

SCIENCE

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FRIDAY, SEPTEMBER 27, 1901.

PROBLEMS AND POSSIBILITIES OF SYSTEM-
ATIC BOTANY.*

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THESE annual summer meetings of our Society, occurring as they do between the close of one year's academic activity and the beginning of the next, offer an excellent opportunity not merely for profitable retrospects, but for such interchange of ideas as may stimulate renewed effort. The summary of results achieved, although a natural and desirable part in the proceedings of an assembly of this sort, is subject to a growing difficulty from the ever-increasing technicality of modern research. We live in an age of great detail and at a time when our subject has branched into many narrowing paths of investigation. Thus, even at a meeting of highly trained botanists, there is less common ground than we could wish, and it is scarcely possible, without the certain ennui of most of our colleagues, to present the finer results in those particular researches which may have stirred us individually to great enthusiasm. On the other hand, the aims and methods in our varying lines are by no means so unlike, and afford an ever-fertile field for discussion and comparison. It may be further maintained, in defiance of any suspicion of prejudice, that the aims and methods of systematic botany should command an especial and very gen-

MSS. intended for publication and books, etc., intended for review should be sent to the responsible editor, Professor J. McKeen Cattell, Garrison-on-Hudson, N. Y.,

* Delivered at Denver, August 28, as an address by the retiring President of the Botanical Society of America.

eral interest. No department of our subject is more intimately associated with every other. No other branch of botany so completely underlies all phases of botanical work. For what botanical investigation does not depend for its value upon a correct identification of the plants with which it deals? An accurate, lucid and complete classification of plants is thus the only secure basis upon which botany as a whole can rest. What is the present strength of this all-important foundation? Is it built upon rational principles? Should we build on or tear down and reconstruct? Is it nearing completion or does it represent as yet only the earliest stages of the desired structure? These are questions scarcely less significant to the physiologist, ecologist, pathologist, horticulturist or pharmacist than to the systematic botanist.

In surveying the taxonomic work of the last decade we see on every hand evidences of great and increasing activity. Small genera have become large; easy groups have become intricate. Thin periodicals have grown marvelously fat—in pages, not, alas! in pecuniary receipts. The number of regular and irregular publications has vastly increased. Species have been made by the thousand. No previous period of similar length has turned out such a bulk of systematic literature. It is true that this copious and now decentralized publication is of all degrees of merit, yet no one would wish to deny to it a reasonably high average of excellence. I speak, of course, of those papers which aim at the record of serious research. From these considerations and in the presence of this extraordinary activity there can be no doubt that systematists are making flattering progress in at least one direction; they are, namely, recording a huge number of facts.

Facts, however, can be accumulated much faster than they can be sorted and arranged. They are, to carry out our figure, only the

bricks for the structure, and to be effective building material must be laid in a firm mortar of law, organization and proper association. The chief difficulty which now besets our subject is the overwhelming accumulation of uncorrelated facts, unmonographed species, disjointed observations, preliminary and fragmentary records. The summit of our structure, instead of presenting a fairly clear and firm surface for further construction, seems to be buried at many points mountain-deep by disorderly heaps of loose bricks, in their way excellent building material, but so carelessly piled together as to impede rather than assist those who are earnestly endeavoring to bring order into this threatened chaos. Let no one understand me as discouraging the accumulation of facts—even very small facts—relating to the classification of plants. We do not know half enough even about the commoner species. I would merely urge that those who publish should take far greater pains to present their facts in an orderly and lucid way, with reasonable terseness and in such a manner as to show clearly their relation to preceding observations in the same field. This is the first possibility for advance in systematic botany and, if I mistake not, many other branches of research are in like case.

In this matter of presentation the natural sciences seem to be at a peculiar disadvantage. In *belles-lettres* a work of crude literary form is damned. Authors, if they would be read, must cultivate a good style. But in the natural sciences, if a work only presents some new and valuable facts it must, in spite of the crudest form, be purchased, read, reviewed, quoted, and the author is often flattered by the seeming success of a paper which may have been little better than an imposition upon his colleagues. Some improvement may undoubtedly be accomplished if the scientific public, especially editors and reviewers, can be

stirred to a more critical attitude toward work defective in form. But important advance can only emanate from the authors themselves. They should take a greater pride in the style of their publications, should realize that lucidity of exposition goes far to carry conviction, while obscurity is positive injustice to their coworkers.

Let us take, for instance, the publication of a new species. The requisites of a good description are generally known. There should be the habital picture, giving in a few words an idea of the general form, size and nature of the plant as a whole; then a considerable number of features should be tersely described; special care should be taken to point out the differential characteristics by which the plant is distinguished from its nearest relatives; and finally full information should be given with regard to the occurrence of the species, its type, locality, collector, date and exsiccation-number of the type-specimen, with a mention of the herbarium in which it is to be found. These are usually simple matters, and their business-like statement in relation to every new species is a generally-recognized obligation of its author to his colleagues, yet it is safe to say that not one-half of the species published during the last year have received descriptions which fulfilled these simple conditions.

On the one hand insufficient characterizations still occur. A well-known botanist has recently described a new leafy-stemmed phanerogam, without mentioning root, stem, branches, leaves, pubescence, calyx or fruit. However, this sort of insufficient description is becoming rare. The need of fullness is widely recognized, and great improvement in this regard has been manifested in recent years. *Enough* is generally said. Quantity in the description is no longer such a desideratum as proper arrangement, judicious selection, and especially some form of emphasis by which the really im-

portant, invariable, and therefore diagnostic, features may stand out in high relief. As I have said, the author of a new species owes clarity to his colleagues. This obligation is not fulfilled by a page and a half of description in which, without particular emphasis, all manner of characteristics are given, ranging from those which concern a group or even family down to others so detailed as to apply only to the single specimen in hand. Here is another possibility for advance, namely, the discrimination and proper emphasis of differential characteristics in description.

Here authors can bring to bear all the keenness of insight which they possess. To estimate correctly the value of plant differences is by no means easy. To a great extent their permanence and consequent taxonomic significance can only be a matter of inference based upon a knowledge of similar differences in other groups. This fact seems to have discouraged some of our systematists to such an extent that they wish to escape all responsibility in relation to the matter. They give what is called a good full description without the slightest effort to show the relative importance of the points they mention. They trust that a future monographer will somehow extract from their miscellaneous statements or find upon their so-called types certain significant differences which will serve to distinguish their plants from all others.

It may be doubted, however, whether a writer is justified in publishing a species until he sees with clearness its differential characteristics, and certainly when he sees them he has no right to hide them without any mark of distinction in a mass of other details of little or no taxonomic significance. Let us hope that, in this regard, the coming decade may see the same improvement which the last has witnessed in the increased fullness of descriptions, and that a systematist's work may be estimated, not by the

number of plants he publishes nor by the pages of descriptions he writes, but by the clearness with which he ascribes true differential characters and the actuality of his species in nature.

Regarding the citation of the type, there is in some quarters still a carelessness or indifference which is little short of astonishing. Species are still, in some cases, and even by persons prominent in systematic botany, published with no more definite information as to habitat, collector or type than the bald statement that the plant appears to be common from Vermont to Michigan and southward to Virginia.

I have heard certain attempts to justify this sort of thing. It is said, for instance, that the citation of a type-number is likely to mislead; that even the best collectors occasionally distribute unlike plants under the same number; that Pringle's 1507 at the Philadelphia Academy of Natural Sciences may not be just the 1507 at the Arnold Arboretum; that a species is more than an individual, and if a single type is cited there will be danger that some will narrow the interpretation of the species until it is artificially confined by those individual characteristics which the type-specimen chances to exhibit. But these are weak excuses. The probability is that Pringle's 1507 will be the same species wherever found, and if by any chance this is not the case a well-drawn description will go far to remove the danger of error. It is, furthermore, always possible, indeed desirable, to state the particular herbarium in which the type is preserved and thus remove all ambiguity. The other objection to the citation of type-specimens has quite as little force, for persons given to such fine-haired discriminations that they separate so-called species on individual traits are bound to interpret a described species in the light of some supposed representative of it, and in the interests of accuracy it is

much better that this individual should be the type rather than some specimen which from its characters or presence in the author's herbarium is merely assumed to represent the species in question. It would seem, then, that an author who does not cite his types is careless or unduly timid, and it is to be hoped that negligence in this matter, of which drastic examples might be given, may be regarded with increasing disfavor. Happily, here, as in the other matters mentioned, there are hopeful signs of improvement, and some of our most important botanical establishments, for instance the United States Department of Agriculture under its present direction, have been exemplary in this regard.

On many accounts it is to be regretted that the commendable custom of describing new species in Latin has been so generally abandoned in America. Still common in England, it is almost universal in continental Europe, and as a means of uniformity it is a source of much convenience. The Latin language by its high inflection and wealth of terse adjectival expressions lends itself exceptionally to the clear and compact presentation of details, and the formal description in Latin undoubtedly requires added attention to subject matter as well as form, while the running characterization, so easily dashed off in the vernacular, is to some extent a temptation to verbosity and hasty publication. The habit of writing descriptions in Latin would also exercise a chastening influence upon nomenclature. An author who could produce an intelligible Latin characterization would scarcely name his plant *pseudolongifolia* or *pulcherrima* or *nationalparkensis*, and these are scarcely overdrawn illustrations of the crudities into which some fall who have utterly abandoned Latin in the presentation of systematic botany. In regard to this matter of names, it may not be remarkable that there are some beginners whose enthu-

siasm in publication far outstrips their general scholarship; but one may express genuine surprise that the heads of important botanical departments and editors of prominent journals let these nomenclatorial solecisms see light in print. Here is another opportunity for easy improvement in the methods of systematic botany.

To this point I have dealt chiefly with the form of presentation. Let us now consider the subject matter. Here the difficulties of improvement are naturally greater.

The first feature of this subject which demands attention is the artificiality which still lingers in our so-called natural system. It is true that the natural arrangement of orders and families has been much improved in recent years. The clues derived from the varying degrees of adnation, connation and zygomorphy of floral parts in the dicotyledons have suggested the first system in which groups of such obvious affinity as the *Caryophyllaceae*, *Aizoaceae*, *Scleranthaceae* and *Amaranthaceae* are found in natural proximity. But much artificiality still remains in the details of modern classification. For instance, we are commonly treating as equivalents in our system things which in nature have widely different values.

There is an old question always coming up, ever fresh for discussion, never very clearly settled, regarding the objectivity of species. Do they exist in nature or are they artificial categories? Much may be said on both sides. It takes, however, no very profound study of plants and their descriptions to reveal the fact that so-called species are of both kinds. Many thousands exist as well-marked entities in nature, but, alas! there are many hundreds more which scarcely extend beyond the subjective. They represent not permanent lines of more or less independent development in nature, but chance combinations of inconstant characters analogous to cross-sections

through some plastic and still unsolidified material.

The cause of this lies partly in the author of the species and is partly inherent in nature. On the one hand, such so-called species may result from the hasty description of plants whose differences, observed in a few herbarium specimens, have not been sufficiently verified in the field. On the other hand, they may come from the simple fact that there are no formed or settled species in the group concerned. The forms of that particular affinity are still in a state of free intergradation and the species *im Werden begriffen*.

There seems to be a wish upon the part of many systematists to ignore this fact; to maintain that this or that form is, in hackneyed phrase, a 'perfectly good species' because it shows certain differences from its slightly removed although copiously intergrading neighbors; in fact, to asseverate that all plants which show differences worthy of remark should, irrespective of their constancy, be classed as species. But notwithstanding these unhappy ideas, nothing can be more certain than that fortuitous cross-sections in the nebulous places of nature are not species in the sense that *Ranunculus pennsylvanicus*, *Juncus trifidus*, *Malva rotundifolia* or *Potentilla tridentata* are. Nor can we hope to escape great artificiality in any system which assigns to like rank and groups in the same category things of such diverse nature and significance.

Species as now recognized are not equivalent things. The category, called specific, is itself a complex, in the same need of critical study, of subdivision, of segregation, as many of its elements. There are species marked by pronounced morphological features, which they never lose and which may always serve to identify them. There are others with characters subject to concomitant variations, in which if one feature varies in a particular direction the change

is regularly accompanied by certain other modifications affecting other members. Such species may be subdivided, and have good subspecies or varieties. On the other hand, there is a totally different type of species in which variation is not concomitant, in which one feature changes without apparent connection with any other, in which, for instance, thorns may be developed or be absent, while leaflets may be few or many irrespective of the presence or absence of thorns, and again, the inflorescence may show further variation quite independent of leaflets and thorns. Such species, exhibiting what Dr. Gray called promiscuous variation, are well illustrated by *Acacia filicina*, *Mimosa asperata*, certain *Aquilegias*, *Delphiniums* and *Lupines*. In these cases segregation or even varietal subdivision, although often attempted, has little or no significance, for the segregates exhibit only kaleidoscopic combinations of ever-changing characters. There are, on the other hand, especially as the result of preponderating close fertilization or vegetative reproduction, species which exhibit a wonderful constancy of small characters, a remarkable fidelity in transmitting from one generation to the next the most obscure traits. Such are the segregates of *Draba verna*, elaborated by Jordan, studied with such keen interest by de Bary during the last months of his life and critically reviewed in the later work of Rosen. Such are also the newly recognized *Alchemillas* of the Alps and our own *Antennarias*.

From these illustrations it is easy to see that species as now recorded in literature are by no means alike and that they cannot be regarded as equivalents in any complete or logical system of classification. Curiously enough, however, the term 'species' seems to be growing more and more popular as it means less and less. Often and on all sides we hear lengthy arguments, and emphatic asseverations to

the effect that this or that plant is a 'perfectly good species'; and if in the course of monographic work a so-called species is let down to varietal rank it rarely fails to find somewhere its ardent defenders, who appear to hold the curious view that the monographer has not merely expressed a scientific opinion, but has somehow perpetrated an injustice upon the plant or its describer. How anxious most discoverers of new forms are that their plants may prove species, not mere varieties, and finally what a fascination the mere binomial appears to exert upon certain minds! Is it any wonder under these circumstances that the specific category has been overcrowded and made to include such widely different elements that the word species has lost nearly all its taxonomic significance?

However, no thoughtful botanist who can rise above a merely subjective attitude toward the few species in which he chances to be particularly interested and take a broader, more objective survey of the whole field, will be satisfied that the present hodgepodge of non-equivalent forms in the specific category represents the finished result of a natural system of classification.

Species must be subjected to a gradual reclassification along more definite lines. Overwhelming as the task may at first appear, it is fortunately one which can be taken up little by little, a work in which every systematist, every collector, every amateur, who will, may take part. The first step is evident enough. Each species must be examined in the light of vastly more copious material than at present exists even in our largest herbaria. Has there ever been a conscientious monographer who has not seen the pressing need of further material in his group, who has not felt that ten or even a hundred times as many specimens would have been necessary to yield a satisfactory knowledge of the directions and limits of variation? Let

us, then, proceed with the accumulation of material, with the collection of specimens which may illustrate each species at every stage of development, in every part of its range, in every environment in which it occurs. In this matter we are much behind zoologists. They often work with hundreds or even thousands of specimens while we try to draw like inferences from dozens. An entomologist recently told me, quite as a matter of course, that he had just completed a monographic examination of more than fifteen hundred specimens representing a single species of orthopterous insect, together with three or four of its varieties. When may we expect that botanists will take similar pains in the interpretation of the limits and variations of a single species?

While on this subject of collection I may be permitted to emphasize an often neglected obligation of the collector to the monographer—that of reasonably full field notes. I realize that this is a wearisome subject, well known and thoroughly appreciated by many conscientious botanists and as persistently disregarded by others. I am acquainted, for instance, with several expert systematists, most scrupulous in all other ways, who appear on this subject of labels to have a curious mental defect. They never seem to have grasped the art of writing them, nor realized in this matter any obligation whatever toward their colleagues. From one of them I recently received some excellent specimens with no data but ‘White Mountains.’ Repeatedly, when working upon a particular species at the Gray Herbarium, I have examined dozens of specimens from many different collectors in the vain hope of learning from the field notes upon the labels such simple facts as the color of the corolla, height of the plant, or nature of the soil where it grows. Here is another opportunity for advance in which nearly every one con-

nected with systematic botany can cooperate.

We have seen, then, that the first requisite for a more thorough proving of species is a much greater and more representative accumulation of material and data. Then, of course, will come the difficult task of interpreting this material and especially of determining for general guidance more definite standards of variation. Regarding this latter possibility I have heard some scepticism expressed; but it seems to offer no greater difficulty than many other problems which have been successfully settled in the natural sciences. It must be admitted, of course, that while our knowledge of particular species is derived from a dozen, or in some instances from only one or two, specimens, no satisfactory standards of variation can be devised or applied. But were we to work with a hundred times this amount of material, it is more than probable that the degrees of natural intergradation could be fairly approximated. It would at least become evident which lines of specific development had attained what may be called a normal distinctness, a condition in which intergrades would be so exceptional as to suggest atavistic reversions, while, on the other hand, many of our so-called species would doubtless be found to be connected by regular, normal and fairly numerous natural intergrades, their lines of development would still be in a state of anastomosis, not having attained habitual distinctness. The interesting question would then arise whether the intergradation were geographically general or local, whether it were morphologically concomitant or promiscuous.

To interpret these matters satisfactorily will require not only the vast accumulation of material the need of which has just been emphasized, but a cautious and judicial attitude of mind, great impartiality, and an

unswerving desire to find out and record the exact truth. I do not mean to imply that systematists to-day have not this desire. Unfortunately, however, many of them, perhaps all, seem never to escape a certain hypnotism caused by particular interests. Trifling matters assume undue importance. Little differences seem so great as to obscure the preponderating similarities, or, on the other hand, superficial likeness blinds the observer to every differing detail. An opinion is quickly formed and perhaps hastily published. It then becomes a matter of personal pride to maintain it, and if any one expresses a doubt concerning its accuracy he is promptly called out to a controversial duel.

Now these things have their bright side and are in their milder form diverting, for somehow after the scrimmage which follows, truth, for the time hidden by the dust of combat, usually shines forth in victory, or more often becomes evident as the result of compromise. Indeed controversy is perhaps the only means which will successfully dispel the narrowing and perverting influence proceeding from the intensive examination of small details, and so often blinding the systematist to the real perspective of his own observations.

If we now turn from the matter of variation to that of distribution, it is equally evident that only a beginning has been made, that inferences are drawn from very insufficient material and that a vast accumulation of further data is requisite to accurate results. Let any one who doubts try to bound the range of some common species, to draw upon a map the sinuous line connecting the outermost recorded stations. The gaps are astonishing. Great lacunæ quickly appear in our knowledge of plant distribution.

No one can doubt the value of much fuller records in this department of our subject, nor maintain that our knowledge

of any plant is satisfactory until the limits of its natural occurrence are accurately determined. While plant distribution, studied from the ecological side, has become a popular subject, comprising many useful observations and theories both valuable and fascinating, the actual record of plant-ranges is on the whole regarded as rather dry business and is a field of investigation in which laborers are few. Mention should be made, however, of Professor A. S. Hitchcock's admirable work in this direction upon the flora of Kansas. What notable advance might be made if each State of our Union could have an equally well-trained systematist similarly interested in this matter of plant distribution!

Could we but know the actual curving boundaries of a few hundreds of our best-defined species, what a wealth of new generalizations could be drawn from them, and how much new information they would yield concerning the factors which govern distribution in general! For, irregular as these lines would be, I can but think that they would in many cases stand in definite relation to lines of other kinds, to isothermals, to altitudinal contours, to degrees of humidity, to the boundaries of geological formations, the limits of glaciation, the ranges of animals, especially pollen-bearing insects, to the paths of bird-migration, and finally, to the course of human traffic. What a field for further investigation is thus suggested by our still very imperfect knowledge of plant boundaries! It is a field, too, which the careful amateur can cultivate almost as easily and as well as the professional botanist. Every one lives near the assumed limits of some plants and might, by directing his attention to the subject, do much to change these as yet vague and hypothetical boundaries into accurately determined and carefully recorded lines.

Not only would these lines be likely to

disclose new and as yet unsuspected relations to forces controlling distribution in general, but they would give us our first accurate landmarks for the observation of plant migration, thus greatly facilitating a study of progressive changes in our flora.

While the accurate determination of plant boundaries has thus great interest it may be remarked that research in this field, as in others, to be successful, must be conducted with care. Reports of occurrence, especially extra-limital stations, should be taken with much caution. In this as in many other matters of science it is impossible to make too sharp a distinction between facts actually observed and those taken on hearsay. In mapping a plant the recorder will do well to indicate this difference. If, for instance, he shows by an umbra the range which rests upon specimens personally examined, let him record unverified reports only by a penumbra. Furthermore, any work of this kind to be of permanent value must rest, at least in great part, upon specimens which are carefully preserved, for segregation is progressing rapidly and no one can foresee its subjects. A plant of supposedly uniform character may at any time prove, upon more critical observation, to be two or more distinguishable species. In such a case it is easy to see that any previous study or records of the composite plant must lose nearly all their value unless specimens have been preserved so that a re-examination will show to which of the segregates the records applied. Similarly, the disappearance of a plant from a given region may lead to a justifiable scepticism as to the accuracy of the records relating to its occurrence in that place. In this case, practically the only valid proof is a preserved specimen accompanied by the original data of collection.

In interpreting and recording this matter of plant boundaries opinions will doubtless differ as to what may be called continuous

range and what is to be regarded as an extra-limital station, or, so to speak, an island in a sea of non-occurrence. This is, of course, all a matter of degree, since in reality no plant has a continuous range, for it is represented by more or less isolated individuals. Yet this offers no serious obstacle. The meteorologist maps the analogous course and limits of a rain-storm composed of separate drops, and the biologist has long recognized the practical continuity of plant and animal ranges which, in a generalized form, are the basis of his so-called 'life-zones.'

Turning now to quite a different field which seems to offer great possibilities, I would call attention to recent researches in plant ontogeny: the investigation of embryonic development, the comparative study of seedlings, and such observations as have been recently made by Professor R. T. Jackson upon the reappearance of juvenile and ancestral traits in offsets and runners. Systematic zoologists have long made use of ontogeny in determining group affinities, but botanical taxonomists have been much less successful in drawing from the early stages of plants like inferences. There are several reasons for this. In the first place, there can be no doubt that plants in their early development do not exhibit such a continuous and complete series of philogenetic stages as many animals do. In plants some stages seem to have dropped out by a sort of morphological and physiological elision or ellipsis. Again, while the classification of animals rests upon general morphology often well suggested even in very early stages of development, the classification of plants is based chiefly upon the mode of reproduction—that is to say, upon a series of structures produced so late in the life of the individual that no suggestion of their character is afforded by embryo or seedling.

But, after all, there can be no doubt that

ontogeny has for the plant taxonomist a wealth of information as yet unrevealed regarding the affinities of genera within the family and species within the genus. In these matters of more intimate relationship, the form, position and venation of leaves, the nature of the petioles, stipules, pubescence and glandularity, all shown in the seedling, are significant.

Here, however, as in the other subjects of which I have spoken, the real obstacle to further inference at present is an astonishing lack of material and data. It is safe to say that of the one hundred and fifty thousand flowering plants recorded in the recently issued *Index Kewensis* not one fiftieth part has been carefully traced through the earlier stages of development. Enough is known, however, to show that species even of the same genus often possess striking differences, and in other cases remarkable similarities, in the seedling stages, that these particular differences and similarities often become lost or obscured as the plants advance to maturity, and the conclusion is unavoidable that these juvenile characteristics must, at least in many cases, show ancestral traits, and, if properly studied, yield even better clues to real affinities than any which we now possess.

By way of summary, it may be said that systematic botany is very far from being a completed subject, that from our present standpoint we can see in various directions long vistas of further possibilities for fascinating exploration and profitable discovery, that among the subjects which seem to invite immediate attention the most important are: (1) The determination of the modes and degrees of variation, an investigation which alone can yield data for a more critical discrimination of plant categories; (2) far more complete study of plant ranges, which can scarcely fail to throw much new light upon the forces controlling distribution; and (3) a further examination of plant

ontogeny as the most hopeful source of information regarding the more intimate affinities and proper arrangement of plants.

B. L. ROBINSON.

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*THE CHANGE OF FRONT IN EDUCATION.**

DR. SAMUEL JOHNSON considered education as needful to the 'embellishments of life.' In his day very few were educated at all, and those few for society or public service. The toiling masses had no education, were supposed to need no education, and while discussing details educators and scholars took no thought of what we call the common people.

Said Johnson (in his 'Life of Milton'):

"The truth is, that a knowledge of external nature, and the sciences which that knowledge requires or includes, are not the great or the frequent business of the human mind. Whether we provide for action or conversation, whether we wish to be useful or pleasing, the first requisite is the religious and moral knowledge of right and wrong; the next is an acquaintance with the history of mankind, and with those examples which may be said to embody truth, and prove by events the reasonableness of opinions. Prudence and justice are virtues and excellences of all times and of all places. We are perpetually moralists, but we are geometricians only by chance. Our intercourse with intellectual nature is necessary; our speculations upon matter are voluntary and at leisure. Physiological learning [by which he means a knowledge of the laws and phenomena of the external world] is of such rare emergency, that one may know another half his life without being able to estimate his skill in hydrostatics or astronomy; but his moral and prudential character immediately appears.

* Address of the Vice-President and Chairman of Section I, Social Science and Statistics, of the American Association for the Advancement of Science, Denver meeting, August, 1901.