EARLY LETTERS TO LADY STOKES.

69, Albert Street, Regent's Park,
London, N.W.

Jan. 5/57.

I had my first lecture to-day. I got on satisfactorily to myself and I hope interested the men. I shall like my lectures better now for I am coming to electricity which is a more interesting subject to lecture on than mechanics.

At Jermyn St. I got my quarter's salary and found a letter from the Abbé Moigno. It was written to induce me to interest myself in an improvement in printing from a photograph, due to a M. Poitevin (if I have read the name right). The Abbé said that they were endeavouring to place me on the list of candidates for the Corresponding Membership of the French Academy at the coming election. He also called my attention to a paper in Poggendorff by a M. Holtzmann, who had repeated with some variation my experiments on the polarisation of diffracted light, but had arrived at an opposite conclusion as to the direction of vibration. I went to the Athenæum and looked over Holtzmann's paper. I had not time to read it carefully through as my lecture was coming on. I think I must repeat Holtzmann's experiments and my own, before I can come to a mature judgment on the matter.

...It looked funny to see mathematical formulæ in a letter from you. Don't be too ambitious for you will not understand these things. They go much beyond Euclid and Algebra, which will be quite enough for you in the way of pure mathematics; but you may be interested by chemistry and physics.

Jan. 10, 1857.

My cold is gone, reduced to only the dregs of the dregs of a cold, an infinitesimal of the second order. After dinner to-day I finished *The Caxtons*. Tell Mrs. Robinson that I have come to the end; not that it was a task, for I like it very well. I suppose you would laugh at me, being in the habit of devouring a novel at a sitting, or two sittings at the most. I take it more by sips, the way a man takes wine. To-day I had my men to an

examination. Alas! I fear they have not been taking in my lectures. It is hard to teach mechanics to men who have had no previous mathematical training. I am now in electricity; a much more interesting subject to lecture on than mechanics.

I doubt if I shall make any further remarks on Prof. Challis's paper. I think I shall send my friend the Abbé Moigno a few lines on M. Holtzmann's experiments to insert in *Cosmos*, a weekly scientific newspaper which he edits. I have got to test the reflecting power of the speculum which Piazzi Smyth used on the Peak of Teneriffe, the reflecting power I mean with regard to the invisible rays. This I hope shortly to do; it won't take long.

I intended to teach you a little Euclid and Algebra just for the sake of strengthening the mind, not for the sake of enabling you to follow my mathematics. I am afraid Euclid and Algebra would not help you much towards understanding what I write on pure mathematics. But I chiefly busy myself about physics, and you will be able to understand something of that after you have seen some experiments.

I am afraid you have puzzled me with your chemistry. The tarnish on the bracelet is, I suppose, a very thin film of sulphuret of silver, and I don't know what will take off that. I tried my hand once on a half-crown belonging to my friend Power of Pembroke. It looked a little suspicious to begin with, and after trying various chemicals I left it looking much more so. It looked about half-way between gold and silver, yet I believe it was perfectly good.

...I send you my smaller colour discs to name your father's by I asked Maxwell to lend your father a copy of his paper if he had one to spare. Please tell me if one arrived.

Jan. 12, 1857.

...I am still on electricity. I have far more and prettier experiments to enliven my lectures by than I had when I was on mechanics. I gave No. 37 to-day and my course consists of 48.

Jan. 17/57.

As no mails go out on Sunday you will not I fear get this for a day and a half after the 55-pager. I am afraid the latter, though it will probably be interesting to you (if I may judge how

I felt about similar letters to me), may have given you some pain. What gave me occasional pain, being no longer pent up, can now give me no more, and I feel so happy now that there is nothing to interfere with my love. The ghosts looked always wrong; but now how silly and contemptible they appear as well!...

The 55-pager explained to you my motives for the first time, and it is better you should love me as what I am than as something else. My letter this morning was for a letter something like Hofmann's methylethylamylophenylammonium for a word: I guess you never got a 55-pager before.

Jan. 21/57.

...So then, my dearest Mary, though the ghosts were bad enough they were not quite so black as you may perhaps have imagined. It grieves me to think that you should look forward to but little love. I fancied I should love you as few wives comparatively get loved, but of course I cannot tell, for I cannot look into other people's hearts. And as to coldness I cannot tell you more of my heart than I know myself, and the heart is said to be deceitful, but it appears to me to be thus. Do you remember how I told you I broke my arm, and how the muscle between the elbow and shoulder fell away when the arm was in the sling and how rapidly it came to again when the sling was laid aside and the arm used? Well, I believe it is even so with my feelings of affection. I have been living so much alone "amidst thoughts and making out things" that they are not so strong as those of one living in the bosom of a family; but they only want an object on which to exercise themselves. And would it be a satisfaction to you to think that you were under God the means of supplying to my character one thing it wants much? Already I feel the difference. To me as well as to you it seems quite natural that people living together, and both trying to do right, though it may be with many faults, should love each other.

I quite approve of your views, and perhaps they are the truer on the whole, but I think mine are admissible too. Why should not love and duty go hand in hand to effect a sacrifice which love alone might have been insufficient for? And may not the love in the end be quite as deep though helped over the initial difficulty by duty? I was capable of being moved, mathematically as it were, by the belief that a particular course

was right; and I do believe that God put these views in my mind, working by means of that which was in me to supply that which was wanting.

...And now the last thing is this; that I do verily hope and expect that I shall be both happier and better for being married to you. It seems to me that I am all the happier from not having made a calculation or estimation as it were of the balance of happiness.

I cannot compare myself with another, but only with myself, and it seems to me that I both do and shall love you better and more surely as it is than I could if I had fallen in love with you in the more ordinary way.

Jan. 24, 1857.

...True, nothing human can be perfect; but any incipient annoyance is prevented by confession from interfering with love.

To reassure you after the great pain I fear I have caused you, I will tell you one thing which will show how I regarded you about two and a half years ago and regard you still. I have never told it to you yet. When the clouds seemed to clear off in that wonderful way, and I saw how I was on the point of sinking into an old bachelor (I mean no disrespect to the *genus*) and felt how much better married life, if carried out as I looked forward to, would be, I felt that perhaps my marriage with you would be even the turning-point of my salvation.

I shall feel anxious to hear that you are again well and happy, but I must have patience.

And now my dearest Mary I hope that henceforth you will be no more afraid of me, but will believe me to be, ever your affectionate, George.

Jan. 27, 1857.

Now that I feel that there is nothing to keep me from opening my heart to you any time in case of any trouble, and find that you are thank God happy again, it seems as if I have got social even in my lonely lodging, and can join your social party at Armagh and form one of the family.

You are quite right in saying that it is well not to go brooding over one's own thoughts and feelings; and in a family that is easy, but *you* don't know what it is to live utterly alone.

Jan. 29, 1857.

...Trials of some kind or other are almost sure to come, and death we know must come at last; but even death itself is only an incident in the bright course if we live as God grant we may. But how many a bright prospect is marred by human fault, from the loss of Paradise downwards. We must bear and forbear, love, comfort, and forgive, until by God's mercy we reach, as I trust we may, that City, into which must enter nothing that defileth, where there shall be no more death, neither sorrow nor crying, neither shall there be any more pain, for the former things are passed away; where, while faith shall have no more exercise, and human knowledge shall have vanished away, love shall abide for ever.

Jan. 30, 1857.

...Last night was a R. S. night. I had a paper to read out on the minute anatomy of the earth-worm and another on a kindred subject. I told General Sabine that in reading such papers I felt as if I were a pair of bellows to be blown for the benefit of the Society. I felt I was a sort of reading machine, the papers were so completely out of my line. Some curious and rather metaphysical discussion arose on one of the papers. What do you think of the doctrine that the queen bee and all the bees of the hive are but one individual?

Jan. 31, 1857. 4 p.m.

I have just finished my Smith's Prize paper, the MS. I mean, and am going to post it to Cambridge to be printed. I have not been out yet to-day, as I wanted to finish it. This paper I find comes in rather awkwardly. It is a paper for the highest Cambridge men, which I have to set to when my thoughts are running in quite a different channel, physical experiments for my lectures at Jermyn St. and so forth. At this time of year at least, Cambridge mathematical books are beginning to look strange to me.

I am going to Cambridge on Monday evening, coming back on Wednesday night, going back to Cambridge on Friday morning, coming back to London on Monday morning.

I have only three more lectures at Jermyn St. which I must give, but I shall give some extra ones.

Pembroke College, Cambridge,
Feb. 3, 1857.

There are nine men now in my room writing away at my Smith's Prize paper, so having nothing particular to do just at present I take up my pen to write you some news, etc., but my fingers are rather numb with cold and not in first-rate writing order.

On Sunday after Church I walked to the Parks, at least Hyde Park, Kensington Gardens, and Regent's Park. There were hundreds, I suppose thousands, on the ice. I did not see any accidents, but I saw by the papers that numbers got in, but no one was drowned.

On Monday (yesterday) after my lecture I called at the R. S. about a paper, and then came here by the 5 p.m. express which arrived at 7. It seemed very cosy getting back to my rooms again and having friends about me. I spent most of the evening in the rooms of different Fellows talking.

And now I must tell you one thing which may be, but I don't think is likely to be, of importance to us, at least at present. If it comes to anything it may make me wish to put off my marriage, as I mentioned to you a day or two after we were engaged. I should not like what I am going to tell you to go beyond you and your father and Mrs R. at present.

Even before I went to the Observatory last August, I thought that as to the time of our marriage I ought not to put it off merely for the sake of saving more money, when I had what prudent and experienced people considered enough (I thought it might be deemed necessary or rather advisable to save money for two or three years in case it should turn out that you had not any, but it was not so deemed either by your friends or mine, but the contrary), but that I ought to put it off a little if my marriage would throw difficulties in the way of University reform so far as relates to the endowment of my own Professorship. Everybody allows that in the abstract the Professorships ought to be endowed, but where is the money to come from? There seems no source but the appropriation of Fellowships to the endowment of Professorships. This would be a very strong measure, like the alienation of private property; for Fellowships are not endowed by the nation, but given by private individuals who founded them.

PEMBROKE COLLEGE, CAMBRIDGE,
Feb. 7, 1857.

On Wednesday night I went again to Town by the 7.10 p.m. train for R.S. work next day. It was a Council day, so we breakfasted with Lord Wrottesley* and discussed business of the R.S. till about one; then I called at Jermyn Street, to speak to Sir R. Murchison, went on to the R.S., arranged my papers, and had just a few minutes to write to William before the Council met. We had done earlier than usual, about 20 minutes before 5. Then dinner of the Philosophical Club at 5.30; then evening meeting of the R.S. at 8.30. I had little to do at this but to listen, as it was the Bakerian Lecture, delivered by Faraday *viva voce* or I should rather say in person; for on other occasions the papers are read by the Secretary. Next morning I got up at 6 h. 20 m., walked to the station 3 or 4 miles, got a cup of tea and morsel of bread and butter, and got off to Cambridge by the 8 a.m. train, got here (i.e. to the station) at 10 and breakfasted in my own rooms. That day (yesterday) was spent in looking over, i.e. completing the looking over of, the Smith's Prize Papers. William came about 1 p.m. for the meeting of the "Family" Club, so he slept here last night, but he breakfasted with his friends Dr and Mrs Paget, who have recently lost their second (by age) child, having now out of three only the eldest left. We met yesterday at 5.30 p.m. to decide the prizes, at the Vice-Chancellor's, Dr Phipott, Master of St Catharine's Hall. The Master of Trinity (Dr Whewell), Prof. Challis, and myself, examined this time. We gave the two prizes to the first two wranglers, but reversed the order, making Savage, the second wrangler, a Pembroke man, first Smith's Prizeman. The decision over, we dined with the Vice-Chancellor; Professor, Mrs and Miss Willis were of the party. When Professor Willis first shook hands with me before dinner he asked me where Mrs Stokes was. I told him there was no Mrs Stokes yet. At dinner I happened to sit on the Vice-Chancellor's left and Mrs Miller on his right. The subject of houses happened to be mentioned, when Mrs Miller said to the Vice-Chancellor that I should be wanting one shortly. It was the first the Vice-Chancellor had heard of it, so he congratulated me, and Mrs Miller was pleased with having had such a piece of

* President of the Royal Society, 1854–8.

news to tell. Here comes the porter for the letters, so I must conclude, but indeed I have told you most of the news. William went off at 10 a.m. to London. He means to return to Denver on Monday. I am going to return to London on Monday morning. To-day I tried my Ruhmkorff's coil for the first time. I got sparks 0·4 inch long with a Callan's battery of 8 cells in indifferent action. I showed it to several of the Fellows as well as to my old bedmaker. She remarked, "You will have a great many things, sir, to amuse a lady." I suppose she meant you, but nothing had occurred to remind her of you. I could not help thinking to-day how very funny it would be, your overhauling such an old bachelor philosopher's den as my rooms are. I had William last night in my bed in my bedroom, so I slept myself on a stretcher with my head right under a lot of chemicals, my feet nearly opposite a shelf of chemicals or rather set of shelves, and the table of the room so blocked up by bottles, funnels, test-tubes, etc. that there was room for nothing and I put my basin, etc. on a plank lying on two chairs.

As to the time of our marriage, I shall proceed on the supposition that it is to be Easter Tuesday unless something occurs to prevent it. I don't think this at all likely as far as changes are concerned.

Ash Wednesday, Feb. 13th, 1857.

It is high time that I should write you a longer letter than I have done for the last two or three days. One thing which prevented me was not knowing where to address. However I think I will post this to-night, addressing to Armagh, unless I get a letter from you by this afternoon's post leading me to address elsewhere.

If I were to marry *now* I should not think of taking a house in London; for the subject of Professorships is fairly before the Council. From all I have heard that has dropped out about their deliberations, I think it probable that they *will* make recommendations as to endowment and probably even as to the foundation of new Professorships; but whether the Senate will sanction their recommendations remains to be seen. Anyhow I expect that a bold effort will be made towards endowment, whatever may become of it. No talk has been made, so far as I have heard, *in the Council* about endowing them in the way I mentioned; probably it will be proposed to tax the Colleges for the benefit of the whole University. As far as we can guess what is probable, it seems likely enough that the Professorships will be endowed,

but it will probably take a few years to effectuate the changes. I that be so I think my plan would be to take a house in Cambridge and take lodgings in London for the winter; to live in fact as live at present, only substituting a house for a College room, wit just *one* great difference, namely having you with me.

There is no harm speculating on the future, so long as we do not so fix our imaginations upon it as to be disappointed should things turn out otherwise. Our present course is to wait till things develop themselves. At present one can only conjecture; by June it seems probable, if it pleases God to prolong our lives to that, that we shall know pretty well how things are likely to go.

I called on the Millers on Monday morning. I wanted to ask Prof. Miller whether anything had been written about the colours of the precious opal, as the Master of Trinity had asked me a question on the subject. I found from him that nothing had been written; it was only conjectured that the colours were due to microscopic crystals of quartz.

Mrs Miller said she had not expected to see me again till I came married. She seemed to have set her heart on being the first to ask you to dinner in Cambridge. I told her of the put off. She spoke of the discussions going on in the Council. Her husband you know is one of those interested in the result, and so you may naturally suppose it is an interesting subject to her as it is to you.

Yesterday I dined with my friend Dr Paget, who as President of the Cambridge Philosophical Society invited the members of the Council. We had a very pleasant party of 15, including Mr Hopkins, Professor Miller, Prof. Challis, and others whom you do not know. To-morrow I am going to dine with Mr Vignoles at Westminster (there is the meeting of the R.S. in the evening) to meet Dr Plarr of Strasburg, who has been writing on the figure of the Earth and was very anxious to see me as I had written on the same subject. I have sent him copies of my papers. I mean to return to Cambridge on Friday morning. I have to examine for the Bell Scholarship next Tuesday in Cambridge, and to give my final examination at Jermyn St. on the following Saturday. So from Thursday afternoon, March 5, till Saturday morning, March 7, inclusive, I expect to be in London, and a letter to reach London then may be addressed Athenaeum Club, Pall Mall, S.W.

It is just 12 and I am going to sit for my photograph* to Power, one of the Fellows who has taken up photography. I shall beg one for you, but I must keep it till I go to Armagh as they are collodion positives on glass. I expect to arrive at Armagh the Saturday before Passion Week.

It is now after dinner. Power took altogether 5 photographs of me. Two of them I think are very good. The afternoon's post brought me your letter of Monday. You don't say how long you are going to stay in Dublin, so I shall send this to Armagh. I return you Mrs Napier's letter as you may like to keep it.

I am putting in a notice of M. Holtzmann's experiments in the next No. of the *Phil. Mag.* Perhaps you will read it, but I don't think you will understand much of it.

I have been very remiss in question-answering. You have asked me 9 marvellous searching questions, which I proceed to answer some of.

1. The virtue you think most of. By think most of I presume is meant think most highly of, rather than reflect oftenest upon. *Answer*:—I think one is more struck with a strong exhibition of virtue, be it of what kind it may, under trying circumstances, than with virtue on account of its being of a particular kind. And with reason; for such a proof indicates the possession of a character which would show itself in other directions if occasion led. Yet comparing virtues in the abstract, I am inclined to think that charity or love (for it is the same word) must rank the highest, for it in a manner includes all others.

2. Favourite hero. Hero I suppose is meant in the usual sense of the word, and I should say in this sense the Duke of Wellington.

3, 4. Prose writer and poet. No decided opinion.

5. Antipathy. *Vide* what was said under virtue. Presuming that you don't mean to go to the assizes but speak of what one meets with in ordinary society, I should be inclined to say a sort of cringing, sneaking, fickle, frivolous, fawning character.

6. Flower. Perhaps a clove-pink.

7. Food. Variety is charming; one would get tired of anything. Though I come from the Emerald Isle I must say the roast beef and plum-pudding of Old England is very good.

* Perhaps the frontispiece to this volume.

8. Occupation. Scientific investigations, especially when they lead to discoveries.

9. Colour. I should say a red-purple.

I must now conclude for I want to write to Mr Vignoles, and then I am going to Dr Clark's to tea.

LONDON, *March* 19/57.

When the cat's away the mice may play. You are the cat and I am the poor little mouse. I have been doing what I guess you won't let me do when we are married, sitting up till 3 o'clock in the morning fighting hard against a mathematical difficulty. Some years ago I attacked an integral of Airy's, and after a severe trial reduced it to a readily calculable form. But there was one difficulty about it which, though I tried till I almost made myself ill, I could not get over, and at last I had to give it up and profess myself unable to master it*. I took it up again a few days ago, and after a two or three days' fight, the last of which I sat up till 3, I at last mastered it. I don't say you won't let me work at such things, but you will keep me to more regular hours. A little out of the way now and then does not signify, but there should not be too much of it. It is not the mere sitting up but the hard thinking combined with it.....

PEMBROKE COLLEGE, CAMBRIDGE,
March 28, 1857.

.....To-day I have been examining some alkaloids sent me by Dr Herapath of Bristol, and trying some other chemico-optical experiments. I find in horse-chestnut bark a crystallizable substance distinct from æsculine, the solution of which is highly fluorescent. The afternoon post brought me a letter from the Prince of Salm-Horstmar enclosing a specimen of a fluorescent substance he had obtained from the bark of the ash. He sent also a cheque for some money for Darker†.

[* *Math. and Phys. Papers*, Vol. IV. p. 77 (*May*, 1857). In modern language this memoir relates to the domains of asymptotic solutions of linear differential equations, and their relations to each other and to the regular solutions. Urged on by the then unaccountable discontinuity above referred to, he succeeded unaided in probing the whole matter to the bottom. Eleven years afterwards H. Hankel (*Math. Ann.* I.) arrived at the same results more systematically, by building on Riemann's work.]

† Philosophical instrument maker.

March 31, 1857.

.....I feel sure that you will love me more than I deserve; and supposing even that in consequence of the letter you love me a bit less, I set it down as an axiom that it must be that I don't deserve it, and I don't blame you a bit, but hope to deserve it better in future.

I too feel that I have been thinking too much of late, but in a different way, my head running on divergent series, the discontinuity of arbitrary constants, etc., etc.* I was thinking to-day that perhaps I would get you at Armagh to bind up the MS. of the paper I am writing for the Cambridge Philosophical Society. I often thought you would do me good by keeping me from being too engrossed by those things. My only chance of finishing the paper is to work at it in the vacation, for my lectures come on immediately after. Yet I was half inclined to lay all these things aside for the vacation.

.....I repay you in kind in one respect; but *my* violets were gathered from the garden where Ridley used to walk up and down learning the Epistles by heart, as he says in his farewell†. But I must stop. The porter has long since been for the letters, but I will take this to the Post-Office myself in hopes, perhaps, of doing something to ease your little troubles. Only just time.

April 1, 1857.

.....Secondly, remember you should make allowance for a situation in which you were never placed, that of having a tiny trouble which you were not allowed to mention to any human being, although it was not a thing concerning yourself alone, and being alone, breakfasting alone, dining alone, taking tea alone, week after week, so as to be deprived of the healthy invigorating effects of social intercourse and the mutual interchange of ideas.

April 2, 1857.

.....I dare say my letters latterly were somewhat cold; it is likely enough, my head got running so on mathematics of a most transcendental character. I felt at the time how good it would be for me to be prevented from having my mind so engrossed. Yet

* See previous footnote.

† Bp Ridley the martyr.

recollect your "manly" (as I called it, which you thought dubiou
praise) sentiment about my researches. Again goodbye and Go
bless you.

38, Dawson Street, Dublin,
April 6, 1857.

I will take things chronologically, that I may not leave ou
what I wanted to say. I went with Lizzie to Dr Stokes's* o
Saturday night. My sleepiness had gone off: I believe I took
a little nap after dinner. Dr Stokes had a small party, not indeed
small in all, but about half of it was made up of his own large
family. There were there Madame Morosini, an Italian, who sings
and plays beautifully, and Mlle C. (I forget the name). Madame
Morosini played, but did not sing as she had a cold. Mlle C.,
who does not speak English, sings most beautifully. Dr Stokes
was in great spirits, and once when Mlle C. was singing some
Neapolitan ballads, accompanying herself on the piano, he got so
charmed that, beginning with moving his hands in time he ended
by taking hold of Mr Otway, a barrister, no chicken in years, and
skipping about the room, much to Mlle C.'s amusement.....

John, though pretty well recovered, did not venture out
yesterday. My mother, Lizzie, and I, went to Church together
yesterday morning to St Anne's, nearly opposite. It was, I believe,
while he was curate of St Anne's that my father fell in love with
and married my mother, then very young. It was sacrament
Sunday there as with you.

April 22, 1857.

.....The change before you—doubly painful to one so loving
and beloved as you—the parting from your friends. But that is
a necessity; and though it cannot be but that your father and
Mrs R. will feel the loss, still I do believe that it is a comfort
to them that you are to be, we all hope happily, married.....

I must soon make up my mind whether to go or not to go to
Dr Gladstone's party. To-morrow will be a thorough R.S. day.

Pembroke College, Cambridge,
April 24, 1857.

It is really the 25th, as it is now 12.45 at night, but I will
write you a line or two before I go to bed. I felt lonely in London
on Wednesday evening after the brightness of Armagh, and a trifle

* Dr W. Stokes, F.R.S., the eminent Dublin physician.

sleepy into the bargain, and not much disposed to work; so I went
to Dr Gladstone's soirée. Crowded rooms as usual; there were
several there whom I knew. Yesterday was a complete R.S.
day. At breakfast at Lord Wrottesley's I heard some particulars
about Sir David Brewster's attachment and marriage. I won't
tell you all at present, it is so late. The lady it seems is about
50 years younger than he. Half a century! What a difference!...
They appear to have become *bonâ fide* attached to each other,
and that pretty quickly. May it be lasting.

.....Adams tells me they are still on the Regius Professorships
in the Council. I expect they won't come to the Mathematical
Professorships for some time. Adams got to-day a letter announcing
his election as a Corresponding Member of the French Institute.
Tell your father that; it will please him. I have managed somehow
or other to get a bit of a cold. I hope my voice will be in trim
for Monday's lecture. High time to go to bed. I have done a bit
more to my paper. Past one now, good night, I hope you are
sound asleep.

Athenæum, London,
May 6, 1857.

.....Indeed I feel that you may help me a great deal. If
your thoughts wander, where are mine? Indeed they range at
large a deal too much. I feel that I am too much engrossed too
with my scientific pursuits. They are all very well in their way,
nay I even look on them as forming a part of the proper work
of my profession; but they occupy too large a share of my mind,
and I am often tempted to neglect the work immediately before
me in following out something new. I believe that you will be of
the greatest use to me, in this respect among others, in keeping
me from being so engrossed. But we must take care not to lean
too much on each other. It seems to me as if, believing that my
marriage to you would be for my great good, I have grown careless,
as if that was to do everything for me. We may indeed help each
other, but we must not expect too much from that. Nothing can
replace individual carefulness, and we must each look for strength
where strength is to be found, that we may be able to help each
other. Without that it may be but the blind leading the blind.

PEMBROKE COLLEGE,
May 11th, 1857.

.....If we are married at the time we are at present thinking of, and go to Switzerland as we talked of, I think I will bring a couple of quartz prisms, a quartz lens, and a piece of uranium glass with me, to observe the spectrum on top of the Rigi or Faulhorn.

I read my paper to-night, so don't suppose that you have put me off finishing it. There were no remarks made on it; it was of too transcendental a nature to permit of remarks being made on the spur of the moment.

As I walked out yesterday the nightingales were singing sweetly. I was thinking that would be something new for you. But perhaps you may have heard them already. Probably not, for you have not been much in England, and I don't think they flourish in the Emerald Isle.

The afternoon's post brought me your short note (the morning post brought the letter) announcing that Mrs R. continued improving, and containing its green enclosure. It needed not that now to assure me that you are no longer afraid of me, though it had a symbolical scent. I hope you won't be knocked up by your nursing. I shall be quite content with such short notes while you have so much to fatigue you. But you must not be too fierce about nursing, or instead of being able to nurse others, you will have to get them to nurse you.

May 16, 1857.

.....Regius Professorships to Trinity College. They had proposed a plan which Trinity College did not accept, and so they had to go back to old matters. But Adams says he does not think they will get back to the Lucasian Professorship for a good while, as they will probably first discuss the question of trying to get money for the Professorships generally.

On Tuesday I dined at the Frosts; on Wednesday with the Master of Caius; on Thursday at the Philosophical Club in London. Next Tuesday I am engaged to dinner at the Master of Christ's and on Wednesday to the Hopkins's. So you see I am rather gay in that way at present. To-day I declined an invitation to dinner from the Master and Fellows of St Catharine's Hall for the 28th, as I am to be in London on that day.....

I have not forgotten little Jack Horner who sat in a corner, so I comprehend the plum, though nowadays the name Horner is rather associated in my mind with Horner's method of solving numerical equations.

I am afraid I cannot help you a bit in the matter of the governess. You must want a regular blue-stocking, to teach Latin and Greek. Of course Euclid and Algebra must be considered a sort of dust on the balance, a trifle not worth mentioning.

June 3rd, 1857.

.....I have got very interested, too interested, in my horse-chestnut bark experiments. One's thoughts run so in one channel, when one is all alone and gets interested in anything particularly. Adams told me to-day they had a meeting of the Council, but were occupied in considering the communication from the Commissioners. I suppose the Council will shortly wind up their deliberations till October; so we must be content to remain uncertain as to our future abode. I have great hopes it may be Cambridge; but there is nothing fresh about that, nor has been this good while. Tell your father that Adams has finished his investigation of the Secular Acceleration of the Moon's Mean Motion, and finds it only half what Laplace made it, so that old eclipses are thrown out considerably.....

[In reply, June 5th, 1857 :—"I am delighted that you have a little time for the Horse Chestnut bark, and I don't see how you can be *too* much interested."]

LONDON, *June* 12, 1857.

To-day has been rather a busy day with me, seeing people on scientific matters, correcting press and so forth. I felt rather fagged, as if I wanted relaxation, and thought if I could only get some talk with you and hear some of your music it would freshen me again and set me up. I ordered a suit for a wedding, as well as 400 cards and envelopes.....

.....But that you may not rush to extremes I must tell you that I felt all along that the investigation [about Horse Chestnut bark] in itself was praiseworthy, and it seemed as if there was even in some sense a motive of charity in it, for it might perhaps save chemists who were working at the subject from spending much labour in vain. But on the other hand I had work to do which this was driving into a corner; and I had papers referred to

me to look over, and if I had a paper of mine under reference would like it to be done soon. It did not seem wrong to go on fro one experiment to another when perhaps important informatio might come out. Whatever I might do by daylight, however, b which alone I could work, I felt as if I ought to be working a my papers, etc., in the evening. But when evening came my brain had been well worked and I wanted relaxation, and yet felt I ought to work but I had no mind for it, could not wel settle down to it, perhaps had not, without relaxation, force enough for it. And so when I felt I ought to be working I kept dipping into chemical books *à propos* to my investigation, and so the thing swallowed up my thoughts. Now I by no means want to pretend that I was not wrong, but only to let you understand better what the nature of the thing was. It began by my softness in thinking I would indulge myself in a few experiments on the Monday after my lectures were over, saying to myself I might treat myself to it in consideration of my lectures, though I felt that that was self-indulgence for I had work to do. And what I ought to have done was perhaps not to refuse to work at all at the experiments, but to break them off with a high hand and stop short in the middle of interesting and perhaps somewhat important investigations, when I found they were interfering with my proper work and swallowing up my thoughts.

And now, my own dear Mary, having told you my weakness in my former letter, and having now explained a little more about it for fear you should be too much troubled, let me tell you this, that the happiness I look forward to in your society was conditional on deep mutual love, and on our striving together to live as we ought to live. That was in my mind the one thing needful. Without that, music or accomplishments, or all you could do in that way to make me happy would be but an empty blank. They are all very well in their way but they only come in the second place. When I was engaged in those experiments I felt that it would be very pleasant to have you to take an interest in them and to take down notes for me, but it did not produce the same tender kind of love as when I opened my heart to you with its weaknesses, a feeling deepened still when I found how my faults pained and alarmed you. It seems to me that truth and love must go together. This calls to my mind the passage "speaking the truth in love."

I suppose your father arrived to-night, I must go and see him to-morrow.

And now, my dear Mary, may God bless you and bear you up under the painful and anxious thoughts attendant upon your great change, and the quitting of the home of your childhood and your dear relations. That it is right I for my part, and looking from my side, feel no doubt, although I cannot wonder that my faults should throw doubts on your mind. I should greatly like to get over to Armagh, but I have full as much to do here as will occupy my time. I am not quite certain whether I shall be able to return to Armagh with your father.

And now somehow or other I feel refreshed in my mind. I felt much during the day as if I had overworked myself and wanted relaxation.

LONDON, *June* 13, 1857.

I half expected a letter by the morning's post but none came from you. Perhaps you wrote to Cambridge, if so the letter will be forwarded. Though I wrote last night I write again this morning, fearing that you may be troubled, and hoping I may be able to say something to make you happier. In the first place I may say that I had a good sleep, and felt fresh again and unfagged this morning. You seem to be distressed because you do not feel your heart more full and satisfied and want to know the reason. I can suggest two, first my faults, secondly my absence. You took me for an angel and were disappointed when you found I was not. But even in this I may perhaps give you some comfort. Had I kept my faults more to myself as many men I take it would, remaining more in a state of proud isolation, you might perhaps have gone on thinking me an angel and having large expectations of happiness and even been overflowing in love to me, and yet I should have felt comparatively coldly towards you. But such a state of proud isolation is utterly subversive of my notions of the deep mutual confidence and love of married life. And I find by experience that opening my heart to you with its weakness makes me love you as I never otherwise should have done.

My absence is another obvious cause. The fact is you have seen but little of me personally, and when people are together there are a thousand little things which tend to produce affection which cannot travel by post.

But for my part the belief has never deserted me that God

Himself guided me in the matter, and if you can feel the same then trust quietly in Him. Perhaps these very troubles were given you to put this trust in exercise.....I do not *share* your troubles, for the prospect before me seems brighter and happier than ever since the time when I first seemed to receive the direction as it were fresh from heaven.

LONDON, *June* 18, 1857.

I have just done dinner and will write a few lines to you. I got *two* letters from you to-day. I wanted to post a letter on scientific business at Charing Cross in time for the evening mails, but I passed by my lodging and called in, hoping to find a letter from you. I found one but I did not read it. I felt *so perfectly* at rest that being afraid of being late for the post, I put it in my pocket and went to Charing Cross to post the letter. As it would not be quite decent to read the letter in the street I came here, and read it here. How happy everything now seems. How thankful we should be that these troubles were sent. It is commonly said that when people are engaged they may as well marry at once, for they will never know each other any better till they are married. It has been very different I take it with us. Why? Because we have not shunned to speak or write the plain truth to each other; caring rather to have a clear conscience than fearing the effect which disclosures might produce. It is put down in the *Vicar of Wakefield* (have you ever read it?) that the vicar put off his daughter's marriage, thinking the interval between engagement and marriage the happiest in one's life, and wishing to prolong his daughter's happiness. And so perhaps it often is; people put the best face on it, and then there are bright expectations, and then disappointment. But with us the interval has oftentimes been a period of trouble; yet it seems to me as if these troubles had all now blossomed and put forth their sweetness..... Our troubles have sprung from our faults, yet mostly, almost entirely mine. I feel that, and yet they seem to have been the very way to cure our faults.....away from it [intellectual worry] altogether by music and so forth, refreshing my mind,—keeping my investigations from swallowing up my mind, keeping me awake, impairing my health, and worse still shutting out the view of the things which belong unto our peace. And I felt too that the presence of one I honoured as well as loved would forbid me to indulge in those investigations to the undue postponement of

my appointed work...But I do indeed hope and trust that I shall (nay, I must not say *shall*, but *should* if you permitted it) have that higher love which would sweeten uninteresting work by the thought that it is for you....

...I know or fear that I am, or perhaps may almost say used to be, cold. With me, in my life of isolation and abstraction, affection has been in the condition of a virtue requiring cultivation, and of which the culture I fear was much neglected, rather than a feeling springing of itself in the daily interchange of domestic kindnesses. Accordingly I have been in the habit of regarding it, as you said of religious feelings, a thing to be felt rather than expressed. (I feel fearful of expressing too much, lest it should be in fact a lie.).....It is a struggle with both of us between affection on the one side and the things given up on the other. In order that affection may be victorious it must be thorough; a whole love or none. The preparations for our marriage are made; the day is named: but even now refuse me if you wish it.

I verily believe that, setting aside what is positively wrong or selfish, there is nothing so fatal to fulness of love as reserve. It damps the affection of the one who maintains it, and that reacts upon the other.

I have work to do, but writing to you must take precedence of it. That most important day of our lives is now near at hand, unless you say it is not to be. What affects the faithfulness with which we make, and the confidence with which we receive each others' promises, if we are to make and receive them, is most important to our happiness. God be with you. Pray to Him to guide you aright, and if you see it to be right to go on, may He be with us in our journey through life, keeping us united in the ties of the deepest mutual affection and in the ways of His commandments, until it pleases Him to call one of us away; and at last when this transitory state of our probation is over may we dwell for ever before Him.

I cannot feel as if you would draw back, but you must take your choice.

1 a.m. on Sunday morning...then it is right that you should even now draw back, nor heed though I should go to the grave a thinking machine unenlivened and uncheered and unwarmed by the happiness of domestic affection. But I will not dwell on this

for I do not believe it can be the case: you mentioned it merely as passing thoughts which troubled you, and which you told lovingly and conscientiously to me, and I don't love you the less for having told them out or even for having had them.

Sunday morning now. Your letter set me so thinking about our feelings that, though it is not far from church time, I will put my thoughts on paper. You spoke of making me happy. Mary, I had no prospect of happiness except on condition of your being happy too, and one part of my own happiness I looked on as consisting in the endeavour to make you happy. It seems to me you did not do well to attempt a compromise after the great letter. It would have been better if you had put it pointedly and openly to me, could I love you so as to make you happy? And for want of that you have gone on contenting yourself with the prospect of a milk and water love, and it is no wonder when your marriage-day drew nigh you had a desolate feeling of unsatisfaction.....Till your yesterday's letter there was nothing to prevent the feeling of full satisfaction with which I could make you my bride, and there is nothing still except your suspicion. The happiness of deep affection outweighs in my mind the happiness of the scientific leisure which I give up, but the happiness of the scientific leisure may outweigh mere milk and water affection. I feel prepared to make my promises provided you feel prepared to believe in them; but a great love on the one side requires a great trust on the other, and you must trust me for love as well as for everything else, nay, as being about the most important point of all. There must be no half measures. If you feel that you cannot trust me, put off our marriage, even for ever if you feel that you never can. And yet I am your faithful, Gabriel.

[She wrote, Sept. 26, 1857, "When you come you will tell me if those great pendulum experiments could not be done in April. You say that there will be a clear month then. I do not think I could like anything so well as knowing that they were going on; sitting in the room with my work or books if it were so allowed."]

PEMBROKE COLLEGE, CAMBRIDGE,
Dec. 31*st*, 1857.

MY OWN DEAREST MARY,

Here I sit to write to you my first letter since you became my wife, in the very place, in the very room, where

I wrote the first letter I ever wrote to you. A very different sort of letter this, just a bit of quiet talk.....I got to College about half-past one. I found Arlett and Ferguson here. In speaking of Professorial matters both declaimed warmly against the absurdity, as seemed to them, of creating a new Professorship before endowing the existing ones. There does not, however, appear the least sign on the part of the University of any movement towards an earlier endowment. It remains to be seen whether the Commissioners will be content. By the Act the power of the University to originate new statutes expires with this year now almost defunct, and then with respect to statutes not yet made they have only the power of accepting or rejecting what the Commissioners offer. The Commission expires Dec. 31, 1858, unless Her Majesty pleases to renew it for one year, which she can do. So we are likely to have an inkling before *very* long of how matters are going.

...I should certainly like very well to be able to settle down in nice quiet Cambridge, but if it may not be we must settle elsewhere and be thankful that we have so much happiness. I think it would be better for me as well as for you to settle in some nice quiet country-like place rather than in a flat or one of Cubitt's houses.

And now I will stop to write to Lizzie before post, and just wish my own Mary a happy new year. Poor Mary! left in the fogs of London while I am enjoying the fresh air and quiet of Cambridge. God keep you. Your loving husband.

PEMBROKE COLLEGE,
Dec. 18, 1857.

MY DEAR STOKES,

I enclose the map of the spectrum which you wish for. I cannot find in the portfolio any other papers relating to the subject. You may well remark on the provision for the Lucasian Professor some eight years hence. Several of us on the Council fought for the common-sense principle of first taking care of what we now have, and adequately endowing the Lucasian before founding the Sadlerian Professorship, but we were beaten. However, I think it very likely that the Commissioners will not be content to look so far forward, and will try to devise some quicker mode of raising the wind.

I am not surprised at Mrs. Stokes falling in love with Highgate, but I wish we could get you away from Hampstead, Highgate and such like places back to old Alma Mater.

Yours very truly,

J. C. ADAMS.

PEMBROKE COLLEGE, OXFORD,
28 June, 1860.

DEAREST MARY,

I intended to tell you, but forgot, that Adams told me in the train that there is a comet now visible with a tail 4° or 5° long. It is (or was) about 23° from the Sun, towards the North, so that it does not set all night. I quite forgot it last night, but at any rate there would I suppose have been no chance of seeing it. Indeed I remember now it was raining.

.....Section A [British Association] broke up early to-day, though there is ordinarily a crush of matter towards the end. A little after 12 I went off to Section D, where there was a paper by Dr Daubeny, "Remarks on the final causes of the sexuality of plants, with particular reference to Mr Darwin's work On the Origin of Species by Natural Selection," which excited a great deal of interest. The room was filled as full as it would hold, and the paper produced discussion in which Owen and Huxley took part.

Saturday, June 30/60.

I certainly was not *vexed* about the accident; though I would rather it had not happened, still I was more sorry at least at first for the annoyance it was to you. Never mind; I dare say a few shillings will repair the damage, and perhaps even it may do as well as it is and it may be only the appearance that is somewhat spoiled. It is well it was not the quartz.

I heard a day or two ago that our friend Fischer is going to be married in about a month. Better late than never. I see no symptoms of the kind about Adams, unless it be that he got a letter he seemed to read with much interest, and stayed late for dinner in order to write something.

OXFORD, *Sunday, July 1st*, 1860.

.....After breakfast at [Bartholomew] Price's, Adams and I went to the University Church, where there was the morning prayer as far as the end of the Litany; followed by a sermon, very

clever, but rather of the new Oxford School, and having a strong tendency, in my mind, to ignore Providence and the use of prayer, though the subjects were not directly touched upon, the latter at least, and though I dare say the preacher may have had some way in his own mind of reconciling the two. I could not help admiring *my* friend Adams afterwards when we met some other friends who were praising the sermon; for while I remained silent he came clearly and boldly out in defence of what *we* hold to be the orthodox view. I could not help feeling rebuked, and still more when I remembered a sentence in your father's letter; for I felt that I would have let the thing pass in silence when I ought to have spoken, though I should not have put the thing as clearly, had not indeed apprehended it as clearly, as Adams put it. We lunched at Price's and afterwards went to the 2 p.m. University Sermon, a very cold affair, as it seemed to me, and so slowly and mouthily delivered for a sermon addressed to the reason that I could not well follow it. After the sermon we took a walk, and at 4 went to Christ Church Cathedral. After the service we fell in with the Willises (Prof., Mrs and Miss) with whom we went about till 6.30.

Yesterday Adams and I dined with the V.C. There were at dinner Prince Frederic of (I think) Sleswig Holstein, Lord Wrottesley, the Willises, Mr Senior, Mr Kay Shuttleworth, and two or three others. After dinner we went to a great crush *soirée* at Dr Daubeny's.

I should guess two days as the most probable time the Commissioners will sit this week, in which case I should probably be back on Wednesday at 9.15 p.m.....

LENSFIELD COTTAGE, *6th Oct.*, 1860.

.....I own I have felt the want of talking, especially the quiet talk of one's own family. Arlett and Power have come back and I am to dine there to-morrow.

.....I paid Miller to-day £3. 4*s*. 3½*d*., the remainder of the cost of the "Glasgitter*," making in all £6. 4*s*. 3½*d*. It is certainly a splendid one. I feel pretty sure the crystals I mentioned are *not* phylloxanthine, but I have come across quite different in appearance which I believe *are*. I was thinking of writing a bit to your father to tell him about my chlorophyll work, but I dosed after dinner and there would not be time now before post. Your

* Optical diffraction grating.

long letter to Lee reached me to-day. They sent it to the Roya Society and Miller brought it for me here.....Otto Struve men tioned at breakfast to-day a curious case of matrimonial felicity a couple of whom one was Dutch who could speak no English and the other English who could speak no Dutch, and their servan was a Pole who spoke German. The couple could only commu nicate with one another in very bad French, and for domesti arrangements the language was I believe an attempt at German..... the time I get back. I can choose whichever seems likely to do best.....

Oct. 12, 1860.

.....You may tell your father I am pretty sure there is a second yellow substance in chlorophyll*. The spectrum of Lens field lawn pretty well satisfied me that there is. I had had suspicions of it before. It was only shortly before I went to London that I observed this spectrum, and therefore I have not yet been able to follow it out.

Royal Society, 12 *Oct.*, 1860.

I am glad of a day for your return being pretty well fixed, but a week seems a good way off. I return to Cambridge to-night, but I own I feel to care very little whether I return or stay till to-morrow. It is not so when a certain person is there, for then I look forward to it with the greatest pleasure. However you or rather I would not gain much if you came back a couple of days earlier, for I am to dine with General Sabine on Wednesday and there is to be a meeting of the Council on Thursday. Don't so fix a day as to cross if it is not settled weather.....

RELATION OF SCIENCE TO THEOLOGY.

Letters to Mr Arthur H. Tabrum†.

Lensfield Cottage, Cambridge,
Jan. 16*th*, 1895.

Sir,

I can reply at once, and with much pleasure, to your enquiries.

(1) As to the statement that "recent scientific research has shown the Bible and religion to be untrue," the answer I should

* See *Math. and Phys. Papers*, Vol. IV. p. 236 (1864).

† An official of the London Post Office.

give is simply that the statement is altogether untrue. I know of no sound conclusions of science that are opposed to the Christian religion. There may be wild scientific conjectures put forward by some, chiefly those whose science is only at secondhand, as if they were well-established scientific conclusions, and which may be of such a nature as to involve, on the assumption that they are true, certain religious difficulties; I would not go so far as to speak of opposition, as for the most part religion and science move on such different lines that there is hardly opportunity for opposition.

But if an appearance of opposition may sometimes arise from this cause, it far oftener, I think, arises from the errors of de fenders of the faith once delivered to the saints, in putting forward propositions which are mere human accretions to it, and present ing the two as if they had equal claims to acceptance. When I speak of the errors of defenders of the faith I am not thinking of learned theologians of the present day, but rather of those of a bygone age, from whom these human accretions passed into the popular theology, and were supposed to be involved in the Christian faith. This mistaken belief afforded infidels a handle for attacking the faith through the error involved in some of the accretions to it.

To illustrate my meaning I will refer to a proposition dogma tically laid down as part of the Christian faith in a standard book written I believe one or two centuries ago. It is that the Christian doctrine of the future resurrection requires us to believe that all the particles of the present body, however widely sepa rated, even though the body may have been burnt to ashes and the ashes strewn to the winds, will be brought together and will be re-animated to form the future body. I dare say many an infidel lecturer has descanted on the difficulties of believing such a proposition as that. But before the Christian apologist replies by simply falling back on the principle that "with God all things are possible," he would do well to consider whether there is any occasion to defend the proposition at all. My own con viction is that there is no such proposition at all involved in the Scriptural doctrine of the Resurrection. The notion that it is involved in it seems to me intensely silly.

I should doubt if you would find a single theologian at the present day who would regard that proposition as connected in

any way with Christianity. But I doubt if infidel lecturers ha yet given up harping on it.

I may appear to have been treating a theological fossil as if it were a living animal. But it may serve very well as an illustratio of my meaning.

(2) You say "as far as my reading goes I am of opinion tha true religion and true science harmonise." I am of the sam opinion.

(3) You ask if it has been my experience to find "the greatest scientists irreligious"? That has not been my experience, but the reverse. To confine myself to my own line of mathematical and physical science, and to those who are no longer on earth, though not very many years dead, I could not well select more eminent scientists, of world-wide reputation, than Faraday, Clerk-Maxwell, and Adams, the discoverer of Neptune. I knew all three very well, especially Maxwell and Adams, with whom I was very intimate. I know that they were all deeply religious Christian men. Yours very faithfully,

G. G. STOKES.

P.S. There is nothing private in this letter. You may do anything you like with it.

LENSFIELD COTTAGE, CAMBRIDGE,
29 *April*, 1899.

I now return you James's *Human Immortality*. The materialist makes human nature monistic—body alone, a wonderful material organism, acting merely by the physical forces, and by them alone performing its functions, thinking included. James's book is good as pointing out the insufficiency of materialism, and suggesting a substitute which is a great improvement. But he is I think radically wrong in making human nature bi-partite instead of tri-partite—consisting of a body acted on from without by an individual something in which personal identity is supposed to consist, and which when relieved from the clog of the body, as it is at death, performs its functions all the more actively. Accordingly he supposes that man is by his nature immortal. He shirks the difficulties as to the lost by setting aside that question. He is I think at the bottom of his heart a universalist, though he hardly confesses it even to himself. In fact his speculations draw him strongly in that direction, though he feels that in some way or other that goal cannot be reached.

There is a striking passage, in this connection, in the writings of Justin Martyr, in his dialogue with Trypho the Jew. Justin relates to Trypho the history of his own conversion, which came as the result of a conversation he had had with an old man of meek and venerable manners, whom he fell in with as he was walking in a meadow by the sea-side. From what we know of probable dates, it does not seem impossible that this old man may have talked with St John. In the course of a conversation about the Platonic philosophy, the old man says (I quote from the Clarke translation):

"For to live is not its (the soul's) attribute, as it is God's; but as a man does not live always, and the soul is not for ever conjoined with the body, since, whenever this harmony must be broken up, the soul leaves the body, and the man exists no longer, even so, whenever the soul must cease to exist, the spirit of life is removed from it, and there is no more soul, but it goes back to the place from whence it was taken."

This is in full accord with St Paul's description of man in his entirety as consisting of spirit, soul, and body, and with our Lord's teaching that man can kill the body, and after that has no more that he can do, but God can destroy both body and soul—both body and being—in Gehenna.

LENSFIELD COTTAGE, CAMBRIDGE,
5 *Oct.*, 1899.

DEAR SIR,

The objection you tell me that sceptics raise to the resurrection of Jesus Christ from the dead, on the ground that it would be scientifically impossible, admits of a very short answer, namely, that science has nothing to do with it. Surely no one on the Christian side contends that that resurrection was natural, but supernatural, and as such it lies outside the ken of science altogether. The only logical standpoint for the scientist who would deny the resurrection on the ground you mention is by maintaining that science covers the whole of the complex nature of man, so that he has nothing to do with anything that lies outside the domain of science. Is that assumption reasonable? Biological science concerns itself with the properties of living things, animal or vegetable. But science never has explained, nor does there seem to be the remotest prospect that it ever will be able to

explain, the origin of life. Science cannot explain the feeling we have of right and wrong. Science does not cover the whole of man's complex nature.

The admission of the resurrection of Jesus Christ, if regarded as a dry isolated fact, would I think be of little or no value. It seems to have been God's *design* that it should *not* be so regarded. We read, "God raised him from the dead and showed him openly, *not unto all the people*, but unto witnesses chosen afore of God." Were admission of the fact of the resurrection the one important thing, the obvious way (if one may so speak without irreverence) to secure it would have been to have shown Him openly. The evidence for the resurrection of Jesus Christ is never to be separated from a consideration of the character and teaching and works of Jesus Christ. The head and the heart must go together. To demand that the alleged fact of the resurrection shall be accepted or rejected on purely scientific evidence, is to act like a judge who in a trial in which there were a great number of witnesses, whose several testimonies would so dovetail into one another as to produce conviction in the minds of the jury, should arbitrarily select one of the witnesses (and he perhaps by no means the most important) and refuse to allow any of the others to come forward.

DEAR SIR, 25 *July*, 1900.

I had to go by an early train yesterday to London, and a little beyond, to be present at the funeral of a late friend, and did not return till latish, so that I could not well have answered your letter much sooner.

If the sceptics affirm that the present condition of things has gone on from a past eternity, and is adapted to go on for an eternity to come, I can only say that they fly in the face of the best, I might almost say universal, scientific opinion, including that of some few scientists who appeared to have been sceptics themselves.

As far as our scientific knowledge goes, matter is indestructible; it can neither come into existence nor cease to exist. But much more than the existence of matter is involved in the continuance of the present state of things. Consider what is required for the continuance of animal life, say man. Man requires food, and that food is derived, directly or indirectly, from vegetable life; directly,

as when I eat bread, made from the seed of a grass, indirectly as when I eat mutton, the flesh of an animal that eats grass. With the exception of fungi, which we may regard as vegetables of prey, vegetables absolutely require light. It is only under the influence of light that the leaves of trees are able to perform a function the reverse of combustion: to decompose the carbonic acid of the air, appropriating the carbon, of which by far the greater part of their dry weight consists, and setting free the oxygen. This light they obtain from the radiation from the sun. The sun is continually giving out an enormous amount of energy, which can be measured like the energy given out from a steam-engine.

But where is the supply? In the case of a steam-engine the ultimate source of energy resides in the coals and the oxygen of the air. But what feeds the sun? The solar energy is derived, not from combustion, but from the energy of descending weights: from the energy given out by its contraction, like that of a descending weight. The wound-up weight of a clock is a source of energy, and keeps the clock going; compensates the small friction which would tend to stop it. But when the weight can come to the bottom the supply of energy ceases, and the clock runs down.

Just so, the particles of the sun tend to fall inwards, and to a certain extent do so fall, under the influence of the gravitation arising from the mutual attraction of the particles, which is a force directed nearly towards the centre of the sun. I say "tend" because the fall is in some measure prevented by the lateral velocity, putting the particles in some degree in the condition of planets. But it is only to a certain extent that the inward motion is thus presented; were it otherwise, there could be no permanent giving out of energy. The contraction it is evident cannot go on indefinitely. For the sun, just as for our steam-engines, we are living upon capital; in the latter case we are drawing upon our bank deposit, we are exhausting our coal-fields. At the present day there is a tolerable agreement among scientific men to regard nebulae as suns in process of formation, while among the stars there are a few smaller ones which are blood red. These are generally looked upon as effete stars; stars in process of extinction.

Next as to the origin of life. The doctrine of abiogenesis,

that life can originate from non-life, is pretty well complete knocked on the head. My late friends, Huxley and Tyndall, who even the sceptics would hardly suspect of being led away from t truth by theological prejudices, are about as strongly against i as any. You only ask about the origination of life. Of cours there is a great deal more to be considered than that of it mere origination. There are vast gaps between the life of a gras that of a fish, that of a bee, that of a man. I do not think that science has succeeded, and I doubt if it will ever succeed, in filling these in. Yet on the other hand what naturalists regard as distinct species of living things, plant or animal, frequently approach each other so nearly that one cannot help thinking that what are commonly called second causes come in.

However, as regards the main question, those who say that the present order of things has gone on from a past eternity, and is calculated to go on for an eternity to come, only, in my opinion, thereby display their own scientific ignorance.

It is high time I should return you your book. I have read the article about my "I," and the Easter Egg. As to the former, it is amusing the way he takes for granted that I knew nothing about Bishop Courtney's nor about Archbishop Whately's book. I have read the former, and have got copies of the passage in the latter referred to in the form of a tract.

As to the "Easter Egg," I have already, I think, discussed with you most of the arguments. In p. 211, about a third of the way down, we see indications of the way in which incautious dogmatism, from which the popular theology is by no means free, puts weapons into the hands of adversaries.

With apologies for having kept the book so long, I remain, yours faithfully,

P.S. I have noted an inaccuracy at the bottom of p. 207. The Paschal lamb need not be a firstling.

3rd August, 1900.

I do not think that what I have written need create in your mind any difficulty. It is merely a question of the meaning that we attach to the word "science." We have first pure science, suppose mathematics, where we deduce certain conclusions from axiomatic premisses, and secondly natural science, where we have to deal with external nature, made known to us through our

senses. In the latter case invariable sequences give us the idea of cause and effect, and we are led to regard observed phenomena as the effect of such and such causes acting together.

In our laboratories we can resolve water into two gases, oxygen and hydrogen; we can burn carbon in oxygen, forming a heavy gas (carbonic acid) which was not there before, and so on. But the weight of the water which disappeared is just equal to the weight of the oxygen and hydrogen which were produced; the weight of the carbonic acid produced is just equal to the weight of the carbon which disappeared plus that of that portion of the oxygen which is oxygen no longer, but was employed in the formation of carbonic acid. So far as we know scientifically, a certain weight belongs to a certain material, which we can neither increase nor decrease by chemical changes.

We are thus led to the contemplation of, and the quantitative measurement of, what we call matter.

As far then as our *scientific* knowledge can teach us, matter can neither be created nor destroyed.

Science, however, leads us to the contemplation of something more. We have reason to believe that matter does not form a continuous plenum, but consists of ultimate molecules; and further that in matter of a given kind, say the gas hydrogen, the molecules are all just like one another. An analogue of this in the works of man is found only in manufactured articles. Thus the Mint turns out a lot of shillings all like one another. This leads us to the idea that hydrogen, for instance, did not exist just as it is from a past eternity, but was in some way made. But there science comes to the end of her tether; how it was made she cannot inform us.

I quite think that the existence of life is one of the strongest arguments for the existence of a Living Being who is the Author of life. In his Belfast address (*Rep. Brit. Ass.*, 1874) Tyndall in attempting to account scientifically for the origin of life was led to attribute emotion to the ultimate molecules of matter in a fiery mass of gas!

I quite think with you that the great gaps which we find in the series of animated things, both plants and animals, weaken the theory that man came in an unbroken chain from some lowly form of life. Yours very truly.

5 August, 1900.

For fear of any misunderstanding, I will just say that the conclusion to which we are led by science—that (say) hydrogen consists of a great number of ultimate molecules which are all alike one another, in that respect resembling manufactured articles, is valid as leading us to say that it must have been caused in some way, instead of having existed as it is now from a past eternity. But *how* it was caused, science cannot say.

Our experiments lead us to affirm that what we call ponderable matter cannot be destroyed or brought into being; to affirm it is a fixed entity. We have evidence of the existence of a *stuff*, as I will call it, which we call the luminiferous ether, which we do not include in the term 'ponderable matter.' We recognise it as possessing inertia, as does ponderable matter. But whether or no ponderable matter is formed from the luminiferous ether, is a matter about which we have absolutely no evidence. We cannot convert hydrogen into ether, or ether into hydrogen, nor, as I observed, do we know whether such conversion is possible, and therefore it is futile to discuss the question whether, if it be, the inertia would remain unchanged.

The 'manufactured article' argument is I think valid as against the notion of ponderable matter, *such as we know it*, having existed from a past eternity. Yours very truly.

7 August, 1900.

In any case we must interpret Genesis i. with considerable latitude. And the only reason why this creates a difficulty from a religious point of view is that religious people insisted on a slavish literalism; insisted on theories of verbal inspiration and so forth; indulged in a sort of Bibliolatry; framed a theory that the Bible must be interpreted in a way just like that in which a lawyer would interpret an Act of Parliament; stuck to the letter rather than attempted to catch the spirit; even though it is said that spiritual things are spiritually discerned.

As to the origin of man it is said that God 'formed man of the dust of the ground, and breathed into his nostrils the breath of life, and man became a living soul.' We have here, it seems to me, a recognition of man in the completeness of his tripartite nature. First we have the ponderable matter of which the body is formed; which the physicist can weigh and the chemist can analyse just like the dust of the ground; then we have a divine energy acting on the ponderable matter, 'breathed into his nostrils the breath of life'; and lastly the result of the interaction, 'man became a living soul.' This accords with St Paul's division (1 Thess. v. 23).

This triple division of man's nature was not arrived at by unaided human reason. Plato and very many followers of Plato suppose that man in the completeness of his being consists of body and soul. Man in the pride of his own strength is disposed to think that he has in him a something which he calls soul, which cannot but live for ever; which is half thought of (though expression is not given to such a thought) as almost in a certain sense independent of God. But St Paul says, 'in him we live and move and are.' I think in the N.T. that which a man gives up at death, that which in some way forms the means of our future life, is never called 'soul,' but 'spirit.' Take again the words on the Cross, 'Father, into thy hands I commend my *spirit*.' Dying Stephen said, 'Lord Jesus receive my *spirit*.' Take again, 'Ye are come unto the *spirits* (not souls) of just men made perfect.' And as to the relation of spirit to the future life, see Rom. viii. 10, 11.

Spiritually I think what we are told in Genesis, in the passage I have quoted as to the formation of man, is in full accord with the teaching of the N.T. But *how* God formed man of the dust of the ground we are not told, nor are we, I think, for spiritual purposes concerned to know. We are by no means committed to the supposition that it was something like the way in which a sculptor moulds his clay into a statue. If we say that mental powers and spiritual aspirations such as those of man were conferred on a previously existing animal, the idea may be somewhat grotesque, but I don't think we can say it is irreligious. I don't see that we need trouble ourselves about that. We may simply say, We don't know, and for aught we can see there is no reason why we need know. An M.D. friend of mine long since dead said to me with reference to the similarity of general structure between the body of man and that of lower animals, 'If we saw the keel of a ship laid down, we could not tell what sort of a ship it was going to be.' The conditions of a somewhat similar mode of animal life in man and quadrupeds may require a somewhat

similar general plan of structure, which weakens the argument for ancestral derivation. But if we entertain the idea of ancestral derivation as a hypothesis (it is I take it a long way off from being an established theory) we may do so; and we are not to dub a man an atheist for holding that view.

I see nothing against your notion that evolution and special creation may have existed side by side. I have had the same idea in my own mind, and am disposed to look favourably on it.

4 Windsor Terrace, Malahide.
17 *Aug.* 1860.

I meant (but I believe I forgot) to have written about Day 4. Some have supposed that the general progress of creation in Gen. i. is described *as it would appear* to a spectator on earth if such there were. On this supposition there appears to be no difficulty in reconciling the general order which seems on scientific grounds the most probable with that of Gen. i.

Day 1. We have first the creation of ponderable matter; and supposing such matter to exist, and then as now to be subject to gravitation, light would be occasioned by collisions of matter on matter, the greater part of which matter (confining ourselves to the solar system) would be collected in a nebulous mass of vague outline,—our sun in a juvenile stage.

N.B. There would be alternations of day and night, though from a dense mantle of cloud the heavenly bodies could not be *seen*.

There is scientific reason for thinking that the earth was originally in a molten condition—at a temperature so high (above the 'critical temperature' for water, Andrews) that there would be a *continuous* change of condition from water to steam.

On further cooling there would be a separation of water into the two conditions of water and steam. There would now be a separate atmosphere, consisting probably mainly of nitrogen, steam, and carbonic acid gas, separating the water under the firmament, the chemical substance H_2O in the physical condition in which we call it water, from the water above the firmament in the form of an enormous number of minute droplets forming cloud, while all through the atmosphere H_2O would be present in the elastic state which we call steam.

Day 2. Not until the cooling had advanced beyond the

preceding stage could there be water with a free surface, and consequently could we have sea and land.

When the cooling had advanced a good deal the temperature would be low enough to permit of the introduction of vegetation. In the early stages it consisted in great measure of gigantic (as compared with what we have now) cryptogams, the remains of which are preserved in our coal measures. The plants would get diffuse light through the mantle of cloud. Even now many kinds of fern thrive best in shady places.

Day 3. This vegetation, under the influence of light, would decompose the carbonic acid in the atmosphere, appropriating (in some state of combination) the carbon for future use as coal, and introducing into the atmosphere free oxygen without which animal life could not exist.

On further cooling the mantle of cloud would no longer be continuous, but there would be intervals of clear sky, permitting the sun and moon to be seen. They existed ages before, but were invisible on account of cloud, as the sun is, probably, to-day to the inhabitants, if any, of Venus. They are accordingly mentioned as 'set' in the firmament of heaven on the 4th day.

Day 5. The earth having been prepared by cryptogamic (and other) vegetation for the introduction of animal life, we now have animal life, first in connexion with the waters and birds. Among very old remains we have saurians, winged reptiles, the archaeopteryx, which seems to form a sort of link of connexion between a winged reptile and a bird, fishes, etc.

Day 6. Now we have mention of land animals, and last of all Man.

I fail to see any such discrepancy, between what we read in Genesis and what science renders probable, as need create a difficulty. We must remember that on the side of religion we are not concerned with more than a broad outline, and on the side of science, as regards forms of life the records are very imperfect. In consequence of different structures and modes of life one kind of creature may be more likely to leave remains that survive in the rocks than another, so that it may not always be true that the relative antiquity of strata in which remains are found corresponds to the order of introduction of the form of life upon the earth.

I shall probably remain here for three weeks or so.

24 *August*, 1900.

The force (such as it is) of the objection which you represent the sceptics as making turns mainly on the assumption very commonly made, but which I believe to be erroneous and unscriptural, that the soul of man is by its nature immortal.

Scripture I think teaches that the *final* end of the wicked is that they are destroyed for ever, not kept alive for ever in a state of misery.

Free agency involves of necessity the possibility of falling away, and the forfeiture, through the agent's own fault, of that eternal life of happiness which he might have had. But there is nothing in this opposed to the statement 'God is love.' Why should God be debarred from creating a free agent, whom He designs for an endless life of happiness, merely because that agent may through his own fault forfeit the happiness for which he was intended? Such is according to the analogy of nature. A tree has many seeds, but not all produce trees themselves. Some come to an untimely end. Is the possibility of this a reason why trees should not be endowed with seed?

It seems to me that the strength (such as it is) of the arguments of the sceptics comes from their introducing into Christianity two things, neither of which belong to it. One is the Turkish fatalism of ultra-Calvinism, the other the Platonic dogma of the immortality of the soul.

LENSFIELD COTTAGE, CAMBRIDGE.
4 *Jan.* 1901.

The question you asked me was I think whether I believed in evolution. It seems to me hardly a correct expression because evolution is not a cause, but the description of a process. Let me illustrate my meaning by a concrete example. It has been known from a very long time that the pole of the heavens, the point, that is, of the heavens about which the stars appear to turn, is not fixed, but slowly changes its position in the starry heavens. It really turns round the pole of the ecliptic, or the point in the starry heavens which is in a direction perpendicular to the plane of the earth's orbit, at the rate of once round in about 25,000 years. The angle between the pole of the heavens, as I called it, that is, the pole of the equator, and the pole of the ecliptic remains nearly invariable—about 23½ degrees. That slow turning of the

pole of the equator about the pole of the ecliptic is called (from one of its manifestations) 'the precession of the equinoxes.'

Now suppose a simple-minded religious person had heard that it had been found that the place among the fixed stars occupied by the sun at either (say the vernal) equinox was not quite the same from year to year, but moved very slowly forwards along the path in the heavens of the earth's orbit, so that the equinox was arrived at a little earlier from year to year than it would otherwise have been, he might very likely have simply accepted it as a fact, not thought of attempting to give any reason for it, but contented himself with resting in the idea that God had so ordained it. Nay, dwelling on the idea that 'the heavens declare the glory of God, and the firmament sheweth his handywork,' he might even think it irreverent to seek for any explanation why there should be any such anticipation, from year to year, of the time of the equinox as compared with the position of the sun in his apparent path among the fixed stars.

It was one of the great achievements of Sir Isaac Newton, the discoverer of universal gravitation, to have shown that the precession of the equinoxes was a consequence of universal gravitation. It is a phenomenon *evolved* from gravitation in the sense of *unrolling* in tracing the sequence of cause and effect. The investigation of precession, and of the allied phenomenon of nutation, is now a subject of mathematical calculation. Nobody thinks there is anything at all irreverent in that.

But if precession is referable to gravitation, what, if anything, are we to refer gravitation itself to? Are we bound to say, Gravitation exists because God willed it so, and it would be irreverent to attempt to go further? or are we bound to say, Just as precession is a consequence of gravitation, so gravitation *must* itself be a consequence of something else?

Newton himself was a religious man, and his writings show it. It is shown by the famous Scholium at the end of his immortal work the *Principia*. He wrote largely on prophecies. Yet he thought it axiomatic that since the sun and the earth attract each other there must be something or other between them or they could not do so.

As to the alternative at the foot of p. 2 there is nothing irreverent in seeking to explain a phenomenon as a consequence of something else, which explanation would constitute a passage

from one link to another in the chain of causation. Nay a the might reasonably regard it as irreverent to forbid such an deavour. For that would come to measuring the mind of Almighty by our own minds, and presuming to assert that wh we do not see a prospect of further progress in the chain (secondary) derivation, no further progress in that direction can possible.

But evolution, as it is usually treated, is chiefly thought of something which went on in the past. Further, it is chiefly spoken of with reference to living things, plants, and more especially animals. Can we in any way explain the origin of species? Are we to suppose that each species, or what we regard as a species, originated in the fiat of an almighty power? Or are we to suppose that we are to go indefinitely backwards, and affirm that a chain of secondary causation is to be continued indefinitely backwards, though we can but trace it a little way if at all?

I should say, Neither the one nor the other. The evil (from a theistic point of view) consists in treating evolution as a cause, which it is not, instead of a mode. The former tends towards the denial of a First Cause, the latter may safely be accepted as a working hypothesis; as a guide which *may* lead us from the last link in the chain of causation which hitherto we have been able to reach to a link yet further on, while still leaving what lies beyond hidden in a cloud. The treatment of evolution as a cause, capable of leading us on indefinitely, tends to shut out the idea of a First Cause; its treatment as a possible mode of sequence, leading us a step or two onwards, still leaves the mind directed towards a First Cause, though 'clouds and darkness are round about Him.'

I have endeavoured to put the thing before you as it appears to my own mind. Remember Evolution does not mean a *cause*.

APPRECIATIONS* BY COLLEAGUES.

By G. D. LIVEING, F.R.S., Professor of Chemistry in the University of Cambridge, and Fellow of St John's College.

I made acquaintance with Stokes in 1850 at the meetings of the Ray Club. That was a society at Cambridge for the cultivation of Natural Science by friendly intercourse, which had been formed in 1837 in order to fill, so far as that could be done without Henslow's† inspiration, the gap left by the cessation of Henslow's weekly receptions of members of the University interested in Natural History. Natural History was still, when I joined the Club, most frequently the subject of conversation at its gatherings, and it may seem surprising that Stokes, who at that time (1850) was best known as a great mathematician, and had just been elected Lucasian professor, should have been a very regular attendant at the weekly meetings of such a Club. Really, however, his bent was to Natural Philosophy, as his work showed, where his great mathematical ability was employed in handling the problems of Nature. His elder brother, William, a fellow of Caius, had been one of the original promoters of the Club, and was a mineralogist and a chemist with whom I fraternised at once; but I very soon found that George Stokes was equally interested in the same subjects, and quite as ready to discuss, with a beginner, questions connected with them on which probably his own conclusions had been reached by a much shorter induction. He did this, to my great delight, with a deeper insight into nature's mechanism and more suggestive remarks than I had then met with from anyone else except Professor Miller, who also was a member of the Club. Stokes was not content with reaching any empiric law of nature,

* The Obituary Notice of Sir George Stokes written for the Royal Society by Lord Rayleigh in 1903 has been reprinted in *Math. and Phys. Papers*, vol. v. pp. ix—xxv., and is therefore not included here.

[† Rev. J. S. Henslow, of St John's College, Professor of Mineralogy 1822–25, of Botany 1825–61, the friend and mentor of Charles Darwin's early years, resided mainly at his living at Hitcham from 1837 onwards. *Life*, by L. Jenyns, 1862.]

but was always pressing behind the scene to try and see how the processes were carried out. It was a pleasure to throw out a suggestion, and hear what he thought of it, and I feel sure it was a pleasure to him to help an enquirer. His thoughts stimulated mine, and it would be hard to tell how much I learnt in that way. In the company of other distinguished men, such as Sedgwick, Peacock, and Whewell, I have been well content to be a listener; but the way to learn from Stokes was to get up a discussion with him. Never had I any difficulty in getting him to talk. His reputation for taciturnity was kept up, if not started, by people who were afraid to talk to him. Never was a worse founded fear, for he was not a specialist who could ride no hobby but his own, much less was he in any degree egotistic or unsympathetic. He read the newspapers and was generally informed on the topics of the day, matters and men, and had his opinions kindly and optimistic about them. He was willing to exchange ideas with people who let him see that they had ideas to exchange, but did not talk for talking's sake, and was not distressed if people, who had nothing to say, said nothing. In truth he was many-sided. I have been associated with him as co-trustee and in practical business of other kinds, and never found him a sleeping partner. He did his part with caution, sound judgment, and promptness, at the same time with full consideration for others, who might be affected by his decision. Frequently when I have spoken with too strong reprobation of somebody's action, he has assented with a softening qualification which implied that he would think no evil of the actor. Those who looked for a brilliant repartee from him, or thought his smile a prelude to a jocular remark on the follies of society, would, of course, be disappointed, nevertheless he enjoyed a good story and sometimes told one. His smile was no cynical one, but the natural expression of sympathy with the bright faces about him. Of late years when I have had one or other of my nieces living with me and he has called at my house, they have, on his departure, expressed astonishment that one whom they had heard spoken of as the most silent man in the University should keep up a continuous conversation during the whole time of his visit. On the ordinary business and occurrences of the day he would say at once what he thought and then perhaps drop the subject; while a philosophical, or any serious question, though he may have said what occurred

to him at the moment about it, did not forthwith pass out of his mind. He would refer to it again and again, sometimes for a whole evening, notwithstanding the interruption of taking part in conversation on various other matters. This persistence in dwelling on a subject on which he had not yet formed a definite conclusion, turning it over repeatedly to get new points of view, was very characteristic, and recalled what Newton said of himself, namely, that he excelled others only in the power of keeping his attention constantly on the problem which he wished to solve. Stokes certainly had, in a remarkable degree, the power of taking pains to do whatever he had in hand as well as he could do it with the means and material at his disposal.

He was generally tolerant of other people's opinions, nevertheless he had a philosophical contempt, shown in his tone rather than in his words, for fanciful suppositions, not proved to have any objective existence, invented to account for facts in nature. Before his explanation of the suspension of clouds appeared, a theory had been current that cloud-drops were bubbles, and in talking with him about his then recently published memoir I mentioned that theory, whereupon he brushed it aside with unwonted brusqueness in the remark, "I can't think how that theory ever came to be accepted—there is absolutely no ground for thinking the drops to be vesicular." His touch indeed had burst that bubble, and no one could put it together again.

The even balance of his mind was that of a strong man. His activity was uninterrupted; he wasted no time in trying to evolve truth out of the speculations of others, or in controversies with fellow workers such as distracted Newton. In truth he never let any occasions of such controversy arise; for he never had any jealousy of other workers in the same field, but was ever ready to communicate to them his views, and let them use them as if they had been their own, repudiating, when it might have been justly claimed, all share in the credit of the outcome. Many brilliant intellects have been lodged in frail bodies, with the result that their wits are sometimes in fine form and at others dull. Stokes' mental and bodily constitutions were closely akin, and had the like patient strength and staying power. His broad chest and firm carriage betokened strength. When he was an undergraduate, and for some time thereafter, the athletic games, now in vogue, were little heard of in the University. There was,

however, more boating and riding and quite as much cricket; but I never heard of Stokes in connexion with such exercises, nor did he ever talk to me about them, as he would have done if he had ever been an active participant in them. One athletic exercise I know that he enjoyed, namely, swimming. About the end of the forties an association of graduates and undergraduates (which later became the Swimming Club) set up a new bathing place in the Grantchester meadows. Stokes and I were among the early members of it, and he was noted among us as a bold and strong swimmer. He has spoken to me with warmth of the keen enjoyment of a battle with the waves when there was a good sea on; and he was just as fond of a sharp walk in the face of a biting "nor'-easter." His bodily organs seemed always to perform their functions healthily, in spite of hard usage. The fatigues of long hours and frequent journeys seemed light to him, and he enjoyed the stimulus of strong tea after dinner without the fear of lying awake in bed after it.

In spite of his activity his mental attitude was essentially conservative, though he contrived to keep an open mind on scientific questions. He did not accept a theory until he had satisfied himself that there was a sound foundation for it, but when accepted he did not hesitate to push it to its logical consequences. At the meeting of the British Association at Edinburgh in 1892, there was a discussion in Section A, on the question whether the well-known yellow rays of a spirit lamp, with salt in the spirit, were emitted by the heated sodium chloride or only by molecules of uncombined sodium. The latter supposition did not seem to commend itself to many of the chemists present, but Stokes remarked as we left the room, "Temperature does not produce any spectrum—a mere motion through space does not affect the aether—there must be some chemical changes going on in the flame." This was, no doubt, a logical deduction from the kinetic theory of gases and the theory of light-waves in an aethereal medium; and he adhered to it to the end of his life, for in a conversation I had with him only a few months before his death he made a similar remark with reference to the spectrum of carbon, a subject to which he often recurred. Nevertheless in that same conversation his cautious mental attitude prevented his giving assent to the argument that, because the thermal effect of a chemical combination can be shown to be the sum of

several parts derived from the several components and dependent only on their chemical natures, such effect must be derived from changes in the intrinsic energies of the components. He had not tested the theory of intrinsic energies, and so his final remark was, "The heat is more probably due to some form of the potential energy of an attraction."

Stokes' mind certainly affected a great many members of the University, as it affected me. Many have acknowledged how much they owe to him, and his influence has contributed a great deal to raise our school of Natural Philosophy, which produced but few men in the earlier half of last century, to its present fruitful state. But that influence was never, so far as I am aware, exercised consciously with a view to reform, though always with a view to promote true science so far as possible. His instruction was particularly effective because he was, as Newton had been, himself an experimenter. His predecessor as Lucasian professor, Dr King, never lectured; nor did Babbage the penultimate occupant of the chair. The Plumian professor of Astronomy and experimental Philosophy lectured in the forties on Hydrostatics and Optics, and showed various pieces of apparatus which Airy had used to illustrate his lectures experimentally, but in Challis' hands they were not even working models. We had to get up Natural Philosophy by a painful exercise of the imagination on diagrams and descriptions, and the abstractions formulated by mathematicians to make calculation possible, which presented Nature as a lifeless statue. Imagination could inspire such a statue with life and activity, but it must be the imagination of a mind already acquainted with nature ever moving and energetic. Stokes at once set the study on a new footing, but it was done at the expense of personal trouble undertaken ungrudgingly for very love. The University at that time possessed no laboratories, and provided no appliances for her professors, much less for students, and did not in all cases provide even lecture rooms. If it had not been for Miller, who left at St John's College apparatus for observing the spectrum of Nitrogen Dioxide, and in his lectures showed us conical refraction and other optical properties of crystals, we had no chance of seeing in Cambridge the optical effects of which we were expected to give the theoretical explanation. I never attended Stokes' professorial lectures, for he had not begun them when I took my first degree, but I have often

seen his experiments. At first these were made in his own rooms in College, with very simple but effective apparatus, usually got together with his own hands and always so manipulated. He appreciated accurately the circumstances necessary to attain success and spared no pains to ensure it. To the end of his life I do not think that he ever had the help of a trained assistant for his experiments. This independence was characteristic, and the experience gained by it made him such a good critic of other people's experimental work, equally of their methods and of their results.

I have often heard it remarked that Stokes seemed to take all the honours bestowed on him as if they were matters of course. That this was far from being the case I feel sure. He was neither cynical nor thick-skinned, but was naturally sensitive, and showed a delicate consideration for the feelings of others which only those who have fine feelings of their own usually show. He was not really indifferent to the recognition of his merits, though he had the control of his emotions which belongs to a strong character. The attainment of that sort of distinction had never been the object of his life; and when it came it was accepted with pleasure, while the withholding of it would never have disturbed his equanimity. His sensitive nature contributed a great deal to his success in helping the labours of other workers at science, for it enabled him to set errors right without offending the authors. How much more anxious he was to advance knowledge than to win honour for himself is fully proved by the amount of thought and trouble he bestowed on his office of Secretary to the Royal Society. Many whom he has assisted testify to it. Anyone less true to his instinct of duty, less willing to spend his own time and labour without reward, would have let many a mistaken investigator publish his errors, or have quashed discoveries of real value because they were ill-presented. Stokes did neither; he took extraordinary pains with papers on physical subjects presented to the Royal Society, first to ascertain that the experiments described were trustworthy, and then to make sure that he understood the author's theory, consulting other experts when he felt any doubt. Eminently conservative as he was, he nevertheless was in no hurry to reject a theory merely because it was not orthodox, but would suggest methods of testing it, or modifications to meet objections, and so, without throwing

good work after bad, he saved a great deal of good work which would else have been wasted because imperfect. That he might have enhanced his reputation as a discoverer by giving the time spent on other people's work to investigations of his own there can be no doubt; but considering how few could have done successfully what he did and how many fewer would have done it, the actual progress of knowledge has, in all probability, been greater through his choice of a path on which he and others pulled willingly together.

By Sir MICHAEL FOSTER, K.C.B., F.R.S.

I saw very little of Stokes (I shall venture in what I have to say, to use the simple name without the titles) until I went to Cambridge in 1870; for in the few relations which up to that I had with the Royal Society I naturally turned to his brother secretary, William Sharpey, who was my great personal friend. Nor in the succeeding ten years did I come into close or frequent touch with him either at Cambridge or at the Royal Society. I joined the latter in 1871; but Sharpey's successor as Secretary was Huxley, and he again was my very close friend; to him I naturally appealed in all matters relating to the Society.

I have however a still vivid recollection of a remarkable feat in the summer of one of the late seventies. It was the last meeting of the Society in June, and at that period the number of papers which, not having yet been read, had to be read at the last meeting was usually very considerable. On that occasion there were I think nearly thirty, and these dealt with very many different branches of science, mathematical, physical, chemical, geological, biological, and other. I remember distinctly how Stokes, standing up, and taking the paper first on the list, gave, in a very few words, a marvellously clear account, such as all of us could understand, of what the paper was, what the author proposed to do in it, and how he did it. Each paper in turn he treated in the same way; of each he gave an exposition, a model of brevity and lucidity; and he seemed to us no less successful with the biological and geographical papers than with those dealing with mathematical and physical themes. As we listened to him, we seemed to grasp quite easily what each author in succession desired to bring before the Society. Only at

the end did he seem to fail. With characteristic self-denial he had placed two papers of his own the last on the list. When he came to deal with these his manner somehow changed; and I, at least, went away with the feeling that I had understood every paper except Stokes' own.

In 1881 I was appointed Secretary to the Society, taking the place of Huxley.

The work of the Society may be described as threefold in character. There is what may be called the internal scientific work, all that relates to the communications made to the Society, to the reading of papers, and to their publication in the *Transactions* or *Proceedings*. Then there is the external scientific work, the negociations with the Government or with other bodies, home or foreign, concerning scientific undertakings or scientific questions, the initiation or superintendence of scientific inquiries or expeditions, and the like. Lastly, there is the domestic work, all that has to do with the organisation of the Society itself, its internal economy, the arrangements for the meetings, the library and the publications, and so on.

Though of the two Secretaries one is chosen for his acquaintance with the physical sciences and the other for his acquaintance with biological sciences, I found, on taking office, that the duties devolving on each secretary were not arranged according to this division. One secretary took the whole, or at least the main part of the internal scientific work, both physical and biological, and the other similarly took the external scientific work, both sharing with the Treasurer the domestic work. Huxley, before me, though junior Secretary, had taken the external work, and, on my succeeding him, Stokes, who naturally had the choice as to which duties he should take up, decided to continue to go on with the internal work, which had been under his care for so many years.

I am not sure, but I believe that the division of labour of which I have just spoken was in practice when Stokes became Secretary in 1854. If so, the charge of the publications of the Society was in his hands for the long period from that date until he became President in 1885. And it would be difficult to overstate the amount of the labour which Stokes bestowed on these duties during this long period. Though he naturally consulted his brother-secretary from time to time on the biological questions which were raised by the biological communications, nevertheless

the sole charge of at least the main part of the communications of all kinds devolved on him, from the receipt of the communication in manuscript until its publication in the *Transactions* or *Proceedings*. He made it his duty to make himself acquainted, so far as it was possible for him to do so, not only with the form but also with the substance of every paper which came in. He spared no pains in his efforts to secure that the form should be as good as possible under the circumstances. It is a matter of common knowledge that a scientific investigator, in making known what he believes to be a new truth, is more anxious about the substance of what he has to say than the way in which he says it, so that the latter at times leaves much to be desired. Stokes, as Editor of the *Proceedings* and *Transactions*, did his best to neutralize this tendency, carefully reading through the whole of the proofs, and, as these passed through his hands, making valuable suggestions to the author with the view of rendering the meaning of the sentences more clear. And, recognizing that the obscurity of a passage is often due to imperfect punctuation, he at times felt it his duty to offer advice to an author even with regard to his commas and semicolons. The papers which the Society published during Stokes' secretaryship are doubtless not in all cases models of lucid and elegant English; but where there is failure in this respect the fault cannot be laid at Stokes' door; he, during long years, did his best to make it otherwise.

Though he thus spent much labour on mere form, and so contributed in an indirect way to the progress of science, which is dependent on new truths not only being made known, but being readily understood, he did not neglect substance. And if it seem to many that his zeal for form brought in reality a great loss to science, since it took up time which might far more profitably have been devoted to one or other of the many researches of his own which he had always on hand, this reproach is not valid, or is not nearly so valid, as regards his efforts to secure that the substance of the communications made to the Society should be as good as possible. Even in the case of biological papers he from time to time offered to the author valuable criticism, by following which pitfalls were avoided, and the value of the paper, when it was eventually published, largely increased. But naturally the aid thus given was most conspicuous in the case of papers dealing with subjects with which he was familiar. Again and again, a

young investigator bringing in a crude form the results of his inquiries to the Society received from Stokes, before the account of the research was published, not merely most valuable negative criticism, through which the vexatious promulgation of false or imperfect doctrines was avoided, but inspiring suggestions, through which the inquiry was so modified or extended that the paper when it finally appeared had a worth wholly different from that which it would have possessed had Stokes' hand never have been put to it. It is indeed difficult to say how much science gained through Stokes' secretaryship by his editorial influence on the work of others, stopping that which was not fit to appear, and moulding a crude, imperfect effort into something worthy of being made known.

It must not be supposed, however, that Stokes took no part in the often heavy and responsible duties imposed by the external work of the Society. On the contrary, he was again and again called upon to take charge of matters of this kind, which his special knowledge fitted him to handle in an effective manner. And the Archives of the Society contain copies of many valuable letters and memoranda on astronomical, meteorological, and magnetic questions, addressed to H.M. Government or to other bodies, all of which bear, besides his signature, the mark of the clear, calm statement and accurate reasoning characteristic of everything to which he put his hand.

And in the domestic policy of the Society he took his share along with his brother officers.

When in 1883 the sad, premature death of Spottiswoode suddenly left the Presidential chair vacant, Stokes might very naturally have expected to be called upon to fill the place. The high position which he by that time had for many years held as a leader in investigation, to say nothing of his long services to the Society, pointed to him as Spottiswoode's successor. But it was felt by those who had the affairs of the Society more directly in charge that, were a suitable person available, it would be desirable to follow the custom through which a President whose acquirements are with the physical sciences is succeeded by one whose acquirements are with the biological sciences. Hence their minds turned to Huxley, whose claims to the honour of the chair were, in his own line, no less than those of Stokes.

Spottiswoode's death took place in the month of June, at a

time when the Society was in recess, and the affairs of the Society were wholly in the hands of the permanent officers. The Treasurer, Evans, the Foreign Secretary, A. W. Williamson, and myself took counsel together, consulting so far as at that season of the year we were able to do so, the chief members of Council and the leading Fellows of the Society. The result of our deliberations was to ask Huxley whether he were willing that we should bring forward his name to the Council, with whom lay the duty of nominating someone for election by the Society as President. Huxley gave us to understand that, highly as he appreciated the honour proposed, he could not accept the nomination unless it were carried out with the cordial support of Stokes. It fell to my lot to make known to Stokes the views of his brother officers. My task, which, had I been dealing with some men, might have been extremely difficult, proved extremely easy; his cordial support was at once unhesitatingly given, and Stokes remained the senior Secretary*.

But only for a short time longer. For reasons of health Huxley was led to resign the Presidency in November, 1885; and Stokes was of course put in the vacant Chair.

The five years, 1885–90, during which Stokes was President, were not marked by any stirring or disturbing events, so far as the affairs of the Society were concerned. He was never called upon to exert, in any crisis, the autocratic power with which, by tradition, the Society invests its President, or, in any emergency, to take up unexpected responsibilities. The circumstances of the Society during his term of office demanded no more than that he should "pursue the even tenour of his way."

That he did with his habitual calm and wonted dignified quietude. His general mental attitude was that which is usually called conservative, and this he brought to bear on the affairs of the Society, to the Society's gain. His Presidential actions were not of the initiative kind; he left the unfolding of new projects to other more progressive Fellows of the Society, some of whom were always to be found on each year's Council. He spoke seldom, and never at any great length, from the Chair, with Presidential authority. But his long experience of the affairs of the Society, his great knowledge of what had been done, the exactitude with which he was able to state what could and what could not be done within the laws and customs of the Society, gave the utmost

[* Cf. Prof. Stokes' letter to Dr Romney Robinson, *supra*, p. 30.]

weight to his brief contributions to the discussions of the Council and of the Society. He did not vehemently urge the Society forward, but he used his powers and position to prevent it hurrying in the wrong direction.

One little incident, and so far as I know one only, marred for a little while and to a slight extent the even tenour of his Presidential way. Some Fellows of the Society held very pronounced views about the nature of the office of the President; they regarded it as being what almost might be called sacred in character, not to incur danger of being defiled by touch with common things; they urged that the virtue of the holder of it should be placed beyond all suspicion. Huxley, as may be seen from his *Life and Letters*, held this view very strongly. So soon as he himself became President he at once resigned all positions already held by him, and refused to accept any new ones which he considered inconsistent with the office. He thought that the President of the Royal Society ought not to be a Member of Parliament, since in that position he would be exposed to party temptations and to party public abuse; and ought not to be the President of such a Society as the Victoria Institute, since the fact of the same man holding both offices might be quoted by some as indicating that the Royal Society took up a position concerning the relations of science to religion. And he wrote in *Nature* an unsigned letter urging that Stokes, who was both Member of Parliament and President of the Victoria Institute, ought to be neither. The letter had no effect on Stokes; he continued to hold both offices. Indeed, he hardly seemed to realize the point of view of the writer of the letter; he fancied that it was dictated by other motives. When he learnt later that the letter had been written by Huxley, his dominant feeling was that of astonishment; he laughingly said that he thought that it had been written by a Radical politician.

If as regards the external scientific work of the Society Stokes during his Presidency was of value as a judge and as a critic rather than as an active ruler, his wide knowledge and his clear, critical insight made his position in the Chair one of extreme importance in respect to the Society's internal work. His power of recognising not only the hidden weaknesses but also the often not too obvious worth of communications made to the Society, as well in biological or geological matters involving in any way physical or chemical considerations as in the subjects more clearly his own, strengthened

as this was by an absolute single-mindedness of purpose, rarely, one might say never, led astray by any secondary influences, gave a valuable positive weight to the decisions of the Council. It guided them to recognise early and to encourage the worth of the initial efforts of young workers who had not presented their work in the best possible way, and who might have been hardly judged by those who could not distinguish so clearly and patiently as did Stokes, the essential from the passing and the irrelevant.

By Sir W. HUGGINS, O.M., K.C.B., Past Pres. R.S.

I willingly adopt your suggestion to put down in a few sentences, from my own personal reminiscences, an expression of my appreciation of the personal character and of the very high intellectual qualities of Sir George Stokes, whose acquaintance and friendship for more than forty years I regarded indeed as a great privilege and possession. I became acquainted with him in the early sixties, a few years after his appointment as Secretary of the Royal Society, in his official character in connection with papers which I communicated to the Society. This acquaintance grew more intimate when I became a member of the Council in 1866. Soon I found that I had gained a true friend, whose ready sympathy and advice during frequent correspondence almost to the time of his death, I regarded as precious beyond words.

One of the most distinguishing characteristic qualities of Sir George, was the generous way in which he was always ready to lay aside at once, for the moment, his own scientific work, and give his whole attention and full sympathy to any point of scientific theory or experiment about which his correspondent had sought his counsel. Notwithstanding the many heavy duties resting upon him, his reply came nearly always without delay by an early post.

Not less remarkable than his ready intellectual sympathy, was the clear mental grasp with which he discussed the point submitted to him, and the new light with which, in a few simple sentences he surrounded it. His scientific instinct was of a very high order, and all but unerring. In a quite unusual degree he combined with unsurpassed mastery and originality in the application of mathematical methods to physical problems, a very

high endowment of that scientific imagination, or to use Bacon's words, that "learned sagacity in the procedure from one experiment to another" upon which rests success and progress in experimental science.

His own experiments, like those of Faraday, were characterized by the extreme simplicity which is possible only to a master-mind.

I consider myself fortunate, indeed, to have had such a man as correspondent and friend for so many years; I cannot even estimate my indebtedness to him.

In later years, Sir George had the consciousness, not too common, that with increasing age some failure of his powers was not improbable. In one of his letters he spoke of the decrease of the sensitiveness of his eyes to violet light, and asked if I had the same experience. In other letters he hoped that he had not forgotten any points, and asked me "to jog his memory" if this were the case. Due, no doubt, in part to his methodical habits, I never noticed the smallest sign of forgetfulness.

I hesitate to add any lighter reminiscences; but in the first type-written letter I received from him I was amused by the comment "that now I shall be able to read my own letters."

He was much interested, many years ago, when after a quiet dinner here, I asked him to examine a mastiff which I then possessed, in elementary mathematics. I claimed for my dog, whom I had named Kepler, that he would answer with accuracy any algebraical questions, of which the answers could be given by a number of barks not exceeding five or six. The dog answered successfully every question which Sir George put. He thoughtfully remarked that there was clearly mind of some kind at work. My own explanation of the dog's success is that he was able to read in my face when he had barked the right number of times, though I endeavoured to avoid giving him any sign.

To sum up;—in Sir George Stokes a great man indeed has passed from us, whose greatness is most appreciated by those who knew him best.

By the Right Reverend GEORGE FORREST BROWNE, D.D., Lord Bishop of Bristol, Hon. Fellow of St Catharine's College.

At a time when it seemed not improbable that a proposal would be made to deprive the Universities of their representation in the House of Commons, some of the University Conservatives raised the question—Was it right that Cambridge should send as its representatives men whose first business it was to be politicians? They agreed among themselves that one of the two representatives ought to be directly concerned in the teaching or in the management of the University; better still, if practicable, both. Professor Stokes was approached privately. Agreeing heartily in the principle laid down, and encouraged by the thought that his predecessor Sir Isaac Newton had held the position now offered to him, he set himself with characteristic simplicity and thoroughness to the one primary question, Could he fully perform all the duties of his Professorship if he undertook this new duty? It was with him much more than a question of trains, and beds, and hours. That side of the question was of grave importance to a man of his age and habits, and, it is unnecessary to say, it was worked out and proved with a minuteness of detail which was a lesson for life to the younger mind of the intermediary. But would the physical labour and hurry unfit him for guiding the intellects of the very flower of the mathematical students of the University? His physical endurance was remarkable. The idea of hurry no one ever associated with his calm demeanour. And he could always sleep, placidly and soundly, whatever was going on. He said yes.

His course as Representative was one of careful sacrifice of personal comfort, and unceasing watchfulness to see whether any part of any question before the House touched the interests of the University itself or of the graduates as a whole. There is no doubt that he keenly enjoyed the position, laborious as he made it by his journeys to and fro by night and day. He did not add to the strain by making speeches. "No man," it was well said of him, "has been silent on so many platforms." He eventually retired, on grounds which every one could understand and appreciate, namely, the labour of the double position for a man of his

age, and the feeling that someone else should have the opportunity of enjoying the honour. But there is no harm in saying, now, that there was another reason. On a question which was sure to come to the front, a question of deep interest to the majority of the graduates, though not touching the University itself or the Colleges, he had made up his mind, after much thought, that if he voted he must vote against the view of the majority, and that he could not conscientiously abstain from voting. That was the culminating reason for his retirement from a position which has become greatly stronger because he has held it. The other Universities of the kingdom have not been slow to follow the example of Cambridge. On his retirement, it may be added, Cambridge set another excellent example. The Conservatives offered the position to one who was known as a University Liberal, Dr R. C. Jebb, the Regius Professor of Greek, who had assured the same intermediary that on three points of policy, which alone were mentioned to him, his views were in general accord with theirs. It is greatly to be regretted that the death of Sir Richard Jebb has prevented the fulfilment of his promise to write an appreciation of his distinguished predecessor in the representation of the University.

As a member of the University of Cambridge Commission, 1877—1881, Professor Stokes took on all questions the side of moderate reform, with emphasis on the "moderate" rather than on the "reform." That remark does not mean more than this, that while he required strong evidence in favour of reform as contrasted with modification, moderation was with him a fundamental demand. In the arrangement of Professorial stipends, he constantly urged that while a "modest provision" should be made for the maintenance of a "modest household," there should be no stipend large enough in itself to form the attraction for candidature. The honour of holding such a position should be the primary attraction. In common with other Commissioners he looked with a longing and hopeless eye to the system of the Scottish Universities, where large pensions for retired Professors are a charge upon the national funds. That the Colleges should make some direct contribution to the income of the University he readily allowed. But he maintained that a grave depression of agricultural prospects had set in, and that those of the Colleges which depend chiefly upon land and tithe, and not on house

property, all, that is, but two or three, would become less able to make contribution without hampering their own work. This was one of the chief controversies which divided a generally harmonious Commission. The contention waxed rather than waned. It was decided that all Colleges must pay an annual contribution, increasing each five years, assessed upon their income, but that the agricultural depression might go further than it had gone, and power should therefore be given to the Chancellor of the University to stop for a period the quinquennial increment (£5000) of the College contributions. The final fight was on the question, should the ultimate maximum of the contribution be £25,000 or £30,000? Professor Stokes was determined that £25,000 was—to say the least—as much as could be borne by the Colleges, and that some of the Colleges would be seriously injured by the necessity of paying their quota towards that amount. The chairman of the Commission, Sir Alexander Cockburn, had ceased for some time to attend the meetings of the Commission, feeling out of harmony with its proposals. Professor Stokes took part in a representation to him, through one of the Secretaries, that at the next meeting it would be decided whether £25,000 or £30,000 should be the limit, and that his vote would settle it one way or the other. This was put to him by the Secretary as a piece of business information, not as a request for his attendance or for his support of the smaller sum. Sir Alexander said that he would certainly attend, and made a very frank utterance as to his vote. That was on a Friday or Saturday. The meeting was on the following Tuesday. Sir Alexander died suddenly on the intervening Sunday, and the ultimate contribution of the Colleges stands at £30,000 a year.

As regards the Statutes of the Colleges, Professor Stokes, who took very little part as a rule in the detailed discussion of various points, always became vocal when questions affecting the tenure of the offices of Tutor and Dean and Chaplain were under consideration. There was a regular iteration of such epithets as parental, kindly, genial, firm, and—oftener than all—tactful. It was clear that if it could have been put into a Statute he would have had an enactment of this kind—"when once you have got a tactful Tutor, don't think of superannuation"; and he would have given a very short shrift to an untactful person in that position. He did all he could to add to the status and importance

of the office of Chaplain and the disciplinary office of Dean. He thought that the offices should be combined, and that a special appointment of a specially suitable man should be made, not of necessity a man fully up to the Fellowship standard. If such a man filled the office well, he held that he had done great service to the best interests of the College, a service which deserved recognition by election to a Fellowship. The religious interests of the young men stood very high in Professor Stokes's estimate of the duties of a place of higher and highest education.

A large experience of Professor Stokes as a member of the Council of the Senate, as also of many Syndicates and other administrative bodies, has left a general impression of the presence of lucid simplicity and the absence of subtlety. As it used to be a matter of weekly interest to note the different ways in which Dr Westcott and Dr Lightfoot, sitting next each other, would deal with a subject, so it was with Professor Stokes and Professor Cayley. Both alike were accustomed to let the ordinary business of the Council or other body take its course without their intervention. They were very unsuspicious of concealed purposes in the management of affairs. When Professor Stokes had to take part in something which called for his intervention, he would make a remark which—as new-comers naturally said—any one might have made. The listener respectfully supposed that it came from profound depths and was moulded into the very simplest form by an intellect of unusual power. Still, thoughts and words alike were free—it may be more true to say were freed—from all appearance of difficulty. He seldom joined in the general discussion of a question, unless it was one which belonged specially to his own sphere of work. He never played the useful part of one who helps others to decide against a view by the manner in which he maintains it. When he expressed an opinion, he spoke with a gravity of manner which shewed his sense of the responsibility of expressing an opinion. But he usually listened and was silent till the time came to vote. Then it was seen how highly sensitive the balance of his mind was. A chairman had often to wait for his vote, and more frequently than with other men to record him as not voting. One seemed to see a testing instrument of the finest delicacy under careful experiment. A grain more, or a grain less, he could vote yes, or no; failing the grain, neither.

Professor Stokes's abstraction of mind and manner gave the impression of a mind formed into compartments, varying greatly in size, with no intercommunication. But no matter which compartment he might have in use at the time, all was orderly, kindly, reverent, charming, loveable. Much there was to enjoy in the present; much, of general impression, there is to enjoy in the retrospect; comparatively little that has stamped itself in minute detail on the memory. For among the many compartments of his mind, there was not a compartment which supplied incisive speech or epigram. There was a compartment of genial humour; and when that compartment was open and in use, there was a smile on the face, and a playfulness of gesture with the hand, and a laugh in the voice, which no one will forget.

His demeanour towards religious services, religious questions, religious things, was itself religious. He took special interest in the difficulties of sceptics. In the later years of his life he noted two facts of observation. The one was, that the doctrine of an endless eternity of punishment by torture was a moving cause of sceptical objections against Christianity as a whole; the other, that neither he nor such of his acquaintance as had entered upon the subject with him accepted the doctrine as part of the faith delivered to the saints. He took pains to press the second of these observed facts upon the class of persons concerned in the first. The result was a series of letters, artlessly written at odd times as occasion served; written conversationally, without copies of former letters, and thus with much repetition. They were published in 1897 (James Nisbet and Co.) under the title of *Conditional Immortality*.

Sir George Stokes, as he had then become, set out with the principle that the "natural immortality" of the soul was a dogma taken from Platonism, and that the combination with it of the Scriptural revelation of the "finality of the perdition of the lost" had created the doctrine of "endless torments." By "natural immortality" is meant, of course, that every soul must go on for ever, that no soul can come to an end. Equally of course, the question raises no doubt of the existence of the soul after the death of the body; it is concerned only with the eternity of such existence. While he freely used illustrations drawn from physical science, he was very clear against drawing argument from the physical to the spiritual. To one who put an argument for the

immortality of the soul from the physical law of the conservation of energy, he replied in words of a directness that Dr Temple might have used,—"I do not think there is anything at all in it"; and he proceeded to give interesting reasons for his opinion. Indeed he went further than this, and would accept neither physical nor metaphysical arguments for the survival of the soul, in place of the revelation made by the Son of God.

Dealing with the soul as separate from the body, his scheme was as follows. The Scriptural account of the creation and fall of man is a guide to the truth. Man's soul was intended to be immortal, the immortality depending upon the condition of obedience. Man's will violated the condition of obedience, and so man's soul lost the offer of immortality. Not that the souls of men from Adam to Christ did not live after the death of the body, but they would not continue to exist eternally. The death of Christ—he was clear against curious examination into the mystery of the Atonement—restored to the soul of man not immortality but the offer of immortality. One of his favourite phrases was "Life in Christ"—no other revealed eternity of existence. "The wages of sin is death, the gift of God is eternal life through Jesus Christ our Lord." We must understand that he attached vast importance to the preaching to the spirits in prison.

He confessed that of the large number of those who had told him that they did not hold the doctrine of "endless torments," the majority believed that all souls would eventually be saved. For himself, he felt it to be much more difficult to make that view accord with the statements of Scripture than the view which he himself held, that at the end—the words are his own—they that have done evil will come forth unto a resurrection of judgment; they will be consumed like tares burnt up in the fire; consigned to a second death from which there is no resurrection. He told with evident pleasure that a Cambridge friend, to whom he had stated his scheme, remarked to him afterwards that "reading the Bible with that idea in the head is like turning a key in an oiled lock."

It is clear that in this scheme there is room for endless speculation as to the condition and treatment of the soul between the death of the body and the final judgment. With such speculation he would seem to have had no sympathy. Purgatorial flames

were as far from his conceptions as were endless torments, if we may judge by his silence. As relating to this point, evidently one of very great importance, a passage may be quoted in conclusion from one of his letters, without criticism or comment.

"What does St John say of conformity to Christ? 'We know "that when He shall appear we shall be like Him, FOR we shall "see Him as He is.' I have long thought that this revelation "would be the means of causing the character of each man to "take its final set for good or for evil. Just as in this life the "preaching of the Gospel may be a savour of life unto life or "a savour of death unto death, so the revelation of the last day "may produce these opposite sets in the character, and the direc-"tion that the set will take may be determined by the character "that has been formed during the state of probation. On those "in whom the set will be for good the gift of eternal life in its "final inalienable possession will be bestowed. As to those in "whom it will be for evil, the general teaching of Scripture "is, I think, that they will be destroyed like tares burnt up "in the fire."

BIOGRAPHICAL AND SCIENTIFIC EVENTS.

1841. Senior Wrangler; first Smith's Prizeman.
Fellowship at Pembroke; vacated on marriage, 1857; re-elected under new statute, 1862.
1849. Lucasian Professor of Mathematics.
1851. Fellow of the Royal Society.
Manchester Lit. and Phil. Soc., Hon. Member: Wilde Medal, 1897.
1852. Rumford Medal of the Royal Society.
1854. Elected Secretary to the Royal Society.
1856. Haidinger Medal of Imperial Vienna Academy.
Rotterdam, Bataafsch Genootschap, Correspondent.
1864. Hon. F.R.S., Edinburgh.
Royal Academy of Göttingen, Correspondent.
1867. Royal Society of Sciences of Upsala, Member.
1869. President of the British Association at Exeter.
1871. Hon. LL.D., Edinburgh.
1873. Royal Irish Academy, Hon. Member.
1877. Gauss Medal of the Academy of Göttingen.

1878. Société Française de Physique, Member.
1879. Royal Prussian Order *Pour le Mérite*.
Académie des Sciences de l'Institut de France, Correspondent succeeding Ångström.
1880. Ophthalmological Society, Hon. Member.
1882. Imperial Academy of Vienna, Foreign Associate.
Royal Academy of Göttingen, Associate.
Madras Harbour Commission.
1883. National Academy of Sciences, Washington, Foreign Associate.
1883 to 1885. Burnett Lecturer, University of Aberdeen.
1885 to 1890. President of the Royal Society.
1886. Royal Institution, Actonian Prize. LL.D., Aberdeen.
1887 to 1891. M.P. for Cambridge University.
1887. Philosophical Society of Glasgow, Hon. Member.
1888. Munich Academy of Sciences, Foreign Member.
R. Accademia dei Lincei, Rome, Foreign Member.
1889. Baronet of the United Kingdom.
American Philosophical Society, Member.
1891 to 1893. Gifford Lecturer, University of Edinburgh.
1892. Royal Berlin Academy of Sciences, Corresponding Member.
Institution of Civil Engineers, Hon. Member.
1893. Copley Medal of the Royal Society.
University College, London, Life Governor.
1896. Imperial Academy of Sciences of Vienna, Foreign Hon. Member.
1897. Imperial Society of Naturalists of Moscow, Foreign Member.
1899. On the occasion of the celebration of his Jubilee by the University of Cambridge he received
The Arago Medal of the French Academy, struck only on special occasions,
The Portrait Commemoration Medal struck in Gold by the University of Cambridge,
Royal Academy of Belgium, Diploma of Associate Member,
and congratulatory addresses from scientific bodies in all parts of the world.
1900. Elected one of the (8) foreign members of the French Academy of Sciences, in succession to Weierstrass.

1900. Royal Academy of Turin, Foreign Member.
Royal Academy of Belgium, Associate.
1901. The Helmholtz Medal, Royal Academy of Sciences, Berlin.
1902. Elected Master of Pembroke College, Cambridge.
Società Italiana delle Scienze, Foreign Member (one of 12).
Doctor of Mathematics of the University of Christiania, on the occasion of the centenary of Abel's birth.
1904. Bronze memorial medallion placed in Westminster Abbey.

In addition the following may be noted:
Member of the Governing Body and *ex officio* Fellow of Eton College.
Member of the Board of Visitors of Greenwich Observatory for 44 years.
Historic Society of Lancashire and Cheshire, Hon. Member.
President of the Victoria Institute, London.
Received Honorary Degrees from the Universities of Oxford (D.C.L.), Edinburgh (LL.D.), Glasgow (LL.D.), Aberdeen (LL.D.), Dublin (LL.D.), Victoria (Sc.D.), Cambridge (Sc.D., LL.D.), Christiania (Math.D.).

GENEALOGY.