Why should not Stanford immediately adopt this as her vital policy? Her position is one of unprecedented freedom. Not trammeled by the service of the state as other universities on this coast are trammeled, independent of students' fees and consequently of numbers, Utopian in the material respects I have enumerated, she only needs a boldness like that shown by her founders to become the seat of a glowing intellectual life, sure to be admired and envied the world over. Let her claim her place; let her espouse her destiny. Let her call great investigators from whatever lands they live in, from England, France, Germany, Japan, as well as from America. She can do this without presumption, for the advantages of this place for steady mental work are so unparalleled. Let these men, following the happy traditions of the place, make the university. The original foundation had something excentric in it; let Stanford not fear to be excentric to the end, if need be. Let her not imitate; let her lead, not follow. Especially let her not be bound by vulgar traditions as to the cheapness or dearness of professorial service. The day is certainly about to dawn when some American university will break all precedents in the matter of instructors' salaries, and will thereby immediately take the lead, and reach the winning post for quality. I like to think of Stanford being that university. Geniuses are sensitive plants, in some respects like *prima donnas*. They have to be treated tenderly. They don't need to live in superfluity; but they need freedom from harassing care; they need books and instruments; they are always overworking, so they need generous vacations; and above all things they need occasionally to travel far and wide in the interests of their souls' development. Where quality is the thing sought after, the thing of supreme quality is cheap, whatever be the price one has to pay for it.

Considering all the conditions, the quality of Stanford has from the first been astonishingly good, both in the faculty and in the student body. Can we not, as we sit here to-day, frame a vision of what it may be a century hence, with the honors of the intervening years all rolled up in its traditions? Not vast, but intense; less a place for teaching youths and maidens than for training scholars; devoted to truth; radiating influence; setting standards; shedding abroad the fruits of learning; mediating between America and Asia, and helping the more intellectual men of both continents to understand each other better.

What a history! and how can Stanford ever fail to enter upon it?

William James.

PLANT FORMS EXISTING IN NATURE AND THEIR RELATION TO BOTANICAL RESEARCH.

Any one who is familiar with the various schools of botanical thought and who has talked with many men in the various lines of research, can not but have been impressed by the diversity of opinions about the so-called plant 'species.'

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The systematic botany of America to-day is, in some respects, in an almost irretrievably chaotic condition, and this condition is unquestionably due to the use of the various manuals which have been published down to the present day. Local workers everywhere determine the names of plants by comparison of six- or eight-line descriptions, and issue floral lists, make botanical surveys, frame ecological deductions, separate 'new species,' work out histological and embryological details, carry out important physiological experiments—everywhere using these manual names to label

their observations; and this is the framework over which the whole tissue of American botany is spread. A remarkable feature of all this is that any plant from any region can always be crowded in under some name found in the pages of the manual, or the six- or eight-line descriptions can always be interpreted so as to cover any plant. I have sent four specimens cut from the same bush to four reputable botanists and received four different names for my plant—names not followed by any question mark either! Three would have involved me in serious error in any future publication. Little wonder that those workers who have been brought up on the manual and never weaned to anything better should be thrown into confusion and consternation when the monographer—the indispensable pioneer in systematic botany—describes a whole grist of 'new species' among plants of his locality, well known to him. Personally, and as throwing light on my point of view, it may be remarked that I have never yet gotten up courage to depend upon the majority of determinations made from the manuals. My only point of departure has become an actual specimen which has been compared with the type, or passed upon by some careful phytographer who is familiar with the type. In the whole chaotic and discouraging condition, there is but one reasonable, safe and sure thing to tie to, and that is the type specimen. Specimens collected by botanists all over the country, and determined by the most careful and conscientious manual methods, quite commonly and naturally gravitate into the great herbaria. If any one needs an eye-opener to awaken him to the condition of American systematic botany to-day, let him go to one of these herbaria and pull down at random almost any species cover containing twenty-five or more sheets from various parts of the country. If he had previously thoroughly learned his own flora with manual in hand, he is now both shocked and distressed to find that the plant he has gathered so many ecological data concerning is not at all the same that X. in his region had written about, too, and he remembers scoring X. as an incompetent and careless observer. He pulls down another species cover and there stares him in the face the unmistakable fact that in his famous controversy with M. over the embryology of this other plant that, alas! he and M. did not really have the same plant in hand; and he blushes a little when he finds that neither he nor M. had the plant to which the name they had used was originally given. Were I to make ecological, or histological, or embryological studies or to publish a floral list, I think I should prepare about thirty to fifty good herbarium specimens of the plant form or forms in question, and send them to all the great herbaria under a uniform set of numbers and notes; and then a good label for the studies would be, for instance: 'Embryology of Physalis No. 250—perhaps lanceolata—deposited by me in all the greater herbaria.' Then might the various 'lumpers' and 'splitters' call it anything they pleased—it would still remain my No. 250, and the whole world could know unquestionably the exact form to which my studies referred. Such a plan would be infinitely more scientific than to use unquestioned some more or less questionable name from the manuals—as is usually done. We should at least be spared the spectacle furnished us in a recent botanical publication where, in embryological work, one investigator questions another's determination of *Cucurbita pepo*.

As it is, a vast number of the published records, observations and studies of plants in American literature are so much wasted printer's ink, except for the most general

purposes. The one thing that the authors could have done to save doubt and dispute and perhaps ultimate oblivion for their work—make good specimens of the plants they discussed and deposit them where others might examine them—they very frequently did not do. I know morphologists who have gone to all the trouble to work out the embryology of a garden plant and have labeled their hard-earned results with some name perhaps placed on the plant by an irresponsible gardener's boy through comparison with pictures in the florist's catalogue, afterwards holding themselves ready to criticize similar observations made under names obtained in a similar manner in other gardens. The very men who exercise the extremest care in studying the minutest details of the changes in the embryo sac are apparently those most likely to be guilty of great carelessness in connection with the identity of the plant they are handling. The highest form of systematic botany is, as some one has recently defined it—a condensed and systematic summary of all existing knowledge of plants, and based as surely upon the histology, embryology and physiology of the plants, as upon a study of their superficial characters. And what is being done to remedy our present unhappy condition in this connection? Has there been any general move towards fuller descriptions and the making of really fine detailed illustrations of all the type forms, together with their interesting variations and mutations, something after the manner, for instance, of Barbey's monograph of *Epilobium*? No—at least not among the American flowering plants—but the making of manuals goes merrily on, with some of the makers still talking of defining species from single specimens, in other words, attempting to define a whole that they have never seen, from a mere mummified fragment, which originally may have been some stray mutant and far from typical of the so-called 'specific' group to which it pertains.

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The anomaly of this old position is most evident when preceded, as it often is, by the attempted definition of a species, in which it is often frankly acknowledged that a 'species' in most cases is a plastic and unknown quantity. Even at this late date I have actually found curators of herbaria who consider each species sufficiently represented by a single specimen; and not long ago a bryologist wrote me: 'Why do you collect these odd and variable forms which only confuse the taxonomist? You had better spend your time searching for good typical forms.' He has not yet explained to me what good typical forms are. And if there is a group of plants in which the most extended studies of the comparative anatomy of individuals is more needed than among the American mosses, I should be interested to know of it.

There is no intention here to decry the use of manuals, especially if they contain full references to certain specimens in certain herbaria, in which case they are useful annotated catalogues. Even the general citation of exsiccati numbers for the older distributions is not to be depended upon, because the loose indefiniteness of the manuals strongly influences collectors also, and in numerous instances a number of wholly distinct forms have been sent out under the same number, rendering the citation of the herbarium also necessary.

Those species with comparatively sharp-cut and unchanging lines are, as a rule, doomed unless quite perfectly isolated. They are of great interest. However, I can not conceive but that the supreme interest of the physiologist, the taxonomist, the ecologist and the morphologist must rest always with the numerous and usually

remarkably plastic 'species' which cover the face of the country in those places where the battle for existence is at its height. In the coast foot-hills near Stanford University there is a common, large, deep purple *Trillium* with richly colored foliage. It presents there many remarkable variations, some of which are apparently independent of locality—for instance, lobing of the leaves—but it is still the same old purple *Trillium*. Northward in the San Francisco peninsula is a deep, rich, isolated valley, in which the trilliums of this type are almost entirely pink or white, and the foliage is pale. Albino forms of most colored flowers occur frequently, scattered among the normal forms. But when a whole valley is filled with them, then there are general causes at work which are well worthy of careful study. Torrey named this form *chloropetalum*. Its origin from *giganteum* seems indicated. Perhaps it has some *ovatum* blood intermingled—who shall say? Who has planted any seed of these things or tried any hybridizing—in fact, who has done anything but guess? I have heard the most emphatic opinion expressed about this form by men who had never even seen it where it grew and certainly intended to make no move to investigate it. It fits quite well the ordinary definitions of species in common use. I had not the least difficulty in collecting fifty quite uniform specimens for distribution and might have easily collected fifty thousand also fairly uniform in the same locality. I issued it as No. 431 of my West Coast Plants, and now it may be referred to as *Trillium giganteum, giganteum* var. *albiflorum, giganteum* var. *chloropetalum, chloropetalum, sessile* var. *chloropetalum,* or any of various other combinations—I care not—it is still my No. 431. Certain that nobody knows anything positive about its relationships, and that no one has added a single item of knowledge about it since Torrey called it distinct under the name *chloropetalum*—I prefer to put aside guess work for the time being, and follow Dr. Greene in calling it *chloropetalum,* without any idea of the taxonomic status of the group of individuals included under the name and with no idea of worrying about its status or entering into any controversy about it, and so it will rest in my mind, until some one gives the subject a little scientific investigation. There are hundreds, if not thousands, of groups of individuals in the United States, which must of a necessity be treated in exactly this same manner until something exact is known concerning them. I have not the opportunity here for the citation of more cases, or I could give many hundred very similar ones and of the utmost interest and suggestiveness in *Castilleia, Trifolium, Draba, Senecio, Amelanchier, Eschscholtzia, Platystemon, Rosa* and numerous other genera. Ecology without the recognition of these forms would impress one like a lamp that gave no light, or a volume with fancy binding from which half of the chapters had been cut. In a recent manual of ecology this feature is given a wholly subordinate place, and is said to be amply provided for by so-called 'preliminary reconnaissances.' Those who have been familiar with outside opinions of American botany for the past twenty to forty years know that it is the preliminary reconnaissance—through the medium of the manuals—that has brought so much discredit upon our work. I maintain most emphatically that the only way to make soil physics and meteorology of living interest to botany is to combine them throughout every step with a minutely detailed and complete study of all possible living plant forms. The results of de Vries have only been possible through such methods and we

have at home a striking object lesson in the case of Dr. MacDougal's calling in expert phytographers to enable him to record his own interesting results. It is only in Europe, where the fullest treatment has been given *Rosa, Alchemilla, Rubus, Draba* and other genera, that the literature of systematic botany furnishes any detailed assistance to the ecologist, of the sort that he absolutely must have. He who handles whole 'specific' groups as single entities is only competent to deal with the broadest aspects of geographical distribution. Ecology is a study of local conditions, and simultaneously a study of the plant products of those conditions within very limited areas and hence must involve congeries of individuals of much less than specific value. Ecology is necessarily a comparative study—in which numerous comparisons of conditions are first made. Why do ecologists stop short of minutely comparative studies of the variable plant forms which are often the exact expression of these conditions? It would do injustice to say that no moves whatever have been taken in the right direction. I have only to refer to one of several articles published by Mr. M. L. Fernald—this one entitled 'Some Lithological Variations of *Ribes*,' and to the recent work of Dr. Sargent on *Cratægus*. The work on *Viola* furnishes another striking example of what interesting investigations still lie before us in the direction of hybridizing and of growing wild plants under varying conditions and in various climates, with seed from known parents.

I have tried to outline as clearly and as forcibly as possible the very serious condition in American systematic botany—familiar already to many. It is necessary now to find a solution of the difficulties and consider methods of reform which may relieve the situation at an early date. The most important and fundamental move in reform must come from the great herbaria where so many types are deposited. These must be more completely described and their anatomical details fully and carefully figured. Such data are absolutely essential to any proper determinations of plants, and such data for the flowering plants are almost wholly inaccessible to the majority of American workers to-day. To make anything like a safe determination of most plants a trip to New York and Cambridge is necessary. It is needless to say that the illustrations should be prepared only under the direction of an expert botanist and a minimum of reconstruction allowed. And to this end fresh material in the form of unquestioned topotypes should be used wherever possible—though in this there are abundant opportunities for error. What a pity such work could not have been done right, from the beginning, and careful illustrations made from fresh material—or even wet specimens preserved. In the present condition of American botany I do not believe that we are justified in using unquestionably any plant name except as meaning that the plant to which it is applied agrees very closely with the actual type specimen, or if it differs appreciably this should be stated as an inseparable part of the determination, for instance '*Paspalum pumilum*, forma spiculis majoribus.' If we allow the indiscriminate application of names depending upon the individual's ability at guessing—and prescribe no limits to the guessing—then just what is the value of the botanical binomial as ordinarily used? To me the application of a binomial name to a plant is a very grave matter—it is a test of scientific honesty, of an ability to recognize anatomical details, and of competency to indicate to the world in a sign composed of two or three words the complete ensemble of clearly appreciable characters of the plant in question—in short, the whole

possibility of indicating any given plant so that other botanists may certainly recognize it. If I apply a name to a certain plant and my friend applies the same name to a quite different thing, one of us has perpetrated an untruth—and when we consider that a large part of American botanical literature is based upon one gigantic fabric of such untruths, the seriousness of the present situation may be appreciated.

There has been so much undignified and unscientific haste to rush out 'new species' and gain priority, that good work has been impossible. A friend of mine calls any field results in the way of specimens a 'grab'—and this name illustrates exactly the character of most of the field work being done in America to-day—'grabbing.' Most collectors are satisfied with a single number to represent what they, in this rapid and very superficial glance, seem to recognize as distinct species. They have not yet learned that they can do more for American botany by concentrated and localized work than by diffuse 'grabbing,' over great regions in the space of a single season. So, of the larger part of the United States we may very truthfully say that the surface has been barely scratched. It does not seem possible that any proper work could be done except by actual residence throughout the season in the single limited region to be investigated. I have found in the west that if a home camp be established in some favorable spot, one will have all that can possibly be attended to by one person during a single season within a day's tramping distance of the camp. From such a limited area I have brought away 25,000 to 50,000 sheets of specimens within three months, and even then knew full well that but a fair preliminary survey had been made, and that during a second season I could take as many more of the host of interesting varieties and extreme rarities, and only then get near a possible interpretation of the flora as a whole. But here the single-specimen men usually step in and forbid me—and I am compelled to do as necessity dictates instead of what I know to be for the best interests of American botany.

I have never yet had the pleasure of seeing an herbarium with a single species fairly represented even from the home locality. There may be many specimens, but from widely separated points. These will, of course, show certain variations, on which grave studies of geographical distribution and taxonomy are often based, and all this when often in single spots a few feet square in the home locality, still wider variations might be discovered by the right kind of critical field work. In the herbarium we haggle over the difference of a few hairs—which must certainly indicate a necessity for more work on the hands and knees in the field. We must learn to sit down among the plants and patiently study them right where they grow, and we shall never have a systematic botany worthy of the name until we do. It may be safely said that when this method of work is generally adopted we shall all see the urgent necessity for a hundred herbarium specimens where one is considered sufficient now. Also, the time will surely come when an herbarium that is no more than a *hortus siccus* will be unreservedly condemned as an efficient aid to the best or the safest work. Such an herbarium belongs to the time of Linnæus. Wet specimens for dissections, drawings and histological work must be prepared at the same time, from the same living plants, and under these same field numbers. Likewise seeds and woods should be collected and carefully associated under the same numbers. If this can not be done, then it would be far better for botany as a whole, if all plants were left in the field and studied only there. What a wonderful thing work in an her-

barium would be where each species was represented by large series of complete and well-prepared specimens from each locality, with full field notes, and where one might refer by the same numbers to wet specimens of the flowers and fruit in all their original form, and to abundant material ready for the knife. An herbarium of this sort fairly complete for any region will do more for botanical science than any of the colossal catacombs of fragmentary and often quite unrecognizable mummies which are being built at the enormous expense of institutions and individuals.

Suppose that individuals and institutions everywhere should undertake the right kind of field work and build broadly for the greatest and best in botanical science—the coordination of all their results would still remain a burning problem. They could, however, easily make a very perfect and immensely valuable coordination in one respect possible from the very beginning, with the use of a very little extra time and at scarcely any expense—a coordination that would benefit all alike, and not bury the results in the larger herbaria which must ever remain inaccessible to the vast majority of outside workers. All this could be accomplished by a well-organized and systematized cooperative exchange. Suppose that fifty institutions or individuals could be found to cooperate, and that, as an example, it was desired to more fully elucidate the genus *Viola*. Suppose that each should collect fifty numbers of the violets of their neighborhood, in series of fifty specimens each, illustrating every possible form, from the so-called typical specimens to all the minor variations of flower and leaf, and accompany each with full field notes, with the determinations as the forms were understood by the collector. Of one's own collecting this would mean an addition of fifty sheets to the herbarium. Through the cooperative exchange it would mean an addition of 2,500 sheets in a single season in *Viola* alone—copious thoroughly annotated material from all parts of the country, under the very names which botanists everywhere are using to label all sorts of field and garden and other observations. It is not necessary for me to dwell upon the extreme interest and widespread value of such a work to all interested in any line of botanical research. I have only to add that it is within easy reach of us—the material is at our very doors with which to carry it out and we shall all be healthier and happier for the little additional exercise the work will impose. It seems as if through a whole season any one might be able personally, or with the help of his students, to handle two hundred numbers of fifty specimens each. With fifty cooperators this would mean an addition the very first season of 10,000 sheets. It is only when we begin to handle them in such numbers that we shall ever know American plants as they are really existing in the field to-day, and cooperation in the work will enable us to reach important results in far less than the time it otherwise would.

However, plants can never be considered statically only, with any just appreciation of their place in nature. Seeds must be planted, seedlings studied, the effect of varying conditions on the life and characters of the plant noted and work in systematic hybridizing undertaken. *It will not serve the full purpose to grow the plants in one spot in a single garden*—this would give us but very few of the more important data so much needed. Firstly, the parent of the seed must be known in its natural surroundings, and then portions from this single known parent planted under widely varying conditions and distributed to widely separated points in different climates, where they should be grown under various conditions through several generations—a series of specimens of all

the cultures being eventually *reassembled on a single laboratory table* and subjected to minutely critical comparison with each other and with a series from the original locality. This is experimental systematic botany or experimental ecology, as you please. It is an endeavor to trace under control and constant observation some of the ordinary every-day processes of nature. A number of years ago I found growing in the spruce woods on Cameron Pass in northern Colorado an enormous strawberry. The plants were the most luxuriant I have ever seen, many of the tufts being more than a foot in height and the size of the flower and fruit clusters something quite remarkable. Its hardiness, fecundity and rapidity of fruiting were qualities which would have attracted any strawberry breeder at once. It differed widely from the other strawberries of these mountains and was named *Fragaria prolifica*. The following winter seeds of this species were planted in Alabama under Professor Earle's direction, and when the plants were well started they were set out in an exposed place in one of the hot, clayey fields of that pine barren country. The plants survived and matured, but there are scarcely two species of the genus in the recent monograph by Rydberg, more strikingly distinct than were these from their parent form. In Alabama they were low, extremely pubescent plants with reduced flower clusters and small leaves, not to be recognized by any botanist unaware of the circumstances as belonging with the mountain form. I believe that seeds of hundreds of high mountain 'species' are washed or carried down to lower levels, there to grow and reproduce and be known as other and well-distinguished species! And the seeds of many of these things will never go back up the hill again. This brings into consideration the whole subject of seed dispersal, not so much the dispersal in the immediate neighborhood of the parent as the distant voyages which are frequently made. I have gathered many notes on different aspects of this subject in years past—all very suggestive, none conclusive—without ever being able to do the only thing that would count for more than circumstantial evidence, *i. e.*, plant seeds. I am told that specimens of *Eucalyptus* grown from Australian seed in other parts of the world have been sent back to Mr. Maiden at Sydney without his being able to recognize them. What a pity that records could not have been kept of their exact parentage! With a little cooperation a most interesting and valuable series of experiments could easily be carried out by American botanists. A permanent high mountain botanical station must surely soon come in America—preferably several—dedicated to these cooperative experiments, which shall be associated with the best and broadest ecological work. Summer students at each station and a specialist to oversee the whole mountain end of the work, could surely be easily supported by the body of American botanists and their respective institutions. Needless to say, these stations could also be used in other important ways. From the parent plants of these mountain gardens could be supplied the uniform series of seed to be used by all caring to cooperate —in all parts of the country. And similarly many things could be sent from the lowlands for planting in the higher and highest altitudes. Some studies in gardens of plants at home have already been made in America. We now need very greatly investigation of their behavior away from home.

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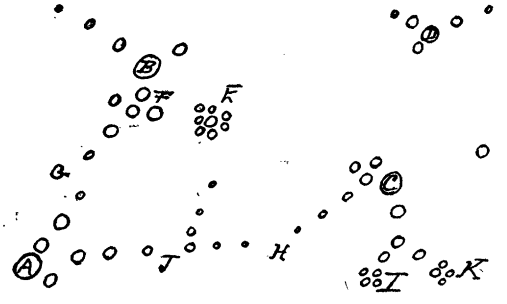
It has been my endeavor to show that much of the future best success of American botany in all of its branches depends

upon the more extended and critical study of plants and all their variable forms—in the field. Just as the origin of plant species does not depend solely upon hybridization, or mutation, or isolation, but upon all these agencies working together, so the salvation of botanical science does not rest in systematic botany, or in ecology, or in physiology or in morphology, but in the closer association of all these, and in more perfect cooperation between them.



The circles in this figure may be taken as representing groups of individual plants, or mountain tops, or meadows, or swamps, or islands in the sea, etc. For instance, *A*, *B* and *C* may represent well-recognized 'species' of plants where they are most at home, *i. e.*, the 'mother group.' Outlying groups in the different directions—a hundred feet or a hundred miles distant—vary from the typical form or the dominant form. At *G* two groups approach each other and hybrids may occur there, though the occurrence of these hybrids would not necessarily bring into question the utter specific distinctness of *A* and *B* unless some superficial investigator should, through lack of sufficient data, erroneously call them 'intermediate forms.' The younger group, *D*, evidently related to the others, perhaps arose by hybridization or mutation, its perpetuation perhaps due to sudden isolation. *J* may represent a descending ridge from the mountain at *A*. The groups *I* and *K* might be very different, and considered specifically distinct if judged by hurried collecting, whereas careful work up the respective cañons towards the summit or center of distribution at *C* would establish their relationship. So changed may *D* have become as to make its relations to *B* and *C*, its nearest relatives, wholly problematical, and an absence of individuals in intervening territory may prevent its reference to either except perhaps after experimental work. Evidently there may be actually in the field a thousand degrees of relationship, and in a systematic botany which shall mirror to some extent actual existing conditions, the terms 'variety' and 'race' have a scarcely intelligible place. The term 'form,' however, may well be in constant use. I do not know of any American species being charted in the above manner. Such work is rendered difficult by the superposition of hundreds of species—it will require a good eye, patience and steadfastness to a single object.

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Scientific Books.

Experimental Electrochemistry. By N. Monroe Hopkins, Ph.D., Assistant Professor of Chemistry in the George Washington University. New York, D. Van Nostrand Company. 1905.

This is an interesting book. Its author has so arranged it that it may be read previous to performing any experimental work, giving thereby an excellent picture of the historical development of electrochemistry. Any person adopting this plan will find himself in possession of many most interesting facts and helpful ideas, which are sure to prove incentives to carrying forward experimentation in this very attractive field of chemical science. Here is a thought which every student who thinks at all of electrochemistry should carefully ponder:

Electrochemical operations are essentially chemical and based upon purely chemical changes, and