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ENVIRONMENTS[1]

By Professor BURTON E. LIVINGSTON
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THE honor of being here this evening as retiring president of the most dignified of our American biological societies is greatly appreciated, and this occasion is for me a very pleasant one indeed. I am specially glad to be allowed to represent the relatively youthful science of plant physiology on the occasion of this semi-centennial celebration of the American Society of Naturalists. With Dr. Conklin's inspiring story of the first half-century of our society fresh in our minds, it is not unnatural for us naturalists to ponder over phases of the probable future of biological science, as its future may be tentatively foreseen from recent thought trends in this field, and it is to some phases of the newer trends that I wish to ask your attention.

For the last decade or two one notable growth change in biological view-point has been a rapid increase of interest in processes and products of living things, as these may be studied and compared in an increasingly quantitative way. Observations become progressively more comparative, and more quantitatively so. Superficial description must, of course, precede everything else, but comparison calls for increased precision of factual knowledge and consequently observation has become, and is becoming, continually more precise and more laborious. This kind of progress is seen partly in the introduction of new characteristics or dimensions, according to which things may be more satisfactorily compared, and partly through improved methods of observation and mensuration. Our language is expanding, with added words and phrases calculated to facilitate precise notation, but such additions hardly keep pace with the recent rapid advance of thought, and some new concepts are widely accepted without adequate ter-

[1] Presidential address presented before the American Society of Naturalists, at its Boston meeting, December 30, 1933.

minology, being represented in some instances by nothing more suggestive than letters of the alphabet. As just one familiar example, consider hydrogen-ion concentration (or hydroxyl-ion concentration) of body fluids and environmental media. This surely important characteristic of solutions was undreamed of in the biology of thirty years ago, but measurements of it have already been recorded for almost every sort of aqueous solution with which biologists deal. Improved methods, for its more precise measurement, are continually being brought forth. Other ion concentrations are beginning to receive attention. Oxidation potential promises soon to play an important rôle in many biological discussions.

Recent progress has involved, among other things, increasing emphasis on the physiological aspect of biology, which deals primarily with processes, changes or transformations that occur in organisms. The etymology of the word "physiology," like that of the word "nature," suggests growth and change. We tend more and more to study and compare developmental and metabolic processes and to institute comparisons and discover relations among all these and between them and the concurrent environmental processes.

The primary measurable characteristic of any specified process is obviously its time rate, and comparisons between different occurrences of the same process are increasingly made in terms of their rates. For example, the simple proposition that green leaves generally give off oxygen and absorb carbon dioxide by day, but generally give off carbon dioxide and absorb oxygen by night, was something worthy of discussion in the earlier days of the last century, but that proposition now arouses no opposition and present interest in it centers about the different and fluctuating rates at which these absorptions and eliminations occur. Recent studies deal with acceleration and retardation of these rates, with the various influences that accelerate or retard them and with accounts of how they fluctuate differently for different forms of green plants or for different developmental stages of the same form. We are even beginning to compare rates of acceleration or deceleration; that is, rates of change of rates of change. The time factor thus becomes increasingly important in experimental and observational studies.

To secure useful descriptions of cells, tissues, organs or organisms, it is usually necessary to give quantitative attention to several different kinds of characteristics or dimensions, according to the sort of comparisons we propose to make. Size, shape, weight and color are perhaps the simplest of these. Anatomical, histological and cytological characters may come next, or our proposed study may lead us to the quantitative estimation of various chemical contents, such as dry weight, nitrogen content, and so on. Finally, rates of metabolic change (such as carbon-dioxide production, for instance) may be the things we wish to compare. If no other means were available one might, I suppose, distinguish between active men and active mushrooms in terms of their respective respiration rates per unit of body weight, body volume, etc., under some suitable standard set of environmental conditions.

It is generally necessary to employ several different sorts of dimensions or characteristics at once. Thus, two men may be compared with respect to body weight, stature, skull shape, complexion, eye color, basic metabolism and the results of intelligence tests; or two apples may be compared with respect to size, shape, color, toughness of skin, water content, sugar content, flavor, keeping qualities, respiration rate, and so on.

Of course you are familiar with all these things and with an exceedingly great variety of related things not mentioned here, but it may not be amiss to point out that, although comparative evaluations based on one sort of criteria may be markedly different from those derived from the study of another sort, yet both sets of conclusions may be quite significant and reliable. It is obviously essential to specify just what criteria are employed when we compare different organisms or when we compare an organism at one time with the same one at another time. To illustrate, one plant may be enlarging more rapidly than another, but the latter may be concurrently producing starch or resin more rapidly than the former. In general, we may note that many apparent discrepancies among published scientific observations seem to be at least partially related to the employment of more or less different criteria for the evaluation of the same thing by different students. The criteria used, whether they are static or dynamic, and the procedures by which their magnitudes are estimated for comparative studies require more specific description than is generally given in our contributions. You will probably all concur with me in this last remark, unless perchance you may be the editor of a scientific journal in these times of increased writing and decreased financial income from subscriptions, when brevity of statement seems to be more desirable than clearness and logical completeness.

Thus far we have been considering organisms and their parts, which may be embraced by the term "internal conditions"; that is, everything that pertains to the organism as distinct from its surroundings. These are the taxonomic, evolutional, morphological, ecological, physiological and sociological characters by means of which we judge and compare our organisms and their various performance or behavior patterns. They may represent simple static observations of *being,* with reference to specified developmental stages—as when we say that the leaves of some plant form are opposite, lanceolate, crenate, stipulate; that horses have four legs; or that yeast cells are ellipsoid. On the other hand, they may represent dynamic characteristics, involving rates of change and their fluctuations—as when we say that some plant is biennial under a specified range of climatic and soil conditions, that a wild duck is fleet of wing when in good health or that an experimental organism was observed to produce carbon dioxide at a specified rate during a specified period of its individual life.

With those considerations in mind, I wish now to turn to my announced topic, "environments." We may remark at once that the spatial division of the universe into organism and environment is arbitrary in many instances, although such division is quite necessary. For example: Is an object held tightly in my clenched hand internal or external to my body; what of the contents of the alimentary canal; what of the fluid lining of the lung cavity? Are the gases of the intercellular spaces in an ordinary leaf to be regarded as internal or external; what of the liquid commonly found in the cavity of a leaf of the Sarracenia pitcher plant? The answer to such questions naturally depends on the nature of our interest, on the problems in hand, but it is always true that many features of an organism are continuous with corresponding features of the environment; there is always interchange of material and energy between these two portions of a more complete system, and organism influences environment while environment influences organism. Consequently, we can not expect to proceed far toward an appreciative understanding of the ordered vital weft of internal features without giving equal attention to numerous features of the just as thoroughly ordered but different environmental weft. In all our discussions the environment is to be consciously considered or at least tacitly recognized; in all our experiments the environment lurks behind the scenes, as it were, ever ready to baffle our efforts and always calling for very watchful alertness on our part. Whether we are studying protoplasm or cells or tissues or organs, whether we are interested in individual organisms as such or in social groups like plant and animal societies, this generalization holds.

Unrecognized or unmeasured environmental influences may play rôles of which we have not yet even dreamed. The same is of course equally true of internal influences that are not adequately recognized and evaluated, but I think these are apt to receive much more attention than is usually given to the possible influences of environmental unknowns. Such apparently surreptitious influences, whether internal or external, may be to blame for many or most of our uncertainties, they seem to inhabit the realm of chance or fortuity, sometimes giving rise to mysticism, wishful thinking and various pseudo-scientific ideas and statements. You can recall many instances where study of a newly recognized environmental feature has recently cleared up, in large degree, an outstanding problem. Think, for example, of osmotic pressure, hydrogen-ion concentration, colloidal relations of sol and gel, the micro-organisms of the soil, the boron and copper concentrations of natural waters. The bringing forward of hitherto unappreciated influences greatly increases the complexity of a problem, but the complexity of a problem must surely be appreciated before satisfactory scrutiny can be applied. We are well used to investigations that first render our discussions more difficult.

Not that we should study organisms less but that we should study environments more, that is my present theme. We know enough of vital processes to realize that they are exceedingly complex, but that complexity is everywhere related to another sort of complexity that is manifest in the outer world. The two are to be studied together.

As ecologists of all sorts easily realize, the environment of an organism embraces the whole universe excepting the organism itself. Our present problems would be greatly simplified, however, if we might proceed outward from the organism only a very short distance, studying immediate or proximate environments not primarily for themselves and with regard to their origins and evolutions, but for their influence on living things. We need especially to try to envisage the structural and performance patterns of a very thin environmental shell, which lies externally against the organism's periphery. This is sometimes called the micro-environment. But we find it difficult, or at present impossible, to confine our attention to that limited region, for we have generally not yet learned how to investigate the microcosmic conditions that prevail there. Therefore, we are constrained to direct our explorations somewhat farther afield, into regions where we are now able to carry on. So we usually study phenomena that do not influence our organisms directly but do influence their immediate surroundings, which in turn influence the organisms. We need to avoid going too far afield, however; a biologist studying environments may do well to keep always, or at least intermittently, in close touch with his biological problems. Soil science and climatology, for example, are largely devoted to discussions that do not impinge directly upon ecology, horticulture, agronomy, but some portions of these sciences of soils and climate do lie clearly within the biological purview and are of very great importance there.

Limitations of my own ability and of my time preclude any attempt to give special attention here to the consideration of organs and tissues, whose immediate environment is other cells and tissues of the same organism, but I think the same principles would be found to apply in the study of the internal microcosm as in that of the external one. I am aware of a great realm of physiological and cytological problems away from which I must turn my face for the present. I am confining myself largely, as you see, to studies of the organism as a whole and of its performance patterns as these are related to the surroundings.

How do we attack the study of environments and how may our campaigns of attack be improved? Of course, we first try to describe our environments, employing, as far as we can, characteristics that promise to show kinds and degrees of environmental influence. In many instances we seem to select for our description environmental characteristics that others have employed before us, or we employ those that appear most easily described. In some instances the invention of a new instrument or method for measuring some environmental condition leads to a vogue or fashion. In taxonomy and field ecology we are constrained to consider only environmental conditions that may be pictured with the least amount of trouble and with the simplest kinds of instrumentation. In partially controlled experiments we use more precise and correspondingly more troublesome methods for environmental evaluation. Such descriptions should of course be adequate for the quantitative comparison of one environmental complex with another and for the comparison of environment with organism.

It is usually convenient to consider the environment of an ordinary plant (with roots in soil and stems and leaves in air) as divided into two portions or regions, the subaerial and the subterranean. You will recognize the two partial environmental systems as *climatic* and *edaphic,* respectively, if you are familiar with recent ecological writing.

Commonly considered characteristics of the subaerial environment are: The chemical make-up of the air about our organisms (its content of oxygen, carbon dioxide, water vapor; sometimes of other gases); barometric pressure; air temperature; air movement; radiation (largely from sun and sky); evaporativity. Add to these: Precipitation (rain, dew, frost, snow, sleet, rime); the mechanical action of flood water, landslides and avalanches; dust fall; the incidence of organisms other than the ones specifically dealt with (air-borne seeds and spores, birds, insects and other animals and their products, including man with his tools). You thus have a representative though incomplete and altogether too general outline of the main things to be considered as comprising the subaerial environment. A similar superficial outline of subterranean conditions would include many of the properties of the soil solution and the wetness or dryness of the soil, the soil gases, barometric and hydrostatic pressure within the soil, movement of material and of organisms in the soil and soil temperature.

Like the vital complex, the environmental complex has many different dimensions, and our task is to learn just how the organism fits into its surroundings. We may be sure that the two complexes do fit very perfectly together; our main question is, what is the nature of the fit? This problem of physiological ecology would be difficult enough if the environmental features to be studied were always of the same nature and intensity, but they, as well as internal features, fluctuate continually; hence we are driven to study rates of change in the environment as well as in the organism and correspondingly our difficulties are enormously augmented.

It is sometimes advantageous to regard the environment as a dynamic system, which operates to supply material and energy to the organism, or to withhold them, and to remove material and energy from the organism. If environmental performance is not suited to the organism, the latter soon shows ill effects, as when the environment fails to supply water or suitable radiant energy at an adequate rate, or permits an excessive rate of water loss from the organism, or fails to remove carbon dioxide as rapidly as it is produced within the organism. The dynamic capacities of an environment to perform such requisite functions of supply and removal are in reality the very environmental characteristics whose quantitative estimate or measurement promises to be most valuable in our study of the interlocking controls by means of which the surroundings influence living things for good or ill. I have dwelt elsewhere on the concept of environments as operating systems and I need not attempt here the elucidation of that concept, but I may mention a few illustrations of dynamic environmental characteristics that have recently become measurable through newly developed methods of instrumental approach. (a) Evaporativity represents the power of the environment to remove water from an organism through evaporation, or to permit aqueous evaporation from plant and animal bodies by maintaining a low magnitude of external resistance to that process. (b) The dynamic capacity of the surroundings to supply radiant energy is a component of evaporativity, but of course it influences organisms in other ways as well. (c) Oxygen-supplying power of the air about an organism is sometimes a limiting condition for health and development. (d) Similarly, the carbon-dioxide-supplying power of the adjacent air is of prime importance when green plants are being considered. (e) Finally, measurement of the water-supplying power, of the oxygen-supplying power and of the carbon-dioxide-supplying power of the soil furnishes indices that seem really to represent the fluctuating soil-moisture conditions and soil-air conditions as these are related to plants. The supplying power of an environment may, of course, be *negative* in some instances, as with regard to the removal of oxygen, carbon dioxide or water from the plant. Of course, a supplying power may be either too great or too small to suit the requirements of an organism, as has been indicated.

All the influential environmental characteristics, whether dynamic or static, not only differ from place to place in nature but generally fluctuate with time, and their respective magnitudes for any plant habitat require integration for specified time periods—such as weeks, months or seasons—before they may be used satisfactorily to enlarge our knowledge of concomitance between plant performance and environmental performance, in the study of environmental control of plant activity. Temperature summations for the growing season furnish a more or less familiar (but not very satisfactory) example of the integration of a fluctuating environmental characteristic. Daily, weekly or monthly totals of water loss from atmometers are automatic integrations of the fluctuating intensity of evaporativity.

When an environmental feature, such as temperature, for example, changes during a specified time period, it is not sufficient to know its mean or integrated value for that period, but we need to take account of the extreme values and the direction of change, whether upward or downward. To illustrate this point we may employ a very simple example, as follows: Consider three plants that are initially alike, suppose them to be exposed to favorable environmental complexes that are alike excepting for air temperature, and suppose that in an observation period the air temperature of one environment decreases uniformly from 30° to 10°, that in another environment this feature increases uniformly from 10° to 30° and that in the third environment air temperature is maintained at 20° throughout the period. Although the mean air temperature representing the observation period is obviously 20° in all three instances, yet the three plants would almost surely behave quite differently. The fundamental temperature relationship thus illustrated has thus far received almost no attention at all, but it is worthy of serious study in connection with the broader problem of environmental appraisal.

As long as we deal with natural conditions—as in most ecological, agricultural and silvicultural studies, and always to some extent when artificially controlled cultures are employed in adequately planned experimentation—an appraisal of environmental components and of environmental complexes must necessarily be based on suitable measurements and integrations. We attempt to depict the pattern of environmental performance in ways calculated to show how the surroundings act to control our organisms. Although available methods and procedures for such environmental appraisal are still crude and inadequate, yet I think enough has recently been accomplished in this connection to show the general direction in which future advance is to be made. We have already learned enough from more or less unsatisfactory efforts towards the evaluation of environmental influences to realize clearly the fundamental necessity for greatly improved methods of environmental instrumentation and for more suitable procedures in the logical analysis of our problems. For example, *depth* of rainfall is of little value in the appraisal of a climatic complex, and we shall have to turn to *duration* of rainfall (which is not yet seriously measured anywhere, I think) and to water-supplying power of the soil before we shall be able to go far in the study of the relations between plant health and precipitation. Again, we shall need to break our easy-going habit of considering air-moisture conditions in terms of the index of *relative humidity,* learning to employ in this connection such newer concepts as *water-saturation deficit* and *evaporativity.* Water content of the soil per unit of dry-soil weight is to be first replaced, in our ratiocination, by water content per unit of natural soil volume and the latter is to give way, in general, to water-supplying power. Similarly, oxygen content and carbon-dioxide content of the soil are to be replaced by oxygen-supplying power and carbon-dioxide-supplying power. Other similar needs will occur to you.

My fundamental proposition is simply that we need to study environments in terms of such environmental characteristics as promise to bring out the terms or components of environmental influence on organisms. The environmental features that we measure and integrate should be, as far as possible, just those features through which the organism is actually influenced. A homely example by analogy may illustrate this proposition: If one is about to erect a new garage for housing one's motor car one does not ordinarily consider the weight or the color or the horse-power of the car, but its length, breadth and height; nor does one primarily consider either floor area or room volume with respect to the storage space required. The new structure would naturally be designed according to dimension features that are immediately applicable to the problem in hand.

In studies dealing with experiments in which some or many of the environmental components are artificially controlled, our problem is correspondingly less difficult, since artificially controlled conditions need not require measurement and in such experiments environmental fluctuation may be limited to certain previously planned patterns of relatively simple nature. Difficult as adequate artificial controls are, it is very much easier to set up and maintain artificial environments that perform in requisite ways than it is to find out by observation just how a natural environment performs. The growing of plants and animals under artificially controlled (and therefore fairly well understood) environments has recently made rapid headway, with the development of increasingly precise techniques for the maintenance of artificial conditions and for bringing a number of closely similar organisms under the influence of the same or different artificial environments at the same time.

Even in the most thoroughly planned experimentation, however, it is usually impossible to apply satisfactory artificial control to all influential features of the environment; comparative experiments generally involve some variables that are fairly well known, others that are known to some degree and still others that may be dealt with only in superficial ways. We naturally aim to bring as many as possible of our environmental variables into my first class.

An experiment carried out with suitable artificial control of the influential environmental features may usually be repeated almost at the will of the experimenter, as is generally true for the much simpler experiments of physical and chemical science. On the other hand, studies of natural environments are almost always impossible of satisfactory repetition, for nature repeats itself only approximately and one may have to wait a year or more for even the approximate recurrence of a set of natural conditions under which a first experiment was carried out.

The simplest problems of environmental relationships are naturally those that involve the fewest kinds of conditions. For this reason we are led, in our search for general or fundamental principles of environmental relations, to the experimental study of simple organisms (such as pure strains of bacteria, molds, seedlings, annuals—to illustrate by means of plant forms) in artificial environments that involve just as few influential features as possible. We sometimes encounter the mistaken notion that it is well-nigh a waste of time and effort to study the relations between simple life forms and artificial environments. I remember once recounting to an eminent chemist how we go to such great pains in conducting solution-culture experiments with ordinary plants. He pointed out that our experimental conditions are highly unnatural and that results obtained from them are not to be interpreted in terms of natural growth and development. It is of course true that the phenomena of highly complex natural systems can not be understood immediately or directly from the results of experimentation with much simpler, artificially controlled systems, but the surprising thing to me was that my eminent friend appeared to imply that our experiments were consequently hardly worth while at all. I still think my retort was passably good; I asked him how far he supposed theoretical chemistry might have progressed if chemists had always insisted on working only under natural conditions—without distilled water, without artificially purified compounds, without controlled temperature? We do hope that the basic principles derived from artificially and (in my present sense) adequately controlled experimentation may be helpful eventually in the study of natural conditions and in the field of applied science; but that hope must generally be a distant one and expectation must fall far short of our ideals. Experimentation directed specifically toward the improvement of biological applications usually needs to be planned and conducted *ad hoc,* with background complexes and experimental variables that in great measure resemble those that usually prevail in the applications considered. Such environments are, as a rule, much more complex than the relatively simple ones of adequately planned experimentation for the advanced theoretical sciences, and they are correspondingly less suitable for the elucidation of fundamental relations. Theoretical considerations usually demand a different sort of approach, after some pioneering has been accomplished—an approach that involves much more arduous planning and much more rigorous technique than are demanded by most practical tests. The foundation of science, as such, lies in this, I suppose, that relatively deep understanding and broad appreciation are pleasant and desirable things in themselves. We surely do not need to point to possible or conceivable practical applications to justify the finer and more elaborate kind of biological study, which is carried on primarily for the elucidation of basic principles.

We are coming increasingly to realize that the relations of an organism to any single environmental influence are themselves conditioned by the whole complex of other concomitant influences. For example, in recent experimentation on young wheat seedlings grown in darkness, it was found that the presence of a specified and maintained concentration of ethylene in an otherwise specified, artificially maintained and generally favorable environmental complex exerted a notable influence on the rate of carbon-dioxide elimination, but this was true only when the maintained environmental oxygen pressure lay within restricted ranges of magnitude. With some ranges of oxygen pressure the presence of ethylene exerted no significant influence on carbon-dioxide output. But with some other ranges of oxygen pressure the ethylene effect was to *retard* that process, and with still other ranges of oxygen pressure the ethylene effect was an *acceleration*. The influence of this ethylene treatment on carbon-dioxide production was, therefore, either positive, zero or negative according to current oxygen pressure. Furthermore, both ethylene acceleration and ethylene retardation of carbon-dioxide production varied in magnitude in specific ways according to current oxygen pressure. With such observations in mind, we realize that oxygen pressure, as well as temperature, salt nutrition, water supply, and so on, must all be quantitatively specified before the nature and magnitude of the influence exerted by this particular ethylene treatment could be seriously studied. Numerous other examples of multiple environmental influences might be mentioned and I think no one now hesitates to accept this general principle. Our most serious need is, as I see things in the present connection, to learn how to take more and more environmental conditions into quantitative account at the same time. It is obviously desirable that we apply artificial controls as far as possible and that we employ enough suitable chosen intensities of one or more environmental features to represent a wide range of possibilities; but it is just as desirable that we describe the remaining conditions with sufficient precision to furnish a background basis or frame of reference, to which our results may be related.

It follows that the results of experimentation are to be considered only with specific reference to the background environmental complexes that prevailed in the experiments dealt with; I mean with reference to all the effective or influential environmental features that were not specified and treated as experimental variables. Otherwise, generalization tends to go too far and too fast. Most biological writers, including myself, are apt to be over-negligent of this principle, although it is likely that all of us would subscribe to it in a general way. For example, the statements just made concerning the apparent oxygen-pressure relations of the influence of our specified ethylene treatment on carbon-dioxide elimination from our young wheat seedlings, were purposely made in the past tense and with limiting phrases that naturally seem somewhat awkward. We can not logically use the present tense of generalization nor can we omit those awkward phrases, no matter what any editor may wish to do in the interest of smaller printing cost. We really know only that those results were obtained from series of tests of specified type, in which a number of different maintained oxygen pressures and one maintained ethylene pressure were employed, along with a number of different maintained temperatures and a specified set of background conditions. It naturally remains an open question whether results similar to those described might be secured with the same experimental variables but with a different environmental background.

It is my hope that these somewhat disconnected paragraphs of mine may have served to bring together in your minds some of the most obvious and pressing needs that characterize our present outlook over the physiological and ecological field of environmental relations. We should not be discouraged by the extent and complexity of that field. Let us not admit the barren thought that environments are hopelessly complex; after all, they are less complex than are the organisms that we study. Let us avoid that kind of scientific hopelessness that would postulate environmental influences that are fundamentally and eternally beyond the reach of patient exploration and inquiry. If some biology students have sometimes seemed to feel or believe that we are approaching the end of our capacity for appreciating the natural control of performance through understandable influences, I am inclined to think that no one has really come to such a view by rational mental endeavor. However that may be, there is surely no reason to think, even for a moment, that the field of environmental influences fails to offer still a very broad range of things that may be profitably studied by experiment, instrumentation and clear ratiocination.

Because organism and environment must be studied together if we hope to increase our understanding of the former, and because both of them are clearly very complex in their modes of activity and with regard to their conditional inter-relations, it naturally follows that our problems now require, more than ever before, cooperative efforts of several or many minds. At any rate, knowledge of many different kinds and constructive thought based on many different view-points will be required for further advance in such studies as we have been considering this evening. As I am sure we all realize, productive cooperative effort, even among devotees of science, is in itself a difficult thing to accomplish; it needs to be based to a considerable degree on mutual understanding among the cooperators and to a much greater degree on mutual appreciation. Understanding and appreciation among us has been facilitated and encouraged by the American

Society of Naturalists, largely through its meetings and symposia, but partly through the intangible but quite significant *esprit de corps* that has so happily prevailed among our numbers. To aid in bringing all kinds of biologists together may well be the main aim of this society.

LOCAL BRANCHES OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

By J. McKEEN CATTELL

AT the meeting of the executive committee of the American Association held in New York on October 21, the question of the organization of local branches was considered and a committee to carry out the work was appointed consisting of Otis W. Caldwell, chairman; J. McKeen Cattell, chairman of the executive committee; Henry B. Ward, permanent secretary; Burton E. Livingston, general secretary, and Karl T. Compton. Members of this committee who could remain in New York the following day discussed the situation and an informal statement of plans proposed is here made in order that they may be brought to the attention of members of the association and secure their cooperation.

Local branches are authorized by the constitution drafted by the present writer in 1918 and adopted at the St. Louis meeting on January 3, 1920. Article 6 reads: "Regional Divisions and Local Branches of the association may be formed by vote of the Council. Such Divisions and Branches may elect officers, hold meetings, appoint committees, enter into relations with other societies, and promote within their fields the objects of the Association."

In accordance with the provisions of this article of the constitution there have been established the Pacific Division, organized in 1915, and the Southwestern Division, organized in 1920. An amendment to the constitution was later adopted providing for representation on the council of the affiliated state academies of science. Previously and since there have become affiliated with the association twenty-six state academies and two city academies. The work involved in the organization of the divisions and the affiliation of the academies has been large, and it is only within the last four years that the association has had other than a part-time secretary, whose time was fully occupied in the arrangements for the meetings and other pressing work of the association. The problem of the organization of branches has not as yet been taken up seriously, though two were established many years ago—one in Rochester and one in Pennsylvania State College.

Some thirty years ago a committee of the association on local branches was appointed, of which the late Dr. R. S. Woodward, then professor of Columbia University and later president of the Carnegie Institution of Washington, and the present writer were the active members. A certain amount of work was done at that time, but it could not be taken up by the central office, for the secretary then was receiving a salary of only $1,500, and, in addition to his official work as head of the Bureau of Entomology, was carrying on research work of importance.

With a membership so large and so widely scattered over the continent it is almost essential to have local and divisional organization. The American Association has its two western divisions and its affiliated academies; it would probably be advantageous to form other divisions covering the United States and Canada. It is the problem of local branches that is of present concern, but these may become the units in a state organization, perhaps under the auspices of the State Academies of Science.

Other professional societies have a more adequate organization than the American Association. For example, the American Medical Association is organized by counties and states; no physician can be a member of the national organization unless he is a member of the state society or of the state society unless he is a member of the county society. The regional divisions of the American Chemical Society cover the country. There are educational associations in every state.

Even without a satisfactory organization the American Association has advanced greatly during the past thirty years in membership and influence. In addition to the affiliated academies there are some 140 associated societies, and the membership had increased in 1931 to nearly 20,000. The comparatively small decrease in 1932 and 1933 has been followed by a moderate gain in 1934, the total membership now being about 18,000. A membership of 20,000 should be reached within the next year or two.

The membership can be increased by the organization of local branches; but the primary object of their organization is to forward in all localities the objects of the association which, according to the first article of the constitution, are: "To promote intercourse among those who are cultivating science in different parts of America, to cooperate with other scientific