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About the Institute
The Hunt Institute for Botanical Documentation, a research division of Carnegie Mellon University, specializes in the history of botany and all aspects of plant science and serves the international scientific community through research and documentation. To this end, the Institute acquires and maintains authoritative collections of books, plant images, manuscripts, portraits and data files, and provides publications and other modes of information service. The Institute meets the reference needs of botanists, biologists, historians, conservationists, librarians, bibliographers and the public at large, especially those concerned with any aspect of the North American flora.

Hunt Institute was dedicated in 1961 as the Rachel McMasters Miller Hunt Botanical Library, an international center for bibliographical research and service in the interests of botany and horticulture, as well as a center for the study of all aspects of the history of the plant sciences. By 1971 the Library's activities had so diversified that the name was changed to Hunt Institute for Botanical Documentation. Growth in collections and research projects led to the establishment of four programmatic departments: Archives, Art, Bibliography and the Library.

GENERA AND SPECIES AND THEIR RELATION TO GEOLOGY AND ECOLOGY.

Once in a while I meet people who ask me sarcastically if I got my ideas about species and genera from revelation or some other source not available to the common crowd. The covert insinuation is, of course, that my information is inaccurate and inadequate or that species and genera are matters of individual opinion. This idea I combat. The idea of species and genera comes only from close observation of nature in a wide range of localities coupled with deep study. The close comparison and analysis involved in classifying the masses of observed detail tax the keenest minds, even those with a naturally good sense of relationship.

Men are often found who foolishly say that all species and genera intergrade. Davenport, who was for some years the leading fern specialist, once stated that you could find intergrades in all the ferns. He must have known that this statement was untrue. The only reason I have for insisting on my opinion in this matter, is that I have spent two generations (more than sixty years) in field study of plants, I have collected more specimens than any man living, and I say without misgivings that as a rule plants do not intergrade. There are a few genera that are in a state of flux, such as Astragalus, Coreopsis, Carex, and nearly all cultivated plants. If you go out into the woods you will never have any difficulty in recognizing the wild crab apple, Pyrus Americana, or the service berry, or the black raspberry, or the devil grass, or the wild oats (Avena). You will, however, not always find it easy to tell a wild plum from a wild cherry, and your mind will be confused by the various names for the wild apricots, Prunus minutiflora, Andersoni, fasciculata, etc., which go under several generic names.

The theory that plants have become as they are by infinitesimal gradation is a myth, entirely unsupported by the evidence. However, it is a manifest fact that genera and species are relatively distinct as they grow in nature.

The fact that no two botanists are likely to agree in all respects

in the limitation of species has been the excuse for most of the riot in the making of new names. That capable botanists differ in minor details is one thing; that they should differ in essentials is quite another thing. The recent eruption of followers of Nuttall and the Continental botanists of Europe in splitting up genera on trivial ground will lead into botanical chaos, if followed. That plants as a whole do not intergrade, and that certain exceptional genera and species may are two very different things.

I assume that it is generally accepted that the development of speci and genera from others closely related to them is a fact and that this development is due to environmental conditions. If there are any left who still cling to the special creation theory, they are people with whom I cannot argue. I used to be one of them myself, but since under undertaking breeding experiments with plants, I have had no patience with the creationists. If environmental conditions cause differentiation it follows as a corollary that the intensity of these conditions will govern the rate of change, and the uniformity of conditions will eliminate change. Now it is definitely established by geological history as written in the rocks that conditions on the Earth, except for slight progressive aridity, have been normally about as at present for about 100,000 years, and that the flora has remained about the same under nearly identical conditions. Possibly at that remote time or before, glacial phenomena caused migrations, and the special conditions resulting caused considerab change in the flora, but the chief areas of differentiation were arid ones such as the Navajo Basin, the Death Valley-Carson Desert area, the Imperial Valley, and the Arizona border region. The Navajo Basin region I have described in Contribution 6 as the desert area east of the Wasatch plateau and south of the Uinta Mountains.

It is evident that no two plants are alike; there is always a limited variation from generation to generation, but there is no valid evidence that specific and generic variation is going on today except in isolated

causes. The great changes which have been made in floras in the past have been due to stress of vegetative conditions forcing plants to adjust themselves to adverse conditions.(See Cont.12) Stress is caused by aridity, alkalinity, elevation, and exposure. It is assumed that there has been a great reduction of carbon dioxide in the atmosphere, and other similar gaseous changes, but, in the nature of things, these things go too far back to be considered here. That there has been a gradual increase in aridity, and therefore a change from acid soil to an alkaline one in many places is evident.

Theorists will assume that the increasing aridity is due to the elevation of land areas in the great west. This, however, could not account for the glacial periods, but may be due to the precession of the equinoxes. I incline to the opinion that a change in the Japan current is more liable to be a main cause, for the reason that conditions of aridity prevail on the islands off the coast of California, just as they do in the interior. This would not occur if aridity was due to elevation of land masses. Doubtless all these causes have had their effect on our flora, however.

The sequence of animal life shows unmistakable change in the diminution in the size of elephants, bear, wolf, wild ox, and camel. I am inclined to think that these animals were contemporary with the flora of today, for the western flanks of the Sierras of southern California show subaerial wash to the depth of at least 400 to 600 feet with no perceptible change in character from today, while the fossils mentioned lie far above the bottom of this wash in the Brea pits. We, of course, do not really know what caused the extinction of these animals, but it seems probable that it was due to the gradual drying out of the vegetation and the consequent death of the forests. The pleistocene fossil plants of the western side of the Mohave desert are manifestly Populus Fremonti and Washingtonia filifera, which now are very scarce in that region.

Now assuming that the vegetation of today is practically that of many thousands of years ago it is reasonable to expect a certain fixity

in species and genera due to the long-continued uniformity of climate. Here and there we are to expect transitional states. We should expect to find the most new species among annuals, and on the arid plateaus, such as the Columbia Basin, the Carson Sink (which term I use to include the whole of the western Great Basin as far south as Owen's valley), the Mohave and Imperial Valley region, and the Navajo and Green River basins. This is what we find. There are certain new genera also in these regions, but whether these are really new or left-overs from more ancient floras we do not know. Heolastocleis, Hermidium, Muilla, Kelloggia, Clarkia, Sprague, Calyptridium, Parryella, Gossosoma, Whipplea, Fendlera, etc. are genera in point.

A few years ago Bailey Willis of Stanford came out with his much-tooted "Age and Area" theory, a theory already known and exploited by Gray and Hooker and others when he was in knee pants. The assumption that the age of a species is determined by the wideness of its distribution geographically Willis had to modify so much to account for ecological facts in each case that at last the theory became so attenuated as almost to vanish.

Sometimes, ~~too,~~ faulty historical botany leads to mistakes in judgment. California is the locus for the development of these peculiar vegetable forms known as Madia, Hemizonia, and Hemizonella. Gray, following the normal and proper course in describing species in these genera, assumed their distinctness from other genera because, as he a assumed, they were the product of local conditions. He erected the genus Hemizonella, but later collections show that this genus is a form of Melampodium, a widely distributed Mexican genus. This fact reveals that we must consider the Mexican flora in working out the genesis of California forms, and must recognize the more ancient connection of the California flora with that of Mexico. Such a comparitive study has hardly yet begun.

It is evident that to get a correct opinion of the specific character

of any genus or species we must consider not only its geological history (something of which we now know little) but also its environmental conditions, namely its reaction to heat, light, exposure, and soil. No one is justified in pronouncing judgment on these things without exact information as to the reactions observable in the field under actual growth conditions. Arguing from analogy based on a plant's relationship in the family is very risky. For Dicoria will not necessarily react to the frosts and drought of fall as Ambrosia and Franseria do , its nearest congeners. Oxytropis nothoxys is not necessarily poisonous because O. L Lamberti is. Nor is it certain that the Astragali will not be poisonous because they belong to a different genus; in fact, we know that several species are poisonous. So I say that the field botanist is the court of last appeal in determining the validity of a genus or species, and not the closet botanist. This is why I have so consistently and continually criticised the conclusions as to specific and generic identity of plants as proposed by the Brittonians. The generic or specific character of a plant is a matter of direct and honest observation free from theory. The supreme test of the worth of a botanical text-book is its practical usefulness in the field. And yet I realize that no book will be entirely satisfactory, for no one botanist can know all the reactions of genera and species in the field covered by his book.

All through botanical history each would-be systematist thinks he has found some obscure character (obscure to others, at least) on which to rest his proposed species. Once in a while a real character is found. It appears also that different families vary in the significance of seemingly trivial characters. Euphorbia, for example, has species that differ only in the wrinkles and angles of the seeds, while the size of the glands is of little importance. And yet the glands are conspicuous. Gray thought he had discovered a good character in the relative development of the pappus of ray and disk in Townsendia, but this falls down. Blake, following Gray, tried to keep up the factitious genera Encelia, Viguiera,

and Verbesina, but with poor success. Jepson thought he had discovered a similar character in the hairs of Calochortus, but this seems to break down. Fernald tried to base species of Antennaria on the tips of the involucral scales but failed. Perhaps the most remarkable of all was Watson's discovery of the markings on the onions' bulb coats, which proved most valuable in diagnosis and which I have figured in each known species. (Cont.10) Another valuable additional character, not as valuable or striking as Watson's, but a real character, was the method of root propagation, which I discovered and brought out in my monograph. (Cont.10:84) I tried in my work on Astragalus to base relationship on the curving of the ventral suture, and had to give it up, but found a most valuable character in geographical proximity.

All this careful and conscientious research is bringing us nearer to a proper conception of relationship. The great activity of German botanists in genetics ultimately led to the adoption of the genetic system by Engler and Prantl in their Pflanzenfamilien and the classificati of families and genera in the natural order of relationship, as understood today. The idea of genus is of great antiquity, being common even before Linnaeus, but only real students have discerned family relations. The arrangement of plants in families (completed mostly in the latter part of the 19th century) was a great advance in systematic botany.

The most disrupting and pernicious present tendency is that of failing to note the family relationships of exceptional cases, or freaks. The great herbaria are fast becoming the receptacles of botanical freaks, vegetable forms never found but once, on which many species have been improperly founded. Watson's Astragalus grallator and my Astragalus hyalinus are cases in point.

As I have pointed out in my Contributions from 1894-1934 (6: 12:85; 13:77; 16:49) it is the primary purpose of Systematic Botany to organize the vast material into an outline simple enough for the mind to grasp.

Th Therefore it is essential that the genera be as few as possible, and that peculiarities of species due to special circumstances be reduced to varieties. Personal ambition to have one's name attached to a genus has no place in the work of a true scientist. We do not have to a advertise ourselves: our capabilities will show in the quality of our work.

Systematic Botany has been evolved almost entirely since I began systematic work, but I do not know a single university that does much more than make a bluff at teaching real systematic botany, and none of them does anything but slop along in ecology. If there is one teacher that knows anything about life zones (See Willow Family & Cont.10:1;13:52) I have not met him. The best they can do is to copy Merriam's antiquated system. When Ph.D's come along and assume to teach me systematic botany, men whose education has been nearly all in morphology, I get the colic. In the early days systematic botanists were dominated by the physiologists, but now systematists pay little heed to the theories of physiologists. Certainly there should be no effort to force upon all botanists the half-baked conclusions of a few in-door, laboratory botanist by an appeal to authority or by monopolizing newspaper and magazine publicity. The vexed questions of species must be solved by patient field study. No theorizing can take the place of actual ecological knowledge.

Genera and Species. to Geology + Ecology.

Once in a while I meet people who ask me sarcastically if I got my ideas about species and genera from Revelation, or some other source not available to the common crowd. The covert insinuation is, of course, that my information is inaccurate or inadequate or is that species and genera are matters of individual opinion. This idea I combat. The idea of species and genera is that of right and wrong comes from close observation of nature, coupled with accentuated study. Men are often found who foolishly say that all species and genera intergrade. Davenport, who was for some years the leading fern specialist, once stated that you could find intergrads in all the ferns. He knew that this statement was untrue. The only reason I have for insisting on my opinion in this matter is that I have spent two generations, (more than sixty years) in field study of plants, and I have collected more specimens than any man living, and I say without misgivings that as a rule plants do not intergrade. There are a few genera that are in a state of flux, such as Astragalus, Coreopsis, Carex, and nearly all cultivated plants. If you go out into the woods you will never have any difficulty in recognizing the wild crab apple, Pyrus Americana, or the service berry, or the black raspberry, or the devil grass, or the wild oats (Avena). You will however not always find it easy to tell a wild plum from a wild cherry, and your mind will be confused by the various names for the wild apricots, Prunus minutiflora, Andersoni, fasciculata, etc. which go under several generic names. However, plants have become as they are by infinitessimal gradation is a myth, entirely unsupported by the evidence, that genera and species are relatively distinct as they grow in nature is a manifest fact.

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Jepson

Good use of space

The Problem of Systematic Botany.

I think it is time that a clear cut distinction should be made in the position of the various critics of certain phases of American botany. There are two schools, the conservative, and the so-called modern, which is nothing but the discarded continental Europaean system, against which the Kew botanists Watson, have always contended, and against which Gray, Engelmann, Boott, Eaton, Davenport, and a host of botanists like Vasey, ~~and others~~ built up American botany. ~~in opposition To~~ Nuttall ~~who~~ was the chief exponent of the Europaean system. The American botanists were in such majority during Gray's time that the Europaean ~~was~~ simply swamped. ~~Then~~ at about the ~~time~~ of Gray's death Britton insinuated ~~his~~ his oily way the homoeopathic suggestion to print a check list of eastern plants for convenience's sake, and got the blank ~~indorsement~~ of the A.A.S. When his ~~his~~ list came out it proved to be a new botanical system, claiming the indorsement of the A.A.S. which it never had. This was the beginning of the so-called American code. All sorts of sugar-coating was used to make this system go down, the main one for some years was the lie that it was based on deep and original study, which lie I have taken occasion to expose several times. There is no denying that everyone has a right to publish what he likes and make as great a fool of himself as he cares to do, and the same right to criticise this work belongs to every other person. What most botanists object to is the sailing under false colors which has been the rule in the ~~Br~~ Brittonian system of botany. was a great advance in systematic botany, and which was completed mostly in the latter part of the past century.

The idea of genus is of great antiquity, and that of species is nearly as old

From very early times men have had an idea of generic differences in the giving of generic names, but only students have discerned family relations.. It has come to pass that a species is considered distinct if it has any one character that is permanent and different from others, but to find such distinctions in species is the difficult thing nowadays, where in fact they do not exist. We who are searching after the real facts of nature are all regarded

Just what was Nuttall's conception of species and genus the botanists of toda do not know, but it is to be assumed that he followed others of his day in assuming that God made all species and genera, a doctrine handed down from th the Catholic church, but the fact is that he was a hopeless splitter, and see seemed to have no idea of the effect of ecological conditions on species and genera. doubtless the men of his day accepted as a fact the interference of G God in nature, but there were many who wanted to know why and how He did it. and never ceased to try to find out., even in the face of the dictum that What God has done is past finding out. and that it was impious to try. The ge general idea of God has been that He is infinite in all his attributes and must have done all things well. When the Linnaean systen came out it was generally accepted as a great human acievement. Nuttall's Genera in the preface has a flatulent reference to it, and Michaux's Flora of North America does th the same thing., all written in Latinspite of all this men generally kept at a study of plant life to find the order of creation. Jussieu proposed a syste and Bentham and Hooker in their Genera Plantarum proposed an order of creatio that was accepted by Gray and genrally followed, putting the highest plants f first and the lowest last and the Polygonaceae and other apetalous families far down in the scale of relationship and entirely out of place ina natural o order. In my early days the Germans came out with a system of relationship that has replaced that of Gray and others. but with exceptions. The arrang ment of plants in families was a great advance in systematic botany, and which was completed mostly in the latter part of the past century.

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as infidels and hopelessly biased by ungodly motives by the religious folks who "Know the truth" through revelation, though they assume as facts things assumed and not proved. I feel lkie commending Breasted's remark in The Dawn of Conscience" where he says "Like most lads among my boyhood associates I was learned the Ten Commandments. I was taught to reverence them because I was a assured that they came down from the skies into the hands of Moses, and that obedience to them was therefore sacredly incumbent upon me. IN later years when I was much older I began to be troubled by the fact that a code of morals which did not forbid lying seemed imperfect. Where did I get the moral yardstick by which I discovered this shortcoming in the Decalogue? When that experience began, it was a dark day for my inherited respect for the theological dogma of "Revelation".

When we see the Cactaceae a family wholly out of harmony with its environment and manifestly a misfit in nature, and know that certain characters in animals are inherited long after they have ceased to be useful to them we begin to doubt the wisdom of the Creator and wonder what He had to do in their creation. if He is infinite in His knowledge and power. while the conviction is forced upon us that the creation of new genera and species is a haphazard thing in the attempts of species to adapt themselves to the conditiobs under which they live.

Religious people continu to hold to the old dogmas of the Jews and to consider them the work of inspiration. Breasted 's Dawn of Conscience gives in detail much of the old Egyptian theology that prevailed thousands of years before the time of Christ. showing that the Jews copied much of those beliefs. in their Bible., and that their ritual of the priesthood including circumcisipn was not revealed but copied, as was their beilef in a coming Messiah. and in their selection as the chosen people of God. The story of Joseph seem to have been appropriated from the Egyptian., as well as their belief inthe one God. and ina pristhood., which lalter has filtered down to the present day in th Christian ministry, based on the fables of the New Testament when the actual facts show its developpmwnt from the necessity of leadership.

X cites whole article out

tanizing in America. See # 15: 74

after the first advent of settlers in the United States there was a great rivalry in getting the native plants for introduction into botanic gardens in Europe, and many collectors were sent out to explore and send over ddsirable novelties for cultivation there. The Indians had names for those which they had found useful to them for food or medicine, but white people began to call things by names of their own, and even scientists began to describe them in Latin bobks. All this led to much confusion for over 100 years. till such men a as Tournefort, Clusius, Dodonaeus, Bauhin, Mitchell, Cambessydes artand Rivinius, and many others began to print generic names for plants, and many specific descriptions and names besides., so that it became necessary for the adoption of some system. In 1735 A Swede by the name of Linne published a Genera Plantarum and called himself in Latin Linnaeus, and in 1753 he followed this by a Species plantarum, in which he gave names to and described all the known plants in the world. He arranged these species in an order based on the number of stamens and pistils an entorely artificial system which prevailed for many years and which was adopted by most botanists who essayed descriptions of species. In the la st f few generations there has been an agreement among botainsts to use the names of Linnaeus both for genera and species used by him in 1753. and they also adopted his stupid style of not describing species in more than 40 words, for the purpose of avoiding multiplication of pages in books. At the close of the 18th. century for a few years no books were printed on American botany but in 1808 Michaux who had traveled extensively in the eastern staes published a flora of America and in 1814 Pursh, a foreigner, got out another flora in which he included plants collected by Lewis and Clark on their expedition to the mouth of the Columbia as a government expedition. These were the days of the trappers and voyagers, and the Government began to send out expedktions for discovery, and colonization. The first plants collected wre not made by botanists but by engineers who took snips of plants and put them in books to dry or mold as conditions permitted and whose facilities for preservation were very meager.. These specimens were later on submitted to Botanists for

Le Conte

Perhaps better omit — Gray — # Watson & Rose

I do not want the inference drawn from my sharp criticisms of Gray in Cont. 16 that I am voicing a personal hostility to him. I never had any such feeling towards him, I have not now. My correspondence with him was always pleasant, and I resented his attempts to shut me up by cutting off publication as the result of his feeling that he was the court of last appeal in botany, a feeling that I never agreed with. We western botanists did him a great favor by sending him critical material for naming and we really made Harvard the court of last appeal, but this did not justify Gray in assuming any dictatorship over us. I never would admit, and do not today admit the right of any clique to tell me what are good species ~~and~~ When the Brown [illegible] people tried to dominate American botany I ridiculed them. That Greene was overbearing and domineering, and fussy and unscrupulous in criticism I know, but I overlooked these things by assuming that he overestimated his importance to American botany and that it was not so necessary for him to complete his great Flora as he thought. I never felt any institutional jealousy toward Harvard, and I always resented the carping criticism of the [illegible] crowd, who never could see any good in Harvard. There never was any botany worth mention in the [illegible] crowd compared with the culture of Harvard, but this did not justify Harvard in assuming any superiority over others. I always felt some resentment towards Watson for his curt expressing of opinions, particularly once when he replied that he supposed I had by this time learned that I could not grow plants to fit the descriptions. I could have replied that it was about time to fit the descriptions to the plants. Watson was too machine-like to suit me, but he was an older man, up to his ears in critical work on the Synoptical Flora and worked to death. I have always resented bitterly any assumed superiority of workers in the Nat. Herb. a [illegible] over field botanists in the West, but they are woefully lacking in ecological knowledge. It has been the stupid policy of the Nat. Herb. to try to publish local floras by using men in the Gov. Service. Even the lovable Rose tried to suppress me by the weight of his opinion when he wrote that he had so [illegible] totally with my position about genera in the Umbelliferae, which

Umbelliferae ?

in #16 Already

used

opinions I only laughed at. [Rose] Lunell was a field botanist, and through speculations allowed himself to be used as a tool [used as a tool]
2 by puffer when I criticised [considered] dripping brains [knaves], and now he
1 used Rydberg, Brittons & many ardent names, [illegible]. I consider the deaths of both are due to disappointment at being discarded by American botanists as leaders. Neither man had the mental equipment necessary for leadership, there were not a good systematic botanists. He had poor judgment of relationships, and no ecological training and experience beyond naming. This is the [original] [illegible] fault of our recent Ph.Ds

the cloth side with LePage's Glue, lay a tar board in the center of the cloth pressing it down tight and folding the edges of the cloth over (corners cut out). I keep them under pressure till dry. I take 4 to 6 of these tarboards along for presses and I use straps made of sole leather cut

six feet log and two inches wide, ~~and~~ with holes punched every two inches ap
apart and with a a heavy bucklt on one end, ~~I use~~ One of these straps around eac [with a tar board cover on each side,]
eachend of ~~the bundle~~ of driers make a press. ~~and I use~~ Two of the prepared [I sit on the bundles facing the press + pull up the straps tight daily. For several years I carried a collecting can with a tight lid but now I use]
~~card~~ board sheets for a protfolio and carry ~~them~~ along in a shawl strap ~~along w~~
with plenty of ~~specimen~~ paper sheets to put specimens in. ~~These card board sheets~~ For specimen sheets I buy unprinted newspaper cut 23 x 18 + folded in quires to fit the driers. I take
~~make the finest press imaginable, and will give to accommodate the sagging~~
along 2 to 6 quires for a trip + strap them up in the portfolio without driers.

As soon as I get any plants, I sit down on the ground, open my portfolio, take out a sheet + write on the lower left hand corner the date, locality and any other notes about color of the flowers etc. Then I lay the specimens on the right half of the open sheet, arranging them so that they do not touch each other. Then I fold the left half over the specimen, and lay a fresh sheet on top, continuing till all are in. If specimens are put in fresh, the flowers + leaves usually lie flat + lifelike, with only ~~the~~ occasional special arranging of a leaf. I pile the empty quires of paper on the filled sheets, + strap up my portfolio + trot along for another picking. When I get all the plants for that trip and go back to camp or home, I transfer all filled sheets to the press without opening the sheets, placing a drier over each sheet. I never open the sheets till dry, for fear of crumpling delicate parts. Usually plants will dry in 3 days to a week. When dry the sheets will feel warm to the touch, not cold. Then they can be taken out of the press, stacked in a pile + strapped up in an empty press. But be sure the stems are dry. If not, put a half dozen sheets, tied up tight, out in the sun for an hour or so.

My 60 yrs. of experience have taught me that to do the

Some people are constitutionally adverse to controversy, as though it were a wicked form of activity. To me it is a part of any critical botanical work. We have to weigh the work of others, and give to it what we think is its due. We often feel compelled to criticise strongly and pointedly where we find a gross lack of proper treatment. Many people take personal affront, as though thinking the criticism personal and not judicial. Such people overvalue themselves

* A bundle of driers with a tag band sewn on each side. To slip around each end of the bundle makes a good press.

[illegible] botanists do not know them and do not care who they are. We very frequently deserve more severe criticism than we get. Professors of botany in institutions of botany in institutions are peculiarly addicted to thinking themselves immune to criticism, when in fact they deserve the criticism aimed at their pupils' work, for American women particularly are abject slaves to fashion and do not dare to take any adverse position to the public trend however stupid that trend is. If we are to stop doing original work for fear of stepping on someone's toes we better get out of the profession. In a monograph we expect and have a right to insist on judicial treatment of all problems, an on the giving of the bases of the judgment of the rights of genera and species to recognition. If a person does not know ecology he has no right to be a monographer. It is fad today to sic young people on to doing monograph work when they have no training but herbarium work.

Geology + Botany

Amsonia, by Woodson. Recently there has been issued a brochure on this genus by Woodson, in which things have been brought up to date, and the species revised. It is my opinion that too many species are recognized in this genus. There are in all probability few genera younger than Amsonia, and the so-called species are too confluent for good species, but the collections are as yet too meager for certainty in defining the proper limitations. I am sure that Amsonia brevifolia and tomentosa are only forms of the same species. The characters given by Woodson do not hold out. I have seen acres of them and studied them in the field quite fully.

~~But it is not this that I care to enlarge upon now, but I want to call particular attention to~~ The maps (geological) accompanying the brochure are open to serious criticism. Mr. Woodson does not pretend that the maps are his own, but are copied from sources which he considered authentic, and ~~so any~~ my criticism ~~I may have on them~~ therefore is not aimed ~~at him~~ but at the author of the maps, who is personally unknown to me.

Any attempt to account for the origin of species geologically must as a basic fact have accurate information on the geology involved.

I have been an active field geologist for fifty years and have traversed most of the regions involved and studied them in detail, besides being familiar with the explorations and reports of Government geologists of the 40th parallel, the Grand Canon of the Colorado, Wheeler's and Hayden's reports, and the geology of the Henry mountains. I have also been personally acquainted with the men, Emmons, Dutton, Gilbert, who did most of the work. I say without any reservation that the outlines of the continent in Cretaceous times as published by Woodson are wrong, no such areas ever existed above the ocean in those times. Later on I will take up in detail this matter.

It is natural to want to find the origin of species during geological time, but up to the present time there is too little authentic material on which to work, and in addition, the paleobotanists who have described most of the species are not competent systematists, and their results are mostly guesswork rather than established facts. I have had a chance to check up the work of Knowlton and Ward on certain geological formations. Take for example Knowlton's work on the Tertiary flora of the upper Snake region. There are certain regions or exposures in which fine leaf impressions are to be found.

Filix Adanson Fam. This genus not having any species appended to it by Adanson must stand on the first description of it by others where species were given with it. This would be of Gilib. Exerc. 1792 where he describes Athyrium filix-foemina. This then becomes the type of Filix. So the contest is between filix and Athyrium as to priority and not with Cystopteris. Therefore Maxon's and Underwood's references of Cystopteris to Filix are unwarranted.

Nothoscordum striatum. This plant has much the same habit as Allium [illegible]. and the markings of the bulb are about as in A. Bolanderi or hyalinum. The bulbs are when mature about an inch long, and ovate, and with the thin coats of the onions. At the junction of the base of bulb to the roots, bulblets are produced outside of the coats and ~~which~~ are ascending and sharp-pointed, several of them to a bulb, and white.

Androstephium violaceum Torr. This has the same kind of propagation as Nothoscordum, but the bulb coats are thick and very fibrous with many vertical ribs.

In Coulter's Flora of Texas he left out the genera Cooperia, Crinus and Zephyranthes on page 430. But he put them in the key on page 429.

Knowlton figured and described certain genera such as Populus, Salix, etc. m making numerous species based on those impressions. Now what competent botanist would dare to identify any species of Populus on the leaves alone, knowing the species as they grow all around him ? And yet here is a paleobotanist who does not hesitate to name a species from the leaves alone. I seriously doubt if he even knew what were Populus and what were Salix species. No field botanist of today could identify certain leaves which I might furnish from Populus angustifolia and Populus dimorpha and tell even the genus to which they belong. When a person does not know the facts on which geological papers are based he is inclined to wonder at the acumen of the botanist who did the work, but when he knows the facts he inclines to think the man a grandstander, and nothing more. Now in this cluttered up condition of paleobotany where will a man land who tries to find the origin of species of plants in geological time ? It is a wildgoose chase.

In my work on Astragalus I made a serious effort to get at the origin of the genus in geological time. I found nothing worth considering prior to the Quaternary, glacial period. What will apply to Astragalus will also apply to any other genus of modern plants, in all probability.

Good if we had plenty of space -

GENERA AND SPECIES AND THEIR RELATION TO GEOLOGY AND ECOLOGY.

Once in a while I meet people who ask me sarcastically if I got my ideas about species and genera from revelation or some other source not available to the common crowd. The covert insinuation is, of course, that my information is inaccurate and inadequate or that species and genera are matters of individual opinion. This idea I combat. The idea of species and genera comes only from close observation of nature in a wide range of localities coupled with deep study. The close comparison and analysis involved in classifying the masses of observed detail tax the keenest minds, even those with a naturally good sense of relationship.

Men are often found who foolishly say that all species and genera intergrade. Davenport, who was for some years the leading fern specialist, once stated that you could find intergrades in all the ferns. He must have known that this statement was untrue. The only reason I have for insisting on my opinion in this matter, is that I have spent two generations (more than sixty years) in field study of plants, I have collected more specimens than any man living, and I say without misgivings that as a rule plants do not intergrade. There are a few genera that are in a state of flux, such as Astragalus, Coreopsis, Carex, and nearly all cultivated plants. If you go out into the woods you will never have any difficulty in recognizing the wild crab apple, Pyrus Americana, or the service berry, or the black raspberry, or the devil grass, or the wild oats (Avena). You will, however, not always find it easy to tell a wild plum from a wild cherry, and your mind will be confused by the various names for the wild apricots, Prunus minutiflora, Andersoni, fasciculata, etc., which go under several generic names.

The theory that plants have become as they are by infinitesimal gradation is a myth, entirely unsupported by the evidence. However, it is a manifest fact that genera and species are relatively distinct as they grow in nature.

The fact that no two botanists are likely to agree in all respects

in the limitation of species has been the excuse for most of the riot in the making of new names. That, capable botanists differ in minor details is one thing; that they should differ in essentials is quite another thing. The recent eruption of followers of Nuttall and the Continental botanists of Europe in splitting up genera on trivial grounds will lead into botanical chaos, if followed. That plants as a whole do not intergrade, and that certain exceptional genera and species may are two very different things.

I assume that it is generally accepted that the development of species and genera from others closely related to them is a fact and that this development is due to environmental conditions. If there are any left who still cling to the special creation theory, they are people with whom I cannot argue. I used to be one of them myself, but since undertaking brreeding experiments with plants, I have had no patience with the creationists. If environmental conditions cause differentiation, it follows as a corollary that the intensity of these conditions will govern the rate of change, and the uniformity of conditions will eliminate change. Now it is definitely established by geological history as written in the rocks that conditions on the Earth, except for slight progressive aridity, have been normally about as at present for about 100,000 years, and that the flora has remained about the same under nearly identical conditions. Possibly at that remote time or before, glacial phenomena caused migrations, and the special conditions resulting caused considerable change in the flora, but the chief areas of differentiation were arid ones such as the Navajo Basin, the Death Valley-Carson Desert area, the Imperial Valley, and the Arizona border region. The Navajo Basin region I have described in Contribution 6 as the desert area east of the Wasatch plateau and south of the Uinta Mountains.

It is evident that no two plants are alike; there is always a limited variation from generation to generation, but there is no valid evidence that specific and generic variation is going on today except in isolated

cases. The great changes which have been made in floras in the past have been due to stress of vegetative conditions forcing plants to adjust themselves to adverse conditions.(See Cont.12) Stress is caused by aridity, alkalinity, elevation, and exposure. It is assumed that there has been a great reduction of carbon dioxide in the atmosphere, and other similar gaseous changes, but, in the nature of things, these things go too far back to be considered here. That there has been a gradual increase in aridity, and therefore a change from acid soil to an alkaline one in many places is evident.

Theorists will assume that the increasing aridity is due to the elevation of land areas in the great west. This, however, could not account for the glacial periods, but may be due to the precession of the equinoxes. I incline to the opinion that a change in the Japan current is more liable to be a main cause, for the reason that conditions of aridity prevail on the islands off the coast of California, just as they do in the interior. This would not occur if aridity was due to elevation of land masses. Doubtless all these causes have had their effect on our flora, however.

The sequence of animal life shows unmistakable change in the diminution in the size of elephants, bear, wolf, wild ox, and camel. I am inclined to think that these animals were contemporary with the flora of today, for the reason that the western flanks of the Sierras of southern California show subaerial wash to the depth of at least 400 to 600 feet with no perceptible change in character from today, while the fossils mentioned lie far above the bottom of this wash in the Brea pits. We, of course, do not really know what caused the extinction of these animals, but it seems probable that it was due to the gradual drying out of the vegetation and the consequent death of the forests. The pleistocene fossil plants of the western side of the Mohave desert are manifestly Populus Fremonti and Washingtonia filifera, which now are very scarce in that region.

Now assuming that the vegetation of today is practically that of many thousands of years ago, it is reasonable to expect a certain fixity

in species and genera due to the long-continued uniformity of climate. Here and there we are to expect transitional states. We should expect to find the most new species among annuals, and on the arid plateaus, such as the Columbia Basin, the Carson Sink (which term I use to include the whole of the western Great Basin as far south as Owen's valley), the Mohave and Imperial Valley region, and the Navajo and Green River basins. This is what we find. There are certain new genera also in these regions, but whether these are really new or left-overs from more ancient floras we do not know. Heclastocleis, Hermidium, Muilla, Kelloggia, Clarkia, Spraguea, Calyptridium, Parryella, Crossosoma, Whipplea, Fendlera, etc. are genera in point.

A few years ago Bailey Willis of Stanford came out with his much-tooted "Age and Area" theory, a theory already known and exploited by Gray and Hooker and others when he was in knee pants. The assumption that the age of a species is determined by the wideness of its distribution geographically Willis had to modify so much to account for ecological facts in each case that at last the theory became so attenuated as almost to vanish.

Sometimes, ~~too,~~ faulty historical botany leads to mistakes in judgment. California is the locus for the development of those peculiar vegetable forms known as Madia, Hemizonia, and Hemizonella. Gray, following the normal and proper course in describing species in these genera, assumed their distinctness from other genera because, as he assumed, they were the product of local conditions. He erected the genus Hemizonella, but later collections show that this genus is a form of Melampodium, a widely distributed Mexican genus. This fact reveals that we must consider the Mexican flora in working out the genesis of California forms, and must recognize the more ancient connection of the California flora with that of Mexico. Such a comparative study has hardly yet begun.

It is evident that to get a correct opinion of the specific character

of any genus or species we must consider not only its geological history (something of which we now know little) but also its environmental conditions, namely its reaction to heat, light, exposure, and soil. No one is justified in pronouncing judgment on these things without exact information as to the reactions observable in the field under actual growth conditions. Arguing from analogy based on a plant's relationship in the family is very risky. For Dicoria will not necessarily react to the frosts and drought of fall as Ambrosia and Franseria do , its nearest congeners. Oxytropis nothoxys is not necessarily poisonous because O. Lamberti is. Nor is it certain that the Astragali will not be poisonous because they belong to a different genus; in fact, we know that several species are poisonous. So I say that the field botanist is the court of last appeal in determining the validity of a genus or species, and not the closet botanist. This is why I have so consistently and continually criticised the conclusions as to specific and generic identity of plants as proposed by the Brittonians. The generic or specific character of a plant is a matter of direct and honest observation free from theory. The supreme test of the worth of a botanical text-book is its practical usefulness in the field. And yet I realize that no book will be entirely satisfactory, for no one botanist can know all the reactions of genera and species in the field covered by his book.

All through botanical history each would-be systematist thinks he has found some obscure character (obscure to others, at least) on which to rest his proposed species. Once in a while a real character is found. It appears also that different families vary in the significance of seemingly trivial characters. Euphorbia, for example, has species that differ only in the wrinkles and angles of the seeds, while the size of the glands is of little importance. And yet the glands are conspicuous. Gray thought he had discovered a good character in the relative development of the pappus of ray and disk in Townsendia, but this falls down. Blake, following Gray, tried to keep up the factitious genera Encelia, Viguiera,

and Verbesina, but with poor success. Jepson thought he had discovered a similar character in the hairs of Calochortus, but this seems to break down. Fernald tried to base species of Antennaria on the tips of the involucral scales but failed. Perhaps the most remarkable of all was Watson's discovery of the markings on the onions' bulb coats, which proved most valuable in diagnosis and which I have figured in each known species. (Cont.10) Another valuable additional character, not as valuable or striking as Watson's, but a real character, was the method of root propagation, which I discovered and brought out in my monograph. (Cont.10:84) I tried in my work on Astragalus to base relationship on the curving of the ventral suture, and had to give it up, but found a most valuable character in geographical proximity.

All this careful and conscientious research is bringing us nearer to a proper conception of relationship. The great activity of German botanists in genetics ultimately led to the adoption of the genetic system by Engler and Prantl in their Pflanzenfamilien and the classificatio of families and genera in the natural order of relationship, as understood today. The idea of genus is of great antiquity, being common even before Linnaeus, but only real students have discerned family relations. The arrangement of plants in families (completed mostly in the latter part of the 19th century) was a great advance in systematic botany.

The most disrupting and pernicious present tendency is that of failing to note the family relationships of exceptional cases, or freaks. The great herbaria are fast becoming the receptacles of botanical freaks, vegetable forms never found but once, on which many species have been improperly founded. Watson's Astragalus grallator and my Astragalus hyalinus are cases in point.

As I have pointed out in my Contributions from 1894-1934 (6; 12:85; 13:77; 16:49) it is the primary purpose of Systematic Botany to organize the vast material into an outline simple enough for the mind to grasp.

Therefore it is essential that the genera be as few as possible, and that peculiarities of species due to special circumstances be reduced to varieties. Personal ambition to have one's name attached to a genus has no place in the work of a true scientist. We do not have to advertise ourselves: our capabilities will show in the quality of our work.

Systematic Botany has been evolved almost entirely since I began systematic work, but I do not know a single university that does much more than make a bluff at teaching real systematic botany, and none of them does anything but slop along in ecology. If there is one teacher that knows anything about life zones (See Willow Family & Cont.10:1;13:52) I have not met him. The best they can do is to copy Merriam's antiquated system. When Ph.D.'s come along and assume to teach me systematic botany, men whose education has been nearly all in morphology, I get the colic. In the early days systematic botanists were dominated by the physiologists, but now systematists pay little heed to the theories of physiologists. Certainly there should be no effort to force upon all botanists the half-baked conclusions of a few in-door, laboratory botanists, by an appeal to authority or by monopolizing newspaper and magazine publicity. The vexed questions of species must be solved by patient field study. No theorizing can take the place of actual ecological knowledge.

Botany in America.

From the advent of the first settlers in America there was a great desire to know the names of native plants. The Indians had names for many or [illegible] useful for food or medicine. But people began to call plants by fancied resemblance to those of Europe. This made much confusion. Many persons gathered pressed specimens on trips to send home to their friends, and then asked specialists to also name growing plants in their gardens to name them for them. This continued till 1753. When a Swede, by the name of Linne, Published a book called Genera Plantarum in 1735. Then later in 1753 he continued this by two books called Species Plantarum, which he adopted a fad of the day by giving generic and specific names to all plants known. These books were printed in Latin, and all his species had Latin names. He tried to adopt a system invented by himself for classifying all plants according to the generation organs. He was a man of such energy and ability that he succeeded in imposing his system on the botanical world. He made a very [illegible] mistake in insisting that every species should be [illegible] in not over 10 words, and this [illegible] in most botanical works to my day and caused much confusion in naming plants, and was followed by Gray and other botanists. In 1803 Michaux published a flora of America and in 1814 Pursh issued a 2 volume flora of America, based as to western plants on specimens gathered by Lewis & Clark on their trip across the country to Astoria. In 1785 Thomas Nuttall was born in England and soon emigrated to America and interested himself in botany so that he attempted to publish a book on the genera of American plants in 1818. About this time the Gov. undertook surveys across the country for exploration, and in 18[illegible] Capt. Wyeth was sent to Walla Walla Oregon to survey a R.R. route and it so happened that Nuttall and another young man started a tramp across the country at the same time carrying their packs on their backs & got to Independence Mo. at the same time as Wyeth's party did. Here they joined the party & journeyed with it to Walla Walla. Nuttall probably tramped afoot all the way and collected [illegible] of specimens as he went along. What was his equipment for pressing specimens no one knows, but it must have been old newspapers or magazines, and not many of them, going afoot with a pack prevents carrying heavy equipment, but all the same he collected a great amount of material. His notations of localities were written on scraps of paper torn off from others sheets. No doubt he had much [illegible] along the way as he went from Ft. Laramie to Green River & Independence Rock and Soda Springs Idaho which was the great recruiting place for worn out stock on its grassy meadows. Then the caravans went down the Port Neuf river past the present site of Pocatello to the bend on the Snake river called Fort Hall where they [illegible] the fort in a better place. Those were the days of the buffalo. Then they crossed

the rivers & went up Lost river to the forts and down on the Snake River near the present Boise. and along the plains to the head of the great Snake R. canyon [illegible] near the present Huntington, where they went over the [illegible] to the Blue Mts, & [illegible] where there ~~[illegible]~~ were great grassy plains and much game. Then again over a high range and down to the ~~[illegible] sea~~ Columbia near Umatilla and along it to Wallawalla. Later on he went to [illegible] and by boat to [illegible] where he stayed some time and then sailed for Hawaii and back home by way of the Isthmus. His return made quite a stir and set him up as the leading botanist of America. He made the Phil. Acad. Sci. his head-quarters and deposited with them all his types of [illegible] and genera prepared by him for his collection. Once he was a teacher of botany at Harvard