Eliot,' these relations were never in any sense sentimental. But they were certainly much more intimate and more prolonged than any of her letters would lead us to suppose. It is surprising to learn that it was he chiefly who urged her to write fiction, an idea which she could not at first entertain. The 'Letters' leave the impression that it was Lewes who played this rôle. Perhaps both equally saw in her this talent before she saw it in herself. It is equally surprising that she should have made Spencer her confidant in the matter of the authorship not only of her first stories, but also of 'Adam Bede.' It is to be regretted that she, too, did not write an autobiography.

Such is a hasty glance at a few of the salient points in the 'Autobiography' of Herbert Spencer. No two persons would select the same points, and no such glance can hope to do justice to the work. Nothing has been said of his inventions, which were numerous but none of them important or successful; of his numerous essays, from his 'Proper Sphere of Government' to his 'Factors of Organic Evolution'; of his 'Descriptive Sociology,' that monumental but costly undertaking; of his 'cerebral hygiene,' which, unlike that of Comte, consisted in reading nothing that he did not agree with, thus warping, as Comte had dwarfed, the growth of ideas; of his more extended travels, including his visit to America, which latter is familiar to us all; nor of his persistent hostility to governmental initiative (*laissez faire*), which formed so prominent a feature in his political philosophy.

With regard to this last it would seem that owing to preconceptions of his youth confirmed during his connection with the *Economist,* he was unduly frightened by the bugbear of collectivism, which is really nothing but social integration, and a necessary part of the very social evolution which he taught. For this must consist, as in both inorganic and organic nature, of differentiation and integration. His inability to perceive this made his system, so broad at its base, a frustum instead of a pyramid.

The 'Autobiography' is written in a much more pleasing style than his other works. It shows its author in all the simplicity of true greatness. His life demonstrates that he was a natural product of his time. He lived at the acme of the Victorian age, the grandest epoch in history, and he was directly in touch with all the powerful forces that characterized that epoch. When we take into consideration his own inherent powers we may say in very truth that his life was 'a continuous adjustment of internal relations to external relations,' and that he was a normal product of the laws of evolution that he expounded.

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*THE WORK OF THE YEAR 1903 IN ECOLOGY.**

AN apology for this paper is necessary and will be forthcoming. The task outlined in the title is by no means voluntary, but has been imposed upon the speaker by your relentless committee; and this—as the secretary will acknowledge—in spite of the speaker's urgent protest. It is always impossible to give a critical summary of current events, because all of us are afflicted with the disease of contemporary blindness. It is more than impossible to do such a task for the field of ecology, since the field of ecology is chaos. Ecologists are not agreed even as to fundamental principles or motives; indeed, no one at this time, least of all the present speaker, is prepared to define or delimit ecology. It is, therefore, a

* Read by invitation of the sectional committee, Section G, American Association for the Advancement of Science, at the St. Louis meeting, December 29, 1903.

certainty that this hasty review will put emphasis where subordination or oblivion is better, and will notice slightly or not at all researches which will loom up in the future. Many titles which the speaker thinks important have been left out from lack of space and time.

If ecology has a place at all in modern biology, certainly one of its great tasks is to unravel the mysteries of adaptation. Are the many structures of animals and plants, which are obviously of use, fundamental or accidental in an evolutionary sense? The Darwinian and Lamarckian theories, which have almost totally replaced the gross teleology of former days, have usually been supposed to imply an evolutionary relation between an organ and its use. The Lamarckians have emphasized the direct response of organism to environment, and the inheritance of useful acquired characters. The Darwinians have emphasized the gradual 'working out' of highly useful structures by the influence of selection upon small fluctuating variations. The two theories are not necessarily inharmonious; the Lamarckians have inquired more as to the origin of variations, the Darwinians as to their survival. The publication of DeVries's mutation theory has occasioned a sharp change of front in many quarters. We hear more now than formerly of adaptation as a secondary thing; that it has little or no significance in an evolutionary sense. The idea that an organ is not explained when we assign it a function is not new; Geoffroy St. Hilaire made this one of the cardinal points of his evolutionary philosophy nearly a century ago, and we find the Greek philosophers debating the question in their day.

Professor Morgan's 'Evolution and Adaptation' has called the adaptation question once more to the fore. Morgan holds that the mutation theory accounts best for incipient organs, now useless, but eventually to become useful when fully developed, for organs that are wholly useless, and for 'over-adapted' organs (such as electric organs in fishes, leaf movements of *Desmodium gyrans*). Many organs that are useless or even harmful may survive because the organism may have some compensatory advantages making it as a whole well adapted. Another whose work tends to entice us from our former idols is Klebs, whose 'Willkürliche Entwicklungsänderungen' is certainly one of the great contributions of the year. Klebs is removed as far as possible from teleological ideas, and explicitly states that they have ruled so long because they are easy and restful ways of solving life's riddles. He holds that the polymorphism of a plant, like that of sulphur, is due to external agents, and that we should not ask for the purpose of the changes in one case more than in the other. The view just outlined is supported by facts from various sources; MacDougal has shown that etiolation is not, properly speaking, an adaptation to the dark; that plants are not to be looked upon as making efforts to reach the light. Etiolation is a response to certain factors, and may or may not be useful. Willis in his studies on the Podostemaceæ finds floral dorsiventrality, *i. e.*, zygomorphy, keeping pace in its development with increasing dorsiventrality in the vegetative organs. Zygomorphy here—so far from being an adaptation to insects—characterizes flowers that are in no sense entomophilous; the only entomophilous flowers of the group are the more primitive actinomorphic forms. If natural selection does not operate here, Willis asks, why may not other cases of zygomorphy be explained apart from insect visitation? Küster's 'Pathological Plant Anatomy' also helps to strengthen the chemico-physical view point of plant structures, in that he treats as alike the result of external agents, harmful struc-

tures, such as galls, and supposedly beneficial structures, such as aerenchyma of water plants, undifferentiated mesophyll of shade plants, etc. That all biologists are not going the way of Klebs and Morgan is evidenced by Francis Darwin's review of Klebs's book; Darwin holds that in the development of structures, adaptiveness must be taken into account, and that there is a difference between the organic and the inorganic. Verworn's biogen hypothesis and Driesch's neo-vitalism are expressions of a supposed difference between the living and non-living.

Nordhausen's experiments seem to support the Lamarckian theory, since he finds that the structural characters of shade leaves of the beech remain in large part in changed conditions. Thus useful characters, originally acquired through the agency of external factors, may be transmitted, at least in part, to later generations. On the other hand, the Lamarckian idea seems not to be supported by the work of Wiedersheim and Ball, who failed to confirm Hegler in the matter of securing an increased development of mechanical tissue in growing plants subjected to tension. Potonié has attempted to attack the problem from another side by a study of fossil plants; he claims that carboniferous plants were less perfectly adapted than those of to-day. This, however, is denied by Westermaier, who thinks that organisms must always have been as well adapted as they are now. Whatever the final outcome concerning this fundamental problem, whether the study of adaptation is scientific or unscientific, it is of value to recognize the presence of the problem; many have taken for granted on one side or the other what ought to be a subject for profound investigation.

Ganong in his splendid paper concerning the Bay of Fundy marshes has expressed another respect in which past study has been at fault, viz., in devoting paramount attention to structural rather than physiological characteristics of plants. We need to know not only about root hairs, leaf shapes and development of so-called protective structures; it is far more important to know a plant's physiological adaptation; its transpiration, its water-absorbing power, its physiological plasticity. From the hasty presentation here given it might be inferred that Lamarckians and Darwinians are necessarily regarded as believers in adaptiveness as a factor in evolution, and mutationists are necessarily supposed to hold the opposite view. This is, of course, incorrect, but it is certainly true that those who hold to mutation have laid the least stress upon the significance of adaptation. To the speaker it seems as if all three theories of evolution, and perhaps others yet unborn, are quite tenable, and that the problem of adaptation is not necessarily to be associated with any particular theory of evolution.

Not all will admit that experimental morphology is a part of ecology, but that its results are of the utmost importance in ecological interpretation can not be denied. The works of Klebs and Küster, to which allusion has been previously made, take a foremost place in this field, but in a summary of this character it will be impossible to specify details. Among the more interesting of recent experiments we may cite some which deal with the phenomena of symbiosis. Bernard's theory that tubers are essentially galls due to fungal attacks has been disputed by Laurent, who shows that concentrated solutions also induce tuberization. Bernard repeats and confirms the work of Laurent, and as a consequence broadens his view as follows: tuberization is induced by factors which cause a greater osmotic pressure within the cell. In nature fungi which penetrate the growing tissues form the chief means of

increasing the osmotic pressure. Bernard has also shown that beyond an early stage the germination of seeds of the orchids *Cattleya* and *Lælia* is quite dependent upon the penetration of an endophytic fungus into the minute embryo. Aseptic cultures into which the fungus is introduced at once show vigorous growth. Thus, as Bernard states, the orchid seedling is dependent upon a fungus for its development, much as an egg is dependent upon fertilization. In this connection it may be noted that Pinoy succeeds in getting Myxomycete cultures only in the presence of bacteria, while Molliard finds that the development of perithecia in *Ascobolus* is highly favored by the presence of other fungi in the culture. The mycorhiza literature has received several additions during the year, but no marked advance has been made in our knowledge. Möller thinks that root fungi have little or no significance in the nutrition of green plants. Tubeuf, on the other hand, holds to the common view. Neger shows that the reason why autotrophic plants flourish better in sterilized soils is because of a change in the soil rather than in the absence of fungi, as Stahl supposed.

Among the important papers of the year we must, of course, include MacDougal's study of the influence of light upon the life of plants; his general conclusion that light does not directly influence growth is of great import in ecology, as is the view that light favors the differentiation of tissues. Eberhardt has now given us a detailed account of his studies concerning the influence of dry and moist air upon plant tissues, but there are few general results which he failed to outline in his preliminary notice. We may note Winkler's study of the causes of leaf position, in which Schwendener's pressure theory is opposed, though most of Winkler's papers, as well as the polemics which they occasioned, antedate the year now closing. The regeneration studies of Winkler, Goebel and several others have an ecological bearing but time will not permit their consideration. Bonnier has made some interesting morphological experiments on orchid roots, as has Benecke on the thalli and rhizoids of liverworts. Benecke finds that impurities in the glassware commonly employed in laboratories are responsible for some results, and in this connection we should note the work of Singer and Richter upon the influence of laboratory air in experimental cultures. These and other considerations demand that as much work as possible should be done out of doors, or at least in well-controlled greenhouses. From an ecological point of view much experimental work that is done in the laboratory or even in the greenhouse is of no direct value. Ganong, in his marsh paper, makes an appeal for field laboratories in connection with future ecological work, and it must be admitted that his argument is sound. The tropical laboratories and the recently installed desert laboratory are steps in the right direction, but even in these cases the experimental work which is to be of the greatest ecological value, must be performed not in the laboratories, but out of doors. In this, which the speaker believes to be the most promising line of ecological research, Bonnier has led the way in his magnificent experiments upon alpine plants. During the past year he has reported upon his parallel cultures at Paris and Toulon, in which portions of the same individual plant and identical soils are employed. He finds that his Toulon cultures from Paris plants are showing characters which the same species show in nature about Toulon, a result in harmony with his earlier alpine studies.

Among contributions based more on observation than experiment are: Paul on the biology of moss rhizoids, in which he maintains that they are primarily of value

as holdfasts; Kraemer on the epidermis, hypodermis and endodermis of angiosperm roots; Grimme on the flowering period of German mosses—a detailed and instructive paper; W. E. Britton on the anatomical features of the plants of the Connecticut sand plains; Bray's anatomical studies of desert plants; Parkin and Pearson on the anatomical characters of the plants of the Ceylon Patanas. These latter authors are surprised to find that the structures are as xerophytic in the wet as in the dry prairies, although it is nearly fifteen years since Schimper showed that xerophytes may be typical of certain wet habitats.

In ecological phytogeography the closing year has witnessed a considerable display of literature in America and England. Possibly no preceding year has afforded so many contributions. In our own country, one must give a prominent place to Ganong's paper on the Bay of Fundy marshes, a paper giving the results of the author's studies during several years in one of the most interesting physiographic areas in the world. As many of us know, Professor Ganong has postponed from year to year the publication of this paper, fearing lest errors might creep in that the study of just another season would rectify. Would that many another might heed his caution, and spare the world the undigested results of a week's ecological excursion! The completeness of detail and the accuracy of statement in Ganong's paper may well serve as models to working ecologists. Probably the harshest criticisms which his paper will receive are contained in his own concluding remarks. One of his suggestions, in addition to those already noted, may be mentioned here, viz., the necessity of finding a means of estimating quantitatively the biological factor, *i. e.*, the exact influence of competition and cooperation in determining the vegetation of a plant association. Another worker, and the only one who has so well exploited his particular field in America, is Bruce Fink, so long and so favorably known for his lichen studies; his recent development of lichen associations has added materially to our knowledge. The speaker has long felt that lichens are among the most interesting of plants ecologically, because they are so closely related to the unmodified physical environment. If any plants will show whether purely chemical factors are of influence in distribution, we should expect rock lichens to be of service in this regard. Apropos of this question of physics versus chemistry, one must mention the recent bulletin of Whitney and Cameron, in which the physical factor is given the dominant place. This view has been accepted readily by most ecologists, ever since Warming, following Thurmann and others, so clearly outlined the overwhelming importance as an ecological factor of the physics of the soil in relation to water. Other important American papers are: Livingston on the vegetation of Kent County, Michigan, presenting a model detailed map which represents a type of illustration too infrequent in American ecology; Transeau on the distribution of the bog societies of North America; Harshberger on the vegetation of mountainous North Carolina. In Britain the work of the lamented Robert Smith has been continued by his brother, who in cooperation with others has given two papers dealing with the vegetation of Yorkshire.

Several papers of more than ordinary interest from the view point of physiographic ecology, apart from Ganong's paper on the Bay of Fundy marshes, are as follows: Cajander's study of the alluvial vegetation of the Lena River, containing excellent analyses of phytogeographic terms as well as discussions on the genetic succession of associations; Penzig's study of the development of vegetation on Krakatoa since Treub's visit some years since; Häyren's

paper on the development of vegetation on the coast of Finland; Düggeli's detailed study of a Swiss valley about to be occupied by a reservoir, giving a basis for a study of the changes which will ensue; Weber's exhaustive study of the development of German moors; Huber's account of the encroachment of vegetation upon new islands in the Amazon. It is a pleasure to see such a list as this, probably the largest and best furnished by any single year to the study of association dynamics or physiographic ecology. While, as indicated above, the interpretation of ecological facts must be regarded as the ultimate end of ecological endeavor, the proximate end must largely be the collection of such facts. We deceive ourselves if we believe that this task has been more than fairly begun. Among the most important facts to be collated are those bearing upon the natural changes which the vegetation of a region undergoes. One may enter a field and make a guess as to what these changes are —this guess may or may not be intelligent; one can find each variety in literature—but the sole way to know what changes occur is to make detailed studies of limited areas year by year. In connection with ecological phytogeography one should mention also the admirable Vegetationsbilder issued by Karsten and Schenck, which serve to give photographic illustrations of distant and especially tropical landscapes. The studies of Engler in German East Africa and Cockayne in New Zealand should be included among the noteworthy contributions to knowledge. And it is, perhaps, in place to recall here the long-promised English translation of Schimper's 'Plant Geography,' which has so recently appeared.

Floristic phytogeography probably should not be classed under ecology, but there are many inter-relations between ecological and floristic aspects, which make a short survey of the field necessary. One of the remarkable contributions of the year is a volume by Hugo Bretzl on the botanical results of Alexander the Great's journey to the east, as reported by Theophrastus. As the speaker pointed out a year ago, too little attention has been paid to the phytogeographic contributions of Linnæus and other former workers. Bretzl's work shows that the Greeks observed and recorded a number of things for which but scanty credit has heretofore been given. The mangrove forests are described with great detail and accuracy; even the relation of various species to saltness is dwelt upon, and correctly. The Greeks were surprised to find conifers on the Himalayas and concluded that the vegetation of tropical mountains resembles that of European lowlands. Theophrastus gives the physiognomy of vegetation in terms of leaf forms; for doing this same thing only a century ago, most writers have given Humboldt the credit of founding phytogeography. Theophrastus anticipated many modern views in morphology and physiology, which of course have no place in this review. Beguinot has shown also that Porta, in his 'Phytognomonica,' published some centuries since, had a knowledge of many principles of distribution. One of the great floristic contributions of recent date is Jerosch's history and origin of the Swiss alpine flora, a volume which makes no pretensions of being more than a compilation, but which places in compact and trustworthy form the results of many workers. Other important floristic works are those of Alboff on Fuegia, R. L. Praeger on Ireland and Parrish on southern California. Among paleontological works bearing on distribution, perhaps the foremost place should be given to Flahault's volume on paleobotany in relation to present vegetation, a work of over two hundred pages and by a master hand. One must at least call by name Seward's presidential address before the

botanical section of the British Association on the geographic distribution of past floras, Wieland's novel but not new view as to the polar origin of life, and Schulz's papers on the geological development of the flora of the Saale and the Suabian Alps.

In closing, a word may be said as to the present status of Briquet's polytopic theory, a theory commonly discarded as untenable, but which the mutation theory and the growing belief in polyphylesis make more probable. The idea that a species may originate in more than one place, simultaneously or not, did not originate with Briquet, but he resuscitated it and christened it the polytopic theory. Though discarded by Jerosch and most writers, as unlikely if not unthinkable, Willis believes that the same step might be taken by species that are far apart, especially in similar conditions; indeed he thinks that this has actually happened within the Podostemaceæ. Arber has favored the idea of homeomorphy or parallelism of descent. Engler has admitted that varieties may originate more than once. It will be recalled that in DeVries's experiments the same species recurred many times, and that too from different parents. Blackman has found that about twenty per cent. of the arctic and antarctic algæ are identical as to species, but not found elsewhere. It will be conceded that in such a case the difficulties in the way of migration during the present or past ages are very great, while the polytopic theory seems to afford an easy explanation. Perhaps it is too easy; in any event it seems adapted for use as a last resort rather than as a general panacea. However, the researches of the past few years have placed the theory of polytopic origins in a position to demand the thoughtful consideration of all students of evolution.

HENRY CHANDLER COWLES.

SCIENTIFIC BOOKS.

Desert Botanical Laboratory of the Carnegie Institution. By FREDERICK V. COVILLE and DANIEL TREMBLY MACDOUGAL. Published by the Carnegie Institution. Washington, November, 1903. Pp. 58, with 29 plates and 4 charts.

This attractive account of a botanical reconnoissance of the desert areas of the southwest will, without doubt, awaken great interest in desert vegetation, and stimulate the thorough investigation of the adaptations of xerophytes. The debt which ecology owes to Drs. Coville and MacDougal for fostering the idea of a desert laboratory, and for carrying it to a successful conclusion must become more and more apparent as the work progresses. The report deals in a very interesting though necessarily general fashion with the vegetation of the areas visited in connection with the location of the laboratory. These were: (1) The arid region of western Texas; (2) the sand dunes of Chihuahua; (3) the White Sands of the Tularosa Desert; (4) the vicinity of Tucson; (5) the gulf region about Torres and Guaymas; (6) the Colorado Desert; (7) the Mohave Desert; (8) the Grand Canyon of the Colorado.

In many ways the most interesting region to the ecologist is that of the White Sands of the Tularosa Desert. These are for the most part mobile dunes, composed entirely of gypsum; they cover nearly four hundred square miles. The soil is necessarily alkaline, a fact clearly indicated also by the abundance of *Atriplex* and *Suæda*. The characteristic vegetation of the dunes consists of woody plants, chief of which are *Rhus trilobata, Atriplex canescens, Chrysothamnus* and *Yucca radiosa*. *Yucca*, by virtue of its striking ability to push up through a sand cover, is a typical dune former. The White Sands when critically investigated should add an interesting chapter to the developmental history of dunes. The selection of Tucson for a laboratory site was based upon the variety and distinctness of its desert flora, as well as upon its being both habitable and accessible. The vegetation in the neighborhood of Tucson consists mostly