

the Koran in one hand and the sword in the other—either submission or death. After a little while came the tribute, or redemption. People redeemed themselves by paying an annual tax, very small, and they lived in peace. Then they extended to Syria, Mesopotamia, Egypt, Tripoli, to the borders of the Alantire, &c. The Arabs are like the Anglo-Saxons. They conquer,—give their language, manners, and customs to the conquered nation,—and in a short time they make them Arabs.*

Botanical Society of Edinburgh.

Thursday, 10th November, 1859.—Mr ANDREW MURRAY, President, occupied the Chair, and delivered the following opening address :—

It is the custom for merchants once a year to make an inquiry into their affairs: to take stock, as they call it, balance their books, reckon their gains and their losses, and see what the progress of their business has been during the bypast year. It is a good and a salutary custom, and one which most of ourselves put in use to a greater or less degree in regard to our own past conduct and life, when the advent of another year invites our thoughts to such considerations. Let us, at the commencement of our New Session, follow this example; let us take a retrospective view of our position, and estimate the gains and losses which we as the representatives, at least as the only embodied representatives, of the Science of Botany in this city, have made during the past year. Let us see how the progress of Botany has been affected by the events of that period.

And first, let us, like stout men, look our losses in the face. These, gentlemen, have not been small. We have no loss in the actual progress of our science to deplore; we have no false step in the mode of conducting our investigations to anounce; we have no fundamental principle to correct. The science is firmly based on the natural system. Its principles are sound, and are being day by day worked out to more and more perfection. Our physiological views have received no rude check; and we are progressing steadily and surely in our search after truth, and in the acquisition of fresh knowledge. But if we have no loss in this respect to regret, we have suffered deep and heavy loss in the persons of some of those who have been mainly instrumental in bringing the science into this satisfactory position. We have to bewail the loss of Alexandre von Humboldt and of Robert Brown, the topmost trees of all the forest. You have already heard the eulogium and the history of their labours from the pen of our excellent Professor, and I shall not do myself the injustice, nor inflict on you the tedium, of retreading the same ground. You have also heard from him an account of the loss we have sustained in the death of Professor Agardh of Lund, the great algologist. Of our young friend and fellow-member, Dr Nichol, he has likewise spoken; and we have recorded in our minutes the sense we felt of his loss; but in speaking of him and of similar losses, I cannot refrain from uttering the reflection which has sometimes forced itself upon me, on the occasion of the death of some of those heroes of Science, who have long stood in the front rank, and full of years as well as honours, have at last succumbed

* The Report of the Proceedings of the British Association is taken partly from the "Athenæum," and partly from Authors' abstracts.

before the assaults of time; that their removal was, perhaps, a less loss to Science than that of some promising young man just entering on his career. They are like old trees which stand out as great landmarks, but which have almost done growing. Their powers of work are nearly exhausted; while the fresh intellect, keen eye, and powers of labour of the young man, might justify us in anticipating a greater harvest from him were he spared than perhaps the giant could now produce. The loss of the great man is felt keenly by those who come in contact with him. His stores of information, applied by a profound and practised intellect, render his loss to them irreparable. To-day they might go to him secure of getting information on any subject they had on hand; to-morrow, not all the reading, nor all the correspondence with all the learned in Europe, could procure it. To them the loss is irreparable. But to you or to me, and the general scientific world, who had no access to his well-stored mind, and look for no new work from his pen, the loss is one of sentiment and feeling. In the pure practical, selfish point of view, the talented young man is probably the greater loss of the two. But in this practical and selfish light, a greater loss than either is that of the matured man, as yet in the full vigour of life, prolific in work, accomplished in science, and eager and zealous in its pursuit. And such a loss we have sustained since we last met. Arthur Henfrey, Professor of Botany in King's College, London (in which chair he succeeded Edward Forbes), has been taken from us in the prime of life and intellectual vigour, and in the full career of laborious and successful research. He was only thirty-nine years of age when he died on the 7th of September last—not a great space of time to acquire the position and to leave the numerous contributions to Science which he has done. His walk was specially vegetable physiology and histology. His contributions to the Royal and Linnean Societies of London were numerous and valuable; the last of which is a paper in this year's Proceedings of the Linnean Society, on the Morphology of the Balsaminaceæ. Not his last literary bequest, however; for he was in course of contributing papers on Vegetable Structure to "The Journal of the Royal Agricultural Society of London;" and the last proof-sheets of the second edition of his papers in the "Micrographic Dictionary" were only out of his hands a few days before his death.

Such, gentlemen, are the losses by which the past botanical year has been distinguished. Let us now turn to the more cheerful side, and reckon up our gains. In doing so I shall pass lightly over the not light labours of descriptive and of systematic botanists. Hooker, Bentham, De Candolle, Lindley, Miers, Bennett, Anderson, Berkeley, with many others, are ably continuing their labours in this department of botany. But time will not permit me to do more than merely notice the fact, that much progress has been made in this most important and most necessary, although rather dry branch of our science. Neither shall I have to detain you with a long list of new botanical works. In that respect, the year has not been fruitful. I see a new German book ("Die Pflanzendecke der Erde von Ludwig Rudolph," Berlin, 1859), which attracted my notice from being a sort of anticipation of one which most of us know is in progress by Professor Balfour and Dr. Greville,—a Climatic Flora; showing the character impressed upon different countries by the vegetation peculiar to them. It is illustrated with a number of coarse woodcuts,

representing the different regions referred to. It shows that the idea has got abroad, or has originated in other minds besides those of our friends; and suggests the desirableness of pushing on more rapidly the work to which we all look forward with so much pleasure.

But if little has been done this year in the way of producing botanical works of a general nature, a great deal has been done in the way of procuring materials for such works in the future. Much new material regarding the Flora of special regions has been published; many short notices of new plants have appeared; and various accounts of the results of public and private expeditions have either already been published, or shortly will be so. It will, I hope, not be uninteresting to the Society, if I give a hasty notice of what has been done in this way. But, first, I would direct your attention to the fact, and claim your mutual congratulations upon it, that by far the greater portion of this work has been done by members of this Society, and men educated in this school. In every one of the public expeditions which have been sent out by Government, the post of Naturalist is filled by one of our body. Of the expedition up the Niger, Dr Balfour Baikie is Chief. In the United Presbyterian Mission at Old Calabar, which, although not a public scientific expedition, has assumed so much scientific interest and importance in the eyes of the public as almost to be looked upon in that light, we have no less than three members of this Society zealously working for us—Mr Baillie, Dr Hewan, and the Rev. Mr Thomson. In Livingstone's Expedition to the Zambesi, we have Dr Kirk and Mr Bankes, who, I think, is also an alumnus of this school; and to Captain Palliser's Expedition to the Rocky Mountains, Dr Hector is Geologist and head of the Naturalist Department. And these are not our only members who are ably working for us abroad. I shall, as I go along, have occasion to refer to others.

I shall now, following the sun round the globe, give a short summary of the progress which has been made in our knowledge of Geographical Botany during the past year.

In Europe, few new discoveries have been made; and in Britain, I believe, none. There may be one or two additions made to our Flora by the discovery of some minute cryptogamic plants which have escaped my notice; but I have better authority than my own (my friend, Mr M'Nab's) for saying that nothing of any moment has been added to our Flora. In Africa, a great deal has been done, although little has yet been published. Mr Charles Barter, of the Niger Expedition, had addressed two letters (dated January and March 1859) to Sir William Hooker, giving an interesting account of the vegetation of tropical West Africa. These letters have been published in the Linnean Society's Proceedings of this year, but I do not find much that is new. He states that orchids were very scarce; aquatic plants, which might have been expected to be numerous, were not found to be so. His list only contains thirteen, among which is the *Papyrus antiquorum*. In regard to the economic expectations from the vegetation of Africa, he says, "Too much must not be expected of Central Africa as a cotton-producing country; the plant needs more moisture than it would obtain in much of the land of the interior, and water-carriage should never be far distant in a country where all loads are conveyed by canoe or on the

heads of men and women. There is plenty of available land near the sea and by rivers; the great valley of the Niger would alone yield an enormous supply. It is here cotton must be looked for, and its growth encouraged. The great plains of the interior are almost as useless in this respect as Sahara itself."

During upwards of two years' exposure to the climate, Mr Barter enjoyed excellent health under the most peculiar and trying circumstances; and it is only recently that the news of his death has reached England, from a rapid attack of dysentery at Balba, and while surrounded with comparative comforts—the first death that has occurred (such has been the care and attention devoted to health) among Dr Baikie's small party. Mr Barter's place has been supplied by the appointment of Mr Gustave Mann, who is to sail for Lagos on 24th November.

Of the Old Calabar station, the fruits are only now beginning to come in,—zoology having taken precedence of botany in the interest of the missionaries. They are now making up for lost time, and both specimens in spirits and living plants in Wardian cases (as well as seeds) have been liberally sent home. The information received from these proves of much importance, and puts us right on some points in which we had fallen into error from imperfect materials. The entire history and structure of the poison-bean is now known, and will, I hope, be described by Professor Balfour either here or in the Royal Society this winter. There are other novelties to be described; and when the seeds and the contents of the Wardian cases develop themselves, there will be still more.

Dr Livingstone's expedition does not seem to have yet reported progress on botanical points. But passing on to India, a good deal has been done. Those of you who heard Dr Hunter's exposition to the Society last winter, and Dr Cleghorn's papers read, will readily admit that the economic department is in good hands. I also notice a valuable and interesting paper by another member of this Society, and alumnus of this school, in the *Journal of the Asiatic Society of Bengal*, No. II., 1859, entitled "Notes on the Flora of Lucknow, with Catalogues of the Cultivated and Indigenous Plants," by Thomas Anderson, M.D. This paper has especial value as regards the geographical distribution of plants in India, inasmuch as he has paid particular attention to distinguishing those plants which have been introduced from those which are wild. This has hitherto been greatly neglected. A botanist coming to India, and finding an Indian plant growing in any locality, is very apt to set it down as indigenous to the spot, although, in point of fact, it has been introduced from some other parts of India by the natives in these gardens, from which it has escaped through boundaries being broken down and the gardens abandoned, the luxuriant soil and climate of India being peculiarly favourable to such naturalisation. Dr Anderson, in eliminating the genuine wild plants of his district from those introduced or cultivated, has performed a most useful service to the Indian botanist, and his example will doubtless be followed in other parts of the Peninsula. Dr Lindley has also, in the *Linnean Society Transactions*, given a valuable contribution to the Orchidology of India.

I must not omit to record a work on the Mosses of India by Mr Mitlen—a laborious work, but one labouring under the disadvantage of

want of plates, an auxiliary almost essential in all minute natural history researches.

Much has been done during the past year in zoology in the Indian Archipelago; but an equal return has not been obtained by botany. Still, if she has had less interest in the scientific researches in that quarter, she has not had to mourn, like zoology, the murder of her votaries; and only grieves for the loss of Mr Motley on grounds common to all humanity.

Passing to Australia, we find that the labouring oar there has been taken by Dr Mueller, Government Botanist for the colony of Victoria. He has contributed several papers to the Linnean Society during the past year, the chief of which are contributions to the knowledge of the *Acaciæ* of New Holland, and a monograph of that curious tribe of plants the *Eucalypti*. Besides these, he has made a report to Government on the plants collected during Mr Babbage's expedition into the north-western interior of South Australia in 1858; and he published the first parts of a separate work on the plants of Australia.

Returning northward by New Zealand, I may notice a paper by Mr Ralph on the Tree-ferns of New Zealand. And proceeding onwards to China, we find Mr Bentham revising some parts of his *Hong Kong Flora*; but since Mr Fortune's last expedition, there has been an intermission in the receipt of botanical novelties from that quarter. Japan, however, makes up for China.

Japan is the country to which all eyes are now turned. This so long hermetically sealed kingdom is at last opened. The merchant is rushing to it with his commodities, doubtless to meet the usual fate of new markets—wealth *primis venientibus*—a glutted market, and ruinous depreciation to those who follow. The naturalist is preparing to follow—nay, has already tasted of the long-forbidden fruit. The Russian expedition which concluded the treaty betwixt Russia and Japan had with them a naturalist, M. Gashkevitch, who made considerable collections, a portion of which has been described and published in the Russian scientific journals, but the much larger portion lost in the shipwreck of the Russian frigate, *Diana*, consequent upon a terrible earthquake. Other collectors have been more fortunate, and the botany of that region has been so far explored as to allow Dr Asa Gray to give a most interesting and valuable report upon it in the “*Memoirs of the American Academy of Arts and Sciences*,” New Series, vol. iv., and abridged in the last number of “*Silliman's Journal*.” I call this paper most important, not only on account of the interest which attaches to Japan at present, but from the philosophic spirit in which he uses his materials, and the important inferences which he draws from them in regard to two most interesting questions now occupying the minds of men of science—the mode of distribution of species, and the question of the origin of species; and as I have come to a different opinion from him on more than one of these points, I shall take the liberty to point them out to you. From Dr Gray's observations, it appears that, notwithstanding the comparative proximity of Japan to Western North America, there are actually more of its species represented in far distant Europe than in that country; also,—showing that this difference is not owing to the separation by an ocean—that far more Japanese plants are represented in Eastern North America than in either. And if, instead of looking at representative species, we regard

the identical species only in the several floras, the preponderance is equally against Western as compared with Eastern North America, but is more in favour of Europe; for the number of Japanese species given by Dr Gray as also found in Western North America is about 120; in Eastern America, 134; in Europe, 157.

In relation to this, Dr Gray further states that he had already pointed out, in his "Statistics of the Flora of the Northern United States," "1. That a large proportion of extra-European types found in America are shared with Eastern Asia; and 2d, That no small part of these are unknown in Western North America. But," he goes on, "Mr Bentham was first to state the natural conclusion from all these data—though I know not if he has ever yet published the remark—viz., that the interchange between the temperate floras even of the western part of the Old World and of the New has mainly taken place *via* Asia. Mr Bentham also calls to mind how frequently large American genera (such as *Eupatorium*, *Aster*, *Solidago*, *Solanum*, &c.) are represented in Eastern Asia by a small number of species, which gradually diminish, or altogether disappear, as we proceed westwards towards the Atlantic limits of Europe; whilst the types peculiar to the extreme west of Europe (excluding, of course, the Arctic flora) are wholly deficient in America. These are among the considerations which suggest an ancient continuity of territory between America and Asia, under a latitude, or at any rate with a climate more meridional than would be effected by a junction through the chains of the Aleutian and Kurile Islands."

So far Mr Bentham; but Dr Gray adds—"The deficiency in the temperate American flora of forms at all peculiar to Western Europe is almost complete, and is most strikingly in contrast with the large number of Eastern American forms repeated or represented in Eastern Asia." "Let it also be noted, that there are even fewer Western European types in the Pacific than in the Atlantic United States, notwithstanding the similarity of the climate."

Now, I pray you to observe, that in Mr Bentham's remarks he is not speaking of identical species—for it will not be denied that the number of identical species found in America, and also in Asia, is very exceptional, and in drawing general conclusions, it is always best, in the first instance, to put exceptional cases out of view—but he is speaking of representative or congeneric species; and although Dr Gray is perfectly alive to the difference between the argument drawn from an identical species and one drawn from a congeneric species, he draws the same inference from congeneric species that he would have done from identical. At one place he says—"The discovery of numerous closely related species, thus divided between two widely separated districts, might not, in the present state of our knowledge, suggest former continuity, migration, or exchange; but that of identical species, peculiar to the two, inevitably would." And yet not half a page distant he says—"That representation by allied species of genera, peculiar, or nearly peculiar, to two regions, furnishes evidence of similar nature, and of equal pertinency with representation by identical species, will hardly be doubted." Now this I utterly and wholly deny. I have satisfied myself, and I trust before I have done I shall be able to satisfy you, or at all events I shall be able to make you understand the grounds which satisfy me, that the

distribution of identical species and the distribution of congeneric species are wholly different things, and not necessarily either subject to the same laws, nor necessarily furnishing evidence of similar nature and equal pertinency. My view is, that the identity of a species is proof that it has somehow found its way from one of the places where it is found to the other; but that the presence of a congeneric species is merely the evidence that similar conditions of life, at the period of their creation, prevailed at the spots where congeneric species were created. Given the same physical conditions in all respects at two different places, say Australia and Japan, I hold that the creative product will be typically the same (not the identical species, but the type will be the same). Of course this does not affect nor take from the value of the inference of connection to be drawn from the fact of two places having had similar physical conditions. On that argument Mr Bentham and Dr Gray may be right—probably are right; but I object to the inference being drawn from the existence of congeneric species in different places, as if that, *per se*, indicated anything. It is the cause of these congeneric existences which may be used as an argument, not the existences themselves. Such abstraction from the argument being made, I might legitimately argue that the very reverse of the course of immigration or distribution indicated by Dr Gray and Mr Bentham was the true one—that the connection was between America and Europe, and not between America and Asia; for Dr Gray tells us that the identical Japanese species which occur in Europe are 157; in Eastern America, 134; in Western America, 120. The natural inference from this, if no other facts come to derange our calculation, is, that these plants have come from Japan to Western America *via* Europe—that is, first to Asia, then to Europe, then to Eastern America, and last to Western America. I do not give any opinion as to this, however. I wish merely to endeavour to adjust correctly the principles on which the reasoning is to be conducted.

Another point on which I suspect I differ from Dr Gray is on the origin of species. He does not commit himself to them; but from the terms in which he speaks of the views of Darwin and Wallace, I should incline to reckon him a supporter of them. It is an exceedingly interesting and important subject, and well worthy of our devoting a few minutes to see how it stands, which I the more readily do, because it gives me an opportunity of saying a word or two in explanation of the feelings which induced me to accept the honour of being your President, when your kindness put it within my power; for you are not to suppose that I have sat in this chair for the last twelve months without frequent qualms of conscience as to the propriety of so imperfect a botanist as I am occupying it, whilst so many so much better qualified in that respect sit around me. Gentlemen, I assure you I have not accepted it in the confidence of self-sufficient ignorance. I am perfectly aware of my deficiencies—much more so than you can be who know less of them—but it appeared to me that the science of Botany was so closely connected with the allied natural sciences, that it might tend to the advantage of both were the cords of attachment drawn nearer, by a President more familiar with one department being occasionally chosen to preside over the other. A similar course has at times been followed in our fellow Society, the Royal Physical; and I felt that, with such a Vice-President as Professor Balfour

to support and prompt me, I might gratify my own feelings by accepting the honour, and occasionally, perhaps, be of use in bringing zoological facts to bear upon botanical questions. The present is a case in point, and, I think, well shows that, in considering any subject of philosophic moment relating to Botany, Zoology must not be left out of view. The position of the matter is this:—Last year, Sir Charles Lyell and Dr Hooker brought before the Linnean Society certain papers, which, by dint [of pressure, they had prevailed upon Mr Darwin to allow them so to use, containing his and Mr Alfred Wallace's views on the origin of species. These papers consisted of an uncompleted essay by Mr Darwin, not originally intended for publication; a letter from Mr Darwin to Dr Asa Gray, the gentleman of whom we are now speaking; and a paper on the same subject by Mr Alfred Wallace, who had, independently of Mr Darwin, come to similar conclusions. These were, that there was no limit to the varieties which might proceed from species; that the breeding of domestic animals had shown the extent to which this might be carried; that these varieties become permanent; and that, under favourable circumstances, the variety might deviate so far as to constitute a new species; that the mode in which such new species might be supposed to take its ground was not according to Lamarck's hypothesis, that the supposed wants or longing of an animal ended in producing the required or desired structure. To use Mr Wallace's words—"The powerful retractile talons of the falcons and the cat tribe have not been produced or increased by the volition of these animals; but among the different varieties which occurred in the earlier and less highly organised forms of these groups, *those always survived longest which had the greatest facilities for seizing their prey.* Neither did the giraffe acquire its long neck by desiring to reach the foliage of the more lofty shrubs, and constantly stretching its neck for this purpose, but because any varieties which occurred among its antitypes with a longer neck than usual *at once secured a fresh range of pasture over the same ground as their shorter-necked companions, and on the first scarcity of food were thereby enabled to outlive them.*" Wallace, p. 6.) With a limited number of very curious facts, which I have not space here to notice (but which all appear to me to be disconnected with the theory by the absence of some link), and with an unlimited amount of time, these gentlemen think they have succeeded in giving, in this way, a clue to the origin of species; and, of course, if it is once admitted as possible to a small degree, there is no reason why it may not be extended to the whole. The theory has excited much attention. It is unnecessary to say it is a most important one. Sir Charles Lyell has formally given in his adhesion to it, in a speech which he made at the meeting of the British Association in Aberdeen; and Mr Darwin has in the press a volume treating of the whole subject; for it will be observed that the hints thrown out in these Linnean Society Papers are very brief, and merely indicate their authors' views on one or two points of a great question, leaving the greater part untouched on. I happen to know, however, from a friend who has seen the proof-sheets, that the views entertained and arguments relied on by Mr Darwin in his book are the same as those in his paper, more expanded and better illustrated; and as my objections go to the root of his theory, I may, without waiting for the book, indicate one or two zoological facts which appear to me

to be inconsistent with it. In the first place, the theory, if good for anything, must be universal. It must not be merely one of several ways, or one of two ways, of creating species; or, at all events, if it does not apply to genera (though where the line is to be drawn I cannot see), it must be the way which is followed in those cases of congeneric species to which we have been alluding. With them, at least, it must be the sole way, if it is a way at all; and, to say truth, Mr Darwin and Mr Wallace do not seem to shrink from this inference; for Mr Darwin says—"Each new variety or species, when formed, will generally take the place of and exterminate its less well-fitted parent. This I believe to be the origin of the classification and affinities of organic beings *at all times*." Assuming, then, the position to be, that this is *the* way in which species are created, or rather developed, if I can point to any instance where a congeneric species exists under circumstances where access to its allied species is morally impossible, I should say the theory must fall. In a paper on an allied subject, now in the press for next number of the "Edinburgh Philosophical Journal," I notice several of such instances. The most striking, and the one which, to my mind, at once disposes of the whole matter, is the existence of species of the same genera of eyeless insects, existing in the vast subterranean isolated caves of Carniola, allied, and exceedingly closely allied, to similar species in the caves of Hungary—to similar but different species in the caves of the Pyrenees—to similar but different species in the caves of Auvergne—and, more than all, to similar but different species of the same genera in the Mammoth Cave of Kentucky. Each of these sets of caves has a different set of species, of the same genera, and all very closely allied. The physical condition of the place being the same, the product has been the same; but not by immigration, nor any means of distribution which we can imagine, can identical species—(for, remember, the theory implies that congeneric species are identical species, or, what is the same thing, their descendants)—be found in caves so widely separated; and it is not the common case of congeneric species found very wide apart, which yet may have traversed the intervening space, because these insects are found nowhere but in the caves, and not in them until you have penetrated far, far into the interior, usually about a couple of miles. Another instance may be drawn from our own coast. We have a small beetle which lives here between high and low water-mark (*Epus fulvescens*), between the leaves of shale. A closely allied form, but quite distinct (*Thalassobius testaceus*) is found in like circumstances on the coast of Chili. Here, again, like physical condition, like product. Take another case—although, perhaps, scarcely so isolated as these two. Of late years, ants' nests have been found to contain a considerable number of species of beetles which live with the ants, are often excessively like them, and sometimes are unprovided with eyes. The same peculiarity prevails here—allied species, and nothing but allied species in ants' nests wherever they are. For instance, among the beetles so found is the curious genus *Paussus*. Seventy or eighty species of *Pauss* are now known, and all are inmates of ants' nests, and confined to them. Species have been found in these localities in Spain, in Natal, in Hong Kong, in India, in Australia, and so on. Here, again, like physical condition, like product. I might draw similar illustrations from the parasites in bees' nests and wasps' nests. Further, it were easy to draw abundance of

proof of the fact that congenerous species are at all events always found in similar physical conditions of life. Dr Gray allows this, although he applies the fact differently from me. He says—"Whether or not susceptible of scientific explanation, it is certain that related species of phænogamous plants are commonly associated in the same regions, *or are found in comparatively approximate (however large) areas of similar climate.*" But I must not dwell longer on this subject. It is a very suggestive one, and readily leads one away to meet or consider objections or difficulties which will occur to any one who thinks over it; and I would only beg any of you, who may think they see a flaw in my theory, not to take it for granted that I have not an answer for it. Perhaps, when Mr Darwin's book appears, I may examine the matter more in detail in another paper, either here or elsewhere.

Another very interesting topic, closely related to this, is also discussed by Dr Gray in this valuable paper. It is the inquiry into the distribution of the ancient flora of the northern half of the globe, as connected with its present distribution. Starting with the fact, which is now pretty generally admitted, that the present vegetation is not of recent creation, he sketches, in a clear and plausible manner, the probable geological changes which have taken place since the Tertiary Period, tracing the variations or oscillations which the climate, and consequently the ancient flora, would sustain. But for this I must refer you to the paper itself. I do not know whether his views of the antiquity of our present vegetation are accepted to their full extent by botanists in general, I rather think they wait for further information. He seems to rest them chiefly on the investigations of Mr Lesquereux, who conceives that he has identified in the tertiaries and subsequent deposits many of our present trees. For instance, in the tertiaries of Vancouver's Island, he identifies the *Sequoia sempervirens*—a tree now found ten or fifteen degrees further south—and one which grows to as great a size as the *Wellingtonia gigantea*. But I must leave Dr Gray, and hurry on to a conclusion.

Crossing from Japan to North America, we have Capt. Palliser's British North American Exploring Expedition, to which, as already mentioned, our member (Dr Hector) is Geologist and Chief Naturalist. A French gentleman, M. Bourgeau, acts as Botanical collector to the expedition; and two letters, respectively of June and October 1858, from him to Sir W. Hooker, are published in last year's Linnean Society Proceedings. They are not without interest, but I do not find anything of particular novelty. The information, both as to the plants and country, quite corresponds with what we know of them from Jeffrey and other sources. A table of the temperature of the earth and of forest trees, made at Fort Saskatchewan, furnishes data which may be useful to the generaliser.

A vast number of new pines have been described, or rather, I should say, announced under names, during the last year or two. These are chiefly from Mexico; and although many may be new, I have no doubt that a still greater number will turn out to be mere varieties or synonyms. The Botanic Garden here is, I may observe, not only well supplied with the cones and leaves, &c. of the Californian and other pines, but also has an exceedingly good collection of fine, healthy growing specimens. These unfortunately, however, are confined to one or two small plots, which might, perhaps, hold two or three examples of the trees when they reach their full size, but of course are quite inadequate to con-

tain anything like the crowd now packed in them. Space must be had for them; and I do hope that Parliament may, in another year, be concussed into doing something for this most urgent object. But it is obvious that this will only be done through force of concussion and external pressure. Members must remember that the Botanic Garden is not a matter alien to them. Next to the University, they may be said to be the parties who have the greatest interest in its prosperity; and I would remind them, that so long as they choose to continue members of this Society, it is their duty to exert themselves on behalf of the Garden. I scarcely think that the members of societies now-a-days sufficiently consider the obligation which they undertake by joining them. They get themselves proposed and are admitted, pay their fees, and think that that is all their part of the contract—in consideration of which they have the privileges of members, come to our meetings when they see any paper in the billet which takes their fancy, and in all other respects conduct themselves as if they were no more members of the Society than of one at the Antipodes. But I should wish much to get them to look at it in another light. I should like them to think that they are part of the Society, and that the Society is part of them; and that it is not optional with them, but a real matter of duty to push on, support, and sustain it by every means in their power. I am not so wild as to dream of introducing the laws of the Oïneromathic—poor Edward Forbes's early chivalrous association—in which everybody was to help everybody, under every circumstances, in purse and person. But it is not utopian to expect members, who feel themselves qualified for it, to take the trouble of giving us occasional papers—and I know several in this Society who might well do it, and do not;—it is not utopian to expect one and all of us to exert ourselves to add to the stores of the Museum—although here, I must admit that this duty is more conscientiously performed; and lastly, and what specially led me to speak upon the subject, it is not too much to expect us, one and all, to clamour loudly and perseveringly, at every fitting season, and at every fitting place, for a more liberal support from Government to the Garden and its Officers.

Gentlemen, I think I have completed the hasty survey of the globe which I undertook, for all that South America has contributed during the last year need not detain us; and in concluding, as the term of my Presidency expires this evening, I have only now to thank you most cordially, in the first place, for having honoured me so highly as to place me in this chair; and in the next place, for the uniform kindness and forbearance which you have extended to me in my imperfect efforts worthily to fill it.

The following Communications were read:—

1. *Letter from Dr JOHN KIRK, Physician and Naturalist to the Livingstone Expedition, relative to the Country near Lake Shirwa, in Africa.* (With a Plate.)

Senna, May 11, 1859.

On board Steam Launch.

MY DEAR DR BALFOUR.—From our former letters you may have heard of the difficulty we found in ascending the Zambezi. With the present steam-boat it is quite impossible; the water is confined when