Yendo (1914) "On the Cultivation of Seaweeds with Special Accounts of their Ecology." In each of these papers attention is called to ecological factors modifying or illustrating the workings of general factors of distribution as well as those concerned in special topographical distribution.

To sum up the general results and to attempt to determine the general subdivisions of the coast lines to satisfy all requirements of geographical distribution, the following seems to be a reasonable, although tentative, arrangement, both as to climatic and as to topographical divisions.

CLIMATIC

I. *Zones,* regulated by temperature of the warmer months, especially to be determined by the mean summer temperatures or in practise by the isotheral lines at intervals of 5° C.;

II. *Regions,* purely geographic segregations under zones;

III. *Provinces,* subdivisions of regions according to mean winter temperatures, in practise by isocrymes, 5° apart or less;

IV. *Districts,* subdivisions under provinces according to geographical remoteness and varying physical conditions of a general nature;

TOPOGRAPHICAL

V. *Formations,* aggregations of algæ of same general form, depending particularly upon substratum;

VI. *Associations,* aggregations of algæ depending for general likeness of plant form, etc., on depth (belts), salinity, light, aeration, etc., generally characterized by the predominance of a single, or at most, of a few species.

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THE CARNEGIE INSTITUTION OF WASHINGTON AND SCIENTIFIC RESEARCH[1]

Numerous references have been made in preceding reports to the growing realization of the world at large that the methods of science are the most effective methods thus far developed for the advancement of learning and for the mitigation of the consequences of the inexorable "laws of nature" which condition existence on our planet. Reference has been made likewise to the contemporary rise and progress of other research establishments and to the introduction of investigation as an economic adjunct to industrial enterprises. These manifestations of popular approval and confidence continue to be among the most noteworthy signs of the times. Indeed, it is plain that we are now witnessing a remarkably rapid evolution of public understanding of the meaning and the value of research. This has been greatly intensified and accelerated by the European war, whose sinister aspects appear to be relieved in some degree by the prospects of an awakened realization of the availability of better methods than those of warfare for settling international disputes, of better methods than those now commonly applied in the government of states, and of better methods in education, in sanitation, in industry, and in biological economy generally. The European war has emphasized to a degree not hitherto attained in the world's history the perils of ignorance, of government by assumed divine right, and of that sort of diplomacy which shades off by insensible degrees into duplicity; and it has emphasized equally clearly the necessity for rational investigation of and progressive reforms in all national affairs.

How the details of this evolution, in which the institution must participate, will

[1] From the report of the president for 1916.

work themselves out is impossible to predict except in general terms. It may be safely inferred, however, from the history of similar developments, that this one will proceed much more slowly and with much more difficulty than many enthusiastic optimists anticipate. Evolution is, in general, a secular process and goes on with a leisurely disregard of individuals. It may be safely inferred also that many of the numerous fallacies which have beset the institution during the brief interval of its existence will recur again and again in the rise of similar organizations, while fallacies of a more troublesome type are likely to beset the introduction of the methods and the results of research in governmental affairs. It is in the latter affairs that the most stubborn opposition to progress is usually met, since there exist, as a rule, in such affairs no adequately developed relations of reciprocity between those best qualified to suggest and to formulate improvements and those who control the machinery for their applications. Such improvements can be secured only by overcoming a stolid adherence to precedent as well as the reluctance of rational conservatism. Thus it happens in governmental affairs that the most incongruous ideas often coexist, as is well shown by the contemporary adoption of the most advanced principles of sanitation in certain European countries which are still dominated by medieval theories of the functions of a state. To cite another illustration readily understood and verifiable, it is an anomalous fact that the United States government exacts no professional requirements for the direction of its highly technical affairs except in a single branch of its service, namely, the legal. And in line with this glaring national deficiency it is notorious that the fiat of an executive can make an astronomer, a geodesist, or a biologist out of a man whose works are unknown in the annals of the science of which he becomes the ex-officio representative. We hear much also in these days of the "mobilization of genius" in the interests of national preparedness for commercial and industrial competition, if not for the more serious exigencies of national defense; but it is to be feared that this mobilization means fruitless attempts to utilize aberrant types of mind, or perhaps the employment of men of talent under the direction of those whose competency for leadership is admitted, if at all, only in quite other fields of activity than those here considered. In the meantime, it is plain enough, in the light of current events, that any nation whose governors mistake necromancy for science, confound invention with investigation, or fail to utilize effectively available and advancing knowledge, is in danger of humiliation in peaceful international competition if not in danger of extinction in international conflict.

Much, perhaps too much, has been said in preceding reports concerning the maxims and the principles which should be observed on the administrative side in the conduct of research. To a great extent these maxims and principles are the same as those developed in the common experience of the race; but to a greater extent they are derived from the more concrete and the more sharply defined experience developed in the evolution of the older sciences. All experience teaches that effective research depends on painstaking labor, arduously, patiently and persistently applied; while all science teaches that research is effective only in those regions wherein something like demonstration can be attained. If investigations can not be well done they are of little worth; if nothing can be proved they are of still less worth, or at best only of negative value. But obvious as these truisms are when stated by themselves, they have been con-

tradicted daily in the plexus of events which make up what our successors will call the history, recorded and unrecorded, of the institution. Thus it has been suggested not infrequently that promising researches be suspended in order that equally or less promising researches might be taken up; and it has happened that proposals to abolish departments of research have been seriously advanced before these departments have had time to prove their rights to existence. It is not infrequently suggested, likewise, by otherwise irreproachable correspondents, that the experts of the laboratories and observatories of the institution be set at work under the direction of amateurs, or, in some cases, of those even who have not reached that earliest stage of capacity in science.

It goes without saying that all such untoward influences should have little effect on the rise and progress of a research establishment; but he would be an incompetent administrator who failed to recognize the existence and the dangers of these influences. Most men are still opportunists; many contemn principles and theories of procedure; while the characteristic defect of deliberative bodies, strikingly illustrated by legislative assemblies, is lack of deliberation. Moreover, what any organization, altruistic or otherwise, may accomplish at any epoch, or during any period, will depend very largely on the status of contemporary public opinion. No organization may be rationally expected to rise much above the level of the ideals of those who support and direct it. The law of averages and the "law of conservation of ignorance" apply in the business of research no less rigorously than in other affairs of human endeavor. The only difference is that in research, from the nature of the case, we are held to stricter accountability; it is incumbent on us to be alive to the ideals and the theories which lead to regress as well as alive to the ideals and the theories which lead to progress.

Although popular opinion continues to look upon the institution as an establishment of unlimited means, and hence of unlimited capacities, it is an easily ascertained fact that such advances as have been attained are due chiefly to concentration of effort in a few fields of investigation, the number of these being necessarily limited by the finiteness of income. Of the agencies which have contributed most to these advances the departments of research must be given first rank when quality and quantity of results accomplished are taken into account. These departments have supplied also a much needed verification of the axiom hitherto admitted in all domains of activity except those of research, namely, that if any good work is required the best way to get it done is to commit it to competent men not otherwise preoccupied. They have verified, likewise, the equally obvious truth that large and difficult undertakings demand foresight and oversight, prolonged effort, and a corresponding continuity of support. The idea that discoveries and advances are of meteoric origin and that they are due chiefly to abnormal minds has been rudely shattered by the remorseless experience of the institution.

Along with these considerations special mention should be made of another of vital importance to the departments of research. This is their complete autonomy within the limits of their annual appropriations. Allusion is made to this matter here partly for the purpose of correcting public misapprehension concerning the relations of these departments to the institution as a whole, and partly for the purpose of stating formally the theory of administration followed by the institution during the past twelve years. Such a degree of freedom accorded to the departments of research is

not only necessary by reason of the extent and the complexity of the affairs of the institution, but it should be regarded as a fundamental principle of sound administration. No one can follow the details of all these varied affairs. A division of labors is indispensable, and to the greatest extent practicable the director of a department of research should be encouraged to be the autocrat of his departmental destiny. But in so far as departments are granted liberty of action it is an equally fundamental principle of administration that they should assume corresponding responsibilities. Autonomous freedom and reciprocal accountability are then, in brief, the essentials of the theory under which the departments of research have evolved.

In consonance with the theory just indicated and in conformity with the precedent set a year ago, no attempt is made here to furnish abstracts of the current departmental reports. They give sufficiently condensed summaries of departmental activities and departmental progress. They are, as a rule, highly technical papers and difficult of adequate appreciation even by those somewhat familiar with the subjects considered. But this is not only just as it should be, but it is inevitable if the investigations under way are worth making. Our confidence in them must be founded in large degree on the general principles revealed in the advancement of science. Great and admirable achievements were attained by the ancients prior to the epoch of recorded history; still greater achievements were attained by the Greeks, the Arabs, and the moderns down to the epoch of Galileo and Newton; while competent judges have estimated that greater progress was secured in the nineteenth century than during all previous history. It is quite within conservative reason, therefore, to assume that if we continue to follow those principles, now grounded in more than twenty centuries of repeatedly verified experience, in the light of accumulated and recorded knowledge, we may confidently expect to achieve corresponding further advances.

The question is sometimes raised as to how the efficiencies of investigators and of departments of research are, or possibly may be, estimated. Occasionally, also, there seems to be entertained along with this question the hypothesis that research is a commodity and that money is the chief agent in promoting its effective increase. But the currently common meaning of efficiency implied in this question and in this hypothesis is too narrow for application here. It applies rather to machines and to aggregates of men working like machinery for predetermined economic ends. In a broader sense, however, the question of efficiency of men and of organizations is worthy of considerate attention. It is, indeed, in this inclusive sense, a question of the greatest importance, especially in all cooperative enterprises of communities and states. But without going into these larger aspects of the matter, it may be said that the efficiencies of the investigators and of the departments of research of the institution are determined in the same way that justification for the institution, as a whole, is determined, namely, by the consensus of competent opinion. In science, the work of an individual is measured on its merits and the work of an organization is weighed in the same manner. Adequate tests and standards for what is not fully known may not be wisely set up in acts of administration. Severer tests and higher standards are supplied automatically and relentlessly by contemporary criticism and by the verdicts of posterity. Hence, given a corps of trained investigators, or an organization of several such, the question of efficiency is happily one which is decided for us mainly

by those who are alone qualified to render adequate judgment.

Like all other branches of the institution, the division of research associates has undergone a distinct evolution. Originally a division which gave rise to excessive and often unrealizable expectations, it has gradually become shorn of its extrinsic appendages and divested of its inheritances from occultism. In spite of these omnipresent obstacles to progress and to efficiency, this division has been highly productive from the beginning and continues to be one of the most important agencies of the institution for the promotion of learning. The main reason for the noteworthy success of this agency is very simple. It was stated in a recommendation concerning research associateships, in the report of the president for the year 1906, in these words:

> The limitation of eligibility for such positions to investigators of proved capacity for and of proved opportunity for research.

In the meantime, the number of those possessing such qualifications has increased much more rapidly than the resources of the institution (or than the resources of all research agencies combined) have increased to meet this and other growing financial needs. Not only has income failed to keep pace with worthy demands, but, as repeatedly pointed out hitherto, the purchasing capacity of income has steadily declined since the foundation of the institution. Thus it happens that now, just as the merits of the system of research associates have come to be generally recognized, it is essential to suspend extension of this system, and it may become essential to curtail to some extent the amounts of the grants hitherto made to those who have helped most to develop this remarkably effective division of the institution's activities.

It should be evident from the preceding paragraphs of this section of the report, as well as from numerous passages in previous reports, that the income of the institution is not only not equal to popular estimates, but that it is not equal even to the legitimate demands on it for research. This proposition is easily verified, although few people believe it and fewer still are willing to undertake the small arithmetical labor essential for its demonstration. On the other hand, it is admitted by everybody that the institution is not doing as much as it could, but the simple reasons for this obvious fact appear to be far from equally obvious. Whether it would be desirable, if practicable, to double, say, the endowment, and hence the income, of the institution is a question well worthy of consideration. But along with many reasons why it would be so desirable there might be adduced also many other reasons why it would not. This is, indeed, a fundamental question whose deliberate consideration should precede the next step. We possess as yet no well-defined and generally accepted theory of a research organization. The institution, plainly enough, stands somewhat in isolation. It would prosper better, probably, and be better understood, certainly, if it had more contemporaries with which to divide not only the vast fields of opportunity, but also the vast aggregate of fruitless labors imposed on those who should be preoccupied with the business of research. In the meantime, while no expansion is permissible under existing income, the current activities of the institution may continue without serious modification of plans or impairment of efficiency.

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SCIENTIFIC EVENTS

WIRELESS TELEGRAPH INSTALLATION AT THE UNIVERSITY OF CHICAGO

A notable addition to the equipment of the Ryerson Physical Laboratory at the Univer-