doing anything that may directly or indirectly conduce to the spread of his influence."

Misconceptions with respect to isolated opinions of such a voluminous writer as Spencer were to be expected, but the general drift of his doctrines ought to have been well understood by this time.

To M. W. KEATINGE.

13 *April*, 1897.

I fear I cannot give you any dictum to serve your purpose, for my opinions are directly at variance with those you suppose.

There is a mania everywhere for uniformity; and centralized teaching of teachers is manifestly in the direction of uniformity. Throughout all organized existence variety tends to life, uniformity tends to death. Competition in methods of education is all-essential and anything that tends to diminish competition will be detrimental.

Your notion of restrictions put upon the teaching profession is absolutely at variance with the views I hold. It is trade-unionism in teaching—it is a reversion to the ancient condition of guilds. It is a limitation of individual freedom. It is part of a general *régime* which I utterly detest.

If, as you apparently indicate, raising the status of teachers and giving them better pay implies increase of taxation, general or local, then you may judge how far I approve of it when I tell you that, from my very earliest days down to the present time, I have been a persistent opponent of all State-education.

That he no longer looked upon his London house as his home may be gathered from the following.

To MISS ———.

1 *April*, 1897.

For practical purposes, as at present carried on, the establishment is much more yours than mine. During my long absences, now covering half the year, the house is occupied by the ——— family, yourselves and relatives; and when I am at home the social intercourse and the administration give the impression that 64, Avenue Road is the residence of the Misses ———, where Mr. Spencer resides when he is in town. . . .

All things considered I do not desire any longer to maintain our relations. . . . On estimating the advantages I derive from the presence of yourself and your sisters in the house, I find them but small—not by any means great enough to counterbalance the disadvantages.

Please therefore accept this letter as an intimation that the

residence of yourself and your sisters with me will end on the first of July next.

A good deal of correspondence passed in May and June between him and a lady at whose house he spent rather less than a fornight as a "paying guest." Through the medium of an advertisement what seemed like a rural paradise had been discovered. Things went on fairly well for a week, save for an occasional murmur; but within a few days he left. This experience as a "paying guest" seems to have prompted the following letter.

To Mrs. Lynn Linton.

15 *June*, 1897.

Let me suggest to you a work which might fitly be the crowning work of your life—a work on "Good and Bad Women."

You have rather obtained for yourself the reputation for holding a brief for men versus women, whereas I rather think the fact is that you simply aim to check that over-exaltation of women which has long been dominant, and which is receiving an éclatante illustration in a recent essay by Mrs. J. R. Green, which is commented upon in this week's *Spectator*.

The flattering of women has been, one might almost say, a chief business of poets, and women have most of them very readily accepted the incense with little qualification; and this has been so perpetual and has been so habitually accepted by men as to have caused a perverted opinion. . . .

The natures of men and women are topics of continual discussion, but entirely of random discussion, with no analysis and no collection of evidence and balancing of results.

If you entertain my proposal I should like very well by and by to make some suggestions as to modes of enquiry and modes of comparison.[1]

In July he went to Boughton Monchelsea, near Maidstone, where he stayed till September. On returning to town he took chambers in Park Place, St. James's, to be near the Athenæum, where he had not been since November of the previous year, and "to acquire by a more enjoyable life, the requisite strength for driving backwards and forwards from Avenue Road." After three days he broke down, went home to Avenue Road, and did not

---

[1] The suggestion apparently led to nothing. See *Life of Mrs. Lynn Linton*, p. 329.

again leave the house until he started for Brighton in October. Considering how little he had been in London, and how little happiness he had enjoyed there during recent years, one may wonder why he continued to keep up a house in town. The explanation lies partly in that hopefulness which always led him to anticipate a change for the better, and partly in his reluctance to sever his connexion with the scene of his literary struggles and successes—with the great city in which had been kept up the closest friendships of his life. At length, however, the final step was taken. "The prospect of passing my last days monotonously in Avenue Road has become a dread to me, and I have decided that they may be passed much better here in front of the sea and with plenty of sun." He moved into 5, Percival Terrace, Brighton, soon after the beginning of 1898, hoping, as he wrote to Mr. Lecky, that his London friends would use his house as an hotel, so that he might see them as often as possible. His first concern was to get two ladies to complete his small domestic circle, musical ability being an essential qualification in one of them. His advertisement for either two sisters, or a mother and daughter, resulted in adding one more to the list of coincidences mentioned in the *Autobiography* (i., 334, 453; ii., 360). Two orphaned sisters of the name of D——— replied to his advertisement. Previously to this Mrs. Briton Rivière had recommended two sisters, also named D———; and he naturally concluded that the ladies who had answered his advertisement were the same as those recommended by Mrs. Rivière. "I should fear that these young ladies being orphans may have tended rather to the melancholy than to the joyous." Mrs. Rivière then informed him that the ladies she had recommended were not orphans, both their parents being alive.

To Mrs. Rivière.

29 *January*, 1898.

In reply to a recent advertisement there came a letter from certain two Misses D——— proposing to accept the position I offered. Remembering that you had recommended certain two Misses D———, the conclusion was drawn without hesitation that they were the same two. It turns out to be otherwise. The two who replied to my advertisement are daughters of a

stockbroker and are orphans. The name is by no means common. Who would have supposed that there should be bearing that name two pairs of sisters both wishing to undertake similar positions? The thing would be considered in a fiction as absolutely incredible.

Throughout 1898, and well into the Spring of 1899, his domestic circle underwent many changes, owing partly to his wanting "a combination of qualities which is not very common," as several of his friends told him. With the help of Mrs. Charlton Bastian he was fortunate in the spring of 1899 in meeting with Miss Key, a skilled musician, whom he engaged for the special duty of playing the piano, which he liked to hear played several times a day; the piece he wished to be played being usually selected by himself. A month or two later Miss Killick took over the duties of housekeeper. These two ladies remained with him till his death, contributing in no small degree by their thoughtfulness and sympathy to the comfort and happiness of his closing years.

During the year 1898 he had on more than one occasion to clear up "misrepresentations." One of these was contained in a paragraph in *Literature* for January, announcing that a forthcoming work by Mr. W. H. Mallock would point out "how Mr. Spencer embodies and gives fresh life to the fundamental error of contemporary 'advanced' thinkers in defining the social aggregate as a body 'composed of approximately equal units.'" Spencer was at a loss to know where Mr. Mallock had found "a passage authorizing this representation."

To W. H. Mallock.

30 *January*, 1898.

After much seeking I have discovered one of the passages to which you refer, but it seems to me that its context affords no justification for the way in which you interpret it. It is a passage on p. 5 of the *Principles of Sociology*, in which, as a preliminary, the social aggregate formed by social insects is distinguished from a human society, because it is in reality a large family and because it is "not a union among like individuals substantially independent of one another in parentage, and approximately equal in their capacities." If here there is an implied conception of a human society, the interpretation of

the words is to be taken in connexion with the contra-distinguished society: the words used should be understood in the light of this distinction. A society of ants, for example, consists of several classes—perfect males and females, workers, soldiers—and these classes differ from one another very greatly in their structures and concomitant capacities. Obviously the intention is to distinguish the *markedly unequal* capacities possessed by units of a society like this and the *approximately equal* capacities of the units forming a human society; and surely it is undeniable that, in contrast with these enormous differences in capacity among the classes of ants, the differences in capacity among human beings become relatively small; *as compared* with the extremely unlike capacities of queens, males, soldiers and workers among ants, the capacities of human beings may fitly be called "approximately equal." I should have thought that it was clear that only when drawing this contrast was the expression "approximately equal" used, and that the word "approximately" is in that relation quite justifiable.

That your interpretation is unwarranted is clearly enough indicated by passages in the *Study of Sociology* accompanying those you refer to, and is quite definitely excluded by large parts of the *Principles of Sociology*. In the *Study of Sociology*, in the chapter entitled "The Nature of the Social Science" . . . [the exposition] sufficiently implies recognition of the effects of superiority and inferiority among the units, for how can there be established the differences referred to unless because the more powerful and more intelligent rise to the top? So that even here your interpretation is tacitly negatived; and then if you will turn to the *Principles of Sociology*, Part V., treating of "Political Institutions," you will find an elaborate exposition still more rigorously excluding it. . . .

So too in the *Principles of Ethics* you will see, in the division entitled "Justice," a variously-emphasized assertion that superiority must be allowed to bring to its possessor all the naturally-resulting benefits, and inferiority the naturally-resulting evils. Moreover, you will find condemnation of the socialistic ideal, with which, apparently, your representation indicated in *Literature* implies my sympathy.

Apparently this did not convince Mr. Mallock, who thought the great man theory "shows itself only accidentally and incidentally, in the body of your work. I am well aware that your sympathies are not with the Socialists; but I confess that I think your method of merging the great man in the aggregate of conditions that have produced him, has furnished socialistic theorists with many of

their weapons." He returned to the charge in the *Nineteenth Century* for August; maintaining that in the non-recognition of "the inequality of individuals as a cardinal social fact" "we have the secret of Mr. Spencer's defect as a sociologist. This great fact of human inequality, instead of being systematically studied by him, is systematically and ostentatiously ignored by him." To these criticisms Spencer replied in the same review the month following.

Another "misrepresentation" had reference to the doctrine of animism, *Literature* representing him as an adherent. This he repudiated, in the issue of February 5, showing how in the Data of Sociology "instead of accepting the doctrine of animism, I have not only avowedly rejected it, but have, throughout the successive parts of a long argument, supplied what I conceive to be direct and indirect disproofs of it." In the same periodical (19 February), he endeavoured to remove the perplexity in which Mr. Andrew Lang was involved in *The Making of Religion*. Under the name of Animism or Fetichism "there is an alleged primordial tendency in the human mind to conceive inanimate things as animated—as having animating principles or spirits. The essential question is: has the primitive man an innate tendency thus to conceive things around? Professor Tylor says Yes; I say No. I do not think it requires any 'revised terminology' to make this difference clear." The matter had to be taken up again in July. The *Spectator* had classed him as one of those who believed that superstitious ideas arose from "the universal conviction or feeling that all things in Nature are endowed with the sentient vitality and the unruly affections of mankind." "I entertain no such belief," he wrote to the editor. "This ascription to me. . . of a belief which I have emphatically rejected, is one of many examples showing me how impossible it is to exclude misunderstanding."

The war between the United States and Spain was weighing heavily on the consciences of many thoughtful Americans, among whom was Mr. Moncure Conway, who asked Spencer whether it would not be possible to form a concert of eminent men, who, whenever a peril of war

arose, should meet as a "supreme court of civilization" and determine the right and wrong, before any declaration of war took place.

To Moncure D. Conway.

17 *July*, 1898.

I sympathize in your feelings and your aims, but not in your hopes. . . . In people's present mood nothing can be done in that direction.

Now that the white savages of Europe are overrunning the dark savages everywhere—now that the European nations are vying with one another in political burglaries—now that we have entered upon an era of social cannibalism in which the strong nations are devouring the weaker—now that national interests, national prestige, pluck, and so forth are alone thought of, and equity has utterly dropped out of thought, while rectitude is scorned as "unctuous," it is useless to resist the wave of barbarism. There is a bad time coming, and civilized mankind will (morally) be uncivilized before civilization can again advance.

Such a body as that which you propose, even could its members agree, would be pooh-poohed as sentimental and visionary. The universal aggressiveness and universal culture of blood-thirst will bring back military despotism, out of which after many generations partial freedom may again emerge.

The reader will remember how, when the Anti-Aggression excitement was on him in 1882, he had endeavoured to induce Miss Bevington to put the indignation he felt into verse. The idea occurred to him again this year.

To Wilfrid Scawen Blunt.

1 *October*, 1898.

For some years I have been casting about for a poet who might fitly undertake a subject I very much want to see efficiently dealt with. At one time I thought of proposing it to Mr. Robert Buchanan, who in respect of vigour of expression and strength of moral indignation seemed appropriate, but I concluded that the general feeling with regard to him would prevent a favourable reception—would in fact tend very much to cancel the effect to be produced. Afterwards the name of Mr. William Watson occurred to me as one who had shown feelings of the kind I wished to see expressed. But, admirable as much of his poetry is, the element of power is not marked: he does not display a due amount of burning sarcasm. Your recent letter in the *Times*, and since then a review in the *Academy* in which there were quotations from

your poem "The Wind and the Whirlwind," lead me to hope that you may work out the idea I refer to.

This idea is suggested by the first part of Goethe's "Faust" —The Prologue in Heaven, I think it is called. In this, if I remember rightly (it is now some 50 years since I read it), Mephistopheles obtains permission to tempt Faust—the drama being thereupon initiated. Instead of this I suggest an interview and dialogue in which Satan seeks authority to find some being more wicked than himself, with the understanding that, if he succeeds, this being shall take his place. The test of wickedness is to be the degree of disloyalty—the degree of rebellion against divine government.

6 *October*.—Thank you for your letter. I am heartily glad to find you entertain my suggestion. . . .

My beliefs are pretty much as pessimistic as those you express—in respect at least of the approaching condition of mankind; but holding though I do that we are commencing a long course of re-barbarization from which the reaction may take very long in coming, I nevertheless hold that a reaction will come, and look forward with hope to a remote future of a desirable kind, to be reached after numerous movements of progress and retrogression. Did I think that men were likely to remain in the far future anything like what they now are, I should contemplate with equanimity the sweeping away of the whole race.

5 *November*.—How to put the greatest amount of feeling and idea in the shortest space is the problem to be solved by every writer, more especially by the poet, for rightly conceived (not as by Browning) poetry is a vehicle in which the friction is reduced to a minimum, and of course everything which is superfluous adds to the friction. I have often thought that nearly all our poets would have greatly benefited by restriction to one-fourth the space. Works of art in general would in nearly all cases profit by restraint. Much architecture and much internal decoration is spoiled by excess, and nearly every painter puts too much into his pictures. Composers, too, even the highest of them, as Beethoven, often spoil their works by needless expansion. To the artist each new idea seems so good that he cannot make up his mind to leave it out, and so more or less sacrifices the effect of the whole to the effect of the part.

Before the appearance of *Satan Absolved*—the title chosen for his poem by Mr. Scawen Blunt—Spencer wrote:—

To Wilfrid Scawen Blunt.

23 June, 1899.

I rejoice to hear that the poem is finished, and that its publication is not far off.

Of course I feel honoured by your proposal to preface some words of dedication to me, and accept with pleasure. Please do not, however, in any introductory words, indicate the origin of the idea which the poem elaborates. You will perhaps be surprised by this request until you understand my reason.

Already my general views, touching as they do in many places upon religious opinions, have from time to time exposed me to vilification both here and in America, and have, in consequence, raised impediments to the wider diffusion of the general philosophical views which I have set forth, and have in various ways diminished both the circulation and the influence of the books. Such being the case I do not want to again rouse, even more strongly than hitherto, the *odium theologicum* and to give it a further handle for attacks, not only upon my declared religious opinions, but also upon the system of thought associated with them, but which is in reality independent of them. It is this contemplation not of the personal, but of the impersonal effects, which makes me wish not to arouse still greater antagonism than I have already done. A further obstacle to the spread of evolutionary views would, I think, be a greater evil than any benefit to be gained.

On receiving a copy of the poem, he wrote in haste to beg Mr. Blunt to omit a passage on the first page. The description of the ante-chamber of heaven "savours too much of the earth earthy, . . . and puts the poem in too low a key."

To Wilfrid Scawen Blunt.

28 October, 1899.

Let me first apologize for my brusquely-expressed letter written immediately on receipt of *Satan Absolved*. . . .

I did not at first recognize the fact that, by calling the poem a "Victorian Mystery," you intended to suggest some analogy to the mysteries of Medieval days, and that you had adopted a mode of treatment implied by this analogy. Hence that assimilation of the divine and the human, which characterized the mystery-plays, had not been understood by me as sequent upon the adoption of the earlier mode of thought, and as a result gave me a sense of incongruity. Though I now see that the adoption of this ancient mode of thought gives consistency to the work, yet it seems to me that we (or at least cultured people) have so far travelled away from that mode of thought

that the revival of it will be apt to excite in many readers an internal protest.

My chief difficulty, however, in forming a judgment arises, as I now see, from the wide difference between the general conception as embodied by you and the conception which I had myself formed and suggested. . . .

This much, however, I can say with all sincerity—that I like it much better on a second reading than on the first; and this I think is a marked evidence of its goodness. Unquestionably, Satan's description of Man and his doings is given with great power, and ought to bring to their senses millions of hypocrites who profess the current religion. I wish you would emphasize more strongly the gigantic lie daily enacted—the contrast between the Christian professions and the pagan actions, and the perpetual insult to one they call Omniscient in thinking that they can compound for atrocious deeds by laudatory words.

During the winter of 1898-99 he wrote two postscripts to Part VII. of the *Psychology*: one on Idealism and Realism; the other in reply to a criticism of the late Professor Green, whose article in the *Contemporary Review* for December, 1877, had recently been republished.[1] In addition to these he wrote a chapter on "The Filiation of Ideas," which he stereotyped and put away for future use.[2] A renewed attempt to introduce the metric system suggested the expediency of issuing a second edition of the brochure, "Against the Metric System," and again distributing it among members of Parliament. Under the name "A Citizen" he wrote to the *Times* four letters, which were included in the pamphlet.

With all his disregard for public opinion as far as concerned his philosophical doctrines—notwithstanding the indifference or even satisfaction with which he contemplated the shocks he occasionally gave to current orthodoxy, whether scientific or religious—he was extremely sensitive to criticism of his character, and had a rooted dislike to his private life and conversation being treated as public property. He assumed that those who enjoyed the privilege of his intimacy would respect the unwritten law of private intercourse by scrupulously refraining from making public

---

[1] *Principles of Psychology*, ii., 505-20, Ed. of 1899.

[2] Reprinted in this volume as Appendix B.

the trivial no less than the important matters of his daily life. Himself taking little interest in personalities and gossip, he never dreamt that unpremeditated remarks made in the hearing of those living under the same roof, might be published abroad, or that the petty details of domestic life might have their pettiness intensified by being taken out of their appropriate setting and held up as a public spectacle. He had a rude awakening in the spring of 1899. Soon after the announcement of his forthcoming book on Spencer, Mr. Hector Macpherson received from a lady quite unknown to him, an offer of "Reminiscences of Herbert Spencer." She and her father had lived at 38, Queen's Gardens during part of the time Spencer was there, and had been in the habit of taking notes of Spencer's sayings and doings, and these notes she now offered for ten guineas, adding that if they were not accepted she could readily find a publisher later on. On hearing of this, and on the advice of his solicitors that he had no power to stop the publication of statements concerning himself, he requested Mr. Macpherson to offer ten guineas for the MS., provided the lady would undertake not to publish any other version of the reminiscences. A legal minute of agreement and sale was drawn up and signed, and in due course Spencer obtained possession of the manuscript.

To Hector Macpherson.

25 *April*, 1899.

You bargained better than you knew. There are many absolutely false statements—false to the extent of absurdity. Here is a quotation:—"Often (!) invited to dine at Marlborough House, but would never go." Imagine the Prince of Wales often repeating his invitations after being declined! The statement is absolutely baseless. Another statement is:—"Gladstone very often came to breakfast, but this was before the Home Rule affair; also George Eliot, Darwin, Tyndall." Again absolutely false. With no one of the four did I ever exchange breakfast civilities save Mr. Gladstone, and instead of his often breakfasting with me I some three or four times breakfasted with him. . . . Some of Mr. ——'s quotations from his diary are, however, of a libellous kind.

Spencer's first idea had been that the lady should be informed by the firm of lawyers who had the matter in hand

that the publication of these reminiscences would render her liable to prosecution. But in the end he took a view of the matter which it seems a pity he did not take at the outset—to treat the proposed publication with indifference, seeing that it contained its own antidote.

The health of Mr. Grant Allen was giving Spencer much concern, his sympathies as usual leading him to try to trace the evil to its source. A visit of some duration from his friend afforded opportunities for earnest entreaties. These were afterwards enforced by appeals to Mr. Grant Allen's scientific knowledge.

To Grant Allen.

Brighton, 2 *June*, 1899.

I am glad to hear that your wife thinks that you have profited by your stay here. I hope that the corner may be by-and-by turned completely.

That it may be turned completely it is clear that you must improve your mastication. . . . If I had to teach children I should give them among other things a lesson on the importance of mastication, and should illustrate it by taking a small iron nail and weighing against it some pinches of iron filings till the two balanced; then, putting them into two glasses, pouring into each a quantity of dilute sulphuric acid, leaving them to stir the two from time to time, and showing them that whereas the iron filings quickly dissolve, the dissolving of the nail would be a business of something like a week. This would impress on them the importance of reducing food to small fragments. That you, a scientific man, should not recognize this is to me astonishing.[1]

When Mr. Grant Allen died in October following, Spencer lost one of his ablest and most chivalrous allies. Writing in June, 1900, to Mr. Edward Clodd on receiving a copy of the *Memoir*, he said:

I was often surprised by his versatility, but now that the facts are brought together, it is clear to me that I was not sufficiently surprised. One of the traits on which I should myself have commented had I written about him was his immense quickness of perception. He well deserved this biography.

---

[1] Mastication formed the subject of a brief essay he began to dictate a few weeks before his death, but did not finish.

The correspondence that follows with Mr. (afterwards Sir) Leslie Stephen regarding the formation of an Ethical Lecturers Fund has an interest apart from its immediate purpose. It throws light upon the question how, with his professed dislike to reading, he was able to amass the immense amount of information contained in his earlier books. This profusion of exemplification and illustration seems inconsistent with his own repeated statements that he was constitutionally, and as a matter of fact, idle—that he was an impatient reader, and actually read little. In one of the following letters to Mr. Leslie Stephen he says that when preparing to write he read up in those directions in which he expected to find materials for his own generalizations, not caring for the generalizations of others. Under the guidance of a generalization he picked out the relevant material, ignoring the irrelevant; as a lawyer restricts his reading in preparing his brief. That he lost by this restricted reading cannot be doubted. It gave colour to the not ill-natured remark of one of his friends: "Scratch Spencer, and you come upon ignorance." But, taking all in all, it may be said that what he lost through lack of diligence in acquisition he made up for, or more than made up for, by the continuous exercise of his wonderful gift of organization. If the word industrious can be so applied, then, as a thinker Spencer was pre-eminently industrious, his mind was incessantly occupied with the logical relations of things. It was the firm grasp he had of these logical relations that enabled him to retain complete mastery over the details, marshalling them at his bidding; giving, perhaps, also the impression of having unfathomable sources of information from which to draw. His literary industry was untiring. Not only were his published writings voluminous, but his correspondence was very great. The limit imposed on the writer of this volume has rendered it impossible to reproduce more than a small fraction of his letters.

To Leslie Stephen.

28 June, 1899.

When I received the circular asking for aid in raising the Ethical Lecturers Fund I at once decided to contribute. On re-reading the prospectus, however, I was brought to a pause

by the paragraph requiring a University Honours degree as the minimum intellectual equipment. If John Mill had been alive and a young man, his candidature would have been negatived by this requirement. And were I a young man and proposed to adopt the career of ethical lecturer, my candidature also would be negatived.

30 *June*.—The expression which you underline does not seem to me to change the essential meaning of the passage I referred to. It implies that there shall be a standard of education substantially like that which a university gives.

I do not know what might have been the case with Mill. I can only say that were I young and a candidate, the regulation would rigorously exclude me. Not only could I have shown no education equivalent to a university honours degree, but I could have shown none equivalent to the lowest degree a university gives. . . .

Naturally, such being my position, I demur to the test specified. Moreover, not on personal grounds only, but on general grounds, I demur to the assumption that a university career implies a fit preparation.

FROM LESLIE STEPHEN.

1 *July*, 1899.

You say that when you were young you could not have shown an education "equivalent to the lowest degree a university gives." It is not for me to dispute that statement. I am, however, sure that when you first published books upon ethical questions, you had somehow or other attained an amount of knowledge upon such topics very much superior to that of the average "honour man," who satisfies the examiners in his department of study. . . . We never thought of suggesting that candidates should have passed any particular course, but that their general hold of intellectual culture should be equal to that implied by capacity to fulfil the ordinary conditions of university success.

TO LESLIE STEPHEN.

2 *July*, 1899.

Your assumption is a very natural one, but it is utterly mistaken. When *Social Statics* was written I had none of that preparation which you suppose.

When with my uncle, from thirteen to sixteen, my acquirements were limited to Euclid, algebra, trigonometry, mechanics, and the first part of Newton's Principia. To this equipment I never added. During my eight years of engineering life I read next to nothing—even of professional literature. Then as always, I was an impatient reader and read nothing continuously except novels and travels, and of these but little. I am

in fact constitutionally idle. I doubt whether during all these years I ever read any serious book for an hour at a stretch. You may judge of my condition with regard to knowledge from the fact that during all my life up to the time *Social Statics* was written, there had been a copy of Locke on my father's shelves which I never read—I am not certain that I ever took it down. And the same holds of all other books of philosophical kinds. I never read any of Bacon's writings, save his essays. I never looked into Hobbes until, when writing the essay on "The Social Organism," I wanted to see the details of his grotesque conception. It was the same with Politics and with Ethics. At the time *Social Statics* was written I knew of Paley nothing more than that he enunciated the doctrine of expediency; and of Bentham I knew only that he was the promulgator of the Greatest Happiness principle. The doctrines of other ethical writers referred to were known by me only through references to them here and there met with. I never then looked into any of their books; and, moreover, I have never since looked into any of their books. When about twenty-three I happened to get hold of Mill's *Logic*, then recently published, and read with approval his criticism of the Syllogism. When twenty-four I met with a translation of Kant and read the first few pages. Forthwith, rejecting his doctrine of Time and Space, I read no further. My ignorance of ancient philosophical writers was absolute. After *Social Statics* was published (in 1851) I made the acquaintance of Mr. Lewes, and one result was that I read his *Biographical History of Philosophy*. . . . And, shortly after that (in 1852), a present of Mill's *Logic* having been made to me by George Eliot, I read that through: one result being that I made an attack upon one of his doctrines in the *Westminster*.

Since those days I have done nothing worth mentioning to fill up the implied deficiencies. Twice or thrice I have taken up Plato's *Dialogues* and have quickly put them down with more or less irritation. And of Aristotle I know even less than of Plato.[1]

If you ask how there comes such an amount of incorporated fact as is found in *Social Statics*, my reply is that when preparing to write it I read up in those directions in which I expected to find materials for generalization. I did not trouble myself with the generalizations of others.

And that indeed indicates my general attitude. All along I have looked at things through my own eyes and not through the eyes of others. I believe that it is in some measure because I have gone direct to Nature, and have escaped the warping

---

[1] In a letter to Prof. Brough, of Aberystwith, in 1895, he said, "I never at any time paid the least attention to formal logic, and hold that for all practical purposes it is useless."

influences of traditional beliefs, that I have reached the views I have reached. . . .

My own course — not intentionally pursued, but spontaneously pursued—may be characterized as little reading and much thinking, and thinking about facts learned at first hand. Perhaps I should add, that my interest all along has been mainly in the science of Life, physical, mental and social. I hold that the study of the science of Life under all its aspects is the true preparation for a teacher of Ethics. And it must be the science of Life as it is conceived now, and not as it was conceived in past times.

If you ask me what test you are to establish, I cannot answer. I simply raise the question—Is it necessary to establish any test? May not the choice be decided by the evidence furnished in each case apart from any specified standard?

While he was at Oakhurst, South Godstone, in July, Mrs. Leonard Courtney sent him an account of visits she had had from two of his admirers—Mr. Hector Macpherson and the Chinese Ambassador, Sir Chih Chen Lo Feng-Luh, whom he had entertained at lunch in June. "Of course," he replied, "I am interested in your account of Mr. Macpherson and the Chinese Ambassador. The latter's opinion that I am a resurrected Confucius is amusing, as is also his opinion that I ought to be a Duke." Writing late in the year to another friend—Mr. Carnegie—acknowledging a present of grouse, he remarks :—

Doubtless it is one of the advantages of being a highland laird that you can thus give gratifications to your friends ; but I can quite believe, as you hint in your last letter, that along with advantages there are increasing responsibilities. It is not only true, as Bacon says, that when a man marries he gives hostages to fortune, but it is also true that he does this when he increases his belongings of every kind.

The letter that follows, written to a lady in Geneva, contains nothing with which the reader is not familiar. But, besides putting the evils of governmental interference and control very clearly, it bears witness to Spencer's life-long consistency with regard to fundamental opinions. It was translated into French and German and read at a Congress in Switzerland.

TO MRS. JOSEPHINE BUTLER.

3 *September*, 1899.

I learn with pleasure that you and some others are opposing the adoption of coercive methods for achieving moral ends.

Briefly stated my own views on such matters are these :—

Nearly all thinking about political and social affairs is vitiated by ignoring all effects save those immediately contemplated. Men, anxious to stop an evil or obtain a good, do not consider what will be the collateral results of the governmental agencies they employ, or what will be the remote results. They do not recognize the fact that every new instrumentality established for controlling individual conduct becomes a precedent for other such instrumentalities, and that year after year philanthropists with new aims urge on further coercive agencies, and that so little by little they establish a type of social organization —a type which no one of them contemplated when he was urging on his particular plan.

The highest aim ever to be kept in view by legislators and those who seek for legislation is the formation of character. Citizens of a high type are self-regulating, and citizens who have to be regulated by external force are manifestly of a low type. Men, like all other creatures, are ever being moulded into harmony with their conditions. If, generation after generation, their conduct in all its details is prescribed for them, they will more and more need official control in all things. . ...

The final outcome of the policy in favour with philanthropists and legislators is a form of society like that which existed in ancient Peru, where every tenth man was an official controlling the other nine ; where the regulation went to the extreme of inspecting every household to see that it was well administered, the furniture in good order, and the children properly managed ; and where the effect of this universal regulation of conduct was the production of a character such that the enfeebled society went down like a house of cards before a handful of Spaniards.

On completing the revision of the *Principles of Biology* towards the end of 1899, he at once took in hand the preparation of a final edition of *First Principles*. Owing to the number and importance of the alterations, he was desirous that the existing translations should be replaced as soon as possible by translations of this final edition. When the German version was completed, Professor Victor Carus wrote : "And now once more, allow me to repeat my most cordial thanks that you allowed me to translate your work anew. It was a very great treat to

me." Below this Spencer has written: "This is the highest compliment I ever received, considering Professor Carus's age and position." It was with no ordinary satisfaction that, towards the end of his eightieth year, he gave the finishing touches to the system of philosophy, on which he had been engaged for forty years. His gratification was enhanced by the cordial greetings from all parts of the world which poured in upon him on his birthday —greetings which he acknowledged in a circular written by his own hand and lithographed :—

> Letters and telegrams, conveying the congratulations and good wishes of known and unknown friends, have reached me yesterday and to-day in such numbers that, even were I in good health it would scarcely be practicable to write separate acknowledgments. I must therefore ask you, in common with others, kindly to accept this general letter which, while expressing my thanks to those who have manifested their sympathy, also expresses my great pleasure in receiving so many marks of it from my own countrymen and from men of other nationalities.

No one will deny that Spencer was entitled to look forward to the enjoyment of undisturbed serenity now that the task, for which he had sacrificed so much, was completed. But, ere the work of revision had been fully accomplished, events were taking place that were to cause him anxiety and vexation during the remaining years of his life. Some time before the outbreak of hostilities in South Africa he had denounced the policy that was drifting the country into war. Whatever one's opinion may be as to the right or the wrong of the war, one must admit that Spencer's attitude towards it was in complete harmony with the principles he had throughout life professed. He was invited to sign a protest.

To James Sully.

10 *December*, 1899.

Who are the "we"? I should not like to give my name in such a case without being made aware with whose names mine would be joined.

Further, I think that the protest is not sufficiently strong, and not sufficiently concise. . . . Among the facts which should be emphasized are (1) that the outlanders were a swarm of unwelcome intruders and had no right to complain of the social régime into which they intruded themselves,

since nobody asked them to stay if they did not like it. (2) They were proved traitors trying to overturn the government which gave them hospitality, and, as Lord Loch's evidence shows, were long contemplating a rising and a seizure of the government of the country. Traitors cannot put in a claim to political power. (3) The Boers have done no more than would inevitably have been done by ourselves if similarly placed, and in doing which we should have regarded ourselves as patriotic and highly praiseworthy. (4) The advocacy of annexation is nothing more than a continuance of our practice of political burglary. (5) We are rightly vituperated by other nations, as we should vituperate any one of them who did similar things, and as we are now vituperating Russia for its policy in Finland, carried out in a much milder manner.[1]

To Mark Judge.

2 January, 1900.

During the last week I have been in communication with the Secretary of the Anti-Vaccination League, and also with the Chairman of the South African Conciliation Committee, and this morning I have a request from the Editor of the *Speaker* to express my sympathy with the course which they are pursuing. In all these cases I am making a favourable response.

I am now nearly eighty, and it is more and more clear to me that I must cut myself off from these various distractions as much as possible for I have still something I want to do, and thinking this, I decide it will be better for me to decline taking any part in this League for Licensing Reform, even in the position of Vice-President. . . . I wish you success in your efforts.

While approving of the attitude of the *Speaker* towards the war, he declined to become a regular subscriber because its political views were "distinctly socialistic or collectivist, if you choose so to call them, and much as I abhor war I abhor socialism in all its forms quite as much." On 5 February the *Morning Leader* had a letter from him protesting against the spirit shown by those who shouted to the departing troops: "Remember Majuba."

To Sir Edward Fry.

6 February, 1900.

Popular passion, excited by political and financial agencies, has gagged all but one of those morning papers which expressed opposition to our war policy in South Africa. The

[1] Spencer was one of the signatories of the memorial to the Czar on behalf of the people of Finland, which His Majesty declined to receive.

Morning Leader is the only one that remains to give voice to those who reprobate the war and desire that the two republics shall maintain their independence. You will see by a copy of the paper, which you have by this time received, that, by the expression of sympathetic opinions, efforts are being made to support this organ of views properly to be called Christian, in opposition to the views of those properly to be called Pagan.

It is not to be expected that much can be done towards checking the war fever, but it may be hoped that by spreading so far as may be sympathy with equitable sentiments and reprobating those who sneer at "unctuous rectitude," something may be done towards preparing the way for a settlement not so utterly inequitable as is now threatened.

Could you help by adding some expression of your opinion to the expressions of opinions already published?

A similar letter was sent to Dr. Edward Caird, Master of Balliol College, Oxford.

To the Right Hon. Leonard Courtney.

24 *February*, 1900.

I daresay you will think me rather absurd in making a suggestion respecting your attitude towards your constituents.

There has grown up the altogether unwarrantable assumption that a man represents that particular part of the constituency which has elected him, and when that part of the constituency —some Conservative or Liberal Association, or what not— through whose instrumentality he was elected disapproves of his course, it seems to be thought by them, and by the public at large, that he is thereupon called upon to resign. But where is there any indication, either in the constitution or in the theory of representation, that a member of parliament represents any particular section of his constituency, any party? So far as I know, the idea of party is not recognized in the representative system at all. A member of parliament represents the constituency and the whole constituency, and not any particular section of it. . . . Hence it results that, if any Liberal or Conservative Association, or any other kind of caucus, calls upon him in a case like the present, to resign, his fit reply may be that as a representative of the whole constituency he cannot even *entertain* the proposition to resign, until it is shown to him that a majority of the whole constituency wishes him to do so.

I do not know that in your case the assumption of such an attitude would be of any advantage, since, probably, the remainder of the constituency is more against you than the part which elected you. Still, I suggest this as a general course of conduct applicable to all cases.

CHAPTER XXVI.

INORGANIC EVOLUTION.

In the preceding chapters little has been said as to the application of evolution to inorganic Nature. This division was passed over in Spencer's programme "partly because, even without it, the scheme is too extensive; and partly because the interpretation of organic Nature . . . is of more immediate importance." While most will admit the cogency of these two reasons, many, after reading the earlier volumes of the series, will agree with Mr. J. S. Mill in desiring to see the working out of the principles of evolution in the omitted division of the programme. Some may even think, with Dr. David Sharp, of Cambridge, that the application of evolution to inorganic Nature was of more importance than the attempt to upset Professor Weismann's theory. Be this as it may, it would be a very perverse judgment that would regard the absence of this division as telling against Spencer's work as a whole. Objection may be made if a writer fails to accomplish what he undertook to do. But, it can hardly be urged against the value of what he has accomplished that he has not done something which, for sufficient reasons, he announced at the outset he did not propose to undertake. To discredit Spencer's teaching, as has been done, now because he attempted too much, and now because he did not attempt more, does not help those who honestly wish to arrive at a just estimate of it.

It is, however, a mistake to assume that Spencer did not apply the principles of evolution to inorganic Nature. Not only was the subject frequently in his thoughts throughout the thirty-six years when he was writing the Synthetic Philosophy; but even before his programme

was issued he had made two important excursions into inorganic Nature—excursions that had no small share in suggesting and developing his system of thought. The purpose of this chapter is to gather together the correspondence bearing upon evolution in its application to the inorganic world. In this way a better idea will be gained of what Spencer accomplished in this domain, than if the subject had been dealt with incidentally, and in piece-meal fashion, in the course of the narrative.

The scientific topics (other than professional ones) that first and chiefly interested Spencer, during the earlier engineering period, were Astronomy and Geology—the two departments of knowledge which, when he issued his programme in 1860, he decided to pass over, or not to treat in detail. Letters to his father during the years 1838 to 1841 contain frequent discussions of astronomical questions. Geology was taken up seriously in 1840, and, during the years he was engaged on railway surveys, he had many opportunities of acquainting himself with it at first hand. Speculation as to the change in the Earth's atmosphere consequent on the abstraction from it of carbon during the deposition of carboniferous strata, took shape in 1843-44 in a paper in the *Philosophical Magazine*.[1] In the same periodical for 1847 he had a paper on "The Form of the Earth no proof of original Fluidity."[2] A theory about nebulous matter was being worked out by the middle of 1851. He had written to Sir John Herschel and Professor Airy, inquiring "whether it had been shewn why nebulous matter must take up a rotatory motion in condensing." Their replies, so he told his father, show "that my idea is new, so I think I have made a discovery worth publishing. I shall write a paper for the *Philosophical Magazine*." He was in no hurry, however, to rush into print; for, though he told his parents in 1852 that he hoped to complete it shortly, it was laid aside for several years, owing to the writing of the *Principles of Psychology*, and subsequent ill-health. But by the spring of 1858 it began to assume a definite shape.

---

[1] *Autobiography*, i., 532. [2] *Autobiography*, i., 313, 546.

TO HIS FATHER.

May, 1858.

The Nebular Hypothesis works out beautifully. The article will contain a great deal that is new, and will, I think, render the argument conclusive. I have had a long talk with Dr. Tyndall on the sundry novelties, which were based upon principles in physics. *He endorses all my conclusions:* though not prepared wholly to commit himself to them, he thinks them rigorously reasoned, and well worth promulgating.

Some months after the publication of the article[1] he mentions that it "had been very favourably received everywhere. It was ascribed to Baden Powell." The early part of 1859 was taken up with a paper for the *Universal Review*, under the title "Illogical Geology."[2] As the primary purpose of "The Nebular Hypothesis" was to prove that the inferences drawn from the revelations of Lord Rosse's telescope were illegitimate, so that of "Illogical Geology" was to direct attention to the inconsistency of the reasonings of geologists. The writing of these two articles, which touched upon the two divisions of Inorganic Evolution as he conceived it, played an important part in the evolution of the scheme of philosophy, which had gradually been growing in extent and definiteness. In the outline sketched during the early days of 1858, the first volume is represented as including, after Parts I. and II., dealing respectively with "The Knowable" and "The Unknowable," Part III., Astronomic Evolution, and Part IV., Geologic Evolution.

Another outline of this first volume, of what he calls the *Deductive Philosophy*, presents the contents of Parts III. and IV. with more detail.

Part III. The Principles of Astrogeny.

Chap.	I.	Primitive Cosmogonies.
,,	II.	A Priori Probabilities of Evolution.
,,	III.	Where are the Nebulæ?
,,	IV.	What are the Nebulæ?
,,	V.	The Comets.
,,	VI.	Motions of the Sun and Planets.
,,	VII.	Specific Gravities of the Sun and Planets.

---

[1] *Autobiography*, ii., 21.

[2] *Autobiography*, ii., 43.

Chap. VIII. Temperature of the Sun and Planets.
" IX. Our Sidereal System.
" X. The Future.
Part IV. The Principles of Geogeny.
Chap. I. Igneous Development.
" II. Aqueous Development.
" III. Geographic Development.
" IV. Meteorologic Development.
" V. Chemical Development.

The omission of Astronomic and Geologic Evolution from the programme issued two years later did not mean that the inorganic world was to be entirely passed over, but only that it would not receive the detailed treatment accorded to Life, Mind, Society and Morality. Readers of *First Principles* are aware of the course followed in the exposition. "The Transformation or Equivalence of Forces," "The Direction of Motion," and "The Rhythm of Motion" are each exemplified, firstly, in astronomical and secondly, in geological transformations, before their operation in organic and super-organic transformations is discussed. The same course is followed in the exposition of "The Law of Evolution," "The Instability of the Homogeneous," "The Multiplication of Effects," "Segregation" and "Equilibration." When treating of "Dissolution" the exposition naturally follows the reverse order. Putting all these expositions together one may obtain a general idea of what the *Principles of Astrogeny* and the *Principles of Geogeny* would have been like had time, energy, and knowledge sufficient been vouchsafed to him.

What he described as "a further development of the doctrines of molecular dynamics" appeared in the *Reader* (19 November 1864) under the title—"What is Electricity?" Nine years later, when writing to Dr. Youmans (12 November, 1873) he said:

> Since I began this letter there has dawned upon me, after this long delay, an extension of that theory of electricity set forth in the *Reader* and published in the *Essays*. I am busy writing a postscript which, when it is in print, I shall submit to Tyndall and other authorities, and, if they do not disprove it, will send you a copy for addition to the American volume.

FROM JOHN TYNDALL.

ATHENÆUM CLUB [1873].

I have glanced over your paper, rather than read it critically. It shows the usual penetration ; but will you bear with me if I advise you not to publish it as it now stands. Its aim is ambitious, and I frankly think it fails in its aim. If you publish it as a speculation, not as an "explanation," no harm can accrue. But I think harm would accrue if it were published in its present garb.

I often wished to say to you that your chapters on the Persistence of Force, etc., were never satisfactory to me. You have taken as your guide a vague and to me, I confess, altogether unsatisfactory book. The greater part of your volume I consider to be of such transcendent merits, putting one's best thoughts into the clearest language, that I feel all the more the transition to the chapters to which I have referred. I expressed, I think, the opinion to you some time ago that they ought to be rewritten.

If you have considered how the disturbance of molecules can generate attraction and repulsion at a distance, you ought to state the result of your thought. If you have not thought of this question, then I think you have omitted the fundamental phenomenon of electricity.

I am hard pressed, and therefore write briefly. You will excuse my frankness. I certainly should grieve to see anything with your name attached to it that would give the enemy occasion to triumph.

TO JOHN TYNDALL.

22 December, 1873.

I quite agree with you as to the undesirableness of publishing this postscript as it stands : indeed, I sketched it out with the expectation that criticism would probably oblige me to remodel it. I quite intended (but I see that I must make the intention more clear) to put forth the hypothesis simply as a *speculation* : apparently having such an amount of congruity with physical principles as made it worth considering—especially in the absence of anything like a satisfactory explanation.

I have had another letter from Clerk Maxwell, which considerably startles me by its views about molecular motion. I should like to talk to you about them. They seem to me to differ from those which I supposed you to hold, and which I supposed were held generally.

Thank you for your reminder respecting the chapter on the "Persistence of Force." I hope to make it worthy of your approval. I am now remodelling it, and the two preceding chapters.

When sending the paper to Professor Clerk Maxwell reference seems to have been made to a remark made to Professor Kingdon Clifford regarding Spencer's views about nebular condensation.

FROM J. CLERK MAXWELL.

5 *December*, 1873.

I do not remember the particulars of what I said to Professor Clifford about nebular condensation. The occasion of it was I think a passage in an old edition of your *First Principles*, and having since then made a little more acquaintance with your works, I regarded it merely as a temporary phase of the process of evolution which you have been carrying on within your own mind. Mathematicians by guiding their thoughts always along the same tracks, have converted the field of thought into a kind of railway system, and are apt to neglect cross-country speculations.

It is very seldom that any man who tries to form a system can prevent his system from forming round him, and closing him in before he is forty. Hence the wisdom of putting in some ingredient to check crystallization and keep the system in a colloidal condition. Candle-makers, I believe, use arsenic for this purpose. . . . But you seem to be able to retard the crystallization of parts of your system without stopping the process of evolution of the whole, and I therefore attach much more importance to the general scheme than to particular statements.

After describing several experiments, which he would not say were inconsistent with Spencer's theory, but which were very important and significant, Professor Clerk Maxwell continues: "As I observe that you are always improving your phraseology I shall lay before you my notions on the nomenclature of molecular motions." One of the terms defined was "the motion of *agitation* of a molecule," namely "that by which the actual velocity of an individual molecule differs from the mean velocity of the group."

On receipt of some remarks by Spencer on the word "agitation," Professor Clerk Maxwell wrote again (17 December, 1873):—

The reason for which I use the word "agitation" to distinguish the local motion of a molecule in relation to its neighbours is that I think with you that the word "agitation" conveys in a small degree, if at all, the notion of rhythm.

If motion is said to be rhythmic when the path is, on the whole, as much in one direction as in the opposite, then all motion is rhythmic when it is confined within a small region of space.

But if, as I understand the word rythmic, it implies not only alternation, but regularity and periodicity, then the word "agitation" excludes the notion of rhythm, which was what I meant it to do. . . . A great scientific desideratum is a set of words of *little* meaning—words which mean no more than that a thing belongs to a very large class. Such words are much needed in the undulatory theory of light, in order to express fully what is proved by experiment, without connoting anything which is a mere hypothesis.

To J. Clerk Maxwell.

30 *December*, 1873.

I must confess that I was taken somewhat aback by the statement that you deliberately chose the word *agitation* because it negatived the notion of rhythm. For I had hardly anticipated the tacit denial that the relative motions of molecules as wholes have rhythm. I feel fully the force of the reason for supposing that, when molecules are irregularly aggregated into a solid, the tensions due to their mutual actions will be so various as to produce great irregularity of motion ; and I have, indeed, in the first part of the speculation concerning electricity, indicated this as a possible cause for the continuity of the spectrum in solids. But, admitting this, there seem to me two qualifying considerations. If, as shown in the lecture you were so kind as to send me, molecules of different weights have different absolute velocities in the gaseous state ; then, must it not happen that when such differently-moving molecules are aggregated into solids, their *constitutional differences of mobility* will still show themselves ? Such constitutional differences cannot well disappear without any results ; and if they do not disappear, must there not result characteristic differences between their motions of agitation in the two solids they form—must not the two agitations differ in the *average periodicities* of the local motions constituting them ? The second qualifying consideration which occurs to me is this. Though molecules, irregularly aggregated into a solid, may be expected to have motions more or less confused by the irregularities of the tensions ; may we not say that, when they are *regularly* aggregated into a solid (as in a crystal), they will be subject to *regular* tensions, conducing to regular motions ? Do not the formation and structure of a crystal imply that its units are all so homogeneously conditioned that they must have homogeneous motions ?

The original draft of the postscript to the article "What is Electricity"? was amended in the light of the criticisms, oral and written, to which it had been subjected at the hands of Professor Tyndall, Professor Clerk Maxwell, and others. Admitting that the hypothesis had received no endorsements, he held that it had not been proved untenable. He published it, therefore, as a speculation only, adding to the postscript another postscript containing suggestions arising out of the criticisms.[1]

The constitution of the Sun, which had formed the subject of a paper in the *Reader* early in 1865, came up again in 1874.

To E. L. Youmans.

16 *October*, 1874.

Proctor, in the last number of the *Cornhill*, has been drawing attention to the conclusions of your astronomer Young that the sun is a hollow sphere. . . . His reasonings are in great measure the same as those set forth in my essay on the "Constitution of the Sun"—reasonings which I have been for the last year past intending to amend, in respect of the particular process by which the precipitated matters form the molten shell. There are mechanical difficulties, named to Clifford by Clerk Maxwell, to the mode of formation as originally described. But, on pursuing the results of the process of precipitation into vapour and then into metallic rain, perpetually ascending and perpetually thickening as concentration goes on, I reached a conclusion respecting a formation of the shell, to which no objection has as yet been made by the authorities with whom I have discussed it. Apart however from this particular portion of the hypothesis which needs amendment, Professor Young's conception of the Sun's constitution and the progress going on in the Sun, are essentially those which I set forth.

He at once set about amending his reasonings "in respect of the particular process by which the precipitated matters form the molten shell." A slip proof of the amended hypothesis was sent to Professor Clerk Maxwell, who, admitting that he did not "quite understand the principal features" of the hypothesis, adduced reasons to show that "a liquid shell supported by a nucleus of less density than itself, whether solid, liquid or gaseous,

---

[1] *Essays*, ii., 176-187.

is essentially unstable." On Professor Clerk Maxwell's letter (December 17, 1874) Spencer has pencilled: "This argument at first convinced me that my hypothesis was untenable. But subsequently the corollaries from Andrews's investigations concerning the critical point of gases, implying that a gas might become denser than a liquid and yet remain a gas, led me to readopt the hypothesis."[1]

This point with others is touched upon in correspondence with his French translator.

To E. Cazelles.

12 *May*, 1875.

I inclose impressions of some passages which will be substituted hereafter for certain parts of the essay on the "Nebular Hypothesis." [One of the alterations] is made as an abandonment of an hypothesis which Professor Clerk Maxwell has clearly proved to me is not tenable.

Respecting your question concerning the calculation of Tait, or rather of Sir William Thomson, I will write to you shortly, when I have refreshed my memory about it. Meanwhile I may say that I believe it to be wholly untenable; for the reason that it sets out with assumptions that are not only gratuitous, but extremely improbable.

20 *July*.—I sent you the other day Huxley's address in which he controverted the conclusions of Sir William Thomson respecting the age of the Earth and of the Solar System. I meant before now to have written to you, giving my own further reasons for rejecting the inference drawn from his assumptions —or rather for rejecting his assumptions.

8 *March*, 1876.—I referred the other day to Sir William Thomson's paper on the solar heat, published in *Macmillan's Magazine* for March, 1862. The aim is to show that the Sun cannot have been radiating heat at its present rate for anything like the time required by the inferences of geologists. The fallacy in his argument, which I remember to have observed when reading, I find to be this:—the calculation which lands him in his conclusion that radiation at this rate cannot have gone on for the required period, tacitly assumes the bulk of the sun to have been something like what it is now; whereas, on the hypothesis of nebular condensation, the implication is, that for vast periods before the Sun reached his present degree of condensation, he was slowly contracting from a larger size, and was all the while radiating heat. Helmholtz has calculated that

---

[1] *Essays*, i., 164.

since the time when, according to the nebular hypothesis, the matter composing the Solar System extended to the orbit of Neptune, there has been evolved by the arrest of sensible motion, an amount of heat 454 times as great as that which the Sun still has to give out. Now since a considerable part of this concentration and radiation must have taken place during the period in which the Sun's mass was receding inwards from the limits of the Earth's orbit ; and, as during all the latter stages of this period (say from the time when the Sun filled the orbit of Mercury) we may assume that the Earth has reached its concentrated form ; it is clear that, during all the remaining period of the Sun's contraction, the Earth must have been receiving its radiations, though in these remote periods the radiations must have been far less intense, *yet since they emanated from a relatively enormous surface subtending at the earth a relatively immense angle*, the total amount of radiation received by the Earth may have been as great or greater. Remembering that, were the Sun double its present diameter, it would need to radiate at but one-fourth its present rate to give us the same amount of heat, and that, did it subtend an angle of $5\frac{1}{2}$ degrees, one hundredth of its present radiation for a given portion of surface would suffice ; we see it to be not only possible, but on the nebular hypothesis quite certain that the Earth has been receiving light and heat from the Sun, adequate for purposes of life, for a period immensely greater than is inferable when the calculation is made on the assumption that the Sun's bulk has been during the time something like the same.

The dispute between the physicists and the geologists as to the age of the Earth and the Solar System has changed its aspect during recent years. Until a few years ago the temperature of the Sun was supposed to be due solely to concentration of gaseous matter and the fall of meteoric stones. Sir William Thomson estimated that the Sun has been giving out heat for a period of some twenty or thirty millions of years, and that geologists must limit their time demands accordingly. But recent discoveries in regard to radio-activity point to the possession by the Sun of other sources of heat. The duration of the solar heat may therefore be indefinitely extended—extended at any rate as far as is necessary to satisfy the geologist, with his indefinite, and, some think, not very modest, claim of from one to five or six thousand millions of years, as the period during which the Earth has been sufficiently cool to permit of the appearance of living things on it.

Across a correspondence with Dr. Charlton Bastian, Spencer has written: "This refers to the fact that Lockyer's speculations concerning the compound nature of the elements, as shown by the changes of the spectra, were pursuant on a remark I made to him expressing that belief."

To H. Charlton Bastian.

25 *November*, 1878.

One Sunday afternoon some four or five years ago, you and I called together upon Lockyer. . . . We chatted with him for some time in his laboratory, and our conversation turned upon Spectrum Analysis. . . . Have you any recollection of this conversation? and can you recall any opinion which I expressed respecting the implications of spectrum phenomena—what I thought was to be necessarily inferred from the more or less numerous lines contained in the spectrum of each element, and what I thought was to be inferred from that transformation in the spectrum of an element, which takes place under certain physical conditions? . . . As we walked away something passed respecting the bearings of what I had been saying upon the views contained in that work [Bastian's *Beginnings of Life*, recently published], leading to the remark that had you entertained the view, you might have begun your exposition somewhat further back.

From H. Charlton Bastian.

27 *November*, 1878.

I recollect the walk quite well to which you refer, our call upon Lockyer, and that there was a conversation in his laboratory in reference to the different spectra yielded by so-called elements, under different conditions of temperature, etc. I know that Lockyer told us about some of his recent results, and that you expressed some opinions in interpretation of the evidence, and concerning the transformations of the spectra to which he referred—but, unfortunately, beyond that I cannot go. The details have slipped from my memory.

I recollect the conversation afterwards to which you refer, and know that the general conclusions from the conversation with Lockyer favoured the view that the so-called elements were themselves products of evolution.

This view of the elements came up again some twelve years after.

From Henry Cunynghame.

30 *May*, 1891.

A short time ago, being in the company of Mr. Crookes, he was good enough to explain to me his theory as to the composition of the elements, which he thinks have been formed

by a process of evolutional segregation. He has devoted some years to experiments upon this question, and the behaviour of the rare earths, such as yttrium under the spectroscope, strongly confirms these views. For by long continued fractionation, different sorts of yttrium seem to present themselves, differing, as different breeds (say) of cows differ from one another. Of course the persistence of type, when once developed makes it practically impossible to transmute metals, just as, to use his own simile, you cannot, without returning to some primitive type, make a cow into a horse.

I said that I thought these experiments would be highly interesting to you as, in one of your works this view had been clearly foreshadowed. Mr. Crookes said that was so, and he had quoted your words in several of his lectures.[1]

On looking through Mr. (now Sir William) Crookes's pamphlets, Spencer wrote of them to Mr. Cunynghame as "yielding verifications of the view I have long entertained, and as tending to show how much more completely evolutionary the genesis of compound matter has been than I supposed. It is marvellous to trace in this field a parallel to the genesis of varieties and species." And to Mr. Crookes he wrote (8 June): "Your views—especially in respect of the development of varieties and species—carry out the evolutionary idea in this field very much further than I have ever dreamt it could be carried." It is doubtless true that if *First Principles* were to be written in the light of recent advances in physics and chemistry, it would in many important respects differ from the book as we know it even in its final form. At the rate of progress of recent years a book on physics, it has been said, cannot appear "that is not already out of date a week after the author returns his proofs." Spencer was aware that his outline of Inorganic Evolution, had reference to the knowledge of the time and was subject to modification with every increase in our knowledge. Granted that "he did not fully nor always rightly utilize the chemistry and physics of his time" (and who has ever done so?), he has the incontestible merit of having foreshadowed some of the most striking chemical and physical discoveries of recent years. The theory that the so-called elements are products of evolution was both

---

[1] Also in his address as President of the Chemical Section of the British Association of 1886.

novel and startling in the seventies. Now-a-days it may be said to be an accepted doctrine. Not only are the atoms no longer considered indivisible, but estimates are made of the number of corpuscles or electrons contained in a so-called atom; and descriptions are given of the struggle for existence continually going on among the communities of corpuscles, ending in the overthrow of the unstable and the continuance of the stable. Like species in the organic world, the atoms are evolutionary products, the result of competition and survival of the fittest.

Writing in July 1880, to Dr. Youmans, he mentions having met Mr. Moulton.

He told me that there had lately been made a discovery which tended to verify my hypothesis with regard to the interior constitution of celestial bodies: the discovery being that made by a Professor Ramsay of Bristol,[1] who, it turns out, is a very competent experimenter. He contributed a paper to the Royal Society, giving results respecting the transition from the gaseous to the liquid state, in which he made it manifest that, at the stage of pressure in which the gas becomes equally dense with the liquid, the line of demarcation of the two gradually becomes hazy and vanishes into a fog, and that, eventually, the liquid and the gas mingle so as to be no longer distinguishable. And Moulton drew my attention to the fact that this makes quite feasible, and in fact almost necessary, my supposition with regard to the gaseous nuclei of the Sun and planets. The result of this will be that I shall have to alter afresh that passage in the essay on the nebular hypothesis which I erased, and shall have to re-instate part of it and modify the remainder so as to incorporate with its arguments this revelation.

No article of Spencer's was subjected to so many revisions as that on "The Nebular Hypothesis." During January and the first half of February, 1883, he embraced the opportunity of a new edition of the *Essays* being called for to subject it to further revision.

To E. L. Youmans.

8 *March*, 1883.

At length I send you the portions of the revision of the article on the "Nebular Hypothesis." They have given me

---

[1] Now Sir William Ramsay, of University College, London.

an immensity of trouble, and I am heartily glad they are out of hand.

The trouble has been in part caused by the fact that I have subjected them to various criticisms, and on minor points have taken advantage of these. As a result I feel quite safe as to the legitimacy of the speculation. Of course it is a case of Speculation *versus* Speculation; and the physical arguments being admitted to be tenable, the thing has as good a basis as can well be given to it.

Towards the end of 1889 he again revised and added to the article, before incorporating it in the final edition of the *Essays*, being assisted by Mr. W. T. Lynn, of the Greenwich Observatory. Copies were sent for criticism to Lord Rayleigh, Sir William Thomson, Dr. Isaac Roberts, Lord Crawford, Mr. Huggins, and to Professors Dewar, Darwin, Williamson, Frankland, and Tyndall. Writing in reply on January 1, 1890, Sir William Thomson said that he felt quite lost when he tried "to think of anything that can be imagined as a *primitive* condition of matter. Of antecedent conditions we may freely reason, and with fairly sure judgment. But of a condition which can come, under known law, from no antecedent, or of a chaos which existed through infinity of past time till a declension of atoms initiated the evolution of kosmos, I can form no imagination. Yet we seem to require a *primitive* condition of matter." Whenever he had thought of it, he had "been led to think of uncombined separate atoms as the primitive condition of matter." "But assuming this to be the case, we see by perfectly definite calculations, that the heat of chemical combination from the condition of detached atoms to the actual state of matter . . . is *very small* in comparison with that due to gravity."

To Sir William Thomson.

3 January, 1890.

I am very much obliged by your letter of yesterday, giving me your criticism in such clear detail. Let me, while thanking you, express my regret that I should have entailed upon you so much trouble. I had not supposed that you would write so fully, or my conscience would scarcely have let me write to you at all, for I should not have liked to intrude so much upon the time of one to whom time is so precious, knowing

as I do by experience how terribly correspondence displaces matters of much importance.

I quite follow and fully appreciate the drift of your remarks, and more especially perceive that which I have not before recognized—the relatively small amount of heat evolved by chemical combinations among the ultimate units of matter, in comparison with the heat evolved by gravitation. It is clear that the amount of molecular motion possessed by each of such ultimate units must be transcendently great, before the quantity of motion lost by unions among them can be comparable in amount to the quantity of motion lost in the course of the journey to their common centre of gravity. Still, I suppose, one may infer that, if preceding unions of such kind had generated a high temperature in the nebulous mass, at a time when it filled the orbit of Neptune, a considerable increase in the time required for concentration into the present solar mass would be implied.

I am much obliged by the copy of the paper which at your request was sent to me by your secretary. I perceive that it contains much matter of interest to me. A good part of it will I fear lie out of the sphere of my comprehension; my mathematics, never very extensive, having become rusty.

Some years before he had urged Professor Tyndall, by way of change of work and scene, to "take up the general question of the condition of the Earth's interior. Recently, the numerous earthquakes and eruptions in various and remote parts of the Earth, sundry of them nearly or quite simultaneous, seem to me to be quite irreconcilable with the Thomsonian view that the Earth's interior is as rigid as steel. Further contraction of this rigid mass, the only possible cause assignable by Thomson, appears to me to be one quite incapable of explaining the facts."

To J. W. Judd.

23 June, 1890.

I recently read with much interest the report given in *Nature* of your lecture to the Chemical Society on the "Chemical Changes in Rocks under Mechanical Stresses." Especially was I struck by the paragraph which states that the "volcanic glass known as *marekanite*" "will, when heated, swell up and intumesce," and that "the brown glass ejected from Krakatau, during the great eruption of 1883, if heated, increases to many times its original bulk, and passes into a substance which, macroscopically and microscopically, is indistinguishable from the pumice thrown out in such vast quantities during that great eruption."

I am reminded, by this paragraph, of certain conclusions concerning volcanic eruptions which I reached after an excursion up Vesuvius during the eruption of 1868. Inclosed is a passage written some years ago, briefly setting forth these conclusions. Though not named in this interpretation (which is simply a note appended to the account of the excursion)[1] the character of pumice-stone had occurred to me as one of the evidences, since the liberation of water and its assumption of the gaseous state under diminishing pressure would, besides producing the effects above described, produce in many cases masses of vesicular substance. It matters not to the hypothesis whether the contained water is mechanically distributed only, or whether it is water of crystallization, or water chemically combined. In any of these cases, if it assumes the gaseous state the effects will be of the general nature described.

But my more immediate purpose in writing to you is to ascertain what is now regarded as the most feasible interpretation of such vast catastrophes as that of which Krakatau was the scene. On glancing at the summary of conclusions contained in the report of the committee appointed to investigate it, I find to my surprise that the eruption or explosion was ascribed to the intrusion of the sea: the implication being that action of a large body of water on a large body of lava would generate an adequate force. Is this probable? Such a co-operation would be limited to the surface of contact of the water and the lava. How could the evolved steam, quickly checked in its genesis by the chilling and solidification of the adjacent molten matter, move so vast a mass. In the first place how is the entrance of sufficient water to be accounted for? Its entrance could be effected only by a pressure greater than the pressure of the body of the lava, part of which extended above sea level. Considering the relative specific gravities of the two, such an intrusion would be unaccountable, even in the absence of greater hydrostatic pressure on the side of the lava. In the second place, apart from mechanical obstacles, I cannot see how intrusion and spread of the water, taking an appreciable interval of time, could have the consequence supposed. The probability appears rather to be that, by the steam first generated, local fissures would be formed, allowing of escape and preventing the requisite accumulation of steam, even could a sufficient quantity be evolved.

If, on the other hand, we suppose a state of things like that implied by the above hypothesis and implied, too, by certain results of the researches you have summarized, we have a force that is both adequate and of the kind required

---

[1] *Autobiography*, ii., p. 181, note.

to account for the various effects. On this hypothesis, the molten matter within the volcano, forming in the midst of its cone a column of, say, several thousand feet high, contains water which can assume the gaseous state only towards the upper part of the molten column, where the pressure is relatively moderate. Suppose that, at some place towards the lower part of the cone, some considerable area of its side has been thinned away by contact with the contained lava; and that, instead of emitting through a fissure a small stream of lava, as commonly happens, it suddenly gives way and collapses over, say, many acres, what must happen? Everywhere throughout the lava which rushes forth, the water and carbonic acid, relieved from pressure, become gaseous. The column of lava, extending high up the cone, suddenly falls perhaps a thousand or two feet, and relieves, from the greater part of the immense pressure it was subject to, the entire body of lava which filled the lower part of the volcano. The water and carbonic acid, imprisoned in every part of it, are liberated; and a mass of matter, of perhaps half-a-mile cube, suddenly explodes.

All the effects produced appear to be natural consequences. Once being ruptured, the sides of the cone, subject to the tremendous force of the escaping gases, would be likely to collapse and be in large measure blown away. Those parts of the molten matter which, not being very far below the crater, had parted with considerable portions of their water and carbonic acid in the shape of ascending and exploding bubbles, would, when wholly freed from pressure, expand in but moderate degrees, and so would form vesicular masses of pumice-stone, which, ejected in large quantities, would cover neighbouring regions, as the sea was covered round Krakatau. Further, the lower portions of the lava, which, subject to high pressure, had, until the moment of the explosion, retained all their water and carbonic acid would, when these were suddenly changed into gases, explode in such a manner as to dissipate their solid substances in small fragments, down to minute particles. Whence would result enormous volumes of dust, such as were produced by the Krakatau eruption and so widely pervaded the atmosphere.

Probably had not other occupations prevented me from being *au courant* with geological speculation I might have learnt that kindred interpretations had been given; but not having met with such, I am prompted by the bearings of your late lecture to inquire what is the present state of opinion on the matter.

In answer to the enquiry as to the present state of opinion, Professor Judd wrote (25 June, 1890):

While a few geologists still maintain that Volcanic Eruptions are produced by the penetration of masses of water to highly heated rocks—many, and I think the majority—following the late Mr. Poulett Scrope, hold that the gradual disengagement of water-gas and other gases *in the midst of a molten mass* (as the pressure is continuously relieved by each ejection) are the really efficient cause in a volcanic outburst.

In 1894 he thought of again calling in question the calculations as to the age of the Earth, made by Sir William Thomson (afterwards Lord Kelvin).

To T. H. Huxley.

1 *October*, 1894.

Has anything of late been said apropos of the controversy between yourself and Lord Kelvin concerning the age of the Earth? I am about to send for his volume of republished essays, but my impression, though a vague one, is that some of his data are inadmissible. I fancy that he is rather famous for reasoning mathematically from assumptions which are of a questionable kind, and then affirming positively the truth of his conclusion; and the world at large have that superstition in regard to mathematicians that they accept as a matter of course a conclusion mathematically reached, forgetting that its validity depends upon the truth of the data.

From T. H. Huxley.

3 *October*, 1894.

Kelvin and I have made no progress that I see. It is as much as I can do to get him to understand that the fact of evolution being proved by fossil remains, the time it may have taken is a question of quite secondary importance.

This information was asked for in view of a letter for *Nature* on "The Cooling of the Earth," which he wrote in the beginning of 1895, Mr. (now Sir) George H. Darwin being consulted. The letter, which was immediately withdrawn lest it should provoke a controversy, was as follows :—

One who is quite incompetent to criticize a chain of high mathematical reasoning may be not incompetent to form an opinion concerning the validity of the premises from which the reasoning sets out. Such premises may be entirely non-mathematical, and, if so, the mathematician cannot claim special authority for them : his assumptions remain open to criticism

by others than mathematicians. Thus looking at the matter, I venture to make a suggestion respecting the calculation of Lord Kelvin and the question at issue between him and Professor Perry.

The reasoning of the one and the criticism of the other are concerned exclusively with processes which have gone on within the body of the Earth. In the one case, a certain interior constitution is assumed, and from the rate of increasing temperatures at increasing depths below the surface, an inference is drawn respecting the time which has been occupied in cooling. In the other case, a question is raised as to the validity of the assumptions in regard to the Earth's interior constitution, and a consequent scepticism about the inferences drawn is expressed. But, in both cases, it appears to be assumed that the condition of things outside the Earth's body has all along been the same as now. It is assumed that whatever may have been the past temperature of the Earth's mass and of its solid or liquid surface, there have been the same facilities for the escape of its heat into space as there are at present. Must this assumption be accepted as beyond doubt? Are we not warranted in demurring to it? May we not even conclude that it is far from being true?

Since the existing heat of the Earth, and that much greater heat which the argument supposes it once to have had, are not otherwise accounted for, it might be contended that the nebular hypothesis (or the hypothesis of dispersed matter in some form), which alone yields an explanation, is tacitly assumed; and it might be fairly held that, if we are to go back upon the nebular hypothesis (or the hypothesis of dispersed fragments) at all, we must go back upon it altogether. Passing over, as not immediately relevant, the early gaseous state (either primordial or produced by collision), and coming at once to the condition in which the elements now mainly composing the Earth's crust were unoxidized, the inference might be that the uncombined oxygen and other gases must at that time have constituted a very voluminous atmosphere, and that the escape of heat through such an atmosphere, especially if it contained any compounds having the form of condensed vapours, must have been extremely slow. But without going back thus far, sufficient reason may be found for a demurrer to the current conclusion.

Let us grant the assumption made that the Earth's body has all along consisted of solid matter, if not such as we now know, yet akin to it in respect of density and conducting power. Evidently the inference drawn from the observed gradient of increasing temperature as we descend, itself implies the belief that the matter of the surface was once, if not at as high a temperature as the interior, still at a high temperature.

Suppose we go back to a time when its temperature was 152° C. At that temperature water boils under a pressure of five atmospheres (four plus the normal). The implication is that maintenance of the Earth's water, or rather part of it, in a liquid form on the Earth's surface, necessitated the existence of a quantity of aeriform water equivalent to more than a hundred feet of liquid water : that is to say, assuming the mean pressure of $2\frac{1}{2}$ atmospheres, the stratum of steam must have been over 70,000 feet deep, or more than 13 miles —an estimated depth which, taking into account the great expansion and indefinite limit of the outer part, would be much less than the actual depth. Even supposing this vast mass of water to have existed as transparent gas, the escape of heat into space must have been immensely impeded : the absorption of radiant heat by the vapour of water being so great. But the water could not have wholly existed—could not have mainly existed—as a transparent gas. It must in large measure have existed as a dense cloud of vast depth. The implication seems to be that, next to the heated surface of the Earth, there was a transparent stratum, but that above it came an opaque stratum of far greater thickness, at the outer limit of which went on condensation into rain. Under such circumstances the escape of heat must have been effected by convection-currents, ascending, expanding, falling in temperature, precipitating at the periphery, and there parting with heat into space. Must we not conclude that during this period the cooling of the Earth went on at a rate relatively small ?

During stages thus exemplified the changes in the Earth's crust, at first of igneous origin only, would begin to be complicated by others of aqueous origin ; and the geological processes which have brought about its present state would be initiated. But, manifestly, throughout the enormous period required for the tolerably complete deposition of the water, and the clearing of the air from its vast stratum of cloud, the rate of escape of heat would be still relatively small, and it would go on only slowly increasing, until there there was reached some such escape as that which now takes place through an air often cloudless, and at most times only moderately charged with water. During this era, the geologic changes would be actively proceeding, and there would be time for the deposit of a vast series of azoic strata—a time to which the present gradient of internal temperature gives no clue.

A long and complicated series of biologic changes would become possible after the temperature had fallen to 100° C. It is true that though some forms of *Protozoa* can exist at that temperature, or even a little above it, we may not infer that therefore life might then have commenced, for the agency of light may have been lacking. Though, with seas at a tem-

perature of 212° F., the stratum of cloud may not have been so dense as to prevent the passage of some light—though the darkness may not have been as great as that which exists at the bottom of the ocean, where nevertheless there is a large amount of life, not only of *Protozoa*, but of *Metazoa* considerably elevated in type—yet it may be contended that, as the life at the bottom of the ocean is dependent on nutritive matter present in sea-water, which has somewhere and at some time resulted from the decomposition of carbonic acid by chlorophyll with the aid of light, we cannot assume that light was not essential. Still the inference may fairly be that when the process of cooling from 212° downwards had gone so far that the universal cloud allowed a certain amount of light to pass, life became possible, and that biologic changes might have commenced at a time when the cooling process was not going on at anything like its present rate, and might have gone through many of their earlier stages before anything like the present rate was reached.

If it should be said, as seems possible, that the inference from the gradient of internal temperature stands by itself, and may be held valid without regard to changes in the Earth's atmosphere, this reply may be made :—Let us assume that the mass of the Earth once had an absolutely non-conducting envelope. Its temperature would then be the same at the centre and the surface, and there would be no thermal data from which its age could be inferred : nothing would negative the inference that it had so existed for an infinite time. Now, suppose the absolutely non-conducting envelope taken away and the Earth left bare. The cooling then commenced would, in course of time, produce a gradient of temperatures analogous to that which is found existing. But the data furnished by this gradient would give no clue whatever to the duration of the pre-existing period, throughout which the escape of heat was prevented. Any inference drawn as to age would be delusive. And if this must be admitted in the case of a sudden change from absolute prevention of radiation to absolute permission of it, then it must be admitted that a gradual change from great prevention to small prevention will also vitiate the inference. The observed gradient when the obstacle to radiation is small will be delusive, if supposed applicable to a time when the obstacle to radiation was great.

To state the case briefly in figurative language—the Earth had once a very thick blanket ; its blanket has in the course of immense epochs gradually thinned away ; and hence it would seem that an estimation of its age from thermal data, which assumes its present thin blanket to have always existed, is open to grave doubt—to say the least.

His last contribution to the Nebular Hypothesis was made in 1900, when preparing the final edition of *First Principles*. When writing section 182*a* [p. 484] he was in correspondence with Dr. Isaac Roberts, whose *Photographs of Stars, Star-clusters, and Nebulæ* he found very instructive. A month or two after the issue of this edition of *First Principles* he returned to the subject in a short paper on "The Genesis of Gaseous Nebulæ," which he intended to be added as Appendix D.[1]

In a short letter to the Editor of the *Fortnightly Review* (April, 1900) on "Professor Ward's Rejoinder," Spencer thus refers to the criticism arising out of the omission of Inorganic Evolution from detailed treatment in the Synthetic Philosophy :—

> He continues to harp upon the fact that the two volumes treating of Inorganic Evolution were omitted by me ; insisting that the fabric of conclusions drawn is vitiated by the omission. Observe the alternatives implied by him. Execution of the works dealing with Organic and Super-organic Evolution was thought by most to be impossible, and if preceded by works dealing with Inorganic Evolution would have been quite impossible. But in the absence of the part dealing with Inorganic Evolution the rest, according to Professor Ward, lacks "adequate foundations" and is valueless. Thus, it was useless to try the one course ; it was useless to pursue the other ; therefore, nothing should have been attempted. It was not allowable to leave the earliest stages hypothetical ; and, beginning with the chemical elements as we know them, to trace out later stages of evolution as conforming to one law. And then, when it was pointed out that the gap was not wholly vacant, but that (in addition to the sketch of Inorganic Evolution in *First Principles*) five sets of evidences I had given implied that the chemical elements have been evolved [*Essays*, i., 155-9], these are cavalierly passed over as having been set forth in three pages of a "fugitive essay."[2]

---

[1] See edition of 1904, p. 473.

[2] Fugitive, in the sense of being a review-article, but not otherwise :—not fugitive, since it contained disproofs of the belief then current among astronomers, but now abandoned, that the nebulæ are remote galaxies (see Proctor's *Old and New Astronomy*, p. 726) :—not fugitive, since the conclusion drawn respecting the Sun s photosphere (at variance with conclusions then held) was, two years after, verified in chief measure by the discoveries of Kirchoff and Bunsen.

CHAPTER XXVII.

HIS LAST BOOK.

(*July*, 1900—*April*, 1902.)

At the age of eighty, and with the purpose of his life achieved, Spencer had established an indisputable claim to complete mental repose during the few remaining years. But, as had been his wont, ere the work was completed on which he was engaged, he was planning another book. In September, 1899, he wrote to Mr. Appleton, of New York, that he wished to have the revision of *First Principles* out of hand "because I want to devote myself to some further work. I have still a little energy left, and still some things to write, which will, I think, make a volume not unlikely to be popular." As he wrote to a correspondent in the following year, mental occupation had become a second nature. "It is difficult after fifty years of writing to emancipate oneself from the habit. Life would be too dreary were the setting-down of ideas brought to a sudden rest."

Had he reflected he would have seen that there was little ground to fear that time would hang heavy on his hands. The widespread, varied, and prolonged influence he had exerted afforded a guarantee that the remaining years of his life would be well filled with the interests his writings and his personality had created or fostered. His characteristic impatience with intellectual error, moral delinquency, or remediable physical evil, would, despite good resolutions to keep out of the fray, continue to plunge him unwittingly into the thick of the fight. Correspondence, never light, had also to be reckoned with. Many of his correspondents were personally unknown; and not a few of them, though ostensibly anxious enquirers for

information, were in reality only common-place autograph hunters. Besides begging letters and applications for interviews, there was a continuous stream of requests for photographs, autographs, mottoes, sentiments; for advice in the bringing-up of children, on the organization of schools, on the management of debating societies; for expressions of his matured opinions on all manner of topics, ranging from the industrial situation in New Zealand to divorce in Italy. The octogenarian was expected not only to favour authors with an authoritative judgment on their books, but to justify this doctrine and to explain that doctrine contained, or supposed to be contained, in one or other of his own writings, extending over half a century. Mr. Andrew Lang says that bores fall into well-defined categories, and that a general lithographed reply should be framed for each category. Spencer had for years adopted some such measure of relief: his lithographed or printed forms having in some cases a space at the end for a sentence dealing with any special feature of the communication replied to. But Mr. Lang admits that it is not so easy as it seems to devise proper replies to some correspondents without employing profane language. From help of this kind Spencer was constitutionally and on principle debarred.[1] To certain requests the only suitable course was to make no reply. What could he say, for example, to the members of a literary institution in India, who asked for a present of all his books? How was it possible to write a satisfactory answer to a Hindu, absolutely unknown to him, and without credentials, whose business had been ruined by the famine, and who asked for a loan of £200? How could he, with his dread of visitors, give a favourable reply to a young Syrian who wished to spend the summer with him: "To accompany you in your daily walks, to hear what you speak, to observe how you act in all the common affairs of life"? While ignoring without compunction the general autograph hunter, he was always willing to send his autograph or photograph to friends. The claims of kinship, even though distant, were responded to, as in the case of a great-granddaughter of his uncle John, to whom, though he had never before heard of her, he sent three

[1] See however, *Autobiography*, i., p. 486.

autographs for her three children. Even bearers of the same name, without any bond of kinship, were occasionally favoured by these small attentions.

In addition to the customary requests from editors for articles, or paragraphs, he had in these later years to meet special requests suggested by special events. For example, —to send "some brief message of congratulation and counsel for the Federating Colonies" at the opening of the first parliament of Federated Australia; to write on "The Guiding Principle of Mankind in the Twentieth Century;" "to rewrite for the common people these two quotations from your admirable works;" to answer the question: "What is the chief danger, social or political, that confronts the coming century?" to send "a brief New Century message to English-speaking women"; to name his favourite author, which of this author's books he liked best, and his reason for the choice; to join in a symposium dealing with the ultimate settlement in South Africa; to write for a Fourth of July number "something in the way of an expression of your opinion regarding Peace amongst men"; to contribute towards a review of the year 1901, an article on "The Parliament of Man, the Federation of the World—to what extent do the Events of the year 1901 foreshadow the Realization of this Ideal in the Twentieth Century"; to express his opinion on "Lord Rosebery's letter announcing his 'Definite Separation' from the Liberal Leader." Not only did the infirmities of age negative compliance with such requests, but the very idea of writing on a text prescribed by others was one which he never could entertain.

The place selected for the summer was the Rectory at Bepton, just under the Downs, to the south of Midhurst in Sussex. "It was," writes Mr. Troughton, "a most charming spot, just the sort of place, in fact, to appeal to a man so passionately fond of the country as Mr. Spencer was. . . . It was here, amid this delightful Sussex scenery that he pondered over 'Ultimate Questions' and put into words the reflection which had more than once occurred to him as old age crept on apace—'Shall I ever again be awakened at dawn by the song of the thrush.'"[1]

---

[1] *Facts and Comments*, p. 202.

Letter writing was easier for him than personal discussion: for this, if for no other, reason that he could choose his time better. Animated conversation, as years went on, more and more upset him. Insomnia became more persistent; yet, so sound was his constitution, that his medical attendant remarked that "old age had scarcely touched him." The restrictions on personal intercourse made him all the more keenly alive to written expressions of sympathy. Thus he acknowledges congratulations from the South Place Ethical Society in July, 1900.

Declining years have their pleasures as well as their pains, and among the pleasures may be named expressions of sympathy, such as those contained in the address you send me on behalf of the South Place Ethical Society. Many, who have spent their lives in the development of their ideas, have not had the satisfaction of meeting with recognition. Only after their deaths have their ideas been appreciated. I have been more fortunate, and, having lived long enough to complete my work, have also lived long enough to see that it has not been without its effect. Thank you for your kind words, and for the expression of your good wishes.

The book he was writing clearly shows how deeply his soul had been stirred by the war in South Africa and the policy that led to it. Probably no political event in the whole course of his life moved him so profoundly. "I am ashamed of my country," was his frequent remark. Liberals equally with Tories were, in his opinion, responsible for the deplorable condition into which the country had drifted. For this, as well as for other reasons, he declined to join the League of Liberals against Aggression.

To A. M. Scott.

26 July, 1900.

I do not desire to be classed among those who are in these days called Liberals. In the days when the name came into use, the Liberals were those who aimed to extend the freedom of the individual *versus* the power of the State, whereas now (prompted though they are by desire for popular welfare), Liberals as a body are continually extending the power of the State and restricting the freedom of the individual. Everywhere and always I have protested against this policy, and cannot now let it be inferred that I have receded from my opinion.

Nor did he desire to be classed with the party that had seceded from the Liberals. In June, 1901, he instructed his secretary to write to the editor of one of the London papers :

When the Liberal Unionists seceded they were never weary of declaring that in all questions save one—the Home Rule question—they remained Liberals ; and so long as this question was prominent they were entitled to stick to the name. But things have changed since then, and their *raison d'être* as 'Unionists' has long since disappeared. . . . They have now nothing in common with the Liberals and everything in common with the Tories. Then why not . . . invariably call them Conservatives or Tories ?

To Moncure D. Conway.

15 *August*, 1900.

Waves of human opinion and passion are not to be arrested until they have spent themselves. You appear to think, as I used to think in earlier days, that mankind are rational beings and that when a thing has been demonstrated they will be convinced. Everything proves the contrary.[1] A man is a bundle of passions which severally use his reason to get gratification, and the result in all times and places depends on what passions are dominant. At present there is an unusual resurgence of the passions of the brute. Still more now than a generation ago, men pride themselves, not on those faculties and feelings which distinguish them as human beings, but on those which they have in common with inferior beings—pride themselves in approaching as nearly as they can to the character of the bull-dog.

To Wilfrid Scawen Blunt.

5 *September* 1901.

When is this dreadful state of things to end ? I hope that there may come a severe financial crisis, for nothing but the endangering of their personal interests will open the eyes of the war party.

7 *October*.—You are doubtless rejoicing, as I am, that the aspect of affairs is black for the Government and for the country. A little pressure on the market, a bank failure or two and a consequent panic, may open people's eyes and make them repent. However heavy the penalty they may have to bear, it cannot be too heavy to please me.

---

[1] To Spencer might have been applied the words of the *Times* regarding a Russian statesman : "His has been that untimely fate—the unhappiest that can befall a reformer—to sit helplessly by while reaction triumphs."

About this time he wrote (by way of suggestion, not for publication) to the editor of one of the London papers.

A strong point might be made against our proceedings in South Africa by quoting a passage from the charge of the Grand Jury, delivered by Lord Chief Justice Cockburn in the case of Governor Eyre and the Jamaica business. In that charge he emphatically asserted that the English constitution knows no such thing as martial law; saying that martial law has no independent basis whatever, but is an agency which comes into action only when the ordinary agency for maintaining law has broken down—is, in fact, nothing else than an armed servant of the ordinary law, which is called in when the ordinary servant is not strong enough to carry out its injunctions. This passage should, I think, be continually emphasized.

To the Right Hon. John Morley.

10 *November*, 1901.

I enclose you a copy of a letter written a little time ago, which had not the intended effect.

I enclose it because I see that in your speech the other day you quoted another distinguished lawyer on the question of martial law; and it occurs to me that if, as I see stated, you propose to bring up the question before Parliament this next session, it will be desirable to add Cockburn's opinion to Campbell's. . . . Martial law as properly understood ought to be nothing more than the calling in of the soldiery, with its accompanying discipline, when the police fails: the whole thing being done under command of the civil power, and ceasing when the civil power withdraws its command.

An interchange of letters took place with Dr. E. B. Tylor touching the controversy of 1877.[1] Spencer had drawn attention to a passage in *First Principles* (chap. ii., § 14, para. 2) in which occur the words "be it in the primitive Ghost-theory, which assumes a human personality behind each unusual phenomenon"—words showing conclusively that his own ideas had been formed before the promulgation of Dr. Tylor's opinions. Soon after, however, his secretary discovered that the passage cited was not in the earlier editions of *First Principles*, having been first introduced as late as 1890. Dr. Tylor was at once informed of this, and a long letter was afterwards written giving an

[1] *Supra*, chap. xiv., p. 190.

account of the genesis of his beliefs, going back to 1853 and concluding thus :—

> I feel bound to recall these evidences, as already said, because I cannot leave you under the impression that I accept your version of the matter, but I do not suppose your opinion will be altered. An idea fixed for thirty years is not easily changed, and it is impossible to change my own conviction, conscious as I am of what the facts were ; so the matter must now drop.

Professor Knight's article in the *Bookman* for January, 1901, was a welcome introduction to the new year. Its very sympathetic and appreciative utterances he prized all the more as coming from one who was in antagonism on more than one point. "In England (though not elsewhere) manifestations of approval have usually been so tepid that yours, being so exceptional, give me much pleasure." In May he was both "surprised and gratified" by an application from Mr. Brant-Sero (an Iroquois) for permission to translate *Education* into the Mohawk language. As if in answer to his complaint that manifestations of approval in England had been tepid, there appeared an article "On the Last of the Great Victorians," in *Black and White* (18 May, 1901)—on article pervaded by a tone of "deep and heartfelt sympathy."

Incidents like these belong to the bright side of 1901. On the dark side were not only the war and the alleged national degeneration ; there was also the continued shrinking of the already narrow circle of his friends : death having recently removed Dr. W. J. Youmans, Mr. John Fiske, Dr. Lewis G. Janes, and Mr. Robert Buchanan.

Occasionally one comes across a letter which shows how he was progressing with his last book.

To Sir Robert Giffin.

17 *May*, 1901.

> Is it possible to state in a rough way—of course in a *very* rough way—what is the amount per head entailed on producers by £100,000,000 of national expenditure in terms of working days? . . . I have in view the extra work entailed on those who are either manually occupied or are necessary regulators of those manually occupied, and on whom extra

taxation entails so much the more labour. I want to state how many extra days work in the year £100,000,000 of expenditure entails on these.

20 *May*.—I am immensely obliged to you for your note and memorandum. It tells me all I wanted. Nothing more than a rough estimate is possible or is requisite for my argument—an argument directed towards showing people that, as in all cases throughout history, those who enslave other peoples enslave themselves.[1]

To Right Hon. Arthur James Balfour.

Petworth, 19 *June*, 1901.

I believe it has been announced that you propose to drop the Copyright Bill. It is now 24 years since I gave evidence before a Royal Commission which sat in 1877, and among the recommendations agreed upon was one that the duration of copyright should be for life and 30 years after death, instead of being as now; and I believe the report of the Commission recently sitting endorsed that recommendation, omitted in the Bill now before Parliament. . . .

Would it not be possible to introduce a short bill doing nothing more than change the duration of copyright, leaving all detailed matters to be hereafter dealt with?

The matter is very important to needy authors who have families, since it is very much a question of leaving a good provision for children or leaving very little.

To me it is a matter of no personal interest, but only of public interest. I have bequeathed my property for the purpose of carrying on the *Descriptive Sociology* after my death. The returns from my books will form part of the revenues which will be available for the undertaking. Under the existing law a large part of these revenues will lapse seven years after my death.[2]

I have, however, a further reason for being anxious that

---

[1] *Facts and Comments*, p. 120.

[2] In his Will Spencer provided that the residuum of his estate should be devoted, under the direction of Trustees, to carrying on the publication of the series of volumes of the *Descriptive Sociology*, commenced in 1867 and stopped in 1881. Mr. H. R. Tedder, Secretary and Librarian of the Athenæum, was appointed general editor of the series. The following volumes are now in preparation:—Chinese, compiled and abstracted by Mr. E. T. C. Werner, H.M.'s Consul, Kiu Kiang, China; Hellenic Greeks, by Dr. J. P. Mahaffy and Prof. W. A. Goligher; Hellenistic Greeks, by the same; Romans, by Mr. E. H. Alton, F.T.C.D., and Prof. Goligher. Arrangements are also being made for a volume on the Ancient Egyptians.

the present law respecting duration should be changed, namely that as the law now stands it will be possible seven years after my death for anybody to publish the imperfect versions of my books of which the copyright has expired, though the perfect versions are still copyright. . . . This I should regard as a disaster.

To Sir Joshua Fitch.

Petworth, 1 *July*, 1901.

In something I am writing I want briefly to enumerate the various ways in which the militant spirit is infusing itself into our teaching institutions of all grades—military discipline, military teaching. . . .

I want to indicate also the way in which the tendency to unification in teaching has been growing. It was shown in the medical profession some years ago by an agitation for some uniform system of examination, but I do not know how that ended. Then there is the present Government's Education Bill, dropped for the time being, which takes away such small variety as arose from school-board management. And there is the endeavour to unify by introducing the ecclesiastical element more widely or, indeed, universally. Private schools are being put more and more to disadvantage, so that they are in course of being crushed out, and there results an increase of uniformity. Moreover, I remember a while ago there was a meeting of Head-masters of public schools, at which something like an appeal was made to the Government to bring them all under some kind of State control—again to unify the system. I wish to illustrate the universal tendency towards regimentation.[1]

He returned to Brighton early in September, feeling so much stronger that he contemplated taking a fortnight in London—an idea which, however, he had not strength to carry out.

A letter from Mr. Leslie Stephen (September 1901), introducing Dr. Stanton Coit, the editor of *Ethics*, induced Spencer to subscribe towards the Ethical Lectures Fund, while adhering to the view expressed in 1899 as to the qualifications of the lecturers.[2] He even assented to allow

---

[1] *Facts and Comments*, p. 134. In April following he wrote to several London papers, recalling a saying of Lord Salisbury's that "their aim must be to capture the Board Schools." "That which was then set forth as an aim is being now carried out."

[2] *Supra*, chap. xxv., p. 416.

his name to be given to one of the lectureships. His misgivings about the scheme presently re-appeared in another form, as one learns from a letter to Dr. Coit in November.

> The drift of the articles in your periodical, *Ethics* . . . opens my eyes to the certainty that there will be no sufficient agreement in the ethical views to be propagated by ethical societies. . . . So clearly do I see that some of the views enunciated will be views from which I profoundly dissent, that I must ask you for an abandonment of the proposal to give my name to a lectureship.

In another letter to Dr. Coit (1 March, 1902) he says: "I cannot without self-stultification continue to co-operate in any way, and I must therefore request that my name may be erased from the list of subscribers to the fund." But he was careful to add that his "dissent from the social ideals, which the Ethical movement, as now directed, will diffuse, must not be taken as evidence of contentment with present social arrangements."

His impatience as a reader, to which he so frequently alludes, was sometimes traceable to intellectual dissent, as in the case of Kant's *Critique*, sometimes to emotional or moral aversion, as in the case of Carlyle. In whichever of those two ways his further acquaintance with a book was put a stop to, the result, as far as concerned his estimate of the author's works, was the same. Instead of keeping his judgment in suspense, he was apt to form a very decided opinion, which in after life he seldom reconsidered. This trait was exemplified when Mr. Collins asked what he thought of Robert Louis Stevenson.

To F. Howard Collins.

18 *October*, 1901.

> Your question about Stevenson I answer just after having listened to a review of his life in the *Times*. I have read very little of him. I began to read many years ago *Travels with a Donkey in the Cevennes*, but was so disgusted with his treatment of the donkey that I gave it up quickly and never looked into another of his books for many years.

His opinions as to the value of learned Academies had long been well-known. Is was, therefore, from a feeling of the courtesy due to an author of distinction, rather than

from any expectation of receiving a favourable response, that he was invited to join the movement for the institution of a British Academy of Letters.

To Sir E. Maunde Thompson.

20 *November*, 1901.

I am obliged by the invitation made by the sub-committee you name to be one of those to receive the charter of the proposed British Academy of Letters. I must be excused, however, if I do not accept the invitation. . . . I have, in contesting the views of Mr. Matthew Arnold, who wished for an English Academy, given expression to sundry objections, and I still hold those objections to be valid.

Sir Joseph Dalton Hooker, Lord Avebury, and Spencer were the sole survivors of the X Club; but they rarely met in these years. Occasionally letters passed between them.

To Sir Joseph Dalton Hooker.

16 *November*, 1901.

It is a long, long time since any news passed between us —a year and a-half, I think. Superfluous letter writing is at your time of life, and even at mine, a thing to be avoided; but still, I should like to have a few lines telling me how you fare in your contest with the inevitable. . . . I am taking my daily drives and doing a fair amount of work.

A sentence in Sir Joseph Hooker's reply—"You have held, and still hold, a big grip on my life"—shows how strong the bond of their friendship was.

From Lord Avebury.

25 *January*, 1902.

You may have seen that the Committee of the Society of Authors, over which I have the honour of presiding, have suggested your name as the one we should put forward from England for the Nobel prize.

The suggestion I may add has been cordially received.

As one of your oldest friends it has been a great pleasure to me to take a part in endeavouring to secure for you this well merited recognition.

Spencer's name was forwarded to the Swedish Academy, but the prize was not awarded to him.

He was trying to answer the question, "What should the Sceptic say to Believers?"[1]

---

[1] *Facts and Comments*, p. 200.

To Mrs. Sidney Webb.

14 *February*, 1902.

My special motive for writing is to ask whether you did not once tell me that your girlhood was often made miserable by your religious convictions—by the thoughts of hell which had been instilled into you. And my reason for asking this is that I am just now about to say a little upon the difficulty of the agnostic in dealing with others—when to leave them alone and when to attempt to change their convictions. There are various cases, and I want to say a little about each kind. There is, I believe, a good deal of religious despondency, and not a little religious insanity, and all this evil has to be set off against what may be said on the other side.

Facts and Comments was published in London and New York on April 25, 1902.

To Alexander Bain.

25 *April*, 1902.

I bait my hook with a book in the hope of catching a letter. You either have received or will shortly receive a copy of *Facts and Comments*, which is my last book, written during these two years at the rate of ten lines a day.

I have heard nothing of you for a long time save the accounts which Duncan has given me on the occasions of his visits down here. You, too, as I gather, are much invalided, but are still able to take a drive daily. This unfortunately I cannot do. . . .

I not unfrequently think of the disgust you must feel at the fate which has overtaken *Mind*. That you, after establishing the thing and maintaining it for so many years at your own cost, should now find it turned into an organ for German idealism must be extremely exasperating. . . . Oxford and Cambridge have been captured by this old-world nonsense. What about Scotland? I suppose Hegelianism is rife there also.

As friend after friend was removed by death, Dr. Bain, like Spencer, cherished all the more warmly tokens of fellowship from those that survived. "I never saw such a beaming smile on Dr. Bain's face as when he showed it [the above letter] to me," said his wife to the present writer. "He was evidently extremely pleased to hear from Mr. Spencer, and Mr. Spencer's sympathy in connection with *Mind* was most highly valued."

Next day Spencer wrote to Professor Masson in a similar strain.

I suppose Hegelianism is rife in Edinburgh as it is in Oxford and Cambridge. This is one of those inevitable rhythms which pervade opinion, philosophical and other, in common with things at large. But our Hegelianism, or German Idealism in England, is really the last refuge of the so-called orthodox. As I have somewhere said, what could be a better defence for incredible dogmas than behind unthinkable propositions?

In December previous he had written to the Editor of *Mind*, with reference to the promise made to Professor Sidgwick at the time *Mind* changed hands, guaranteeing his financial support.

Since that time *Mind* has been becoming more and more conspicuously an organ of the Hegelians, or of German Idealism. The result was that, just before my first annual subscription became due, I wrote to my bankers to erase my name as a subscriber. Of course I should regard it as quite appropriate that each school of philosophic thought should have its say, but of late one school has been having very much more say than the rest. It cannot be expected that I should aid the survival of a periodical so largely devoted to the expression of views diametrically opposed to my own.

The appearance of his last book just two days before his eighty-second birthday lent additional meaning and fervour to the annual greetings.[1] Thus Lord Hobhouse wrote :—

Though, alas! the generation is froward; and some of your good seed has been devoured by fowls of the air; and some fallen on barren rock; and some choked by thorns; a great deal has fallen on good ground, and has brought forth fruit manifold, and will assuredly bring forth more in more favourable seasons.

To Lord Hobhouse.

4 *May*, 1902.

Among the many congratulations received on the occasion of my eighty-second birthday I can say very sincerely that none have been so appropriate, and therefore so pleasurable to me, as that for which I have to thank you.

---

[1] Among the greetings from abroad was the usual letter and birthday gift from M. Geza Schulek, of Buda Pesth. Three years before this date he and his wife had come to England expressly to see Spencer for a few minutes.

It is, as you say, doubtful whether the event itself is one to be rejoiced over, but you express my own feeling fully, when you say that it is a matter of rejoicing to me that I have lived long enough to complete the work, which half a century ago I conceived and soon after definitely undertook. Some small aims of no great moment remain unfulfilled ; but, passing these by, I have the satisfaction, which I suppose is rare, of having done what I proposed to do ; and it adds to this satisfaction to receive this expression of your sympathy.

You too have been working towards ends which the course of things is thwarting, and we must both be content with contemplating a remoter time when good efforts made now will have some effects, though they may be infinitesimal.

An envelope, containing a lock of his hair, encloses also a note, of which the following is a facsimile :—

CHAPTER XXVIII.

THE CLOSE OF LIFE.

(*April*, 1902—*December*, 1903.)

Facts and Comments had been definitely announced as his last book. This circumstance, together with the varied nature and contentious character of the work, tended to excite more than the usual interest. Professor Masson thought it "eminently readable and interesting—none the less that much of it is provocative of dissent, and is sure to be protested against in various quarters. I refer especially to the questions concerning the war and other present-day questions. If I say that here and there I am among the dissenters in this department, that will not, I am sure, distress you much." Sir Joseph Dalton Hooker was a dissenter, or at least a partial dissenter, about the war. Professor Bain thought the "showing up of Matthew Arnold's absurd claim for the State-Church as the exclusive nursery of men of genius was a very deserved and important correction. But perhaps the part of the book that aroused my deepest interest was your concluding remarks on Ultimate Questions." While recognizing it as "the conclusion of strenuous, honourable, consistent work," the *Times* noted in these "slight, sketchy, and imperfect" utterances "a tone of persistent egotism," too great to be quite excusable. The essay on "Some Light on Use-Inheritance," "has the charm of copious and felicitous illustration in which Mr. Spencer is unsurpassed." "We should have liked the latest words of one who has deeply influenced his generation to be measured, calm, equitable, peaceful. In some of these essays are present these qualities . . . . But in too many of the *Facts and Comments* is a tone of acerbity." The *New York Saturday* of May 17 was gracious enough to excuse this "excursion into the domain of fads,"

on the ground that "a man of eighty-two is too old to work and may play if he likes. If in setting his desk in order he comes across scraps of disconnected literary output, which did not fit anywhere in his earlier books, and he chooses to gather them into a haphazard collection . . . why should he not do so?" Readers in the United States were naturally interested in "A Few Americanisms," and were not unwilling to avail themselves of the invitation, conveyed in the last paragraph of the article, to expose deteriorations in the English language as spoken in Great Britain. Among the causes that contributed to create more than the usual demand for the book on the Continent, not the least were its denunciations of the South African War: these denunciations seeming to afford a justification for the general dislike to Great Britain during those years. So popular was it in France that three translations were offered. In Germany more than one version was proposed; but, instead of translating the whole book, selections were made from it and from *Various Fragments*. At one time it looked as if there would be no Italian translation, Spencer having intimated that rather than tolerate the persistent repudiation of an author's rights he would prefer to let the book remain untranslated. "It is not that I care about the actual amount receivable. In proof of this he handed over to the translator his own share of the amount paid by the publisher. Russia, so long in the front rank, had years ago fallen behind. Spencer's books continued to be objects of suspicion to the Russian authorities, whose blundering ignorance is shown in the *Times* of July 28, 1903. A student, on being examined for admission to the University, was charged with being a socialist, on the ground that he had been seen in the street at the age of 15 with Spencer's *Sociology* under his arm! Nevertheless, *Facts and Comments* appeared in a Russian dress before it was published in French or German.[1]

In May, 1902, he went on what was to be his last visit

[1] Since the year 1865, when proposals to translate his books were first thought of, most of Spencer's principal works had been rendered into Russian, French, German and Italian. Portions of them had also been translated into almost all the other languages of Europe, as well as into the chief languages of India and into Japanese and Chinese. During his last years translations of *Education* into Arabic and Mohawk were mentioned.

to the country, Leith Vale, Ockley, in Surrey, being the place selected. How he enjoyed himself was thus described at the time by Mr. Troughton : "Above all he is delighted with the multitude of song-birds hereabouts. Listening to the birds the other day, while sitting outside under the verandah during a short spell of sunshine, Mr. Spencer said, 'This is what I have been looking forward to for the last six months.'" His absence from Brighton deprived him of the pleasure of meeting one with whom he had corresponded a great deal, but whom he had never seen—the Dowager Countess of Portsmouth, who first became interested in him through her brother, the Hon. Auberon Herbert.

To the Dowager Countess of Portsmouth.

6 *June*, 1902.

I am very unfortunate. Some years ago you honoured me with a call at Avenue Road, and I was out. And now that you are about to visit Brighton I am away from there. . . .

The contretemps is very provoking, since I should have been greatly pleased to see one from whom I have received so many kindnesses. I fear I thus lose my last chance, for being now eighty-two, the probability that you will again visit Brighton during my life is but small.

To Mrs. Bray.

6 *June*, 1902.

Allow me at eighty-two to shake hands with you at eighty-eight! I say shake hands rather than offer congratulations, since you know as well as I do, or better, that the infirmities and weariness of advanced years are such as render continuance of them not a cause for congratulation. . . .

I managed three weeks ago to get to this place, which is in all respects charming, and I am on the average profiting by the change.

The requests for contributions from his pen were varied and numerous. He was invited by the Danish Minister of the Interior, through Mr. Goschen, the British Minister, to write a short article for a journal which was to be issued weekly during the Exposition Historique de la Presse Danoise, the subject prescribed being an inquiry as to the direction in which social development was tending—whether towards socialism or individualism. This invita-

tion was declined "because the amount of thought required would be too great a tax." The approaching Coronation brought many such appeals. A few lines "on the subject of the Trust in Atlantic Steamships" were solicited by one of the London daily papers. *The Neue Freie Presse* was eager to get a contribution for its Christmas number—"Anti-semitism" being suggested as a topic. Mr. Spielmann begged for a few words on the condition of the Jews in Roumania. The *Giornale d' Italia* sought his opinion about the suppression of the Religious Orders in France. "A few words of sympathy and support" were sought by a small number of people in Melbourne, who were forming a society bearing his name.

Peace had been proclaimed and there had now to be faced the consequences of the war. The condition of the sufferers, whether Boers or Britons, aroused the active sympathy of all parties. Among those who had suffered most was Ex-President Steyn, whose fortune and health were completely shattered by his heroic efforts to save the independence of his State. While Mr. Steyn was on his way to Europe, to obtain the best available medical assistance, Spencer was asked to give his name to a movement to send some token of the sympathy and admiration of well-wishers. He readily assented on condition that the matter would be kept entirely private, and that the secretarial work would be done by the friend who had made the suggestion.

The gift was transmitted with the following letter :—

To Ex-President Steyn.

10 *August*, 1902.

A few friends in England have paid me the compliment of making me the medium for transmitting to you the accompanying testimonial of their sympathy and high admiration. They believe, as I do, that nowhere among historic characters is there to be found one whose persistence in upholding a cause he believed to be right has been more conspicuous. Even enemies must admit that sacrifices of position, property, and health, which have ended in a prostration so extreme as that which you now suffer, imply a heroism rarely to be found among men. To emphasize their belief and accompanying admiration, they beg your acceptance of this proof of their great regard, joining to it the hope that with care, and the attention of sympathetic friends, you may yet recover.

Needless to say, this spontaneous recognition of his honesty of purpose and of the self-sacrificing devotion with which he had pursued the course he believed to be right, was gratefully appreciated by Mr. Steyn. The value of the gift was enhanced by the medium through whom it was transmitted, Spencer's having been an honoured name in South Africa, long before the outbreak of the war.

Spencer was eagerly waiting for public intimation of some centre of co-operation for the collection of subscriptions to the Boer Fund, and represented to General Botha and his colleagues, who were then in London, the impolicy of delay.

To General Louis Botha.

24 *October*, 1902.

I have been both astonished and greatly annoyed by the way in which the Boer Relief Fund has been managed in England. We have a maxim, "Strike while the iron is hot"; whereas the course pursued seems to have been "Wait till the iron is cold"!

If, immediately after your interview with Mr. Chamberlain, there had been an advertisement, naming a committee of some three or five, with an indication of the bank to which subscriptions might be paid, there would at once have been a response from a great many who now have become almost indifferent from mere lapse of time. Two months have passed, and the feelings of the sympathetic have been allowed to die away before anything practical has been done. . . . The whole thing, in my opinion, has been dreadfully bungled. Pray have the thing put in such business form as is always taken by any body which proposes to raise subscriptions.

General Botha shared Spencer's regret that so much precious time had been lost. But being without experience in circumstances entirely new, he and his brother delegates had to be guided by the advice of their friends.

A request made by the Rationalist Press Association for permission to publish a cheap reprint of the first part of *First Principles* was declined for reasons stated in the following letter.

To George J. Holyoake.

26 *August*, 1902.

Two mischiefs are apt to arise from reading separately the first part of *First Principles*: (1) Those who are opposed to its views conclude that the second part, being as they think based