

Road, written probably towards the end of 1858 or very early in the year following, he says :—

Had not the announcement of coffee prevented, I had hoped to carry much further the discussion we commenced on Saturday evening. Lest you should misunderstand me, let me briefly say now what I wished to say.

In the first place, I fully recognize, and have all along recognized, the tendency to ultimate equilibrium ; and have, after sundry other chapters on the general laws of change, a final one entitled "The Equilibration of Force." Indeed, of the general views which I have of late years been working out, this was oddly enough the first reached. Among memoranda jotted down for a second edition of *Social Statics*—memoranda written towards the close of '51, or early in '52—I have some bearing on this law in its application to society. . . . Thus, you see, that my views commit me most fully to the doctrine of ultimate equilibration.

That which was new to me in your position enunciated last June, and again on Saturday, was that equilibration was death. Regarding, as I had done, equilibration as the ultimate and *highest* state of society, I had assumed it to be not only the ultimate but also the highest state of the universe. And your assertion that when equilibrium was reached life must cease, staggered me. Indeed, not seeing my way out of the conclusion, I remember being out of spirits for some days afterwards. I still feel unsettled about the matter, and should like some day to discuss it with you.¹

The completed volume was published the third week of June, when he gave himself up to relaxation, acting as guide at the International Exhibition to his parents and others. This summer's visit to Scotland, whither he went after two weeks in Wales, is described with more than usual fulness in the *Autobiography* (ii., 77-83), owing probably to its having been mainly a walking tour. Of the scenery from Invergarry to Loch Alsh he writes : "I have seen some magnificent scenery—the finest I have seen in the kingdom." This enjoyable tour was made shorter than he intended by the arrival from New York of Dr. and Mrs. Youmans, whom he arranged to meet in Glasgow. After a few days spent with them, he went South, taking Derby and Coventry on the way, London being reached by the middle of September.

¹ See Appendix. "The Filiation of Ideas."

While away he had been on the look-out for notices of his book. "No reviews of *First Principles* have reached me yet," he writes in July. "It is a book that reviewers are glad to put off as long as they can." October brought several notices: among them the article on "Science, Nescience, and Faith," in the *National Review* for October, which is thus referred to:—

TO HIS FATHER.

9, GLOUCESTER SQUARE,
3 October, 1862.

It is . . . evidently by Martineau. When you get to the end of it you will see pretty clearly that it is animated by a spirit of retaliation for the attack I made upon him. It is clever, as might be expected; but it contains sundry cases of the usual Martineau Jesuitisms and dishonesties—ascribing to me things which I have not said, and misinterpreting things which I have said

The *Westminster Review* devotes three pages to the book. . . . But the tone of it is somewhat tepid—as I expected it would be.

The *British Quarterly* has given me an agreeable surprise. In its epilogue on books and affairs under the head of Science, there is a brief notice very cordially expressed, stating that they . . . propose to devote an article to it in their next number. It is rather odd that here, where I had expected most antagonism, there is most recognition. . . .

I am well and getting on with my work satisfactorily. I was strongly tempted to go to Cambridge [to the British Association], but concluded that the excitement would be too much for me.

13 October.—I think I told you that I had promised to go to Paris with Mr. Silsbee. . . . I shall have a quantity of work with me to revise, which will occupy me during my stay of three weeks or so; so that I shall not lose any time.

As usual, he very soon got weary of Paris and returned in a fortnight. As soon as he came back he took up Mr. Martineau's review of *First Principles*. In a letter to the *Athenæum* (November 8) he mentioned that the National reviewer has classed him "with a school whose religious conclusions I repudiate, alike on logical grounds and as a matter of sentiment." In a second letter he adduced evidence to show that he had ample justification for charac-

terizing the reviewer's statements as "misrepresentations and grave ones." The "agreeable surprise" given by the *British Quarterly Review* in October was not long-lived. The next issue showed that his original expectation was not far wrong. For in the promised review his analysis of ultimate religious ideas is described as sounding "like a great blazon of trumpets over a very small victory." The analysis of ultimate scientific ideas "is occasionally tedious, but if the tediousness be overlooked, is very amusing, or might easily be made so."

Spencer objected to press notices of his books being used for advertizing purposes. But to satisfy Dr. Youmans, whom he wisely recognized as the best judge of what was expedient for the American public, he sent, early in 1863, a collection of such notices. "As I told you, I do not propose to have them quoted in England; having a decided dislike to the practice. But I have no objection to their being used in the United States, if you think it will be desirable." There was indeed an urgent necessity for something being done there to sustain the interest that had been aroused in 1860. The unsettled political condition during 1861 had been "deadly to generous support" of such literature as did not bear upon the struggle in which the country was engaged.

TO E. L. YOUMANS.

27 February, 1862.

When next you favour me with a letter I shall be very glad to hear from you what is the present state of affairs and opinions respecting the secession. One sees here nothing more than extracts from American papers, and these are mostly from papers which probably do not truly represent the real feeling that prevails among you.

FROM E. L. YOUMANS.

4 April, 1862.

The fact is incontestible that the North is fighting for liberty, order, free industry, education, and the maintenance of stable government, while the South is contending for the opposite—slavery and its dark concomitants. Here the thing is well understood, but wherefore England should sympathize with the South, I confess, is not understood. How the views of large classes in England may be warped by their interests

is plain ; and how the newspapers may be influenced is also obvious ; but how your dignified and philosophic Quarterlies can be brought to utter such unjust sentiments and flagrant misconceptions as their last issues contained, we are unable to explain. Do those Reviews fairly represent British feeling ?

TO E. L. YOUMANS.

15 May, 1862.

I am glad to see by the last account that the North is making great progress. I have held all along that whatever may be the solution to be finally desired, it is quite necessary that the North should show its power ; and I rejoice to see it now doing this. I think Dr. Draper, in common with most other Americans over-estimates what adverse feeling there is here. . . . Such adverse feeling as does exist is due to what we consider here to be the perverse misinterpretation of our motives—the suspicion that our commercial interests *must* bias us in favour of the South, and then the twisting of whatever we said and did into proof that we *were* biassed in favour of the South. As far as I had the means of judging, the feeling here was at first *very decidedly* on the side of the North ; and the change that has taken place has, as far as I have been able to observe, [been] wholly due to the cause I have assigned. But that feeling, however caused, has now very considerably abated.

14 February, 1863.—I am grieved to see the recent news respecting the state of the war. Though, as you know, I have all along held that it was both a necessary thing, and a desirable thing, that the separation should take place, yet I have always hoped to see the South restrained within narrow limits, and regard as disastrous, both for America and the world at large, anything which looks like a possibility of extension in their territory.

CHAPTER X.

BIOLOGY.

(September, 1862—March, 1867.)

ON his return from his holiday in September, 1862, he at once began the *Principles of Biology*, the first number of which was issued in January, 1863, and the second in April.

The announcement of Mr. Mill's *Utilitarianism* afforded him an opportunity of stating more clearly than he had done in *Social Statics*, his attitude towards the doctrine of Utility. The greater part of his letter of February 24, 1863, appears in the *Autobiography* (ii., 88). It concludes by expressing the hope that "the above explanations will make it clear that I am not really an antagonist to the doctrine of Utility. If not a Utilitarian in the direct sense, I am still a Utilitarian in the transcendental sense."

FROM J. S. MILL.

25 February, 1863.

I am obliged to you for your letter, and if the sheet is not struck off (which I fear it is) I will add to the note [pp. 91-2] in which you are mentioned, what is necessary to prevent the misapprehension you desire to guard against.

Your explanation narrows the ground on which we differ, though it does not remove our difference; for, while I agree with you in discountenancing a purely empirical mode of judging of the tendencies of human actions, and would on that subject, as well as on all others, endeavour to reach the widest and most general principles attainable, I cannot admit that any of these principles are *necessary*, or that the practical conclusions which can be drawn from them are even (absolutely) universal.

As I am writing I cannot refrain from saying that your *First Principles* appear to me a striking exposition of a consistent

NOTE.—*Autobiography*, ii., chaps. xxxix., xl., xli., xlii., xliii., xliv.

and imposing system of thought ; of which, though I dissent from much, I agree in more.

To J. S. MILL.

1 March, 1863.

I am greatly obliged to you for having not only made the desired alteration, but allowed me to see the proof. Taking advantage of your permission, I have ventured to make, in pencil, such changes of expression as are needed more completely to represent my view.

20 March.—I am much obliged to you for the copy of your reprinted essays on *Utilitarianism*. . . . Let me also thank you for having so readily acceded to my request respecting the explanation, as well as for having introduced the modifications of expression in it which I suggested.

TO HIS FATHER.

9 June, 1863.

For myself I am well and busy—going out a good deal, and indeed rather too much. Saturday and Sunday I spent at the Lubbocks, along with Huxley and his wife, and Tyndall. On Wednesday I dine out again, on Friday again, and again on Sunday. . . . On the Friday I am going specially to meet Lady Lubbock, who, Mrs. Lubbock says, is “dying to see me.”¹

Having issued the third number of the *Biology*, and taking with him the *Essays* he proposed to revise, he went about the middle of July to see his mother at Scarborough. While there he writes to his father : “I find my mother looking pretty well and in tolerably good spirits. . . . Your hand is *very much bolder*, whence I infer that you are considerably better. . . . For myself I am very well and comfortably placed, and like Scarborough much for its variety and picturesqueness.” From Scarborough he went to the West of Scotland, in company with Mr. Lott and another friend, eventually reaching Ardtornish.

His interests in the United States were being well looked after by Dr. Youmans, who thus describes the two objects to be aimed at : “To circulate your writings as extensively as possible, and to do it in such a manner that you might share the pecuniary results. It has been comparatively easy to accomplish the first object unembarrassed by the

¹ See *Autobiography*, ii., 71.

second." Mr. Appleton being ready to *share* the risk of publishing a selection from the *Essays*, though not seeing his way to take the whole of it, Dr. Youmans set about securing the necessary support, which was more liberal than Spencer could accept, though he warmly appreciated the generosity that prompted it.¹ But he acquiesced in an arrangement according to which his American friends were to provide the publishers with the stereotype plates, on the understanding that no royalty should be paid to him until his friends had been recouped their outlay. In the *Autobiography* he says: "This was, I believe, the course eventually adopted. Funds were raised to pay the cost of reprinting the several volumes named, and after those who furnished them had been recouped, I began to receive a royalty on all copies sold." Subsequent correspondence does not bear out his recollection of "the course eventually adopted."

FROM E. L. YOUMANS.

1 January, 1865.

As respects the copyright money sent you, or the certificates of its investment, I have only to say that it accrues to you from the sale of your books, and if you do not draw it, D. Appleton & Co. will have the benefit of it.

When your letter, refusing to accept anything from the avails of your books until all who had subscribed to their republication were repaid, was received, I circulated it among those principally interested. They appreciated your feelings in the matter, but said your scruples were groundless, as you totally misconceived the case—that they had aided to republish the works for public reasons, as they had a perfect right to do, and were ready, if desirable, to increase their contributions, but not to receive back what they had so gladly given. They have not regarded it at all in the light of a personal matter, nor can they be made to do so now. While they consider themselves richly compensated by the success of your works, and the unmistakable symptoms of their powerful influence upon public opinion, the fact that the author gets his just compensation is regarded as an agreeable incident of the enterprize.

And now allow me to remind you of a remark you made some time since to the effect that you had better leave this business of reprints to us on this side and take no responsibility in the matter.

¹ *Edward Livingston Youmans*, p. 161. *Autobiography*, ii., 97.

TO E. L. YOUMANS.

23 January, 1865.

I am very much impressed by the manifestation of sympathy and generosity implied in your explanation respecting the proceeds of copyright. I should have preferred that the matter should have been transacted in the modified way that I originally requested, and I feel somewhat uncomfortable under the much heavier obligation entailed on me by the course pursued; but, at the same time, this extreme self-sacrifice displayed by my American friends is a source of very pleasurable feeling to me, not only in its personal aspects, but also as a testimony of their interest in the propagation of the views with which I am identified.

The success of the *Essays* had suggested the expediency of bringing out an American edition of *Social Statics*.

FROM E. L. YOUMANS.

12 April, 1864.

I think you once remarked to me that certain of your views had been considerably modified since the publication of *Social Statics*, but as you intimated that the change consisted in a divergence from the democratic views there expressed, the volume may be more acceptable to us in its present form than it would be after your revision.¹

TO E. L. YOUMANS.

18 May, 1864.

Respecting *Social Statics*, I gave you a somewhat wrong impression if you gathered from me that I had receded from any of its main principles. The parts which I had in view, when I spoke of having modified my opinions on some points, were chiefly the chapters on the rights of women and children. I should probably also somewhat qualify the theological form of expression used in some of the earlier chapters. But the essentials of the book would remain as they are. When you come to the reprinting of *Social Statics*, should that project be persevered in, I should like to put a brief prefatory note, stating my present attitude towards it.²

Dr. Youmans hoped that Spencer would devote sufficient space to put readers in full possession of his later views. When the promised preface was sent in November, Spencer

¹ *Edward Livingston Youmans*, p. 176.

² *Ibid.*, p. 180.

wrote: "I fear it will disappoint you in not containing any specific explanations. But I could not make these in any satisfactory way without occupying more space than would be desirable and more time than I can now afford. I think, too, it will be better policy at present to leave the disclaimer in the comparatively vague form in which I have put it."

FROM E. L. YOUMANS.

1 January, 1865.

I was not disappointed in it as a disclaimer, but was somewhat so that it was *only* a disclaimer. I had hoped there would be a little of something else to relieve it of its naked, negative character. . . . But the effect of this preface in its present form will undoubtedly be bad upon the work. . . . If I had followed my own preference I would have written a preface saying certain things which I could very well have said, and absorbing the entire contents of your preface into it as a private letter, stating your present attitude to the work. . . . I do not purpose to change a word nor to neutralize its influence by counter-statements; but simply, by distributing it through another medium, to somewhat diminish the injurious effect which it will have by being placed and read alone.

TO E. L. YOUMANS.

23 January, 1865.

Pray do as you think best respecting the preface to the American edition of *Social Statics*. Probably it will be better to embody the explanations I have made in an introduction of your own, as you propose. All I wish is, to make it understood that the book must be read with some qualifications; and this end will be as well achieved in your own [words] as in mine.

"After repeated attempts to comminute and macerate" the preface Dr. Youmans had to give it up, and let it appear as Spencer had put it.

The fourth instalment of the *Biology* had been delayed partly owing to his having devoted some three months to the revision of a second series of *Essays*. A more serious interruption was caused by his having turned aside to set himself right as to his relations to Comte and Positivism. In a letter to the *New Englander* towards the end of November, 1863, he repudiated being classed as a follower of Comte.

On all . . . points that are distinctive of his philosophy, I differ from him. I deny his hierarchy of the Sciences. I regard his division of intellectual progress into three phases, theological, metaphysical and positive, as superficial. I reject utterly his religion of humanity. And his ideal of society I hold in detestation. Some of his minor views I accept; some of his incidental remarks seem to me profound; but from everything which distinguishes Comtism as a system, I dissent entirely.

When he wrote to the *New Englander* he had no idea of going beyond the immediate purpose of correcting the misapprehension in the United States. But it now occurred to him that it might be well to set forth his views in full, and immediately on his return from Derby in January, 1864, he set about doing this. Once more he was led further than he at first intended. While preparing the pamphlet on "The Classification of the Sciences," there appeared M. Laugel's article in the *Revue des Deux Mondes* for February 15, 1864. "I find the impression that I belong to the school of Comte is so general," he tells his father, "and so likely to be confirmed by M. Laugel's article, that I am about to write a full denial on all points." Proofs of "Reasons for Dissenting from the Philosophy of M. Comte" were sent to Mr. G. H. Lewes.¹ Several scientific men were invited to say whether Comte had influenced them, or any men they knew. The gist of their replies was embodied in the pamphlet, but after it had been put in type this paragraph was omitted.

TO E. L. YOUMANS.

26 March, 1864.

I enclose along with this a slip-proof of a portion which I suppressed from the pamphlet, from a desire not to seem needlessly hostile to the Comtists here. I do not suppose you will find any use for it. But if you do, do not make use of my name. Since having suppressed it from the pamphlet here, it is not desirable that I should assert it elsewhere.

The suppressed paragraph, besides summarizing the denials of Tyndall, Huxley, Herschel, Babbage, Lyell and

¹ *Autobiography*, ii., 111; also Appendix B, p. 485. *George Eliot's Life*, ii., 381.

Faraday that their course of thought had been influenced by Comte, points out how scientific conceptions and methods repudiated by Comte had, nevertheless, gained wider acceptance and greater definiteness. "Thus not only have M. Comte's teachings failed to change in any appreciable way the course of scientific thought in England; but its most marked advances have been in directions which he says it should not take."

With reference to this question, he wrote to Mr. Mill enquiring whether a letter dated 29 July, 1858,¹ was still in existence. "Unless I am very much mistaken respecting its contents, this letter would form tolerably conclusive evidence" as to the actual origin of the system of philosophy.

FROM J. S. MILL.

3 April, 1864.

I am, fortunately, able to send you the letter you want. No Englishman who has read both you and Comte, can suppose that you have derived much from him. No thinker's conclusions bear more completely the marks of being arrived at by the progressive development of his own original conceptions. . . . But the opinions in which you agree with Comte, and which, as you truly observe, are in no way peculiar to him, are exactly those which would make French writers class you with him; because, to them, Comte and his followers are the only thinkers who represent opposition to their muddy metaphysics.

I myself owe much more to Comte than you do, though in my case also, all my principal conclusions had been reached before I saw his book. But in speculative matters (not in practical) I often agree with him where you do not, and, among other subjects, in this particular one, the Classification of the Sciences. The fact you mention of your having read only a portion of his *Cours de Philosophie Positive*, explains some things to me which I did not understand previously; for, if you had read the entire book, I think you would have recognized that several of the things which you urge as objections to his theory, are parts of the theory.

I have lately had occasion to re-read, and am still reading, your *Principles of Psychology*. I do not agree any more than I did before with the doctrine of the introduction; but as to the book itself, I cannot help expressing to you how much my opinion of it, though already high, has been raised (I hope from a progress in my own mind) by this new reading. There is

¹ *Autobiography*, ii., 23.

much of it that did not by any means strike me before as it does now ; especially the parts which show how large a portion of our mental operations consist in the recognition of relations between relations. It is very satisfactory to see how you and Bain, each in his own way, have succeeded in affiliating the conscious operations of mind to the primary unconscious organic actions of the nerves, thus filling up the most serious lacuna and removing the chief difficulty in the association psychology.

TO J. S. MILL.

8 April, 1864.

I am greatly obliged to you for your letter of the 3rd, enclosing the letter I referred to, which has been so fortunately preserved. Your expression of opinion on the question at issue was also extremely satisfactory to me : coming as it does from one so fully acquainted with the facts of the case, and so free from bias. It has served to confirm me in the belief that the position I have taken is a justifiable one.

Let me thank you also for the very gratifying expression of your opinion respecting the *Principles of Psychology*. I need scarcely say that, coming from you, this favourable criticism gives me a better assurance than any which I have yet had, that the book has not been written in vain.

Respecting the doctrine of the introduction, I have hitherto postponed returning to the question until the time when a second edition afforded an opportunity to do so. But as you have referred to it, it seems proper now to say, that I believe the disagreement arises mainly from a difference of verbal interpretation. It did not occur to me when I used it as I have done, that the word "inconceivable" was liable to be understood as the equivalent of "incredible." By an *inconceivable* proposition, I, in all cases, meant an *unthinkable* proposition—a proposition of which the elements cannot be *united in consciousness*—cannot be thought of in *direct relation*.

Towards the end of his letter of 3rd April, Mr. Mill mentions with approval the work being done by Professor Bain and Spencer, each in his own way. The differences between these two on philosophical questions seemed but to bring out more clearly their regard for one another—a regard (already strong in those early days) which grew in strength as year succeeded year. The following is an acknowledgement of the second edition of *The Senses and the Intellect*, which Professor Bain had kept back as long as possible in order to be in possession of Spencer's latest utterances in the *Biology*.

TO ALEXANDER BAIN.

March, 1864.

I see that you have made some references to my speculations and criticisms ; and have done so in a very friendly spirit. I am the more gratified by this, because I feared that you might be somewhat annoyed by my review of your second volume. I am very glad to find that the differences of opinion which I freely expressed in it, have not induced any disagreeable feeling. I am, indeed, impressed with the great generosity of nature which your reception of them implies.

His health and power of work were about this time better than usual. The excitement accompanying his criticism of Comte had, he thought, done him good. The social excitements of the London season were also borne for a time without injury. But by midsummer he had to admit that he had been going out too much. After bringing his mother home from Matlock, and spending a short time with the Lotts in Wales, he went to Scotland.

TO HIS FATHER.

ARDTORNISH, 28 August, 1864.

I have now been here nine days ; and the time has passed very pleasantly. I have been very cordially treated—more so than usual, I think. . . .

I have declined Bain's invitation. I did not dare to run the risk of discussions.

7 October.—The opening article in the *North American Review* for July is one on the Nebular Hypothesis—taking for text my second series of Essays. It disagrees on some points, but is very civil and complimentary.

About the middle of October, the concluding number of the first volume of the *Biology* was issued. "Fancy my disgust," he writes next month, "on reading in the list of the books of the week in the *London Review*, my own just published volume announced as *Electro-Biology*, vol. i. . . . I am getting on with my writing satisfactorily, and am working out the Morphology of Plants with unexpected success." The issue of the first number of vol. ii. was delayed "in consequence of the number of woodcuts I am having prepared in illustration of vegetal morphology. The choice and arrangement and execution of these gives a great

deal of trouble, and keeps me tied here. I shall not, in consequence, be able to get down to Derby till after Christmas." As if his biological work were not enough to have on hand at one time, he took an active part along with a few friends in an attempt to reorganize the *Reader*. He himself contributed four articles: "What is Electricity?" "The Constitution of the Sun," "The Collective Wisdom," and "Political Fetichism." Endeavours were made to obtain the co-operation of men of standing.

TO CHARLES DARWIN.

22 April, 1865.

We are getting our staff of the *Reader* into better working order; and are proposing forthwith to use all the means available for making a more decided impression, and establishing our position. Profs. Huxley and Tyndall, Mr. J. S. Mill and myself, have severally agreed to write a few leading articles by way of giving the intended tone and direction.

Among other means of making the public aware of the character of the *Reader*, we propose to obtain, so far as possible, occasional brief letters from the leading men of science, announcing such interesting novelties as admit of being understood by the general public, and are of fit nature to be quoted from our columns. I have a letter from Sir John Herschel consenting to aid us in this way. Sir Charles Lyell, too, has promised the like aid. Can you in like manner give us, occasionally, the valuable help of your name? . . .

A letter of a *dozen lines* would suffice the purpose of giving us the weight of your name, and making it apparent that you joined in the effort to establish a scientific journal, and an organ of progressive opinion.

TO J. S. MILL.

26 May, 1865.

I hope you are better satisfied with the *Reader*. It is rapidly improving in circulation; but I fear we shall now have to pass through a trying period, during which the want of advertisements will be very much felt.

It appears that the putting of initials to articles is not of very much service—many of the public having been quite in the dark as to who "J. S. M." was. It is suggested that the full names should be put. What is your feeling on this point?

The article some weeks since on the *Edinburgh Review* caused a breach with the Longmans, as was to be expected. They had not advertised for some time previously, and of course have not done so since.

FROM J. S. MILL.

AVIGNON, 29 May, 1865.

With regard to the *Reader*, I like the plan of full signatures. . . . But, to admit of this it would [be necessary for the *Reader* to give up the plan it has recently adopted of making slashing attacks to right and left, with very insufficient production of evidence to justify the vituperation ; and in a manner which gives to an indifferent spectator the impression either of personal ill-will in the particular case, or of general flippancy and dogmatism. Contributors will not like to identify themselves by name with a publication which would embroil them with an unlimited number of angry and vindictive writers, together with their friends and their publishers. I myself should not like to be supposed to be in any way connected, for instance, with the attack on the *Edinburgh Review* (for which I am at this very time preparing an article)—an attack of which I totally dislike the tone, and agree only partially with the substance : and it happens that the article singled out from the last number for special contempt, my name, too, being cited against it, is by a personal friend of my own, a man of very considerable merit, whom I was desirous of securing as a recruit for the *Reader*, and who is very naturally hurt and indignant at the treatment of him. I am by no means against severity in criticism, but the more it is severe, the more it needs to be well weighed and justly distributed.

It was now some years since Spencer had written an article of any magnitude or importance for the larger Reviews. The reason for breaking his rule in 1865 is given in the following correspondence, which, apart from its bearing on the questions discussed, has value as a revelation of the generous catholicity of the writers.

FROM J. S. MILL.

BLACKHEATH, 11 March, 1865.

Dr. Chapman will send you in the course of a day or two a copy of an article of mine on Comte, which is to be published in the forthcoming *Westminster*. In forming an estimate of him, I have necessarily come into collision with some of your opinions—a thing for which I should never think of apologizing to you or any advanced thinker ; but it has so happened that though our points of agreement very greatly exceed in number and importance those of difference, the latter are those respecting which, accidentally, most has been said to the public, on my side at least. What I have now written, however, will give a very false impression of my feelings, if it raises any idea

but that of minor differences of opinion between allies and fellow-combatants. In a larger volume which I shall soon have the pleasure of offering to you, there will be little or nothing to qualify the expression of the very high value I attach to your philosophic labours.

TO J. S. MILL.

13 March, 1865.

I am greatly obliged to you for your note of the 11th; and appreciate the kind feeling which dictated it.

I thoroughly sympathize with your view respecting the candid expression of differences of opinion. My own practice has always been that of pointing out what appears to me erroneous, quite irrespective of any personal considerations; and I am quite prepared to have the opinions I express treated by other thinkers with a like individual regard for the interests of truth. Moreover, I am fully convinced that what you may think it needful to say, in opposition to anything that I have said, must always be something which it is well to have said: either as an indication of a mistake, or else as the indication of some imperfection in the argument or some fault of exposition which needs rectifying.

On receipt of the promised copy of Mr. Mill's *Examination of Sir William Hamilton's Philosophy* Spencer deemed it necessary to deal with the question at issue between them, namely, the ultimate test of truth. This was done in the *Fortnightly Review* for July.

FROM J. S. MILL.

12 August, 1865.

When I received your article in the *Fortnightly Review*, the reprint of my book on Hamilton was too far advanced to admit of my correcting at the proper place the misconception which you pointed out in p. 536 of the *Review*. I consequently added a note at the end of the volume, of which, in case you have not seen it, I enclose a transcript.¹

I do not find that the distinction between the two senses of the word inconceivable, removes or diminishes the difference between us. I was already aware that the inconceivability which you regard as an ultimate test, is the impossibility of uniting two ideas in the same mental representation. But unless I have still further misunderstood you, you regard this incapacity of the conceptive faculty merely as the strongest proof that can be given of a necessity of belief. Your test of

¹ *Essays*, ii., 195. Mill's *Examination*, p. 175, note †, third edition.

an ultimate truth I still apprehend to be, the invariability of the belief of it, tested by an attempt to believe its negative.

I have, in my turn, to correct a partial misunderstanding of my own meaning. I did not assert that a belief is proved not to be necessary by the fact that some persons deny its necessity, but by the fact that some persons do not hold the belief itself ; which opinion seems as evident as the other would be absurd.

On the main question between us, your chief point seems to be that the Idealist argument is reduced to nonsense if we accept the Idealist conclusions, since it cannot be expressed without assuming an objective reality producing, and an objective reality receiving, the impression. The experience to which our states of mind are referred is, *ex vi termini* (you think), experience of something other than states of mind. This would be true if *all* states of mind were referred to something anterior : but the ultimate elements in the analysis I hold to be themselves states of mind, viz., sensations, memories of sensation, and expectations of sensation. I do not pretend to account for these, or to recognize anything in them beyond themselves and the order of their occurrence ; but I do profess to analyze our other states of consciousness into them. Now I maintain that these are the only substratum I need postulate ; and that when anything else seems to be postulated, it is only because of the erroneous theory on which all our language is constructed, and that, if the concrete words used are interpreted as meaning our expectations of sensations, the nonsense and unmeaningness which you speak of do not arise.

I quite agree with you, however, that our difference is "superficial rather than substantial," or at all events need not and does not affect our general mode of explaining mental phenomena. From the first I have wished to keep the peace with those whose belief in a substratum is simply the belief in an Unknowable. You have said what you deemed necessary to set yourself right on the points which had been in controversy between us. I am glad you have done so, and am now disposed to let the matter rest. There will probably be other and more hostile criticisms, by Mansel or others, and if I should think it desirable to reply to them, I could on the same occasion make some remarks on yours, without the appearance of antagonism which I am anxious to avoid.

To J. S. MILL.

ARDTORNISH, 21 August, 1865.

I am much obliged by your courtesy in sending me a copy of the note to the new edition of your work on Hamilton. . . . Thank you very much for the very candid explanation which the note contains. It sets the matter quite straight.

The partial misstatement of your own view which you point out I will endeavour to set right should there occur (or rather as soon as there occurs) an opportunity for doing so.

I am much gratified to find that the discussion has been thus far carried on, and, indeed, for the present concluded, without having produced anything beyond intellectual difference.

LONDON, 11 *October*.—Many thanks for the copy of the sixth edition of your *Logic*, which you have been so kind as to send me. I shall have to study it afresh before preparing the second edition of my *Psychology*, should I ever get so far; and I am very glad to have, thus brought up to date, the latest developments of your views on the many important questions dealt with.

I have of course already read the chapter in which you discuss the chief questions at issue between us; and am obliged to you for the care you have so promptly taken to restate my position as recently explained afresh. Without entering upon any of the chief points of the argument as it now stands, I will just refer to the fact that on one of the issues the question is greatly narrowed—coming, as it does, to a direct opposition between the verdicts of your consciousness and my own. You remark—“When Mr. Spencer says that while looking at the sun, a man cannot conceive that he is looking into darkness, he means, a man cannot *believe* that he is doing so. For he is aware that it is possible, in broad daylight, to *imagine* oneself looking into darkness.” To these interpretations of my meaning I demur; I do really, in this case as in other cases, mean the words “cannot conceive” to be used in their rigorous sense. The verdict of consciousness, as it seems to me (and I find it the same with three competent friends to whom I have put the question), is that when looking at the sun I not only cannot *imagine* that I am *then* and *there* looking into darkness (and this is the point at issue), but I cannot even imagine darkness at all. The attempt to imagine myself looking into darkness, I find can be carried to the extent of imagining some other scene in which I have before experienced darkness; but while I can imagine the various elements of the scene which accompany the darkness, I cannot imagine the darkness itself. I can bring into consciousness all those impressions along with which I have experienced the darkness of a cellar, but I cannot bring with them the impression of darkness itself, *while my consciousness is occupied with the vivid impression of light*. Even did I find that I can, under such conditions, imagine darkness in the abstract, this would not be equivalent to finding that I can, under such conditions, think, or conceive, or imagine, that I am actually *at the moment*, looking into darkness; and this, I take it, is the real point.

FROM J. S. MILL.

AVIGNON, 29 October, 1865.

I have kept your letter by me unanswered, partly for want of time, and partly in hopes that the delay might enable something to occur to me which would throw light on the rather subtle matter of difference between us which you bring to my notice. It is evident that I have again a misapprehension of your opinion to confess and correct, since you do not acknowledge it as yours in the mode in which it is stated by me. We seem to differ on two questions, one a question of fact, viz., whether it is possible, while looking at the sun, to imagine darkness. You, and your three friends, think it is not, while my consciousness seems to tell me that it is quite as possible to imagine darkness in its absence, as anything else in its absence. Of course, the stronger present impression of an actual sensation makes the simultaneous consciousness of a mere recollection seem feeble by comparison. But it appears to me perfectly real, and as like the impression of sense which it corresponds to as most reminiscences are to their originals. But, you say, even if I could, under such conditions, imagine darkness, it would not follow that I could imagine that I am actually at the moment looking into darkness. To me it seems that to imagine an object of light is always to imagine myself actually at the moment seeing it. I think one never imagines anything otherwise than as an immediate and present impression of one's own. Indeed, when the object to be conceived is darkness, there is absolutely nothing else to imagine than oneself trying to see and not seeing; for darkness is not a positive thing. It seems to me, then, that I can, in broad daylight, *conceive myself* then and there looking into darkness. Is this the same thing, or not the same thing, as what you mean by the words "*conceive that I am* then and there looking into darkness?" It strikes me that this change of the expression to the form *I am*, just marks the transition from conception to belief—from an imagination of something thought as absent from the senses, to an apprehension of something which is thought to be present to the senses; of which two states of mind I hold the former to be, in the assumed circumstances, possible, the latter impossible. It was in this way I was led to think that you were here using the word conception in the sense of belief. Even now, I cannot see how the phrase, to conceive that I am, or that anything is, can be consistent with using the word conceive in its rigorous sense.

These letters bring out (as clearly perhaps as the subject permits) "the rather subtle matter of difference" between Mr. Mill and Spencer concerning the ultimate test of truth.

The last page of one of my proofs, which has been criticised by Huxley, and in which he signifies by the sketch that he cannot lay hold

of anything.

differentiated in proportion as their relations to incident forces become different. And here, as before, we see that in each unit, considered by itself, the differences of dimension are greatest in those directions in which the parts are most differently conditioned; while there are no differences between the dimensions of parts that are not differently conditioned.*

* It was by an observation on the forms of leaves, that I was first led to the views set forth in the preceding and succeeding chapters on the morphological differentiation of plants and animals. In the year 1851, during a country ramble in which the structures of plants had been a topic of conversation, with a friend—Mr G. H. Lewes—I happened to pick up the leaf of a buttercup, and drawing it by its foot-stalk through my fingers so as to thrust together its deeply-cleft divisions, observed that its palmate and almost radial form was changed into a bilateral one; and that were the divisions to grow together in this new position, an ordinary bilateral leaf would result. Joining this observation with the familiar fact that leaves, in common with the larger members of plants habitually turn themselves to the light, it occurred to me that a natural change in the circumstances of the leaf might readily cause such a modification of form as that which I had produced artificially. If, as they often do with plants, soil and climate were greatly to change the habit of the buttercup, making it branched and shrub-like; and if these palmate leaves were thus much overshadowed by each other; would not the inner segments of the leaves grow towards the periphery of the plant where the light was greatest, and so change the palmate form into a more decidedly bilateral form? Immediately I began to look round for evidence of the relation between the forms of leaves and general characters of the plants they belonged to; and soon found some signs of connexion. Certain anomalies, or seeming anomalies, however, prevented me from then pursuing the inquiry much further. But consideration cleared up these difficulties; and the idea afterwards widened into the general doctrine here elaborated. Occupation with other things prevented me from giving expression to this general doctrine until Jan. 1859; when I published an outline of it in the *Medico-Chirurgical Review*.



The microscopic investigations undertaken while dealing with morphology and physiology had opened up an interesting enquiry regarding circulation in plants and the formation of wood, his earlier preparations being shown to Dr. Hooker, Professor Huxley, and Dr. Busk in January, 1865. "Most naturalists will regard it as an audacious speculation," he tells Dr. Youmans, "but as Hooker and Huxley are inclined to endorse my reasonings, I feel at ease on the matter."

The usual respite was taken after the issue of his serial in June, 1865. During his stay at Ardtornish this year he mentions having caught a salmon of $9\frac{1}{2}$ lbs.—the largest he had ever yet got. He expected "to be in first-rate condition" by the time he left.

TO HIS FATHER.

LONDON, 3 *October*, 1865.

I am getting on satisfactorily with my work and expect to send you some proofs in less than a fortnight. The theory of the vertebrate skeleton works out far more completely than I had expected.

11 *December*.—Meanwhile I am busy with No. 16; . . . By the 20th I hope to get ready as much MS. as will give me something to do in revising while I am down at Derby. If nothing intervenes I propose to come down to you about the 21st, and stay with you till the end of the month; after which, if you feel equal to it, you had better come and spend a week with me in town. I am glad to gather that my mother has borne up so well during your late attack. I hope she continues to do so. Give my love to her and say I shall see her shortly.

During the few days at home he carried on his microscopic study of the circulating system of plants, returning to town in time for the usual New Year's dinner at the Huxley's, to which he refers when writing to his father.

TO HIS FATHER.

3 *January*, 1866.

Our evening was a very pleasant one. Among other guests was Mr. Ellis, an ardent educationist, who has done great service in popularizing Political Economy for schools. . . .

I am busy while I dictate in re-examining my preparations, which, while I was at Derby, I had only so far examined as

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.”

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

CHAPTER XI.

SUNSHINE AND SHADOW.

(December, 1865—July, 1867.)

THE number of the *Biology* issued in December, 1865, contained a notice to the effect that on completion of the volume the series would cease. The circumstances that led to this decision, and the efforts made to prevent its being carried out, are narrated in the *Autobiography* (ii., 132, 491). One of the first to interest himself was Mr. J. S. Mill.

TO HIS FATHER.

15 *February*, 1866.

I enclose you a very gratifying letter which I received from John Mill some ten days ago. It shows great generosity. I have, however, declined both the offers it makes. As you will see, he clearly does not understand the nature of the loss which led me to issue the notice—he thinks that it is nothing more than the difference between the receipts from the subscribers and the cost of printing; and that were the bare expenses of publication met I should have no difficulty in going on. I have explained to him how the matter stands.

Williams and Norgate hinted to me the other day that there was a movement in progress to do something that would meet the case in a way that I could agree to. I learn also that John Mill has called upon them since he got my reply to his letter.

TO E. L. YOUMANS.

2 *March*, 1866.

Count Limburg Stirum . . . one of Comte's executors, has written to Lewes, sending through him to me a draft for £10 towards a publication fund, and proposing to form a committee for the furtherance of the matter, and wishes that the *Fortnightly Review* should make itself the organ for carrying out

The first of these is the fact that the
number of cases is increasing rapidly.

The second is the fact that the
cases are occurring in all parts of the
country.

The third is the fact that the
cases are occurring in all classes of
society.

The fourth is the fact that the
cases are occurring in all ages.

The fifth is the fact that the
cases are occurring in all seasons.

The sixth is the fact that the
cases are occurring in all parts of the
world.

The seventh is the fact that the
cases are occurring in all parts of the
country.

The eighth is the fact that the
cases are occurring in all parts of the
country.

The ninth is the fact that the
cases are occurring in all parts of the
country.

The tenth is the fact that the
cases are occurring in all parts of the
country.

such a plan. Of course, in pursuance of the attitude I have taken up, I returned him the draft, explaining how matters stood.

A proposal that came from some of his friends that those interested "should subscribe for a sufficient number of copies to secure the author from loss," was not so easily disposed of.

TO T. H. HUXLEY.

11 April, 1866.

My reflections over the matter of our talk the other night have ended in a qualified agreement to the arrangements—an agreement under conditions.

In the first place, as to the number of copies to be taken. This is too great. I do not know how 250 was fixed upon. . . . Thus, then, I conclude that an extra circulation here of 150 will suffice, joined with what I may otherwise fairly count upon. And to this number, I should wish that the additional copies taken may be limited.

In the second place, as you say that the wishes to further the continuance of the work have, in the main, acted spontaneously, I will yield to your argument that the acts are in a sense public ones, with which I am not personally concerned—but with one reservation. I can take this view of the matter only in those cases where the sacrifices involved are not likely to be seriously felt. Those to whom guineas come in some abundance may be allowed to spend them in this way; but those who have to work hard for them, and have already heavy burdens to bear, cannot be allowed to do so. Having granted your premises, my exception to your conclusion may be quite illogical; but I must, nevertheless, make it. No re-assertion of the position that the act is public and the motive impersonal would suffice to get rid of what would be to me an intolerable consciousness, were any save those who are quite at their ease to join in these transactions.

Subsequently he withdrew this conditional acquiescence for the reason given in a letter to Professor Huxley, written from Derby in May, about three weeks after the death of his father.¹

Not less eager were his friends in the United States to avert, if possible, the threatened calamity. On learning the facts towards the end of 1865, Dr. Youmans

¹ *Autobiography*, ii., 491.

had remarked: "You will not object to my using them here in any way that seems desirable." Had Spencer known what his friend thought of doing, he would certainly have taken measures to prevent it. No one knew this better than Dr. Youmans. "Of course it won't do to let Spencer know what is going on at all. He would spoil it, sure as fate." By the middle of June, 7,000 dollars had been collected. "So the Spencer affair is finished, all but the most troublesome part," he told his sister; anticipating difficulty in persuading Spencer to accept the gift. It had not all been smooth sailing. Besides having to rouse the enthusiasm of disciples, he had to counteract the effects of adverse criticism, which "embargoes 'Liberal Christianity' and leaves us to raise money out of 'the world, the flesh, and the devil.'" A criticism in the *Christian Examiner* (March, 1866) was described as "the ablest thing yet against Spencer," and for a time he feared the effect it might have on his appeal.

As the bearer of the letter from Mr. R. B. Minturn, announcing the handsome testimonial, Dr. Youmans came to London. Writing to his sister, he describes the astonishment and pleasure with which Spencer read Mr. Minturn's letter.¹ Other letters of sympathy and encouragement accompanied this token of America's good will.

FROM WILLIAM R. ALGER.

18 June, 1866.

We do this in a pure spirit of loyalty to truth and humanity, without the slightest egotistical thought of ourselves or of you. We do it as a simple act of justice. We shall be deeply disappointed if you do not rise above every disagreeable personal feeling, and accept this offering in the spirit in which it is made, in the service of science and society.

FROM HENRY WARD BEECHER.

June, 1866.

The peculiar condition of American society has made your writings far more fruitful and quickening here than in Europe. We are conscious of great obligations to you, and perplexed because we cannot acknowledge them as we could were we your fellow citizens.

But we cannot consent to lie under such obligations with-

¹ *Autobiography*, ii., 140. *Edward Livingston Youmans*, p. 213.

out some testimonial of our feelings respecting your eminent service to us, and to the cause of the emancipation and enlightenment of the human mind, so dear to us all.

And we are sure that you will not allow any scruples of personal delicacy to make you unjust to us, or to compel us to forbear the only action which is possible to us at this distance, and in our circumstances.

In the last, or one of the last, of his letters to his father (March 27) he enclosed a letter from St. Petersburg, "which will give you an agreeable surprise, as it did me." The agreeable surprise was a request for permission to translate his books into Russian. "The Classification of the Sciences," was the first to appear. To meet his objection to this being selected to start with, he was informed that books discussing religion or politics would not be tolerated by the authorities. The "Classification" passed successfully; but a translation of the *Essays* was seized, owing to the essay on "Manners and Fashion," which was supposed to call in question the validity and eternity of the monarchical principle and of divine right! For attempting to publish it the translator had rendered himself liable to prosecution for a criminal offence, the penalty for which varied from six years penal servitude to eight months imprisonment in a fortress. The translator requested Spencer to be in readiness to insert a paragraph in the *Times*, in the event of an adverse verdict. It was not till March of the following year that Spencer learnt that the charge had been withdrawn—"s'est terminé parfaitement à la russe."

A French translation of *First Principles* was being made by Dr. E. Cazelles, who was strongly recommended by Mr. J. S. Mill. Writing to his father in October, 1865, Spencer had enclosed a letter from M. Renan informing him that the book was likely to be translated. On hearing from Dr. Cazelles, towards the end of the following year, that half of it had already been translated, Spencer urged him to wait for the second edition before proceeding further.

Of his other doings during the second half of 1866 little is known. When narrating the occurrences of this time, he admitted that his memory was not very clear. He missed the letters to and from his father, which hitherto

had served as milestones and sign-posts by which to follow the course of events. That his memory should have failed him is not surprising. The shock of his father's death, anxiety about his mother, depression consequent on the contemplated discontinuance of the *System of Philosophy*, and the unexpected manner in which that trouble had been removed, all these tended to prevent the course of things leaving a permanent impression. Before going to the meeting of the British Association, at Nottingham, Dr. Youmans and he spent a few days at Aberdovey, in Merionethshire. While there the article in the *Christian Examiner* by Mr. F. E. Abbot was discussed with a view to a reply Dr. Youmans intended to publish on his return to New York. "We are taking it up point by point," Dr. Youmans tells his sister. "Spencer talks, and I am amanuensis. . . . I have myself learned some matters and things worth knowing. Spencer doesn't recede or budge a hair, but he interprets."¹

At Aberdovey, and afterwards in London, there were frequent talks about a lecture which Dr. Youmans was to deliver at the College of Preceptors on the "Scientific Study of Human Nature." How he took the manuscript to Spencer, and what Spencer thought of it, are related by Dr. Youmans (28 September):

I arranged to call to-day at eleven to read my production to him. With my tail feathers spread and in a state of infinite complacency I went, and returned trailing my glories in the infernal London mud. Poor man! What could he do? There was but one thing to do, and he did it, you had better believe. Faithful indeed are the cruelties of a friend. My lecture was fairly slaughtered. I had such nice authorities for everything. What are "authorities" to Herbert Spencer. The pigs went to the wrong market this time. "A little too much effort at fine writing"—forty-five pages. "You have lost your point at the fifth page and not recovered it. Why, I thought you wished to make a sharp presentation of science in its bearings upon the study of human nature, and you seem to have entered upon a systematic treatise on physiology interlarded with bad psychology." The unfeeling wretch! "Strike out half, put the rest in type and work it up," was the final injunction.²

¹ *Edward Livingston Youmans*, p. 220.

² *Ibid.*, pp. 223 and 451.

In March, 1867, Spencer took up what he calls the "agreeable task" of reorganizing *First Principles*. As the earlier portions could be done out of London it occurred to him to take a short holiday in Paris.

TO E. L. YOUMANS.

3 May, 1867.

I start for Paris on Sunday (very glad so to utilize that day) ; and expect to remain away ten days or so—taking a little work with me to revise, but devoting myself mainly to sight-seeing.

7 June.—I went for a fortnight, and came back before the week was out. Perpetual sight-seeing soon became a weariness ; and I was heartily glad to get back.

19 June.—The second edition of *First Principles* is working out very satisfactorily—even more satisfactorily than I had anticipated. In its reorganized form it will be extremely coherent all through—the thread of the argument will be unbroken ; and it will, I think, have the obvious character of completeness.

The present seemed a favourable opportunity for introducing a distinctive general title for the series. In his letter to Dr. Youmans of 7 June, he mentioned that the evils arising from the want of such a title had just been thrust upon him afresh by the new edition of Lewes's *History of Philosophy*. "The Positive Philosophy will continue to be understood as the philosophy of Comte, and as I so distinctively repudiate the philosophy of Comte, it is needful to take some step to prevent the confusion. So long as there is no other title in use to express a philosophy formed of organized scientific knowledge, one cannot expect people to discriminate." Fearing that, in giving his reasons for adopting the new title, he would make it the occasion for emphasizing afresh his antagonism to Comte, Dr. Youmans advised him to avoid the Comte discussion in the preface to the revised edition. Before the book was published in the autumn he had given up, though with reluctance, the idea of using the new title. "I discussed the matter with Huxley and Tyndall, and though I do not think that the objections raised were such as to outweigh the manifest advantages, still there doubtless are objections ; and in the midst of conflicting

considerations I eventually became so far undecided as to let the matter stand as it was."

In the midst of the anxieties arising from the prospect of having to relinquish his work, there came in April, 1866, the shock of his father's sudden illness and death. What this signified to him can be understood only by one who has, like the present writer, read the correspondence between father and son, carried on for three and thirty years. It must have seemed to Spencer a cruel fate that the premature abandonment of the System of Philosophy should so nearly coincide with the loss of one who had watched over its inception and been consulted in every detail. Although not indebted to his father for the leading doctrines of evolution, he was largely his debtor for the intellectual discipline which had made it possible to plan and so far elaborate his scheme, as well as for literary and expository criticism step by step as each chapter passed through the press. In the soundness of his father's critical judgment, he retained to the last the greatest confidence.

Hardly had he recovered from the shock of his father's death when he had to face the loss of his patient and gentle mother, who died in May, 1867. This event, although it did not come upon him, like the death of his father, with little warning, and although it did not mean the ending of an intellectual companionship which had been for so many years a precious possession, appealed nevertheless in a special manner to the emotional side of his nature. He knew that his mother had little sympathy with his intellectual pursuits, but he also knew that his welfare and happiness ever held a chief place in her thoughts, and that no sacrifice on her part would have been grudged, if by it she could have promoted his interests. The death of his father deprived him of one with whom he had walked in the closest communion of thought since boyhood; by the death of his mother he was bereft of one in whom he saw embodied in no small measure those feminine affections for which, as he repeatedly tells us, his nature craved.

For years the health of his parents had been a source of increasing anxiety. As for his mother, he had long given up the hope of arresting, even if only for a time, the downward progress towards confirmed invalidism. Unlike

his father, whose chronic nervousness tended to aggravate his disorders, his mother paid too little attention to premonitory symptoms, and neglected ordinary precautions. With his father, his endeavour generally was to convince him that he was not so ill as he thought he was; with his mother his aim was to get her to realise that she was worse than she thought she was. The ups and downs of his father's health had their sources in the condition of the nervous system which led to acts of imprudence when he was well, and to undue depression when he was ill. His mother's permanent ill-health was due to overdrafts on a system of low vitality. Her conservative obstinacy was proof against advice and remonstrance. The difficulty with his father was not occasioned by obstinate adherence to an adopted course of living, but to ever-changing views regarding his numerous ailments, and endless experimentation in the matter of diet, clothing, and therapeutics. Concern for his father, keen while it lasted, had months of respite, during which he felt at ease, provided that no unforeseen imprudence was committed. But as regards his mother his anxiety had never been relieved by periods of hopefulness or satisfaction.

These remarks have been suggested by reperusal of his letters home during the last eight or ten years of his parents' lives: letters full of advice, expostulation, and entreaty. Here are a few characteristic extracts—mostly to his father.

It would almost seem as though you acted upon the maxim—
“Of two evils choose the greater.”

I think you are wrong in taking such liberties with yourself as you describe. . . . I wish you would be less particular about small risks and more particular about great ones.

All the reasons you name for *not coming* are so many reasons for *coming*. You are evidently nervous, and as usually happens with you in such condition, make mountains out of mole-hills. The various things which you say you want to settle, leave unsettled; and settle them on your return. In your present state, you are not a fit judge of what is best for you. Therefore, do just what I tell you. Pack up your carpet-bag and write me word by return of post at what hour on Sunday or Monday I shall meet you at King's Cross or Euston Station.

It is useful in this life to tolerate annoyances, and to think as

little about them as possible. Everyone has lots of things to worry him. . . . In respect to present arrangements of the household, we must be content if matters can be made to go with some approach to regularity and smoothness. It is out of the question under such circumstances to avoid small evils.

You did not tell me when I asked you some time since how you were going on in respect of money. Pray do not borrow from anyone, but let me know if you fall short.

I think you had better give up your lessons. It will be very bad policy to make yourself ill over them. You must do as much teaching as will amuse you, but no more.

If (this to his mother) I thought it would be any good I would say a good deal in the way of exhortation that you should take care of yourself. But you are so incorrigible in the matter, that I expect you will do much the same whether I expostulate or not. I fear that nothing I can say will have any effect. I can only hope that you will behave better in this respect when I am absent than you do when I am present.

I hope (he writes to his father) you will *insist* on her not exerting herself by making the needless journeyings into the town which she does. They ought not to be allowed, whatever may be the reasons she assigns ; for she will make any reason a sufficient one.

I am sorry to hear that my mother is becoming still feebler, and still persists in over-exerting herself. There is no remedy but *positive prevention*—using as much peremptoriness as may be needful.

You must do what you can to prevent her from fidgeting herself, and make her feel that it is *better to let things go a little wrong, rather than make herself worse by trying to keep them right.*

Your accounts of my mother are depressing ; but I fear it is needful to reconcile ourselves to them, and to the expectation of such symptoms becoming more decided. You are doubtless right in thinking it may be needful to have more assistance. Pray do so whenever it seems requisite ; do not let expense be a consideration.

It is sad to hear the accounts of my mother, though what you tell me is not more than what was to be expected. We may, however, be glad she is free from pain and is usually in pretty good spirits. This is as much mitigation as can well be looked for. . . . Give my love to my mother, and you may add that she will probably see me shortly.

The following appears to be the last letter received from home.

FROM HIS FATHER.

9 April, 1866.

With regard to myself I am mending but slowly, if any. I seem to tire sooner, but so long as it does not get like the other attack, I shall do. I liked your proofs very much and hope my memoranda may not annoy you. When shall I be favoured with the next? . . . Your mother wonders from day to day that you don't come to see her. [He had just been to see her, but her memory was gone.] My back aches, so excuse more.

It has been the fashion to speak of Spencer's character as if it were all intellect and no feeling. The falsity of such an opinion was, however, well known to his friends. No one who knew him at all doubted his absolute sincerity when giving expression to his feelings; but even those who knew him intimately were apt to underestimate their intensity. Of the purity and depth of his affection for his parents, his letters, written during a period of more than thirty years, furnish a testimony that is conclusive. Some might think these letters lacking in gushing terms of endearment. But it is unsafe to draw conclusions as to the strength or the weakness of the emotions from the language employed to express them, unless account be taken of the character of the writer. In Spencer's case it would be a mistake to conclude that his feelings were of a low degree of intensity because he gave expression to them in subdued terms. Some people unintentionally use the strongest terms in the language to express the most ordinary degrees of emotion. Others deliberately try to conceal the shallowness of their feelings in a turbulent torrent of superlative words. Spencer's dislike to exaggeration led him, of two or more ways of expressing his feelings, to choose the least highly coloured. Add to this his singular sincerity, which would not brook the use of language to conceal or miscolour his sentiments. And after all, mere emotional display of interest in the welfare of others is a form of sympathy which costs the giver as little as it benefits the receiver. In reading the letters to his parents, in which he enters into the minutest details regarding bodily ailments, or family misunderstandings, or business misfortunes, or mental distress, one cannot

help thinking how much easier it would have been to have contented himself with offering the usual sympathetic platitudes. Time and trouble spent in trying to put right what he considered wrong were never grudged; no detail was too wearisome. If the emotional manifestations of sympathy were more subdued than usual, the rational considerations involved were dwelt upon with a minuteness and care rarely met with. Writing home was never a perfunctory duty coming round at stated intervals. The most remarkable feature of the correspondence is the revelation it affords of the closeness of the communion of thought and feeling between him and his father. With a qualification this holds true as regards his mother also. For, though aware that she took little interest in his writings, he kept nothing back from her. His almost invariable custom was to send home all letters he received, whether relating to his writings, to his plans and prospects, or to his social engagements. In this way his father and mother were kept informed of every detail of his life. This openness on his part was reciprocated by a like openness on theirs. Rare indeed are the instances in which father and son have laid bare their minds so freely to one another. Rarer still are the instances in which father and son have for over thirty years carried on their correspondence on such a high level of thought and sentiment. Fortunate it has been for the writer of these memoirs that the son was so unsparing with his letters, and the father so careful in preserving them. They have been the main sources of information down to this time.

CHAPTER XII.

PSYCHOLOGY AND DESCRIPTIVE SOCIOLOGY.

(July, 1867—October, 1872.)

THE holiday of 1867 embraced Yarrow, Glenelg, Ardtornish, Scarborough, Stourbridge, and Standish, followed by a walking tour in Surrey with Mr. Lewes, in the course of which, passing through Weybridge, he introduced his companion to the family of Mr. Cross, afterwards to become the husband and biographer of George Eliot. His own acquaintance with Mr. Cross dated from 1858.

Views expressed in *Social Statics* had led him to be looked upon as a supporter of the admission of women to the suffrage. Hence a request from Mr. J. S. Mill in August to join a society about to be formed to promote that object. Some two months before this Miss Helen Taylor had requested permission to include, in a series of papers she was bringing out, the chapter in *Social Statics* on "The Rights of Women."

TO J. S. MILL.

28 May, 1867.

You will, I am sure, understand that in the course of the seventeen years that have elapsed since *Social Statics* was written my thoughts on various of the matters it deals with have assumed a more complete form; and you will, I doubt not, sympathize in my reluctance to have reproduced in their original shape, any of them which I should now present in a better shape. At the same time, . . . I cannot, without too much deranging my plans, undertake to re-write the parts with which I am dissatisfied.

Had he been more explicit in the above letter it would have been known how far he had receded from the position held when *Social Statics* was written, and Mr. Mill would

have understood that it was futile to ask him to join the proposed society. He had now to make his position clear.

TO J. S. MILL.

YARROW, 9 August, 1867.

Your note has reached me here, where I have been spending a few days with Prof. Masson on my way north.

Probably you will remember that in a letter some time since, written in reply to one of yours, I indicated that my views had undergone some modification since the time when I wrote *Social Statics*. The modification goes as far as this, that while I should advocate the extension of the suffrage to women as an *ultimate* measure, I do not approve of it as an *immediate* measure, or even as a measure to be shortly taken. I hold, as I doubt not you also hold, that political liberties or powers, like that of voting, are simply means to an end. That end, you would probably say, is the securing of the good of the individuals exercising such powers; or otherwise, as I should say, it is the securing the greatest amount of individual freedom of action to them. The unhindered exercise of faculties by each, limited only by the equal claims of others, is that which the right of voting serves to obtain and to maintain. This is the real liberty in comparison with which right of voting is but a nominal liberty.

The question with me then is: How may this substantial liberty to pursue the objects of life with least possible restriction be most extended? And as related to the matter in hand the question is: Will giving the suffrage to women, which is in itself but a nominal extension of liberty, lead to a real extension of liberty.

I am decidedly of opinion that it will not. The giving of political power to women would, I believe, restrict, and indeed diminish, liberty in two ways. It would strengthen the hands of authority, both political and ecclesiastical; for women, as a mass, are habitually on the side of authority. Further, it would aid and stimulate all kinds of state administrations, the great mass of which are necessarily antagonistic to personal freedom. Men in their political actions are far too much swayed by proximate evils and benefits; and women would be thus swayed far more. Given some kind of social suffering to be cured or some boon to be got, and only the quite exceptional women would be able to appreciate detrimental reactions that would be entailed by legislative action. Political foresight of this kind, uncommon enough in men, is extremely rare in women.

Of course, whoever holds that the minds of men and women are alike, will feel no difficulty of this kind. But I hold them to be unlike, both quantitatively and qualitatively. I believe

the difference to result from a physiological necessity, and that no amount of culture can obliterate it. And I believe further that the relative deficiency of the female mind is in just those most complex faculties, intellectual and moral, which have political action for their sphere.

When the State shall have been restricted to what I hold to be its true function—when it has become practically impossible for it to exceed that function—then it will be alike proximately and remotely equitable that women should have political power.

To put the right construction on these reasonings of mine, you must bear in mind that to me the limitation of the functions of the State is the question of questions, in comparison with which all other political questions are trivial; and that to me electoral changes and other changes in forms of government are of interest mainly as they promise to make men freer, partly by the removal of direct injustices, and partly by the removal of those indirect injustices which all undue legislative action involves.

I greatly regret not to be able to coincide with you on this matter; and the more so because I recognize the nobility of your motive, and, could I reconcile it with my conscience, would fain follow your example.

Two years later he had an opportunity still further to explain his views.

TO J. S. MILL.

9 June, 1869.

Thank you for the copy of your essay on *The Subjection of Women*. . . .

Meanwhile I will just remark that I think the whole question, under its social and political aspects, is being discussed too much upon the assumption that the relations among men and women are determined only or mainly by law. I think a very trenchant essay might be written on the *Supremacy of Women*, showing that, in the present state of civilization, the concessions voluntarily made by men to women in social arrangements have become an organized set of laws, which go far to counterbalance the laws that are legally enacted; and that throughout a large part of society the tyranny of the weak is as formidable as the tyranny of the strong.

Mr. Mill was in full agreement with Spencer in thinking "that in a great many cases women tyrannize over men," and "that it is generally the best of men who get most tyrannized over. But . . . two contradictory tyrannies do not make liberty."

He returned to town in the beginning of October, eager to commence the revision of the *Principles of Psychology*, about which he says in a memorandum :—

Nominally, this was a second edition, but it was more nearly to be regarded as a new work ; for besides the fact that sundry of the parts were considerably further developed, there were four divisions which did not exist in the work as originally published. . . . This I had now to execute, and entered on the task with considerable zest ; for I had much interest in what I saw would be the working out of the harmony between these further views and those previously enunciated.

I had a further satisfaction in the preparation of an edition more completely developing the general views which I first had set forth, since there was now a widely different attitude in the public mind in relation to this view from that which existed when the first edition was published. In 1855, this view got scarcely any attention, and what little it did get brought upon me little else than vituperation. The tacit assumption, and towards the close of the work the avowed belief, that all organisms had arisen by evolution, and the consequent conception running throughout the whole work that the phenomena of mind were to be interpreted in conformity with that hypothesis necessarily, in 1855, roused not sympathy, but antipathy. It was only after the publication of Mr. Darwin's *Origin of Species*, some four years subsequently, and only after this work, drawing so much attention—causing so much controversy—began presently to affect deeply the beliefs of the scientific world, that the views contained in the *Principles of Psychology* came to be looked at more sympathetically. . . . Not, however, that the book began at once to get that credit which had been originally withheld ; for now, with this change in the current of opinion, there came other books setting forth this advanced view, and which, with the change of the times, were sympathetically received. Especially was this so with the work of Dr. Maudsley on the *Physiology and Pathology of the Mind*, which, proceeding throughout on the evolution view of mind, and adopting the cardinal conception of the *Principles of Psychology*, without at all indicating whence that conception was derived, was reviewed with applause and had a great success. In now returning to the *Psychology*, therefore, for the purpose of further developing it, I had the consciousness that something would be done towards rectifying the arrangement in which I had got all the kicks and others the halfpence.

He was also looking ahead to the time when he would enter upon the sociological portion of his scheme, for which ample provision would have to be made. His changed

circumstances, there being now no one dependent on him for support, led him to take a step he had for some time deemed desirable, namely, to get this preparatory work done by deputy. He took counsel with Professor Masson, on whose recommendation the present writer was engaged as secretary. Simultaneously, therefore, with the commencement of the *Psychology* he began to put into shape his idea of "making tabulated arrangements of historical data, showing the co-existence and succession of social phenomena of all orders." For an hour or so before dinner he would listen while his secretary, pencil in hand, read from books of travel. "Mark that," he would say when anything pertinent was met with. After familiarity with the work had in this way been gained, the present writer was left largely to his own discretion. One of the instructions was characteristic. Impressed with the magnitude of the undertaking and the time it would occupy, he urged his secretary to avoid reading many books. "If you read, say, three trustworthy authors on any one tribe, that will be enough." This instruction had tacitly to be disregarded. For, in addition to the considerations that few travellers had the ability to note "social phenomena of *all* orders," that many had no interest in certain aspects of savage or semi-civilized societies, and that others lacked opportunities for studying any but the most superficial features of a community, there was the obvious reflection that a traveller's trustworthiness could ordinarily be ascertained only by the perusal of what he had written. Spencer's own account of these preparatory occupations is as follows :—

Some little time was passed in elaborating a method of classification, for it did not prove easy to devise any method of presenting all the phenomena of society in a form at once natural and methodic. But eventually I pretty well satisfied myself as to the system of arrangement, and by the time Mr. Duncan had been familiarized, by reading aloud to me and receiving the needful suggestions, with the nature of the work to be done and the heads to be filled up, I finally decided upon a form of table for the uncivilized races, and had it lithographed in blank form with the headings of columns. All this was done simply with the intention of having prepared for my own use the required materials. . . . But when some of the tables had been filled up and it became possible to appreciate

the effect of thus having presented at one view the whole of the essential phenomena presented by each society, the fact dawned upon me that the materials as prepared were of too much value to let them lie idle after having been used by myself only. I therefore decided upon publishing them for general use. Thereafter Mr. Duncan did his work in the consciousness that it would be not lost in the fulfilment of a private end merely, but that he would have the credit derivable from it on publication. And thus was initiated *Descriptive Sociology*.

With the year 1868 came an attack "of greater nervousness than usual." Hence the question put to Professor Tyndall: "Do you know any lively, pleasant fellow who would make a good travelling companion?" Rackets, played in a court attached to a public house in Pentonville, was resorted to again. Having never played before, the present writer was, if not a very formidable, a very exasperating antagonist; an ill-directed ball not infrequently disappearing among the neighbouring houses, to be presented a few minutes later by a messenger claiming compensation for a broken window. But Spencer took it all—broken windows and poor play—in good part. After some twenty minutes he would sit in one of the adjoining sheds and dictate for about the same length of time; then another game, and so on during the forenoon. On one occasion Professor Tyndall was persuaded to come. There was a look of amused incredulity when Spencer told him that the *Psychology* was being written in such a piecemeal fashion and amidst such unattractive surroundings. If the day was unsuitable for rackets, billiards would be resorted to. In warmer weather he would betake himself to the Serpentine, where the forenoon would be spent in rowing and dictating by turns, or to Kensington Gardens, where short periods of dictation while sitting under a tree would be relieved by short periods of walking. With his election to the Athenæum early in 1868 a new source of enjoyment was opened up.

Having waited in vain for a "lively, pleasant fellow" as a travelling companion, he made up his mind by the end of February to start for Italy alone. About this tour enough has been written in the *Autobiography* (ii., 178-98). He was back by the middle of April not much better—"too

idle and out of spirits to write letters," but hoping that, if unable to do much work, he and his secretary might "at any rate get through some reading."

The supervision of the preliminary sociological work and the state of his health furnish only a partial explanation of the slow progress of the *Psychology*. His good resolutions notwithstanding, he was continually being drawn aside from regular work. One such interruption arose out of a lecture, delivered while he was in Italy by Mr. Kingdon Clifford at the Royal Institution, "On some of the Conditions of Mental Development." Thinking that the lecture conveyed an erroneous impression as to the authorship of the doctrines discussed, he consulted Professor Tyndall.

TO JOHN TYNDALL.

11 May, 1868.

[The lecture contains] nothing more than brief and popularized statements of some of my already published doctrines. . . . My impression is that there is scarcely a proposition, save quite familiar ones, that is not to be found somewhere or other in my book, either in the same shape or some kindred shape. . . . I feel it the more necessary not to let this occurrence pass without notice, because by it, and by another occurrence of kindred nature, I am put in a very disagreeable position. . . . I am now so placed that in reproducing some of my own ideas I shall run the risk of being supposed to have appropriated the ideas of others. The circumstances are these. There was published last year, by Dr. Maudsley, a book on the *Physiology and Pathology of the Mind*. . . . Dr. Maudsley is now being cited as the authority for these doctrines which he appropriates from me. . . . See, then, my predicament. I am beginning to prepare a second edition of the *Principles of Psychology*, in which these doctrines that are being widely diffused in connection with other men's names will reappear. . . . The reproduction of my own thoughts will render me liable to the charge of plagiarism!

When the matter was brought to Mr. Kingdon Clifford's notice, he settled it to Spencer's satisfaction by means of a letter in the *Pall Mall Gazette* of June 24.

Part of the autumn holiday of 1868 was spent at Inveroran, where he had good fishing on the Orchy. In a memorandum he compares, with almost boyish satisfaction, his own success with that of two others staying at

the hotel; one of whom "got up at 5 o'clock, and fished all the pools before us (Scotchman like!)." His stay was, nevertheless, cut short for the characteristic reason that he got "quite sick of the food, so wanting in variety."

Towards the end of the year his work was again interrupted. In an article on "Philosophical Biology" in the *North American Review*, for October, Mr. F. E. Abbot examines Spencer's answers to certain fundamental questions. To the question—What is the origin of life? "We find no definite reply of any sort in the volumes before us." To the question—What is the origin of species? Mr. Spencer "returns substantially the same answer as Mr. Darwin." To the third question—What are the causes of organic evolution? Mr. Spencer traces it entirely to mechanical and physio-chemical forces, and recognizes no force or forces to be called *vital* in any special sense. And yet he makes the very assumption which he condemns, namely, that of an "inherent tendency, or power, or aptitude," or an "organic polarity." He abandons the mechanical theory and practically adopts the vitalist theory. These criticisms led to what Spencer calls, in one of his memoranda, "a small controversy."

I had been charged with inconsistency because I did not accept the current doctrine of spontaneous generation, which was supposed to be not only harmonious with the doctrine of evolution as I held it, but was thought to be a part of it, which I was in consistency bound to adopt. Feeling that there was some ground for the representations made, I had to write a rejoinder, explaining my view of this question; and, as commonly happens, strove to get this done by a fixed day, thinking that I could just do this before I rested. It is always these efforts to achieve some proximate end before leaving off that do the final mischief; for nearly always the threatened mischief comes before this proximate end is reached. It did so in this case. I had to break away from my work and leave town; going first of all to Malvern, then afterwards to Ben Rhydding.

Spencer's reply (the facts and arguments of which had "the unqualified endorsement of Huxley, Tyndall and Frankland") did not appear in the *North American Review*, as he intended it should.¹ Dr. Youmans, reflecting prob-

¹ *Principles of Biology*, i., Appendix D, p. 696.

ably American opinion, told him the reply was "of no value for the general public—they cannot understand it. . . . Now what I wish is to take up the subject myself and put it so that they can understand it, always provided that I can arrive at a proper understanding of it myself. But before going into it I should like to hear whether you consider that the inquiry has at all changed aspects. Is it, in fact, needful for you to commit yourself to either side of the question as at present contested?"

TO E. L. YOUMANS.

16 March, 1869.

Respecting the reply to the *North American Review*, you need not be alarmed at the aspect of the "spontaneous generation" question, and the way in which I have committed myself upon it. Huxley has lately been experimenting on the matter, and reaching remarkable results; and though he says that they confirm some of the observations of Pouchat, he considers that they make the hypothesis of "spontaneous generation" more untenable than ever.

It will be very well to have the ideas . . . popularized, if, as you say, it is not comprehensible to the generally intelligent. But I should like the reply as it stands to be made accessible for the benefit of such as can understand it.

While his prudence stood him in good stead in preventing him from joining the newly-formed Metaphysical Society, for the atmosphere of which his temperament would have proved ill-suited, it failed to prevent him from going out of his way, about the middle of 1869, in an endeavour to remove the anti-British feeling which had prevailed in the United States since the Civil War. This was not a sudden resolve; for, as far back as 1866, he had endeavoured to get Mr. Moncure Conway to take the matter up. Those who have read the *Autobiography* are aware of the circumstances which led him to write the letter, as well as of the reasons that induced him to withdraw it in deference to the strongly expressed disapproval of his American friends.¹

When he returned from Scotland towards the end of September, 1869, he had barely reached the middle of the first volume of the *Psychology*. He would fain

¹ *Autobiography*, ii., 210, and Appendix E, p. 497.

have gone to Switzerland to see Professor Tyndall, who was laid up in consequence of an accident; but "I have been idle for so long a time, I am anxious to get some work done." It is questionable whether such an extension of his holiday would have added to his working power; for he was wont to say that Tyndall's "infectious vivacity" was too exciting. By sticking to work he succeeded in issuing three instalments of the *Psychology* between December, 1869, and March, 1870, inclusive, thus affording a prospect that the remaining part—"Physical Synthesis"—although it covered entirely new ground, would be finished before the autumn holiday. This holiday of 1870 was longer and more varied than usual. It included ten days at the Argoed, near Monmouth, with Mr. and Mrs. Potter; a fortnight with the Lotts on the north coast of Wales; and a visit to Ireland and to Scotland. Of Ireland he wrote: "I spent three days in Dublin, which has things in it worth looking at. But I found the living bad—slovenly and dirty. . . . Having heard that they had room at Inveroran, I determined to go there by way of Belfast. Belfast I found worse than Dublin—the most stinking place I was ever in, indoors and out; and I was glad to get away as quickly as possible." A ramble with Professor Tyndall in the Lake District, after the meetings of the British Association in Liverpool, was an enjoyable ending to his holiday.

In the expectation that the "Physical Synthesis" would be completed before he went away he had been disappointed. It "is very difficult to treat satisfactorily. But I see that it will form a very important addition to the general argument." The volume was published in December—fully three years after the revision had been begun. This seemed a favourable opportunity for carrying out his intention of dedicating the System of Philosophy to his American friends. Dr. Youmans was, of course, consulted: a proof of the proposed dedication being sent for suggestions. To his great surprise the proposal was strongly disapproved of. While it "no doubt would please American vanity amazingly," "it would be unjust to your sincere friends in other countries." Thereupon he cancelled the dedication and ordered the type to be distributed.¹

¹ *Edward Livingston Youmans*, p. 262.

The second volume of the *Psychology* progressed more rapidly than the first had done, five instalments being issued in 1871, and the three remaining in 1872. The forecast given in the following letter was to prove very far from correct. Twenty-four, instead of twelve, years were needed to finish the Synthetic Philosophy.

TO E. L. YOUMANS.

12 October, 1872.

I have just finished the second volume of the *Psychology*. . . . I find on looking back that it is just twelve years since I commenced. Having now got half through, it might be inferred that it will take another twelve years to finish. I have reason for hoping, however, that ten will suffice. Considerably more than two years, I believe, have gone in interruptions—partly due to occasional relapses of health, partly to the second edition of *First Principles*, partly to various incidental essays and articles, and partly to the arrangement and superintendence of the *Descriptive Sociology*, which, during the earlier stages, occupied much time. Indeed, now that I put them down, these interruptions account, I think, for more than two years' loss of time. As I am much better now than I was when I commenced, and as I do not see the likelihood of much incidental writing hereafter, I am inclined to hope that, after completing the *Study* [*of Sociology*], ten years will suffice to carry me through.

The other main occupation during those years—the superintendence of the *Descriptive Sociology*—was disturbed in 1870 owing to his secretary (the present writer) going to Madras. Having been led by Dr. Youmans to believe that there were many young men with the requisite qualifications who would gladly undertake the work, his disappointment was all the keener when he failed to find one. After endeavours continuing for nearly a year, he secured the services of Mr. James Collier. The printing of the extracts and tables had not gone far, when the cost began to look serious, partly owing to the manuscript being sent to the press in the original rough draft. Before leaving for India the present writer had drawn attention to the fact that the manuscript was not in a fit state for publication, and had suggested taking it with him for revision; but the risk was thought too great. One may wonder that, in view of the cost so far exceeding his expectations, he did not suspend

the work altogether. Instead of that he was on the lookout for a third compiler to undertake the *Extinct Civilized Races*. Through Mr. Lewes, towards the end of the year, he, for this work, entered into an arrangement with Dr. Scheppig.

The supervision of the *Descriptive Sociology* had, from time to time, suggested interesting lines of thought, tempting him to turn aside from the *Psychology*. One of these was connected with the worship of animals, his conclusions being embodied in an article in the *Fortnightly Review* for May, 1870, in which he sought to answer the question, "how primitive men came so generally, or universally, to believe themselves the progeny of animals, or plants, or inanimate bodies." Another line of thought led to the strengthening of previously formed convictions regarding the origin and growth of moral opinions and sentiments.

TO E. L. YOUMANS.

3 March, 1871.

I am about, after getting rid of this forthcoming part, to make another short parenthesis in my work. The representations of my doctrine respecting the genesis of moral sentiment, which Mr. Hutton made in *Macmillan's Magazine* about a year ago, have been spreading through other channels, and I find it needful to put a stop to them. I had intended to let the matter stand over until I came hereafter to deal with it in the course of my work : but Mr. Hutton will now have to pay the penalty a good deal sooner. I am going to prepare the article for the next *Fortnightly*.

TO CHARLES DARWIN.

3 March, 1871.

What I have read [of the *Descent of Man*] has surprised me by the immense accumulation of evidence, interesting in itself and doubly interesting by its implications, which you have brought to bear on the questions you discuss. I had no idea that such multitudinous proofs of the action of sexual selection were forthcoming.

I am glad that you have so distinctly expressed your conviction on the more special question you treat. It will, I doubt not, raise afresh the agitation on the general question ; since many who have in a considerable degree reconciled themselves to the conception of evolution at large, have never had represented to them, in a positive way, these ultimate implications

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 “Pangenesi,” 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.

most ingenious and remarkable theories ever put forth on a question of Natural History," and as "a most promising line of research."

Such were the opinions of "experts." Here is the opinion of one who, if not an expert in any branch of natural science, was one of the leading thinkers of the time.

FROM J. S. MILL.

2 December, 1868.

This I may say, that I have seldom been more thoroughly impressed by any scientific treatise than by your *Biology*; that it has greatly enhanced my sense of the importance of your philosophical enterprise as a whole; and that, altogether apart from the consideration of what portion of your conclusions, or indeed of your scientific premises, have yet been brought into the domain of proved truth, the time had exactly come when one of the greatest services that could be rendered to knowledge was to start from those premises, simply as a matter of hypothesis, and see how far they will go to form a possible explanation of the concrete parts of organisation and life. That they should go so far as they do, fills me with wonder; and I do not doubt that your book, like Darwin's, will form an era in thought on its particular subject, whatever be the scientific verdict ultimately pronounced on its conclusions; of which my knowledge of the subject-matter does not qualify me to judge.

Academical honours were of no value in his eyes, except as indications that his work was appreciated. He declined to be put forward for the Lord Rectorship of the University of St. Andrews, or to be nominated for the degree of LL.D.,¹ or to accept an honorary membership of the St. Andrew's Medical Graduates' Association. To the Secretary of the Association he stated his reasons in full (December 16, 1871).

Some years ago, while occupied in biological enquiries, I should doubtless have been able to make much use of the advantages which such an election would have given me; but I fear that, as now my studies lie almost wholly in other directions, these valuable facilities will be almost thrown away upon me. Doubtless it is true that honorary memberships in such cases are not supposed to imply habitual participation,

¹ *Autobiography*, ii., 233.

either in the advantages or in the proceedings of the bodies giving them. . . .

Beyond the general objection I have to all names and titles that are not descriptive of actual function, there rises before me in this case an objection of another order, which will very possibly be regarded as no less peculiar. I see that one of the purposes of the Association is "the maintenance of the interests of the Medical Graduates of the University." . . . I think that very probably any public action the Association might take would be one I should disapprove. The doctrines I have long publicly held respecting the functions of the State and the liberties of the subject, are of a kind quite at variance with the policy pursued by the Medical Profession, when it has brought its combined power to bear upon legislation. . . . I fear that this letter will be regarded as a very ungracious response to the compliment which your Association has paid me. But, as I hope your Council will see, my course is one taken altogether irrespective of the particular circumstances. The principles I have indicated are principles long since adopted, and from which I have not hitherto swerved.

The French translation of *First Principles* was expected to appear early in 1868. When spring of the year following came without any sign he was "beginning to get a little anxious." About the middle of 1870 he found out that the delay was due in part to the difficulty experienced in preparing the prefatory note, which was growing to the dimensions of a volume. There were three points Dr. Cazelles wished to bring out: "To determine your place in the experimental school; to trace the evolution of your idea of Evolution; in fine, to mark the differences which separate your philosophy from the only scientific generalization known in France—the positivism of Comte." To this Spencer replied at great length in June, 1870, tracing the development of his thought. Being now in possession of the required information, Dr. Cazelles expected to have the translation published in July or August, little thinking of the disaster that was about to overtake his country.

TO E. CAZELLES.

10 March, 1871.

I have not endeavoured to communicate with you during this period of dreadful disaster for France that has elapsed since I last heard from you about midsummer, 1870. My silence has been partly due to the feeling that the entire

absorption of your thoughts and feelings by these unhappy events made attention to any other matters out of the question ; and partly to the belief that during this reign of confusion, a letter would very likely not reach you. Now that I do write it is more to express my sympathy with you in this time of national misfortune, than because any matter of business requires me to write ; for I conclude that, in the state of prostration under which France must for some time suffer, literary activity is likely to be almost entirely suspended. My chief hope is that when social order becomes fairly re-established and the corrupting effects of the Imperial *régime* partially got rid of, the result may be a turning of the national energies into more healthful channels.

The long-delayed translation at length saw the light during the troublous days of the early summer of 1871. Neither Dr. Cazelles nor M. Ribot could tell how the book had succeeded in escaping from Paris. The Introduction pleased Spencer greatly.

TO E. CAZELLES.

3 May, 1871.

The lucidity of your brief statements is admirable, and, in many cases, presents to me my own ideas with a freshness and neatness which gives them almost the effect of novelty. Indeed, I cannot better express the effect produced on me by what I have read, than by saying that it seems to me as though I were looking at myself in a glass, not having before known "what manner of man I was," as seen externally. This effect is in part due, I doubt not, to the comparative brevity with which you have sketched out the System of Philosophy in its essentials—so giving me, free from superfluous details, that which is habitually present to me under more involved forms, and in part to the quite different *order* in which you have exhibited these essentials. This new concatenation, considerably unlike that through which my thoughts habitually run, enables me to judge of the *ensemble* from a fresh point of view, and thus gives me an impression of it which I can look at as though it proceeded from some one else. . . . I am so struck by its lucidity and by the vividness due to a presentation of the main features in rapid succession, that I should like very much to have it diffused in a separate form. . . .

I should some time since have replied to your letter of March 21st, had not the occurrence of this dreadful second disaster in France [the excesses of the Commune] led me to suppose that a letter would probably miscarry. I wish I could do something towards mitigating that despondency which must accompany the view you take respecting the future of

France. I wish this the more because in the interests of civilization at large, as well as in the interests of France, I should be glad to find rational grounds for taking a more favourable view than that which you take. But, though I shall hope to see society in France re-organize itself in a more satisfactory form, I cannot with candour say that my hope is at all a sanguine one. It has seemed to me for these many years past that from some cause difficult to trace (race, or the particular mixture of races, being perhaps at the root) there has arisen an obstacle to further development. The nature of the social units seems to have become different from that required for a higher type of social structure, and, in fact, there seems to be no type that is suitable. In the *average* French nature there appears to be an intolerance of despotism along with an unfitness for freedom—or, at least, if these characters do not co-exist in the same individual, they co-exist in the individuals of the same society, and prevent that society from organizing itself into a type under which the units can co-operate harmoniously.

10 July.—I am glad to see affairs in France assuming so much better an aspect and promising tranquillity for some time to come, at least. I should have greater hope for the future were there not already so many indications that the dominant feeling is that of revenge, and were there not a consequent determination to still further exhaust the national resources by military preparations, and so to entail a further retardation, if not arrest, of social growth.

Unknown to Spencer, M. Ribot had already completed a translation of the original edition of the *Psychology*. It was now arranged to substitute a translation of the new edition. Meanwhile, to give the public some idea of the psychology of evolution, M. Ribot wrote, for *La Philosophie Positive*, an article which M. Littré had agreed to publish, but on seeing it drew back. "Without contesting the great merit of M. Herbert Spencer," wrote M. Littré, "there are between him and us differences so profound that we could not receive your article as it is." He was willing to publish the article, however, on condition that it were "transformed into an exposition pure and simple."

CHAPTER XIII.

THE STUDY OF SOCIOLOGY.

(December, 1869—January, 1874.)

THE adoption of a general title for the System of Philosophy, reluctantly laid aside in 1867, came up again in connexion with Mr. Fiske's lectures at the Harvard University, which were published as delivered in the *New York World*, then owned by Mr. Manton Marble. Spencer was gratified to know that his philosophy was to be expounded by a disciple so able and enthusiastic, but the title of the course was not to his liking.

TO E. L. YOUMANS.

4 December, 1869.

I am very much annoyed that he should have used the title he has done. . . . What he has called Positive Philosophy . . . has little or nothing in common with the philosophy of Comte ; for even the relativity of knowledge, joined with the *deliberate ignoring* of an unknown cause of phenomena is a quite different thing from the relativity of knowledge joined with the *deliberate assertion* of an unknown cause of phenomena. And while this general doctrine, which Fiske calls Positivism, is not what the Positivists mean by that title, it is a doctrine which is held by those who distinctly repudiate the name Positivists. So far as I can judge from his programme . . . a title, which is applied both by its adherents and opponents to one system of thought, he is applying to another system of thought, the adherents of which do not acknowledge the title. Among other evils, one result of this will be that all who wish to direct against the doctrine of Evolution such objections and prejudices as have grown up against the system of Comte, will be able to cite justification for doing this.

TO JOHN FISKE.

2 February, 1870.

If the word "Positive" could be dissociated from the special system with which he [Comte] associated it, and could be connected in the general mind with the growing body of scientific thought to which he applied it, I should have no objection to adopt it, and by so doing accord to him due honour as having given a definite and coherent form to that which the cultivated minds of his time were but vaguely conscious of. But it seems to me as the case stands, and as the words are interpreted both by the Comtists and by the public, the amount of correct apprehension resulting from the adoption of the word will be far out-balanced by the amount of misapprehension produced.

In so far as I am myself concerned, I still hold that the application of the word to me, connotes a far greater degree of kinship between Comte and myself than really exists. . . . Such elements of my general scheme of thought as you have brought into prominence as akin to those of Comte (such as the relativity of knowledge, and the de-anthropomorphization of men's conceptions) . . . *have been all along quite secondary to the general doctrine of Evolution, considered as an interpretation of the Cosmos from a purely scientific or physical point of view.* . . . If you bear in mind that *my sole original purpose* was the interpretation of all concrete phenomena in terms of the redistribution of matter and motion, . . . you will see why I regard the application of the word Positivist to me as essentially misleading. *The general doctrine of universal Evolution as a necessary consequence from the Persistence of Force is not contained or implied either in Comtism or in Positivism as you define it.*

By the end of the following year Mr. Fiske had come to the conclusion that Spencer was right "in refusing to accept the appellation 'Positivist' in any sense in which it is now possible to use the word. . . . I should like also to know what you think of the terms 'Cosmic Philosophy' and 'Cosmism.'" To these also Spencer objected. Thinking the time had now come to give effect to his former intention, he set aside the reasons that had been urged against the title "Synthetic Philosophy,"¹ and forthwith had a new general title-page inserted in the unsold copies of *First Principles, Biology, and Psychology*. His objections to "Cosmic" and his reasons for choosing "Synthetic" are set forth in a letter to Mr. Fiske in 1872 or 1873.

¹ *Supra*, p. 131.

To put my view in its most general form, I should say that a system of philosophy, if it is to have a distinctive name, should be named from its method, not from its subject-matter. Whether avowedly recognized as such or not, the subject-matter of philosophy is the same in all cases. . . . Though every philosophy is more or less synthetic, it seems to me that that which formulates and elaborates the "Doctrine of Evolution" is synthetic in so especial a manner that it may fitly take from this peculiarity its distinctive title.

It is synthetic as recognizing avowedly that philosophy is a synthesis of all knowledge—that which unifies the partial unifications achieved by the several sciences.

It is synthetic as uniting Science and Nescience as the correlative parts of an integral conception of the Universe.

It is synthetic as recognizing each derivative law of force as a demonstrable corollary from the ultimate law, the Persistence of Force.

It is synthetic as proceeding consciously to the interpretation of phenomena as caused by a co-operation of forces conforming to these derivative laws.

It is synthetic as proceeding to deduce from the general law of the redistribution of matter and motion the successive orders of concrete phenomena in their ascending complexities.

Further, it is synthetic under sundry more special aspects as combining and reconciling opposing views—as those of the transcendentalists and the experientialists.

And yet once more it is synthetic in its conception of the Universe as objective, since it regards the progress of things which brings about evolution as being itself a synthesis—a reaching of more and more complex products through successive increments of modification.

Mr. Fiske did not think "that Synthetic, any more than Cosmic, will apply, as a *distinctive* name to your philosophy. The differential mark of your philosophy is, not that it is Synthetic or that it is Cosmic, but that it is based upon the conception of Evolution as opposed to the conception of Creation."¹ The term Cosmic would, however, in Dr. Youmans's opinion, "probably come under popular use in this country. Nothing short of the Cosmic will satisfy the American spread-eagleism."

Into the project for an "International Scientific Series" Spencer entered with the utmost cordiality, the proposed arrangements seeming to "practically amount to inter-

¹ *Edward Livingston Youmans*, pp. 290-92, note †.

national copyright.”¹ The international character of the scheme gave occasion for the clashing of interests, the exciting of jealousies, unlooked-for delays, and many disappointments. After some six months negotiations in Europe Dr. Youmans found, on returning to New York, that American writers had to be propitiated. “There was unanimous and much bitter complaint on the part of the press at the absence of any American element, and it was urged upon me all round in the interest of the undertaking that the omission should be supplied as early as possible.” From the side of the public came complaints of overlapping of subjects, of over-prominence given to certain topics, and of inequality in the amount of matter. Even the size of the page agreed upon had to be strictly adhered to. “Books of that kind we cannot sell,” wrote Dr. Youmans, with reference to a proposal to introduce a larger paged book into the series. Altogether, the task which Professor Huxley, Professor Tyndall and Spencer took upon themselves when they agreed to act as a London Advisory Committee, proved by no means light. On Spencer from the very outset fell the burden of the Committee’s work.

Spencer’s hands being full, he had at first no idea of himself contributing to the series. But for several years he had been impressed with the necessity of preparing the way for Sociology by an exposition of the method by which, and the spirit in which, the phenomena of society should be studied. Failing to find any one to do this, or to collaborate with him in doing it, he at last yielded to Dr. Youmans’s persistent suggestion that he should write the book himself and include it in the Series. The result was an arrangement that the *Study of Sociology* (the name to be given to the book) should first appear as articles in the *Contemporary Review*, with simultaneous publication in an American periodical, about the standing of which he was very particular.² No sooner had Dr. Youmans seen the first article than he made up his mind to start a magazine (the *Popular Science Monthly*)

¹ *Edward Livingston Youmans*, pp. 266-94. *Autobiography*, ii., 227, 230-2.

² *Edward Livingston Youmans*, p. 295.

forthwith, with this article in the opening number. Spencer was rather taken aback to find that the articles got so little attention in the United States. Of the chapter on "The Bias of Patriotism," he writes :—

As its contents are varied, and part of it has a considerable interest distinct from that of the argument in general, its length will perhaps not be objectionable. Especially, I suppose, the castigation of Arnold will excite some attention. . . . You will see how the sales of my books are increasing. If things go on thus, I shall make a fortune by philosophy.

For the chapter on "The Political Bias," he had asked Dr. Youmans to send him "a supply of typical illustrations of the way in which your political machinery acts so ill—its failures in securing life, property and equitable relations. I want to use the case of America as one among others to show how baseless is the notion that the form of political freedom will secure freedom in the full sense of the word."

Mr. Martineau's article in the *Contemporary Review* for April, on "The Place of Mind in Nature, and Intuition of Man," caused a brief interruption in the *Study of Sociology*, for the purpose of writing a reply. To this he refers in the course of a letter to Dr. Youmans: "I have just had a very enthusiastic letter from Darwin¹ about the article, which is, of course, satisfactory; for I feel since the article was published that he might think I ought to have referred to him personally in connexion with the doctrine defended."

TO CHARLES DARWIN.

12 June, 1872.

I cannot consent to let your letter pass without saying how much gratified I am by your approval. I should very well have liked, had time permitted, to deal somewhat more fully with the metaphysical part of Mr. Martineau's argument. If, as I expect him to do, he makes some reply, it will probably furnish the occasion, after an interval, for a fuller exposition; by which I hope to make clear to quite ordinary apprehensions, the absolute emptiness of all such propositions as that with which Mr. Martineau deludes himself and his readers.

¹ *Life and Letters of C. Darwin*, iii., 165.

TO J. E. CAIRNES.

21 March, 1873.

At present I have done nothing more than just dip into your essay on *Laissez-faire*. Without being quite sure that I seize your meaning exactly, I feel inclined to object to that current conception of *laissez-faire* which you appear to accept and argue upon. You say that "the able men who led the agitation for the repeal of the Corn Laws promised much more than this. They told us that the Poor Laws were to follow the Corn Laws; that pauperism would disappear with the restrictions upon trade, and the workhouses ere long become obsolete institutions."

Now as a Poor Law is itself a gross breach of *laissez-faire*, in what I conceive to be the true meaning of it, this passage seems to me to be tantamount to an expression of disappointment that obedience to *laissez-faire* in one direction has not cured the evils caused by continued disobedience to it in another direction.

I do not think that *laissez-faire* is to be regarded simply as a politico-economical principle only, but as a much wider principle—the principle of letting all citizens take the benefits and evils of their own acts: not only such as are consequent on their industrial conduct, but such as are consequent upon their conduct in general. And while *laissez-faire*, as I understand it, forbids the stepping between these private acts and their consequences, it is quite consistent with the doctrine that a government should, far more effectually and minutely than at present, save such individual from suffering evils or claiming benefits due to the acts of others.

About this time he became acquainted with Mr. Arinori Mori, the Minister of Japan to the United States. "He came," says Spencer, "to ask my opinion about the re-organization of Japanese institutions. I gave him conservative advice — urging that they would have eventually to return to a form not much in advance of what they had, and that they ought not to attempt to diverge widely from it."

Spencer's aversion to self-advertisement comes out in his reply to a suggestion made by Dr. Youmans regarding the final chapter of the *Study of Sociology*.

TO E. L. YOUMANS.

3 June, 1873.

To your suggestion that in the concluding chapter I should outline the coming treatment of the subject in the *Principles of*

Sociology, I fear I cannot yield. The concluding chapter, as I have outlined it, in thought, will make no reference whatever to the *Principles of Sociology*, and could not do so without an obvious departure from the proper limits of the book. To the advantage that might result from indicating the scope of the *Principles* I am entirely indifferent—about any probable increase of sale I do not care in the least. Indeed, so far from being tempted by an opportunity for something like an advertisement, I should be tempted to avoid it if it came naturally. Even as it is, I feel a certain distaste for the inclusion of the two chapters, "Preparation in Biology" and "Preparation in Psychology"; since these may be regarded as indirect advertisements of my own books. I would escape this implication if I could; and I shall solicitously avoid any such further implication.

His sojourn in Scotland this year was cut short owing to "very bad weather, very little amusement, and unsatisfactory health." Taking into account the expediency of proceeding at once with the *Principles of Sociology*, one would have thought he would have avoided outside entanglements. But the writing of the *Study of Sociology* had revived his former active interest in the question of the separation of Church and State, leading to meetings and discussions with those in favour of the movement.¹

In the last chapter of the *Study of Sociology* he had singled out Mr. Gladstone as "the exponent of the anti-scientific view." Mr. Gladstone repudiated the interpretation put on his words: "Whether there be or not grave differences of opinion between us, they do not arise from the words in question." Spencer thereupon withdrew the erroneous construction, and took steps to counteract it.² Thinking the small controversy between them had thus been amicably terminated, Mr. Gladstone did not read the proof which Spencer had sent him showing how it was proposed to correct the misinterpretation in future editions. When he did look into this some five weeks afterwards, he found there a reference to the other passage quoted, the manifest meaning of which he had not disclaimed. This "other passage," Mr. Gladstone wrote, had not been disclaimed because it was not in the *Contemporary Review* article, but appeared for the first time in the book itself. Moreover,

¹ *Autobiography*, ii., 258-60.

² *Study of Sociology*, note 5 to chapter xvi.