters of acknowledgment are worth quoting, Mr. Darwin's being one to which Spencer attached great importance.

FROM CHARLES DARWIN.

25 *November* [1858].

Your remarks on the general argument of the so-called Development Theory seem to me admirable. I am at present preparing an abstract of a larger work on the changes of species; but I treat the subject simply as a naturalist, and not from a general point of view; otherwise, in my opinion, your argument could not have been improved on, and might have been quoted by me with great advantage.[1]

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[1] *Life and Letters of Charles Darwin*, ii., 141. *Autobiography*, ii., 27.

FROM CHARLES DARWIN.

2 [*February*, 1860.]

From your letter I infer that you have not received a copy of my book, which I am very sorry for. I told Mr. Murray to send you one, *amongst the first distributed* in November. . . . I have now written a preface for the foreign editions and for any future English edition (should there be one), in which I give a very brief sketch [of the progress of opinion], and have with much pleasure alluded to your excellent essay on Development in your general Essays.

TO EDWARD LOTT.

10 *February*, 1860.

Have you got a copy of the "Theory of Population," and if so, can you find it? I have no copy left save one that is cut into parts for future use.

I am just reading Darwin's book (a copy of which has been searching for me since November and has only just come to hand) and want to send him the "Population" to show how thoroughly his argument harmonizes with that which I have used at the close of that essay.

I shall shortly be sending you something which will surprise you.

At the foot of a copy of this letter Spencer has noted: "This makes it clear that the programme of the 'System of Philosophy,' *in its finished form* was drawn up before I read the *Origin of Species*." Along with the pamphlet on "Population," he sent Mr. Darwin a note, acknowledging the *Origin of Species*, and apparently remarking on it.

FROM CHARLES DARWIN.

23 [*February*, 1860].

I write one line to thank you much for your note. Of my numerous (private) critics, you are almost the only one who has put the philosophy of the argument, as it seems to me, in a fair way—namely, as an hypothesis (with some innate probability, as it seems to me) which explains several groups of facts.[1]

You put the case of selection in your pamphlet on Population in a very striking and clear manner.

The issue of the programme seemed a favourable opportunity for carrying out the intention, expressed some years

[1] See also *Life and Letters of Charles Darwin*, ii., 290.

to see that they were worth preserving. I find they now furnish me with far more beautiful cases than I had before perceived. While I was travelling up I hit upon the idea needful for the complete interpretation of plant circulation. I have the whole thing now as satisfactorily demonstrable as can well be imagined.

15 *January.*—Since I wrote last I have been showing my preparations to Hooker, Busk and Huxley. The results turn out to be new. These structures in certain classes of leaves were unknown to them all; and they could find no descriptions of them, and they recognize their significance. It turns out, too, that though there have been experiments on the absorption of dyes, they have been limited to the cases of stems, in which the results are, when taken by themselves, confusing and indeed misleading. They were all of them taken aback by the results I have shown them; which are so completely at variance with the doctrines that have been of late years current; and they have nothing to say against the hypothesis based on these facts which I have propounded to them. It is proposed that I should put the facts and arguments in the shape of a paper for the Linnæan Society; and it is probable that I shall do so, eventually including it in the appendix to the *Biology*.

24 *January.*—I am half through, or more, with my paper for the "Linnæan." The argument works out very satisfactorily.

30 *January.*—I am using as a dye, infusion of logwood, which I find answers in some respects much better than magenta. I shall be able, I think, very completely to demonstrate my proposition. I am getting much more skilled in making preparations, and have hit on a way of doing them with readiness and efficiency. On Sunday I discovered some spiral and annular structures of marvellous size—four or five times the diameter of any that I have previously found, or seen figured. They exist in the aberrant leaf of an aberrant plant, which I daresay has never been before examined.

26 *February.*—I should have written before, but I have been so very busy preparing specimens, making drawings, and revising my paper for the Linnæan Society. It is announced for Thursday next.

The paper was read on 1st March. Further examinations and experiments in revising it for inclusion in the Transactions of the Society occupied him during the month. After a visit to his parents at Easter he set to work on the fourth number of vol. ii. of the *Biology*, which was issued in June. Of this number Mr. Darwin wrote to Dr. Hooker :—

"It is wonderfully clever and I daresay mostly true. . . . If he had trained himself to observe more, even at the expense, by the law of balancement, of some loss of thinking power, he would have been a wonderful man."[1] On his return to London in September, he took up his abode at 37, Queen's Gardens, Bayswater, which was to be his home for many years. Here he set to work, amid many interruptions, to complete the volume, three numbers of which still remained to be brought out. Towards the close of February, 1867, he was able to tell Dr. Youmans : "I am in the middle of the last chapter but one of the *Biology*; and make sure of getting the volume out before the end of March, if no unforeseen hindrance occurs. It will be a cause of great rejoicing with me to have got through so trying a part of my undertaking."

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[1] *Life and Letters of C. Darwin*, iii., 55.

of it. Many such will doubtless fight against them; and out of the fighting there is sure to come further progress.

I very much wish that this book of yours had been issued somewhat earlier, for it would have led me to introduce some needful explanations into the first volume of the *Principles of Psychology*, lately published. One of these explanations I may name. Though I have endeavoured to show that instinct is compound reflex action, yet I do not intend thereby to negative the belief that instincts of some kinds may arise at all stages of evolution by the selection of advantageous variations. I believe that some instincts do thus arise; and especially those which are operative in sexual choice.

The Descent of Man indirectly led to another "parenthetical" bit of work, foreshadowed in the following letter:

To Charles Darwin.

2 *May*, 1871.

It has occurred to me that it may be worth while to write a few lines to the *Contemporary Review à propos* of Sir A. Grant's article.[1] I think of drawing his attention to the *Principles of Psychology* as containing proofs both analytic and synthetic, that the division between Reason and lower forms of Intelligence, which he thinks so unquestionable, does not exist.

Before deciding on this course, however, I think it is proper to enquire whether you propose to say anything on the matter; seeing that the attack is ostensibly directed against you.

Apparently Mr. Darwin was not induced to take the matter up. Hence the short paper on "Mental Evolution," published in the *Contemporary* for June, to which reference is made in a letter to Dr. Youmans (5 June).

I enclose a brief article just out. I wrote it partly as a quiet way of putting opinion a little right on the matter. Since the publication of Darwin's *Descent of Man*, there has been a great sensation about the theory of development of Mind—essays in the magazines on "Darwinism and Religion," "Darwinism and Morals," "Philosophy and Darwinism": all having reference to the question of Mental Evolution, and all proceeding on the supposition that it is Darwin's hypothesis. As no one says a word in rectification, and as Darwin himself has not indicated the fact that the *Principles of Psychology* was published five years before the *Origin of Species*, I am obliged to gently indicate this myself.

[1] "Philosophy and Mr. Darwin," *Contemporary Review* for May.

Towards the end of the year he was drawn into a controversy with Professor Huxley, whose address on "Administrative Nihilism," while dealing with the objections raised to state interference with education, criticized adversely the view that Government should be restricted to police functions, and set aside as invalid the comparison of the body politic to the body physical, worked out by Spencer in the article on "The Social Organism." Spencer replied in the *Fortnightly Review* for December in an article on "Specialized Administration," expressing at the same time his reluctance to dwell on points of difference from one he so greatly admired.

"The *Nation*," wrote Dr. Youmans (May, 1869), "gave you a little thrust the other week, and our friend, Henry Holt, of the firm of Leypoldt and Holt (publishers of Taine), took them to task in last week's paper." The "little thrust" was made in the course of a notice of Taine's *Ideal in Art*, in which it was said that "it is Herbert Spencer's reputation over again; all very well for the 'general public,' but the chemists and physicians, the painters and the architects, are disposed to scoff at the new light." The point of this innuendo must have been very illusive, for when first Mr. Holt, and afterwards Mr. Fiske, adduced evidence to prove that, taking Spencer as a philosopher, "it is clearly not the 'experts' that do the scoffing," the editor retorted that both of them had missed it.[1] "The correspondence in the *Nation*," wrote Dr. Youmans, "has elicited a good deal of comment, not concerning your doctrines, but yourself. Emerson, Agassiz, and Wyman are quoted against you on the ground that a man who attempts so much must be thin in his work." Spencer could treat such criticisms with equanimity, knowing the esteem in which he was held by experts.[2] Mr. Darwin, for example, showed no inclination to scoff. "I was fairly astonished," he writes, "at the prodigality of your original views. Most of the chapters [of the *Biology*] furnished suggestions for whole volumes of future researches." Nor did Spencer write to Mr. Darwin as if he were liable to be scoffed at

[1] The *Nation*, from 20 May to 3 June, 1869.

[2] *Life and Letters of C. Darwin*, iii., 120. *Autobiography*, ii., 216.

by the great naturalist. Witness the following (dated 8 February, 1868), written on receipt of the *Variation of Animals and Plants under Domestication* :

> I have at present done little more than dip here and there—paying more special attention, however, to the speculation on "Pangenesis," in which, I need hardly say, I am much interested. It is quite clear that you do not mean by "gemmules" what I mean by "physiological units"; and that, consequently, the interpretations of organic phenomena to which they lead you are essentially different from those I have endeavoured to give. The extremely compound molecules (as much above those of albumen in complexity as those of albumen are above the simplest compounds) which I have called "physiological units," and of which I conceive each organism to have a modification peculiar to itself, I conceive to be within each organism substantially of one kind—the slight differences that exist amongst them being such only as are due to the slight modifications of them inherited from parents and ancestry. The evolution of the organism into its special structure, I suppose to be due to the tendency of these excessively complex units to fall into that arrangement, as their form of equilibrium under the particular distribution of forces they are exposed to by the environment and by their mutual actions. On the other hand, your "gemmules," if I understand rightly, are from the beginning heterogeneous—each organ of the organism being the source of a different kind, and propagating itself, as a part of succeeding organisms, by means of the gemmules it gives off.
>
> I must try and throw aside my own hypothesis and think from your point of view, so as to see whether yours affords a better interpretation of the facts.[1]

The year before the *Nation* made its "little thrust," Dr. Hooker, in his presidential address to the British Association, gave Spencer's observations on the circulation of the sap and the formation of wood in plants, as an "instance of successful experiment in Physiological Botany." "It is an example of what may be done by an acute observer and experimentalist, versed in Physics and Chemistry, but above all, thoroughly instructed in scientific methods." Another expert, Mr. Alfred R. Wallace, in his Presidential Address to the Entomological Society in January, 1872, spoke of Spencer's view of the nature and origin of the Annulose type of animals as "one of the

[1] See *Life and Letters of C. Darwin*, iii., 78, 80.