some cases at least come to with much misgiving.

It must be remembered that though some of the schedules in question have been tested (and corrected) by being actually applied to a considerable mass of literature, they will all be severely tested by the actual experience even of the first year.

Speaking now entirely for myself and giving only my own opinion, I cannot expect otherwise than that the experience of the first year (or possibly even a shorter experience) will compel the International Council to authorize changes in the schedules. This, if done, will have the disadvantage of making the second year's Catalogue not wholly homogeneous with the first year's; but this I venture to think will be more than balanced by the better character of the second year's issue. Similarly with the third and fourth years. When the fifth year is reached, the Catalogue if it is to answer the expectations which it has raised may be expected to have passed through at least its main metamorphoses and to have assumed its adult form. If at the end of the fifth year, the office will be able, and it is hoped it will be able, to issue not only the Catalogue for the fifth year, but also a combined catalogue of all the five years taken together, then if that five year Catalogue and the fifth year Catalogue are not fully worth, both to workers and to libraries, the money asked for them, the whole enterprise had better be abandoned. And I would venture to urge very earnestly that the worth of the Catalogue should not be judged by its first issues; they must necessarily be most imperfect; they can only present not the achievement but the promise of what is intended.

5. As a substitute for cards, it was suggested that a bimonthly issue might be adopted, or an issue on an accumulative plan; but the Conference decided in the first instance to be content with the simple annual book issue. There seems, however, to be no reason why special arrangements should not be made by which a subscriber should receive the Catalogue printed on one side only of the paper. Scissors and paste would soon convert this into a card catalogue.

6. The subscriptions asked for by the Smithsonian Institution, namely for complete sets of the 17 volumes a year, for five years, represent part of the financial basis of the enterprise. The calculations made show that an annual sale of about 300 sets of 17 volumes representing 17 branches of science at an *average cost* per volume of £1, *i. e.*, £17 or $85 for a complete set will put the whole undertaking on a sound financial basis. The price of each volume will of course not be the same, some volumes (*e. g.*, Zoology) will be much larger than others (*e. g.*, Mineralogy). The exact price of each volume cannot be determined until the size, *i. e.*, the number of entries in that volume, is approximately known. Hence at present subscriptions for individual volumes at a stated price cannot be invited. Ultimately of course not only each volume, but in certain cases at least, parts of a volume, *i. e.*, indices of subdivisions of a branch of science will be offered for sale. The prices and conditions of sale will be determined hereafter.

M. FOSTER.

GARRISON-ON-HUDSON, N. Y.,
    September 21, 1900.

ADDRESS OF THE PRESIDENT OF THE BOTANICAL SECTION OF THE BRITISH ASSOCIATION.

THERE has been considerable difference of opinion as to whether the present year marks the close of the nineteenth or the beginning of the twentieth century. But whatever may be the right or the wrong of this vexed question, the fact that the year-date now begins with '19' instead of with '18' suggests the appropriateness of

devoting an occasion such as the present to a review of the century which has closed, as some will have it, or, in the opinion of others, is about to close. I therefore propose to address you upon the progress of Botany during the nineteenth century.

I am fully conscious of the magnitude of the task which I am undertaking, more especially in its relation to the limits of time and space at my disposal. So eventful has the period been that to give in any detail an account of what has been accomplished during the last hundred years would mean to write the larger half of the entire history of Botany. This being so, it might appear almost hopeless to attempt to deal with so large a subject in a presidential address. But I trust that the very restrictions under which I labor may prove to be rather advantageous than otherwise, inasmuch as they compel me to confine attention to what is of primary importance, and thus to give special prominence to the main lines along which the development of the science has proceeded.

STATISTICS.

We may well begin with what is, after all, the most fundamental matter, viz, the relative numbers of known species of plants at the beginning and at the end of the century. It might appear that the statistics of plants was a subject susceptible of very simple treatment, but unfortunately this is not the case. It must be remembered that a 'species' is not an invariable standard unit, like a pound or a pint, but that it is an idea dependent upon the subjectivity of individual botanists. For instance, one botanist may regard a certain number of similar plants as all belonging to a single species, whilst another may find the differences among them such as to warrant the distinction of as many species as there are plants. It is this inevitable variation in the estimation of specific characters which

renders it difficult to deal satisfactorily with plants from the statistical point of view. However, the following figures may be regarded as giving a fair idea of the increase in the number of 'good' species of living plants.

It is generally stated that about 10,000 species of plants were known to Linnæus in the latter half of the eighteenth century, of which about one-tenth were Cryptogams; but so rapid was the progress in the study of new plants at that time that the first enumeration of plants published in the nineteenth century, the 'Synopsis' of Persoon (1807), included as many as 20,000 species of Phanerogams alone. Turning now to the end of the century, we arrive at the following census, for which I am indebted mainly to Professor Saccardo (1892) and to Professor de Toni who has kindly given me special information as to the Algæ:

Species of Phanerogams indicated in Bentham and Hooker's 'Genera Plantarum' (Durand, 'Index,' 1888).

Dicotyledons	78,200
Monocotyledons	19,600
Gymnosperms	2,420
	100,220
Estimated subsequent additions (Saccardo)	5,011
Total Phanerogams	105,231

Species of Pteridophyta (indicated in Hooker and Baker's 'Synopsis'; Baker's 'New Ferns' and 'Fern Allies').

Filicinæ (including Isoëtes), about	3,000
Lycopodinæ, about	432
Equisetinæ, about	20
Total Pteridophyta	3,452

Species of Bryophyta (Saccardo's Estimate).

Musci	4,609
Hepaticæ	3,041
Total Bryophyta	7,650

Species of Thallophyta.

Fungi (including Bacteria) (Saccardo)	39,663
Lichens (Saccardo)	5,600
Algæ (incl. 6000 Diatoms) (de Toni)	14,000
Total Thallophyta	59,263

Adding these totals together—

Phanerogams..................................105,231
Pteridophyta............................... 3,452
Bryophyta................................... 7,650
Thallophyta................................. 59,263
                                           ———————
we have a grand total of...............175,596

as the approximate number of recognized species of living plants.

These figures are sufficiently accurate to show how vast have been the additions to the knowledge of plants in the period under consideration, and they afford much food for thought. In the first place, they indicate how closely connected has been the growth of this branch of Botany with the exploration and opening-up of new countries which has been so characteristic a feature of the century. Again, no one can consider these figures without being struck by the disparity in the numbers of species included in the different groups ; a most interesting topic, which cannot, however, be entered upon here. It must suffice to point out in a general way that the smaller groups represent families of plants which attained their numerical zenith in long past geological periods, and are now decadent, whilst the existing flora of the world is characterized by the preponderating Angiosperms and Fungi.

We may venture to cast a forward glance upon the possible future development of the knowledge of species. Various partial estimates have been made as to the probable number of existing species of this or that group, but the only comprehensive estimate with which I am acquainted is that of Professor Saccardo (1892). He begins with a somewhat startling calculation to the effect that there are at least 250,000 existing species of Fungi alone, and he goes on to suggest that probably the number of species belonging to the various other groups would amount to 150,000 ; hence the total number of species now living is to be estimated at over 400,000. On the basis of this estimate

it appears that we have not yet made the acquaintance of half the contemporary species ; so that there remains plenty of occupation for systematic and descriptive botanists, especially in the department of Fungology. It is also rather alarming, in view of the predatory instincts of so many of the Fungi, to learn that they constitute so decided a majority of the whole vegetable kingdom.

In spite of the great increase in the number of known species, it cannot be said that any essentially new type of plant has been discovered during the century. So far as the bounds of the vegetable kingdom have been extended at all, it has been by the annexation of groups hitherto regarded as within the sphere of influence of the zoologists. The most notable instance of this has occurred in the case of the Bacteria, or Schizomycetes, as Naegeli termed them. These organisms, discovered by Leeuwenhoek 200 years ago, had always been regarded as infusorian animals until, in 1853, Cohn recognized their vegetable nature and their affinity with the Fungi. These plants have acquired special importance, partly on account of the controversy which arose as to their supposed spontaneous generation, but more especially on account of their remarkable zymogenic and pathogenic properties, so that bacteriology has become one of the new sciences of the century.

CLASSIFICATION.

Having gained some idea of the number of species which have been recognized and described during the century, the next point for consideration is the progress made in the attempt to reduce this mass of material to such order that it can be intelligently apprehended; in a word, to convert a mass of facts into a science ; ' Filum ariadneum Botanices est systema, sine quo chaos est Res Herbaria ' (Linnæus).

The classification of plants is a problem

which has engaged attention from the very earliest times. Without attempting to enter into the history of the matter, I may just point out that, speaking generally, all the earlier systems of classification were more or less artificial, the subdivisions being based upon the distinctive features of one set of members of the plant. When I say that of all these systems that proposed by Linnæus (1735) was the most purely artificial, I do not imply any reproach : if it was the most artificial, it was at the same time the most serviceable, and its author was fully aware of its artificiality. This system is generally regarded as his most remarkable achievement ; but the really great service which Linnæus rendered to science was the clear distinction which he for the first time drew between systems which are artificial and those which are natural. Recognizing, as he did, his inability to frame at that period a satisfactory natural system, he also realized that with the increasing number of known plants some more ready means of determining them was an absolute necessity, and it was for this purpose that he devised his artificial system, not as an end, but as a means. The end to be kept in view was the natural classification : 'Methodus naturalis est ultimus finis Botanices' is his clearly expressed position in the 'Philosophia Botanica.'

There is a certain irony in the fact that the enthusiastic acceptance accorded to his artificial system throughout the greater part of Europe contributed to postpone the realization of Linnæus's cherished hopes with regard to the attainment of a natural classification. It was just in those countries, such as Germany and England, where the Linnean system was most readily adopted that the development of the natural system proceeded most slowly. It was in France, where the Linnean system never secured a firm hold, that the quest of the natural system was pursued ; and it is to French botanists more particularly that our present classification is due. It may be traced from its first beginnings with Magnol in 1689, through the bolder attempts of Adanson and of Bernard de Jussieu (1759), to the relatively complete method propounded by Antoine Laurent de Jussieu in his 'Genera Plantarum,' just 100 years later.

The nineteenth century opened with the struggle for predominance between the Jussiean and the Linnean systems. In England the former soon obtained considerable support, notably that of Robert Brown, whose 'Prodromus Floræ Novæ Hollandiæ,' published in 1810, seems to have been the first English botanical work in which the natural system was adopted ; but it did not come into general use until it had been popularized by Lindley in the thirties.

Meantime the Jussiean system had been extended and improved by Auguste Pyrame de Candolle (1813–24). It is essentially the Candollean classification which is now most generally in use, and it has been immortalized by its adoption in Bentham and Hooker's 'Genera Plantarum,' one of the great botanical monuments of the century. In Germany, however, it has been widely departed from, the system there in vogue being based upon Brongniart's modification (1828, 1850) of de Candolle's method as elaborated successively by Alex Braun (1864), Eichler (1876–83), and Professor Engler (1886, 1898). It must be admitted that for the last fifty years the further evolution of the natural system, at any rate so far as Phanerogams are concerned, has been confined to Germany.

One of the more important advances in the classification of Phanerogams was based upon Robert Brown's discovery in 1827 of the gymnospermous nature of the ovule in Conifers and Cycads, which led Brongniart (1828) to distinguish these plants as 'Phan-

érogames gymnospermes'; and although the systematic position of these plants has since then been the subject of much discussion, the recognition of the Gymnospermæ as a distinct group of archaic Phanerogams is now definitely accepted.

Moreover, the greatly increased knowledge of the Cryptogams has involved a considerable reconstruction in the classification of that great sub-kingdom. One of the most striking discoveries is that first definitely announced by Schwendener (1869) concerning Lichens, to the effect that the body of a Lichen consists of two distinct organisms, an Alga and a Fungus, living in symbiosis ; a discovery which was so nearly made by other contemporary botanists, such as de Bary, Berkeley, and Sachs, and which can be traced back to Haller and Gleditsch in the eighteenth century.

But the discoveries which most affected the classification of the Cryptogams are those relating to their reproduction. Whilst it had been recognized, almost from time immemorial, that Phanerogams reproduce sexually, sexuality was denied to Cryptogams until the observations on Liverworts and Mosses by Schmidel and by Hedwig (of whom it was said that he was born to banish Cryptogamy) in the eighteenth century ; and even as late as 1828 we find Brongniart classifying the Fungi and Algæ together as 'Agames.' But in the middle third of the nineteenth century, by the labors of such men as Thuret, Pringsheim,' Cohn, Hofmeister, Naegeli, and de Bary, the sexuality of all classes of Cryptograms was clearly established. It is worthy of note that, although the sexuality of the Phanerogams had been accepted for centuries, yet the details of sexual reproduction were first investigated in Cryptogams. For it was not until 1823 that Amici discovered the pollen-tube, and it was more than twenty years later (1846) before he completed his discovery by ascertaining the true signifi-

cance of the pollen-tube in relation to the development of the embryo ; whilst it remained for Strasburger to observe, thirty years later, the actual process of fertilization.

The discovery of the reproductive processes in Cryptogams not only facilitated a natural classification of them, but had the further very important effect of throwing light upon their relation to Phanerogams. Perhaps the most striking botanical achievement of the nineteenth century has been the demonstration by Hofmeister's unrivaled researches (1851) that Phanerogams and Cryptogams are not separated, as was formerly held, by an impassable gulf, but that the higher Cryptogams and the lower Phanerogams are connected by many common features.

The development of the natural classification, of which an account has now been given, proceeded for the most part on the assumption of the immutability of species. As Linnæus expressed it in his 'Fundamenta Botanica,' 'species tot numeramus, quot diversæ formæ in principio sunt creatæ.' It is difficult to understand how, with the point of view, the idea of affinity between species could have arisen at all ; and yet the establishment of genera and the attempts at a natural system prove that the idea was operative. The nature of the prevalent conception of affinity is well conveyed by Linnæus's aphorism, ' Affines conveniunt habitu, nascendi modo, proprietatibus, veribus, usu.'

But a conviction had been gradually growing that the assumed fixity of species was not well founded, and that, on the contrary, species are descended from preexistent species. This view found clear expression in Lamarck's ' Philosophie zoologique,' published early in the century (1809), but it did not strongly affect public opinion until after the publication of Darwin's ' Origin of Species' in 1859. Regarded from this point of view the problems of

classification have assumed an altogether different aspect. Affinity no longer means mere similarity, but blood-relationship depending upon common descent. We no longer seek a 'system' of classification; we endeavor to determine the mutual relations of plants. The effect of this change has been to stimulate the investigation of plants in all their parts and in all stages of their life, so as to attain that complete knowledge of them without which their affinities cannot be accurately estimated. If the classification of Cryptogams is, at the present monent, in a more satisfactory position than that of Phanerogams, it is just because the study of the former group has been, for various reasons, more thorough and more minute than that of the latter.

PALEOPHYTOLOGY.

The stimulating influence of the new doctrine was not, however, confined to the investigation of existing plants; it also gave a remarkable impulse to the study of fossil plants, inasmuch as the theory of descent involves the quest of the ancestors of the forms that we now have around us. Marvelous progress has been made in this direction during the nineteenth century, by the labors more especially of Brongniart, Goeppert, Unger, Schimper, Schenck, Saporta, Solms-Laubach, Renault, on the Continent, and in our own country of Lindley and Hutton, Hooker, Carruthers, and more especially of Williamson. So far-reaching are the results obtained that I can only attempt the barest summary of them. I may perhaps best begin by saying that only a small proportion of existing species have been found in the fossil state. In illustration I may adduce the statement made by Mr. Clement Reid in his recent work, 'The Origin of the British Flora,' that only 270 species that is, about one-sixth of the total number of British vascular plants, are known as fossils. Making all due allowances for the imperfection of the geological record, for the limited area investigated, and for the difficulty of determination of fragmentary specimens, it may be stated generally that the number of existing species has been found to rapidly diminish in the floras of successively older strata; none, in fact, has been certainly found to persist beyond the Tertiary period. Certain existing genera, belonging to the Gymnosperms and to the Pteridophyta, have, however, been traced far down into the Mesozoic period. Similarly, the distribution in time of existing natural orders does not coincide with that of existing genera; thus the Ferns of the Carboniferous epoch apparently belong, for the most part, if not altogether, to the order Marattiaceæ, but they are not referable to any of the existing genera.

Moreover, altogether new families of fossil plants have been discovered: such are, among Gymnosperms, the Cordaitaceæ and the Bennettitaceæ; among Pteridophyta, the Calamariaceæ, the Lepidodendraceæ, the Sphenophyllaceæ and the Cycadofilices. It is of interest to note that all these newly discovered families can be included within the main subdivisions of the existing flora; in fact, no fossil plants have been found which suggest the existence in the past of groups outside the limits of our Phanerogamia, Pteridophyta, Bryophyta and Thallophyta.

It cannot be said that the study of Paleobotany has as yet made clear the ancestry and the descent of our existing flora. To begin with the angiospermous flowering plants, it has been ascertained that they make their first appearance in the Cretaceous epoch, but we have no clue as to their origin. The relatively late appearance of Angiosperms in geological time suggests that they must have sprung from an older group, such as the Gymnosperms or the Pteridophyta; but there is no evidence to

definitely establish either of these possible origins. Then as to the origin of the Gymnosperms, whilst it cannot be doubted that they were derived from the Pteridophyta, the existing data are insufficient to enable us to trace their pedigree. The most ancient family of Gymnosperms, the Cordaitaceæ, can be traced as far back as any known Pteridophyta, and cannot, therefore, have been derived from them; but the fact that the Cordaitaceæ exhibit certain cycadean affinities, and the discovery of the Cycadofilices, suggest that what may be termed the cycadean phylum of Gymnosperms (including the Cordaitaceæ, Bennettitaceæ, Cycadaceæ, and perhaps the Ginkgoaceæ) had its origin in a filicineous ancestry, of which, it must be admitted, no forms have as yet been recognized.

Turning to the Pteridophyta, the origin of the Ferns is still quite unknown: the one fact which seems to be clear is that the eusporangiate forms (Marattiaceæ) are more primitive than the leptosporangiate. With regard to the Equisetinæ, the Calamariaceæ were no doubt the ancestors of the existing and of the fossil Equisetums. Similarly, in the Lycopodinæ, the paleozoic Lepidodendraceæ were the forerunners of the existing Lycopodiums and Selaginellas. The discovery of the Sphenophyllaceæ seems to throw some further light upon the phylogeny of these two groups, inasmuch as these plants possess characters which indicate affinity with both the Equisetinæ and the Lycopodinæ, thus suggesting the possibility that they may have sprung from the same ancestral stock.

To complete the geological survey of the vegetable kingdom I will briefly allude to the Bryophyta and the Thallophyta. Owing no doubt to their delicate texture, the records of these plants have been found to be very incomplete. So much is this the case with the Bryophyta that I forbear to make any statement concerning them. The chief point of interest with regard to the Fungi is that most of those which have been discovered in the fossil state were found in the tissues of woody plants on which they were parasitic. In this way it has been possible to ascertain, with some probability, the existence of Bacteria and of mycelial Fungi in the Paleozoic period. The records of the Algæ are more satisfactory; they have been traced far back into the Paleozoic age, where they are represented by siphonaceous forms and by the somewhat obscure plants known as *Nematophycus* and *Pachytheca*.

In a general way the study of Paleobotany has proved the development of higher from lower forms in the successive geological periods. Thus the Tertiary and Quaternary periods are characterized by the predominance of Angiosperms, just as the Mesozoic period is characterized by the predominance of Gymnosperms, and the Paleozoic by the predominance of Pteridophyta. And yet, as I have been pointing out, we are not able to trace the ancestry of any one of the larger groups of plants. The chief reason for this is that the geological record, so far as it is known, has been found to break off with such surprising abruptness that the earliest, and, therefore, the most interesting, chapters in the evolution of plants are closed to us. After the wealth of plant-forms in the Carboniferous epoch there is a striking falling-off in the Devonian, in which, however, plants of high organization, such as the Cordaitaceæ, the Calamariaceæ and the Lepidodendraceæ, still occur. In the Silurian epoch vascular plants are but sparingly present— but it is remarkable that any such highly organized plants should be found there —together with probable Algæ, such as *Nematophycus* and *Pachytheca*. The Cambrian rocks present nothing but so-called 'Fucoids,' such as *Eophyton*, etc., some of which may be Algæ. The only known fos-

sil in the oldest strata of all, the Archæan, is the much-discussed *Eozoon canadense*, probably of animal origin ; but the occurrence here of large deposits of graphite seems to indicate the existence of a considerable flora which has, unfortunately, become quite undeterminable. Thus, whilst there is some evidence that the primitive plants were Algæ, there is at present no available record of the various stages through which the Silurian and Devonian vascular plants were evolved from them.

<div style="text-align:center">MORPHOLOGY.</div>

If inquiry be made as to the cause of the great advance in the recognition of the true affinities of plants, and consequently in their classification, which distinguishes the nineteenth century, I would refer it to the progress made in the study of morphology. The earlier botanists regarded all the various parts of plants as 'organs' in relation to their supposed function ; hence their description of plants was simply 'organography.' The idea of regarding the parts of the plant-body, not in connection with their functions, but with reference to their development and their mutual relations, seems to have originated with Jung in the seventeenth century (1687): it was revived by C. F. Wolff about seventy years later (1759), but it did not materially affect the study of plants until well on in the nineteenth century, after Goethe had repeatedly written on the subject and had devised the term 'morphology' to designate it. For a time this somewhat abstract mode of treatment led to mere theorizing and speculation, so much so that the years 1820–1840 will always be stigmatized as the period of the 'Naturphilosophie.' But fortunately this time of barrenness was succeeded by a veritable renascence. Robert Brown and Henfrey in England ; Brongniart, St. Hilaire, and Tulasne in France; Mohl, Schleiden, Naegeli, A. Braun and

above all Hofmeister in Germany, led the way back from the pursuit of fantastic will-o'-the-wisps to the observation of actual fact. Instead of evolving schemes out of their own internal consciousness as to how plants ought to be constructed, they endeavored to discover by the study of development, and more particularly of embryogeny, how they actually are constructed, with the result that within a decade Hofmeister discovered the alternation of generations in the higher plants ; a discovery which must ever rank as one of the most brilliant triumphs of morphological research.

With the knowledge thus acquired it became possible to determine the true relations of the various parts of the plant-body ; to distinguish these parts as 'members' rather than as 'organs'; in a word, to establish homologies where hitherto only analogies had been traced—which is the essential difference between morphology and organography.

The publication of the 'Origin of Species' profoundly affected the progress of morphology, as of all branches of biological research ; but it did not alter its trend; it confirmed and extended it. We are not satisfied now with establishing homologies, but we go on to inquire into the origin and phylogeny of the members of the body. In illustration I may briefly refer to two problems of this kind which at the present time are agitating the botanical world. The first is as to the origin of the alternation of generations. Did it come about by the modification of the sexual generation (gametophyte) into an asexual (sporophyte) ; or is the sporophyte a new formation intercalated into the life-history ? In a word, is the alternation of generations to be regarded as homologous or as antithetic ? I am not rash enough to express any opinion on this controversy ; nor is it necessary that I should do so, since the subject has twice

been threshed out at recent meetings of this Section. The second problem is as to the origin of the sporophylls, and, indeed, of all the various kinds of leaves of the sporophyte in the higher plants. It is suggested, on the one hand, that the sporophylls of the Pteridophyta have arisen by gradual sterilization and segmentation from an unsegmented and almost wholly reproductive body, represented in our day by the sporogonium of the Bryophyta; and that the vegetative leaves have been derived by further sterilization from the sporophylls. On the other hand, it is urged that the vegetative leaves are the more primitive, and that the sporophylls have been derived from them. It will be at once observed that this second problem is intimately connected with the first. The sterilization theory of the origin of leaves is a necessary consequence of the antithetic view of the alternation of generations; whilst the derivation of sporophylls from foliage-leaves is similarly associated with the homologous view. Here, again, exercising a wise discretion, I will only venture to express my appreciation of the important work which has been done in connection with this controversy—work that will be equally valuable, whatever the issue may eventually be.

I will conclude my remarks on morphology with a few illustrations of the aid which the advance in this department has given to the progress of classification. For instance, Linnæus divided plants into Phanerogams and Cryptogams, on the ground that in the former the reproductive organs and processes are conspicuous, whereas in the latter they are obscure. In view of our increased knowledge of Cryptogams, this ground of distinction is no longer tenable; whilst still recognizing the validity of the division, our reasons for doing so are altogether different. For us, Phanerogams are plants which produce a seed; Crypto-

gams are plants which do not produce a seed. Again, we distinguish the Pteridophyta and the Bryophyta from the Thallophyta, not on account of their more complex structure, but mainly on the ground that the alternation of generations is regular in the two former groups, whilst it is irregular or altogether wanting in the latter. Similarly the essential distinction between the Pteridophyta and the Bryophyta is that in the former the sporophyte, in the latter the gametophyte, is the preponderating form. It has enabled us further to correct in many respects the classifications of our predecessors by altering the systematic position of various genera, and sometimes of larger groups. Thus the Cycadaceæ have been removed from among the Monocotyledons, and the Coniferæ from among the Dicotyledons, where de Candolle placed them, and have been united with the Gnetaceæ into the sub-class Gymnospermæ. The investigation of the development of the flower, in which Payer led the way, and the elaboration of the floral diagram which we owe to Eichler, have done much, though by no means all, to determine the affinities of doubtful Angiosperms, especially among those previously relegated to the lumberroom of the Apetalæ.

ANATOMY AND HISTOLOGY.

Passing now to the consideration of the progress of knowledge concerning the structure of plants, the most important result to be chronicled is the discovery that the plant-body consists of living substance indistinguishable from that of which the body of animals is composed. The earlier anatomists, whilst recognizing the cellular structure of plants, had confined their attention to the examination of the cell-walls, and described the contents as a watery or mucilaginous sap, without determining where or what was the seat of life. In 1831 Robert Brown discovered the nucleus of

the cell, but there is no evidence that he regarded it as living. It was not until the renascence of research in the forties, to which I have already alluded, that any real progress in this direction was made. The cell-contents were especially studied by Naegeli and by Mohl, both of whom recognized the existence of a viscous substance lining the wall of all living cells as a 'mucous layer' or 'primordial utricle,' but differing chemically from the substance of the wall by being nitrogenous : this they regarded as the living part of cell, and to it Mohl (1846) gave the name 'protoplasm,' which it still bears. The full significance of this discovery became apparent in a somewhat roundabout way. Dujardin, in 1835, had described a number of lowly organisms, which he termed Infusoria, as consisting of a living substance, which he called 'sarcode.' Fifteen years later, in a remarkable paper on *Protococcus pluvialis*, Cohn drew attention to the similarity in properties between the 'sarcode' of the Infusoria and the living substance of this plant, and arrived at the brilliant generalization that the 'protoplasm' of the botanists and the 'sarcode' of the zoologists are identical. Thus arose the great conception of the essential unity of life in all living things, which, thanks to the subsequent labors of such men as de Bary, Brücke, and Max Schultze, in the first instance, has become a fundamental canon of Biology.

A conspicuous monument of this period of activity is the cell-theory propounded by Schwann in 1839. Briefly stated, Schwann's theory was that all living bodies are built up of structural units which are the cells : each cell possesses an independent vitality, so that nutrition and growth are referable, not to the organism as a whole, but to the individual cells. This conception of the structure of plants was accepted for many years, but it has had to give way before the advance of anatomical knowl-edge. The recognition of cell-division as the process by which the cells are multi-plied—in opposition to the Schleidenian theory of free cell-formation—early sug-gested doubts as to the propriety of regard-ing the body as being built up of cells as a wall is built of bricks. Later the minute study of the Thallophyta revealed the ex-istence of a number of plants, such as Myxomycetes, the phycomycetous Fungi, and the siphonaceous Algæ, some of them highly organized, the vegetative body of which does not consist of cells. It became clear that cellular structure is not essential to life ; that it may be altogether absent or present in various degree. Thus in the higher plants the protoplasm is segmented or septated by walls into uninucleate units or 'energids' (Sachs), and such plants are well described as 'completely septate.' But in others, such as the higher Fungi and certain Algæ (e. g., *Cladophora*, *Hydrodic-tyon*), the protoplasm is septated, not into energids, but into groups of energids, so that the body is 'incompletely septate.' Finally there are the Thallophyta already enumerated, in which there is complete continuity of the protoplasm : these are 'unseptate.' Moreover, even when the body presents the most complete cellular structure, the energids are not isolated, but are connected by delicate protoplasmic fibrils traversing the intervening walls ; a fact which is one of the most striking dis-coveries in the department of histology. This was first recognized in the sieve-tubes by Hartig (1837) ; then by Naegeli (1846) in the tissues of the Florideæ. After a long period of neglect the matter was taken up once more by Tangi (1880), when it at-tracted the attention of many investigators, as the result of whose labors, especially those of Mr. Gardiner, the general and per-haps universal continuity of the protoplasm in cellular plants has been established. Hence the body is no longer regarded as

an aggregate of cells, but as a more or less septated mass of protoplasm : the synthetic standpoint of Schwann has been replaced by one as distinctively analytic.

Time does not permit me to do more than mention the important discoveries made of late years, mainly on the initiative of Strasburger, with regard to the details of cytology, and especially to the structure of the nucleus and the intricate dance of the chromosomes in karyokinesis. Indeed, I can do but scant justice to those anatomical discoveries which are of more exclusively botanical interest. One important generalization which may be drawn is that the histological differentiation of the plant proceeds, not in the protoplasm, as in the animal, but in the cell-wall. It is remarkable, on the one hand, how similar the protoplasm is, not only in different parts of the same body, but in plants of widely different affinities ; and, on the other, what diversity the cell-wall offers in thickness, chemical composition, and physical properties. In studying the differentiation of the cell-wall the botanist has received valuable aid from the chemist. Research in this direction may, in fact, be said to have begun with Payen's fundamental discovery (1844) that the characteristic and primary chemical constituent of the cell-wall is the carbohydrate which he termed cellulose.

The amount of detailed knowledge as to the anatomy of plants which has been accumulated during the century by countless workers, among whom Mohl, Naegeli, Unger, and Sanio deserve special mention as pioneers, is very great—so great, indeed, that it seemed as if it must remain a mere mass of facts in the absence of any recognizable general principles which might serve to marshal the facts into a science. The first step towards a morphology of the tissues was Hanstein's investigation of the growing point of the Phanerogams (1868), and his recognition therein of the three embryonic tissue-systems. This has lately been further developed by the promulgation of van Tieghem's theory of the stele, which is merely the logical outcome of Hanstein's distinction of the plerome. It has thus become possible to determine the homologies of the tissue-systems in different plants and to organize the facts of structure into a scientific comparative anatomy. It has become apparent that, in many cases, differences of structure are immediately traceable to the influence of the environment ; in fact, the study of physiological or adaptive anatomy is now a large and important branch of the subject.

The study of Anatomy has contributed in some degree to the progress of systematic Botany. It is true that some of the more ambitious attempts to base classification on Anatomy have not been successful ; such, for instance, as de Candolle's subdivision of Phanerogams into Exogens and Endogens, or the subdivision of Cormophyta into Acrobrya, Amphibrya, and Acramphibrya, proposed by Unger and Endlicher. Still it cannot be denied that anatomical characters have been found useful, if not absolutely conclusive, in suggesting affinities, especially in the determination of fossil remains. A large proportion of our knowledge of extinct plants, to which I have already alluded, is based solely upon the anatomical structure of the vegetative organs ; and although affinities inferred from such evidence cannot be regarded as final, they suffice for a provisional classification until they are confirmed or disproved by the discovery and investigation of the reproductive organs.

PHYSIOLOGY.

The last branch of the botanical science which I propose to pass in review is that of physiology. We may well begin with the nutritive processes. At the close of the eighteenth century there was practically no

coherent theory of nutrition; such as it was it amounted to little more than the conclusion arrived at by van Helmont a century and a half earlier, that plants require only water for their food, and are able to form from it all the different constituents of their bodies. It is true that the important discovery had been made and pursued by Priestley (1772), Ingen-Housz (1780), and Sénébier (1782) that green plants exposed to light absorb carbon dioxide and evolve free oxygen; but this gaseous interchange had not been shown to be the expression of a nutritive process. At the opening of the nineteenth century (1804) this connection was established by de Saussure, in his classical 'Recherches chimiques,' who demonstrated that, whilst absorbing carbon dioxide and evolving oxygen, green plants gain in dry weight; and he further contributed to the elucidation of the problem of nutrition by showing that, whilst assimilating carbon dioxide, green plants also assimilate the hydrogen and oxygen of water.

Three questions naturally arose in connection with de Saussure's statement of the case: What is the nature of the organic substance formed? What is the function of the chlorophyll? What is the part played by light? It was far on in the century before answers were forthcoming.

With regard to the first of these questions, the researches of Boussingault (1864) and others established the fact that the volume of carbon dioxide absorbed and that of oxygen evolved in connection with the process are approximately equal. Further, the frequent presence of starch in the chloroplastids, to which Mohl first drew attention (1837), was subsequently found by Sachs (1862) to be closely connected with the assimilation of carbon dioxide. The conclusion drawn from these facts is that the gain in dry weight accompanying the assimilation of carbon dioxide is due to

the formation, in the first instance, of organic substance having the composition of a carbohydrate; a conclusion which may be expressed by the equation:

$$CO_2 + H_2O = CH_2O + O_2.$$
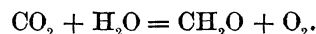
The questions with regard to chlorophyll and to light are so intimately connected that they must be considered together. The first step towards their solution was the investigation of the relative activity of light of different colors, originally undertaken by Sénébier (1782) and subsequently repeated by Daubeny (1836), with the result that red and orange light was found to promote assimilation in a higher degree than blue or violet light. Shortly afterwards Draper (1843), experimenting with an actual solar spectrum, concluded that the most active rays are the orange and yellow; a conclusion which was generally accepted for many years. But in the meantime the properties of the green coloring matter of plants (to which Pelletier and Caventou gave the name 'chlorophyll' in 1817) were being investigated. Brewster discovered in 1834 that an alcoholic extract of green leaves presents a characteristic absorption spectrum; but many years elapsed before any attempt was made to connect this property with the physiological activity of chlorophyll. It was not until 1871–72 that Lommel and N. J. C. Müller pointed out that the rays of the spectrum which are most completely absorbed by chlorophyll are just those which are most efficient in the assimilation of carbon dioxide. Subsequent researches, particularly those of Timiriazeff (1877), and those of Engelmann (1882–84) based on his ingenious Bacterium-method, have confirmed the views of Lommel and of Müller, and have placed it beyond doubt that the importance of light in the assimilatory process is that it is the form of kinetic energy necessary to effect the chemical changes, and that the function

of chlorophyll is to serve as the means of absorbing this energy and of making it available for the plant.

These are perhaps the most striking discoveries in relation to the nutrition of plants, but there are others of not less importance to which brief allusion must be made. We owe to de Saussure (1804) the first clear demonstration of the fact that plants derive an important part of their food from the soil; but the relative nutritive value of the inorganic salts absorbed in solution was not ascertained until Sachs (1858) reintroduced the method of water-culture which had originated centuries before with Woodward (1699) and had been practiced by Duhamel (1768) and de Saussure. Special interest centers around the question of the nitrogenous nutrition of plants. It was long held, chiefly on the authority of Priestley and of Ingen-Housz, and in spite of the contrary opinion expressed by Sénébier, Woodhouse (1803), and de Saussure, that plants absorb the free nitrogen of the atmosphere by their leaves. This view was not finally abandoned until 1860, when the researches of Boussingault and of Lawes and Gilbert deprived it of all foundation. Since then we have learned that the free nitrogen of the air can be made available for nutrition—not indeed directly by green plants themselves, but, as Berthelot and Winogradsky more especially have shown, by Bacteria in the soil, or, as apparently in the Leguminosæ, by Bacteria actually enclosed in the roots of the plants with which they live symbiotically.

We turn now from the nutritive or anabolic processes to those which are catabolic. The discovery of the latter, just as of the former, was arrived at by the investigation of the gaseous interchange between the plant and the atmosphere. In the eighteenth century Scheele and Priestley had found that, under certain circumstances, plants deteriorate the quality of air; but it

is to Ingen-Housz that we owe the discovery that plants, like animals, respire, taking in oxygen and giving off carbon dioxide. And when Sénébier (1800) had ascertained for the inflorescence of *Arum maculatum,* and later de Saussure (1822) for other flowers, that active respiration is associated with an evolution of heat, the connection between respiration and catabolism was established for plants as it had been long before by Lavoisier (1777) in the case of animals.

Among the catabolic processes which have been investigated none are of greater importance than those that are designated by the general term *fermentations.* The first of these to be discovered was the alcoholic fermentation of sugar. Towards the end of the seventeenth century Leeuwenhoek had detected minute globules in fermenting wort; and a century later Lavoisier had ascertained that the chemical process consists in the decomposition of sugar into alcohol and carbon dioxide; but it was not until 1837–38 that, almost simultaneously, Cagniard de Lateur, Schwann, and Kützing discovered that Leeuwenhoek's globules were living organisms, and were the cause of the fermentation. Shortly before, in 1833, Payen and Persoz extracted from malt a substance named *diastase,* which they found could convert the starch of the grain into sugar. These two classes of bodies, causing fermentative changes, were distinguished respectively as *organized* and *unorganized* ferments. The number of the former was rapidly added to by the investigation more especially of the Bacteria, in which Pasteur led the way. The extension of our knowledge of the unorganized ferments, or enzymes, has been even more remarkable: we now know that very many of the metabolic processes are effected by various enzymes, such as those which convert the more complex carbohydrates into others of simpler constitution (diastase, cytase, glucase, inulase, invertase); those

which decompose glucosides (emulsin, my-
rosin, etc.); those which act on proteids
(trypsins) and on fats (lipases); the oxi-
dases, which cause the oxidation of various
organic substances; and the zymase, re-
cently extracted from yeast, which causes
alcoholic fermentation.

The old distinction of the microorgan-
isms as 'organized ferments' is no longer
tenable; for, on the one hand, certain of
the chemical changes which they effect can
be traced to extractable enzymes which
they produce ; and, on the other, as Pasteur
has asserted, every living cell may become
an 'organized ferment' under appropriate
conditions. The distinction now to be
drawn is between those processes which are
due to enzymes and those directly effected
by living protoplasm. Many now definitely
included in the former class were, until
lately, regarded as belonging to the latter;
and no doubt future investigation will still
further increase the number of the former
at the expense of the latter.

The consideration of the metabolic proc-
esses leads naturally to that of the func-
tion of transpiration and of the means by
which water and substances in solution are
distributed in the plant. This is perhaps
the department of physiology in which prog-
ress during the nineteenth century has
been least marked. We have got rid, it is
true, of the old idea of an ascending crude
sap and of a descending elaborated sap, but
there have been no fundamental discoveries.
With regard to transpiration itself, we know
more of the detail of the process, but that
is all that can be said. As for root-pres-
sure, Hofmeister (1858–82) discovered that
'bleeding'—as the phenomena of root-
pressure were termed by the earlier writers
—is not confined, as had hitherto been
thought, to trees and shrubs ; but the cur-
rent theory of the process, allowing for the
discovery of protoplasm and of osmosis,
has advanced but little upon that given by

Grew in the third book of his 'Anatomy of
Plants' (1675). Again, the mechanism of
the transpiration-current in lofty trees re-
mains an unsolved problem. To begin with,
there is still some doubt as to the exact
channel in which the current travels.
Knight (1801–8) first proved that the cur-
rent travels in the alburnum of the trunk,
but not, he thought, in the vessels, for he
found them to be dry in the summer, when
transpiration is most active ; a view in
which Dutrochet (1837) subsequently con-
curred. Meyen (1838) then suggested that
the water must travel, not in the lumina,
but in the substance of the cells of the
vessels, and was supported by such eminent
physiologists as Hofmeister (1858), Unger
(1864, 1868), and Sachs (1878) ; but it has
since been strongly asserted by Boehm,
Elfving, Vesque, Hartig, and Strasburger
that the young vessels always contain
water, and that the current travels in the
lumina and not in the walls of the vessels.

Now as to the force by which the water
of the transpiration-current is raised from
the roots to the topmost leaf of a lofty tree.
From the point of view that the water
travels in the substance of the walls, the
necessary force need not be great, and would
be amply provided by the transpiration of
the leaves, inasmuch as the weight of the
water raised would be supported by the
force of imbibition of the walls. From the
point of view that the water travels in the
lumina, the force required to raise and sup-
port such long columns of water must be
considerable. Dismissing at once as quite
inadequate such purely physical theories as
those of capillarity and gas-pressure, there
remain two theories as to the nature of this
force which resemble each other in being
essentially vitalistic, but differ in that the
one involves pressure from below, and the
other suction from above. In the one, sug-
gested by Godlewski and by Westermaier
(1884), the cells of the medullary rays and

of the wood-parenchyma are supposed to absorb liquid from the vascular tissue at one level and force it back again by a vital act at a higher level : this theory was disposed of by the fact that the transpiration-current can be maintained through a considerable length of a stem killed by heat or by poison. In the other, suggested by Dixon and Joly (1895–99), and also by Askenasy (1895–96), it is assumed that there are, in the trunk of a transpiring tree, continuous columns of water which are in a state of tensile stress, the tension being set up by the vital transpiratory activity of the leaves. Some idea of the enormous tension thus assumed is given by the following simple calculation relating to a tree 120 feet high. Not only has the liquid to be raised to this height, but in its passage upwards a resistance calculated to be equal to about five times the height of the tree has to be overcome. Hence the transpiration-force in such a tree must at least equal the weight of a column of water 720 feet in height ; that is, a pressure of about twenty-four atmospheres, or 360 lbs. to the square inch. But there is no evidence to prove that a tension of anything like twenty atmospheres exists, as a matter of fact, in a transpiring tree; on the contrary, such observations as exist (*e. g.*, those of Hales and Boehm) indicate much lower tensions. Under these circumstances we must regretfully confess that yet one more century has closed without bringing the solution of the secular problem of the ascent of the sap.

The nineteenth century has been, fortunately, more fertile in discovery concerning the movements and irritability of plants. But it is surprising how much knowledge on these points had been accumulated by the beginning of the century : the facts of plant-movement, such as the curvatures due to the action of light, the sleep-movements of leaves and flowers, the contact-movements of the leaves of the sensitives, were all familiar. The nineteenth century opened, then, with a considerable store of facts ; but what was lacking was an interpretation of them ; and whilst it has largely added to the store, its most important work has been done in the direction of explanation.

The first event of importance was the discovery by Knight, in 1806, of the fact that the stems and roots of plants are irritable to the action of gravity and respond to it by assuming definite directions of growth. Many years later the term ' geotropism ' was introduced by Frank (1868) to designate the phenomena of growth as affected by gravity, and at the same time Frank announced the important discovery that dorsiventral members, such as leaves, behave quite differently from radial members, such as stems and roots, in that they are diageotropic.

It was a long time before the irritability of plants to the action of light was recognized. Chiefly on the authority of de Candolle (to whom we owe the term ' heliotropism '), heliotropic curvature was accounted for by assuming that the one side received less light than the other, and therefore grew the more rapidly. But the researches of Sachs (1873) and Müller-Thurgau (1876) have made it clear that the direction of the incident rays is the important point, and that a radial stem, obliquely illuminated, is stimulated to curve until its long axis coincides with the incident rays. Moreover, the discovery by Knight (1812) of negative heliotropism in the tendrils of *Vitis* and *Ampelopsis* really put the Candollean theory quite out of court ; and further evidence that heliotropic movements are a response to the stimulus of the incident rays of light is afforded by Frank's discovery of the dia-heliotropism of dorsiventral members.

The question of the localization of irritability has received a good deal of attention. The fact that the under surface of the pul-

vinus of *Mimosa pudica* is alone sensitive to contact was ascertained by Burnett and Mayo in 1827; and shortly after (1834) Curtis discovered the sensitiveness of the hairs on the upper surface of the leaf of *Dionœa*. After a long period of neglect the subject was taken up by Darwin. The irritability of tendrils to contact had been discovered by Mohl in 1827; but it was Darwin who ascertained, in 1865, that it is confined to the concavity near the tip. In 1875 Darwin found that the irritability of the tentacles of *Drosera* is localized in the terminal gland; and followed this up, in 1880, by asserting that the sensitiveness of the root is localized in the tip, which acts like a brain. This assertion led to a great deal of controversy, but the researches of Pfeffer and Czapek (1894) have finally established the correctness of Darwin's conclusion. It is interesting to recall that Erasmus Darwin had suggested the possible existence of a brain in plants in his ' Phytologia ' (1800). But the word ' brain ' is misleading, inasmuch as it might imply sensation and consciousness : it would be more accurate to speak of centers of ganglionic activity. However, the fact remains that there exist in plants irritable centers which not only receive stimuli but transmit impulses to those parts by which the consequent movement is effected. The transmission of stimuli has been found in the case of *Mimosa pudica* to be due to the propagation of a disturbance of hydrostatic equilibrium along a special tissue; in other cases, where the distance to be traversed is small, it is probably effected by means of that continuity of the protoplasm to which I have already alluded.

Finally, as regards the mechanism of these movements, we find Sénébier and Rudolphi, the earliest writers on the subject in the nineteenth century, asserting, as if against some accepted view, that there is no structure in a plant comparable with the muscle of an animal. Rudolphi (1807) suggested, as an alternative, that the position of a mobile leaf is determined by the ' turgor vitalis ' of the pulvinus, and thus anticipated the modern theory of the mechanism. But he gives no explanation of what he means by ' turgor '; and the term is frequently used by writers in the first half of the century in the same vague way. Some progress was made in consequence of the discovery of osmosis by Dutrochet (1828), and more especially by his observation (1837) that the movements of *Mimosa* are dependent on the presence of oxygen, and are therefore vital. But it was not, and could not be, until the existence of living protoplasm in the cells of plants was realized, and the movements of free-swimming organisms and naked reproductive cells had become more familiar, that the true nature of the mechanism began to be understood; and then we find Cohn saying, as long ago as 1860, that ' the living protoplasmic substance is the essentially contractile portion of the cell.' This statement may, perhaps, seem to put the case too bluntly and savor too much of animal analogy ; but the study of the conditions of turgidity has shown more and more clearly that the protoplasm is the predominant factor. The protoplasm of plant-cells is undoubtedly capable of rapid molecular changes, which alter its physical properties, more particularly its permeability to the cell-sap. It may be that these changes cannot be directly compared with those going on in animal muscle; but if we use the term ' contractility ' in its wider sense, as indicating a general property of which muscular contraction is a special case, then Cohn's statement is fully justified. This is borne out by the observations of Sir J. Burdon-Sanderson (1882–88) on the electrical changes taking place in the stimulated leaf of *Dionœa*, and by Kunkel's (1878) corresponding observations on *Mimosa publica* : in both cases the electri-

cal changes were found to be essentially the same as those observable on the stimulation of muscle. We find, then, that the advances in Physiology, like those in Anatomy, teach the essential unity of life in all living things, whether we call them animals or plants.

With this in our minds we may go on to consider in conclusion, and very briefly, that department of physiological study .which is known as the Bionomics or Œcology of plants. In the earlier part of the century this subject was studied more especially with regard to the distribution of plants, and their relation to soil and climate; but since the publication of the 'Origin of Species' the purview has been greatly extended. It then became necessary to study the relation of plants, not only to inorganic conditions, but to each other and to animals; in a word, to study all the adaptations of the plant with reference to the struggle for existence. The result has been the accumulation of a vast amount of most interesting information. For instance, we are now fairly well acquainted with the adaptations of water-plants (hydrophytes) on the one hand and of desert-plants (xerophytes) on the other; with the adaptations of shade-plants and of those growing in full sun, especially as regards the protection of the chlorophyll. We have learned a great deal as to the relations of plants to each other, such as the peculiarities of parasites, epiphytes, and climbing plants, and as to those singular symbioses (Mycorhiza) of the higher plants with Fungi which have been found to be characteristic of saprophytes. Then, again, as to the relations between plants and animals: the adaptation of flowers to attract the visits of insects, first discovered by Sprengel (1793), has been widely studied; the protection of the plant against the attacks of animals, by means of thorns and spines on the surface, as also by the forma-

tion in its tissues of poisonous or distasteful substances, and even by the hiring of an army of mercenaries in the form of ants, has been elucidated; and finally those cases in which the plant turns the tables upon the animal, and captures and digests him, are now fully understood.

CONCLUSION.

Imperfect as is the sketch which I have now completed, it will, I think, suffice to show how remarkable has been the progress of the science during the nineteenth century, more particularly the latter part of it, and how multifarious are the directions in which it has developed. In fact Botany can no longer be regarded as a single science: it has grown and branched into a congeries of sciences. And as we botanists regard with complacency the flourishing condition of the science whose servants we are, let us not forget, on the one hand, to do honor to those whose life work it was to make the way straight for us, and whose conquests have become our peaceful possession; nor, on the other, that it lies with us so to carry on the good work that when this Section meets a hundred years hence it may be found that the achievements of the twentieth century do not lag behind those of the nineteenth.

S. H. VINES.

THE METHOD OF TYPES IN BOTANICAL NOMENCLATURE.*

FOR many decades the systematic botany of the United States can scarcely be said to have had a history separate from that of Europe, so extensively were our treasures exploited by transient visitors, while resident students of the science long remained

* Read at the New York meeting of the Botanical Club of the A. A. A. S., through the kindness of Mr. Charles Louis Pollard. On motion the paper was referred to the Committee on Nomenclature and the author was requested to offer it for publication in SCIENCE.