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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.

Put this under Hist. Amer. Botany

Contr. U.S. 139

Development of System. Botany

Botanists whom I have known.

There is a woful lack of information about the makers of the systema botany of thday. The collectors and writers on western botan.

It has been my good fortune to have belonged to the middle period. Pursh and Michaux were the only leaders who were gone when I began to botanize in 1875. Nuttall was still alive though old, as was Gray who was born in 1810 (some 9 years before my father's birth) and passed through all the reconstruction period of botanical research from the Linnaean system to the present genetic system, having adopted the ideas of Bentham and Hooker in their Genera Plantarum, and their method of publication. ectors whom I have known either directly or by proxy. My personal relations to the various great men need not clutter up the record.

Typed

#2 This should be in. Cuts Port. Asa Gray.

The engravings of Gray, Watson, and Torrey given ~~here~~ in this volume were copied from the Botanical Gazette and Torrey Bulletin. That of Gray is to be preferred to that published in the Bulletin at the time of his death, which ~~photo was one~~ showed him a worn old man; the ~~other~~ shows his steely black eyes indicative of his fighting nature. The photo of Torrey was issued by the Bulletin at the time of his death, revealing the kindly nature, like that of Sir Joseph Hooker, which endeared him to all whom ~~xx~~ he knew., The photo of Watson was from the Botanical Gazette, and which was a copy of a photo at the Pomona college herbarium. ~~It is a noticeable fact drawn from~~ the articles published at ~~their~~ the deaths of the three men, that there is no tribute of personal friendship in the review of Watson's life, just a cold statement of fact, nOthing like the warm personal regard shown for Torrey. Watson's meteoric advent into botany led many to ask about his origin, ~~this question was asked Torrey once, and he made no reply but just pointed his finger heaven ward,~~ the records show he graduated from Yale in 1847, and was born in 1826, some 26 years before the writer was. He was a teacher in Iowa College (Now Grinnell) in 1867, and was given the M. A. degree there about the time of my Colorado explorations in 1879. The absence of evidences of personal regard show him to have been as unsocial as I have represented him to have been.. ~~It is said of him that one night he appeared in the U.S. Gov. camp in Nevada carrying his showes in his hand and asked for a job with them. This was when W.W. Bailey was the botanist and was unable to work from ill health,~~ Watson's report of the work of the 40th. parallel was a model of thoroughness and the best thing ever gotten out on a Gov. survey except the magnificent Cactaceae of the Mexicam Boundary by Engelmann. I have already written much about Watson in the past and of my high regard for the quality of the work. and of his pemchant for short descriptions which I hav always condemned. This last of the stupid dicta of Linnaeus persisted a century after his death, and was the last to go, after the discard of his generative sysyem

Typed

Watson B N Sug Cont. 16 Vasey

~~In saying to miss Watson I may have seemed to slur him, but this was not my intention.~~ ~~As a man Watson was as cold and unsympathetic as ice.~~ As a botanist however he stood nearly at the top. No one but Engelmann was worked as much precise plan.. as Watson. ~~He was a very high class botanist, but a cypher socially.~~ Another man whom we all admired for his worth, but whose calibre was not that of Watson was Dr. Vasey, botanist of the U.S. Dept. of Agric. whose [illegible] [illegible] ~~effort to kill.~~ Vasey was a real man in every way, always prompt and reliable, ~~and~~ giving personal attention to his work. There was no man in the profession for whom I held a stronger personal regard. See @ ~~[illegible]~~ See #15

(6) Dr. George Vasey Botanist U. S. Dept of Agriculture

~~Dr Vasey~~ He was always a genial correspondent and attended promptly to specimens of grasses sent him; I used to bundle up all my specimens that I could not name and send them to him, In 1882 I sent him grasses I got in Mexico on the trip to Ensenada with Pringle and Parry. ~~and~~ I considered one species to be new and ~~reserved the name for it~~ told him I would name it later on. He must have shown it to Scribner, who was trying to be the grass specialist, almost immediately I received ~~about that time he sent me an urgent~~ a letter from Vasey urging me to publish my new species at once before ~~saying that~~ Scribner ~~was going to~~ should steal it from me. I replied that no honorable man would do such a thing; ~~and I doubted Scribner would do it.~~ but he did just that and called it Melica frutescens.

This was the last letter that I received from Vasey. ~~He became quite familiar with the grasses and had as~~ His assistant, L. H. Dewey, who after his death, ~~swat~~ was to take his place in charge of the grasses, ~~but~~ was pushed aside by Scribner who remained in charge till he was removed. See Cont. 15, R.S. In 1894, when I was in Washington, Dr. Rose had as his stenographer Miss Vasey, the daughter of the doctor, ~~in 1894~~ She was a very social person who, at Rose's suggestion, took me over to the Corcoran Art Gallery and showed me all around.

(See 15:30)

The lovable Dr. Rose, himself was no field botanist, although very conscientious. It was a pity that he & Rydberg allowed Britton to dominate them and so sacrificed what leadership they might have had among American botanists. I believe they died disappointed. Rose was not a good systematic botanist, for he had poor judgment of relationship and no ecological training or experience worth naming. Yet in the superior manner of the National Herbarium he tried to suppress me by the weight of his opinion when he wrote that he had no sympathy with my position regarding genera in the Umbelliferae. This was really amusing, for without adequate Ecological background his opinion could have little weight.

Typed

Two sheets on Pringle - work into Bot. I have known.

Botanists Whom I have known. #16.7.43

Supplementing remarks made earlier about C.G.Pringle I will state that I spent 17 days at Guadalajara in October 1930. Rev. Howland was the minister of the Congregational church there when Pringle botanized at Guadalajara. I was much surprised to learn that Pringle made ~~his~~ Howland's place his home there for eight years while he explored the great Barranca and adjoining regions. Pringle endeared himself to the Howlands, and they always speak of him kindly. *1.

It ~~will be a pleasure~~ has been interesting to check up on Pringle to see how many of his plants I collected in my short stay ~~there~~, in that botanical paradise. The people in Mex. who knew Pringle ~~in Mexico~~ are all gone now with the single exception of Prof. Puga, a kindly old Mexican prof. of pharmaceutical chemistry, with whom I hdda pleasant visit.

My study of the flora of Guadalajara, which I secured ~~Insert Line~~ in 1930 has now progressed to the point where I am justified in saying that my early criticism of Pringle for being a poor field botanist is fully justified. He missed many species that grew in that region, some of them new, but a person who spent 8 years in a region should have got the entire flora, ~~which he did not get.~~ Evidently Pringle did not do so.

Insert above C G. Pringle *1.

Adding to what I have written about Pringle Mr Wright, a neighbor here of mine in Claremont, who was a missionary ~~minister~~ at Guadalajara, Mexico, when Pringle was botanizing there and with whom he stayed at times, says that Pringle was a true Quaker in his thinking and never would carry a gun nor kill anything and that on his ~~er~~ trips he would carry some cheese and tortillas as food and be gone a week ~~or~~ at a time. He also says that in the time of the great Rebellion (Civil War) Pringle was drafted into the army, refused to fight and was tied to a post and exposed publicly as a slacker, by some half-witted army officer.

Typed

BOTANISTS WHOM I HAVE KNOWN.

There is a woeful lack of information about the makers of the systematic botany of today,-the collectors and the writers on western botany. It has been my good fortune to have belonged to the middle period. Pursh and Michaux were gone when I began to botanize in 1875. Nuttall was still alive, though old. He died in 1859 when I was seven years old. Torrey lived until I was twenty, but he had dropped entirely out of botanical work. Douglas was dead also, and many of the smaller lights were gone. But the great men, Gray Nuttall, Engelmann, Boott, Eaton, Watson were still building when I began to collect. Gray, who was born in 1810 (some nine years before my father's birth),lived through all the reconstruction period of botanical research from the Linnaean system to the present genetic system, having adopted the ideas of Bentham and Hooker in their Genera Plantarum, and their method of publication. As my own life has covered a rather long period, it has seemed fitting to me to add my mite to the records of systematic botany and collectors whom I have known personally or by proxy.

The engravings of Gray, Watson, and Torrey given in this volume were copied from the Botanical Gazette and the Torrey Bulletin. This one of Gray is to be preferred to that published in the Bulletin at the time of his death, which showed him a worn old man; This onw shows his steely black eyes indicative of his fighting nature. (See comments about Gray and others in Cont.16.) The photo of Torrey was issued by the Bulletin at the time of his death, reveals the kindly nature which, like that of Sir Joseph Hooker, endeared him to all whom he knew. The photo of Watson is from the Botanical Gazette. There is also a copy of this photo at the Pomoma College herbarium.

It is noticeable that in the articles published at the deaths of the three men, there is no tribute of personal friendship in the in the review of Watson(s life, just a cold statement of fact, nothing like the warm personal regard shown for Torrey.

Watson's meteoric advent into botany led many to ask about his origin. The records show he graduated from Yale in 1847, and was born in 1826, some 26 years before the writer was. He was a teacher in Iowa College (now Grinnell) in 1867, and was given the M.A. degree there about the time of my Colorado explorations in 1879. The absence of evidences of personal regard shows him to have been as unsocial as I have represented him to have been. (Cont.16:47) As a botanist, however, he stood nearly at the top. No one but Engelmann has evoked as much praise from me as Watson.

DR. GEO. VASEY

Another man whom we all admired for his worth, but whose caliber was not that of Watson, was Dr, Vasey, botanist of the U.S.Dept. of Agriculture, whose chair Coville has rattled around in. Vasey was a real man in every way, always prompt and reliable, giving personal attention to his work. There was no man in the profession for whom I held a stronger personal regard. He was always a genial correspondent, attending promptly to the many puzzling specimens of grasses I sent to him to name. In 1882 when I sent him the grasses I got in Mexico on the trip to Ensenada with Pringle and Parry, I wrote that I considered one of the species new and would name it later on. He must have shown it to Scribner, who was trying to be a grass specialist. Almost immediately I received from Vasey a letter urging me to publish my new species at once, before Scribner should steal it from me. I replied that no honorable man would do such a thing. But he did just that and called it Melica frutescens. This was the last letter that I received from Vasey. His assistant, L.H.Dewey, who, after his death, was to take his place in charge of the grasses, was pushed aside by Scribner, who remained in charge until he was removed.(Cont.15:30) In 1894 when I was in Washington, Dr.Rose had as his stenographer, Miss Vasey, the daughter of the doctor. She was a very social person and, at Dr. Rose's suggestion, took me over to the Corcoran Art Gallery and showed me all around.

DR. ROSE

The lovable Dr, Rose (Cont.15:30)was no field botanist but very

conscientious. It was a pity that he and Rydberg allowed Britton to dominate tham and so sacrificed what leadership they might have had among American botanists. I believe they both died disappointed. Rose was not a good systematic botanist, because he had poor judgment of ~~rela~~ relationship and no ecological training or experience worth naming. Yet in the superior manner of the National Herbarium he tried to suppress me by the weight of his opinion, when he wrote that he had no sympathy with my position regarding genera in the Umbelliferae. This was really amusing, for without adequate ecological background his opinion could have very little weight.

C. G. PRINGLE

Supplementing remarks made earlier (Cont. 16:43) about C.G.Pringle, I will state that I spent 17 days at Guadalajara in October 1930. Rev. Howland was the minister of the Congregational church there when Pringle botanized at Guadalajara. I was much surprised to learn that Pringle made Howland's place his home for eight years while he explored the great Barranca and adjoining regions. Pringle endeared himself to the Howlands, who always speak kindly of him.

Mr. Wright, a neighbor of mine here in Claremont, who was a missionary at Guadalajara, Mexico, when Pringle was botanizing htere, and with whom he stayed at times, says that Pringle was a true Quaker in his thinking and would never carry a gun nor kill anything. On his trips he would carry some cheese and tortillas for food and be gone a week at a time. He also says that during the great Rebellion Pringle was drafted into the army. When he refused to fight, he was tied to a post by some half-witted army officer and exposed mublicly as a slacker. The people in Mexico who knew Pringle are all gone now with the exception of Prof. Puga, a kindly old Mexican professor of pharmaceutical chemistry, with whom I had a pleasant visit.

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plants I collected in my short stay in that botanical paradise. My study of the flora of Guadalajara which I secured in 1930 has now profressed to the point where I am justified in saying that my earlier criticism of Pringle for being a poor field botanist is fully justified.

initials (?) D. H. HILLMAN

One of the best botanical artists in the profession was D.H.Hillman, professor of botany at Reno, Nev. He was another very capable man who was lost, submerged, in an unexplained way intthe Dept. of Agriculture. Everyone knows of the jealousies that prevail in the departments at Washington, but no one yet has had the courage to expose the persons responsible for swamping such good men. I have a very strong suspicion as to the guilty ones. Nothing but a thorough investigation of the government departments, such as we hoped for from Theodore Roosevelt, will ever right matters. There never has been to this day a man in the presedential chair pledged to a real house-cleaning.

P. A. RYDBERG

On July 25, 1931 there occurred the second tragedy in American botany, the death of Rydberg before his time, the first tragedy having been the death of J.N.Rose. Their disappointment at the treatment accorded them by the botanical public was, however, partly their own fault; for no one has a right to subordinate his own judgment to that of another, as I think they didito Britton's, for the sake of financial support.

Rydberg was a kindly Christian man, well liked by those who knew him best. He was capable, sloppy, and a mule. I say "capable" because when he really settled down to work on a thing he could do it well. I say "sloppy", because he was too much inclined (like Standley) to rush through a thing before he had looked on all sides of it thoroughly, and so made many blunders. I say "a mule", because when he was criticised for his faults he set up his back and stuck to his errors like a mule, instead of acknowledging them and changing his course. This is particularly evident in his silly Astragaloid genera. He was almost as bad a splitter as Greene,

characterizing most of his species on single specimens without taking into consideration well known ecological factors, and without attempting to collect the intergrading material in the type localities of his proposed species. He had no conception of genus, and a very poor one of species, and no conception of the effect of ecological conditions on species. How onyone could be a botanist and neglect these things is a mystery. Rydberg's first work, on Potentilla, was to me his best. He made too many species there, but the work was good in the main.

There was a time in his early years when workers in the Bronx befan to swell out with what they considered justifiable pride at his wonderful insight and deep and original studies, but I punctured this bubble by showing that all his so-called original work was simply giving new names to things that were discovered and published before he was born. Since that time we have been spared any more of this bunk.

Rydberg was inclined in his later years to consider himself persecuted. I never knew that he had any personal enemies. Nor fdo I think any one ever got after him for personal reasons. I certainly never did, though once or twice I had some reason to feel that he had not treated me fairly in private correspondence. In one of his comments on my species, he refers to having seen my type material, in order to strengthen his comment on it, but at that time he never had seen any of my types nor ever asked to see them. Only once did he ever call on me and that was just a social call in Salt Lake City. I was always willing to let anyone look at my types but I never loaned them to anyone, for the reason that Britton had refused point blank to loan me any Astragalus material in 1895, which necessitated my taking a special trip east in 1897 to see types, at great cost to me. At that time it was a general rule not to loan types, but this has been modified since then. I still feel that it is too risky.

ON DOMINATION

I do not wish my drastic comments on Brittonianism to give the impression

that I am fighting Britton. That is merely a convenient term for that type of standardization which was first put into concrete form by Britton, and used to hide his real intent, namely to dominate American botany. There never can be any real freedom, intellectual or otherwise, until men are relieved from the threatening domination of the higher ups; this is what I have been fighting for. We see the sinister results of this spirit in the somersault of L.R.Abrams in his second edition of the Flora of Los Angeles. No man with a particle of self-respect could ever have done such a thing. It is also evident in the monographs put out by students working for their M.A. and Ph.D. degrees. These monographstoo often represent not the opinions of their ostensible authors, but those of the professors under whom the students worked. I have often asked such persons why they do not take an independent stand and express up-to-date ideas. Their reply is always the same: "I wo ld not get the credits I am working for if I antagonized the professor." So everywhere we find servility to the detriment of scientific research. This I consider the most vicious tendency of today. J.N.Rose told me in 1895 that he considered it more important to have a chance to work than to stick for any special form of nomenclature. This was when the fight was on against the beginnings of B rittonianism. So as the years went by, he became more and more a slave, till he lost all sense of proportion, and his critical judgment was impaired.

The only objection to my criticism of the botanical dictators which I have received (I received many grateful expressions of approval) came from a woman who did not like me to write "mean things". I should be duly squelched, but, like the Irishman's turtle that still wiggled its tail after its head was cut off, I am "dead but not sinsible of it". I do not wish to be spiteful at all, but I do approve of frankness, honest criticism, and independence. I have chosen to attack the dictators by ridicule because they are impervious to argument. When people make so much fuss

about priority as they have made, and then deliberately adopt "once a synonym always a synonym", and brazenly relegate to oblivion all the names of families like Coniferae, Leguminosae, and Compositae because they are not founded on a genus, nothing but ridicule will get under their elephantine hides.

FREEDOM AT POMONA

Such domineering intolerance in scientific work is not necessary at all. Here at Pomona College, where Dr. P.A.Munz is head of the botany department, the scientific branch centering in Crookshank Hall forms a kind of patriarchial family, in which we cultivate individuality and the growth and development of personality, and frown on the subterfuges of society such as false dignity, etc. There is a sort of free masonry that is very enjoyable. The girls especially are addicted to doing their own thinking and expressing their opinions, which are genuine even if not always complimentary to the older heads. I have had enough experience as a teacher to know that any attempt to dominate people is fatal to progress. It is not quantity but quality of work that counts. To be sure, there is much waste in getting quality, but there is waste in all progress,- the scrapping of the effete. Probably this is the reason for the death of the aged. We get too hide-bound by our traditions and environment.

It has been my pleasure to meet in recent years many of the younger generation who are coming along slowly picking their way to efficiency in the school of hard knocks. Among them are Johnston of Harvard, a former Pomona man, Prof. Cory of Texas, Prof. Cottam of Utah, Miss Mathias of the Missouri Botanical Garden, and others. I have also found much pleasure in association with the members of the Los Angeles Nature Study Club, and with members of the Cactus Society which issues Desert, one of the best publications on succulents. In fact, I have been amazed at the number of capable women in southern California who are doing botanical work. So, in spite of the recent eruption in systematic botany, there is much hope for the future. There are many younger botanists coming along, with some of whom I have worked on intimate terms in the last few

years, whose places are yet to be made. Among them are Vesta Newsom, Martha Hilend, Elizabeth Crow, Helen Sweet, Tom Craig and Ray Fosberg.

If I were trying to be giving a fair estimate of the botanical work done during my time, I should have to go into the work of Trelease, Miss Alice Eastwood, Piper, Thornber, Conrad, Nelson, Rydberg, Payson, Pennell, Munz, Johnston, Hillman, Dewey, Holm, Elrod, and possibly others,-work which I shall never be able to do.

all copied Dr. William A. Setchell Can you add anything to this?

[Inasmuch as Prof. Jones had had a cut made of Dr. Setchell to put into Cont. 13 Cont. 18, it is evident that he intended to write something about him. I can find nothing, however, except remarks made in personal letters to me. When Father was in Berkeley at the University Herbarium for many weeks working on the identification of his Mexican plants, Dr. Setchell persuaded him to stay several weeks longer in order to dictate to a secretary furnished by Dr. Setchell the story of his life, especially of his scientific work. He had promised to write it out himself long before but never seemed to get around to it. Mrs. H.P.Bracelin laboriously took down in long hand his rather scattering reminiscences. She and I expect to work this over into some coherent order soon. Father thought a great deal of Dr. Setchell, often speaking of him as "a prince of a fellow", and a fine botanist, superlative praise from him. Since Father's death, Dr. Setchell has lost his wife. It would grieve him, I know, to hear of his friend's sorrow. M.J.B.]

Botanists Whom I Have Known —

5 C. G. Pringle — additional Remarks { Howlands — Wright. Purpa my criticism.

4. Dr. Geo. Vasey

#2 Photos of Gray, Torrey, Watson

3 Sereno Watson — additional Remarks.

6. (?) D. H. Hillman + unfairness in Dept. of Agric.

9 Free spirit at Pomona —

7. Rydberg — + Brittonianism

1. The Records of Systematic Botany — lack of information. I in middle period — Who gone — who still working — Watson Gray, Torrey, Nuttall, Engelm., Boott, Eaton

~~Systematic Botany — recently evolved — no univ. teaching it or life zones.~~

10 Botanical work estimate — those in my time — not take up — Trelease, Eastwood, Piper etc —

11 Recent, younger botanists — Johnson, Cory — Collano + Pomona grads —

8. "Mean things" + dictators. (Just before Pomona)

~~[illegible]~~

Gray.

Good to publish

P. A. Rydberg. On July 25 1931 there occurred the second tragedy in American botany, the death of Rydberg before his time, the least tragedy known to be No. I in the death of J. N. Rose. In my judgment N. L. Britton used these two men to put out before the public the Brittonian propaganda. Both men died disappointed, feeling hurt at the treatment accorded them by the botanical public, though their treatment was just for no one has a right to subordinate his own judgment to that of another, for financial support.

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He was almost as bad a splitter as Greene, but without the viciousness of that renegade clergyman. He did not go quite to the lengths of Greene, though he characterized most of his species on single specimens without taking into consideration well known ecological factors, and without attempting to collect intergrading material in the type localities of his proposed species. He had no conception of genus, and a very poor one of species, and no conception of the effect of ecological conditions on species. How anyone could be a botanist and neglect these things is a mystery.

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typed

Rydberg was inclined in his later years to consider himself persecuted. I

never knew that he had any personal enemies. Nor do I think that anyone ever got after him for personal reasons. I certainly never did, though once or twice I had some slight reason to feel that he had not treated me fairly in private correspondence, for once when I owed him a lot of material he scolded me for delay in repaying him in kind, and criticised some of my material by saying that it was "faded'. He did not explain how one can keep material for forty years and not have it fade. I had ample excuse for my dilatoriness because of the tragic death of my father and mother which put me out of botanical work for a year or more. But these things I forgot or overlooked. In one of his comments on my species he refers to having seen my material in order to strengthen his comment on it, but at the time he made the comment, he never had seen any of my types nor ever asked to see them. Only once did he ever call on me and that was just a social call in Salt Lake City. I was always willing to let anyone look at my types but I never loaned any to anyone, for the reason that Britton had refused point blank to loan me any Astragalus material in 1895, which necessitated my taking a special trip east in 1897 to see types at great cost to me. At that time it was a general rule not to loan types, but this has been modified since. I still feel that it is too risky to loan types.

In my drastic comments on Brittonianism I do not want to give the impression that I am fighting Britton, but this is a convenient term for that standardization which was first put in concrete form by Britton, and used as a motif to hide his real intent, namely to dominate American botany. I am a descendant of the Puritan fathers, and of the men who fought and won in the Cromwellian period, the fight for individualism. Brittonianism is simply a form of what is euphemously called standardization, and which reached its most repulsive form as a result of the world war. The purpose is to put in the saddle a lot of half wits whose objects are purely selfish. The first we see of this spirit is in the adoption of civil service, which assures government employees of the safety of their positions, a worthy object, but which in its administration has filled the departments of the government with fossils and political lefovers. These men in furtherance with the spirit have created state educational boards to standardize education, and which has put education in a rut from which it is hard to extract it. Again it has greatly enhanced the power of the [illegible]

and the contemptible [illegible] replaced [illegible] in Agrostology. One of [illegible]
botanical artists in the profession was F. H. Hillman Prof. of botany at Reno Nev. The best
He was another very capable man who was lost, submerged, in an [illegible]
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in the Depts. at Washington, but no one yet has had the courage to reveal
the persons responsible for swamping such good men. I have a very
strong suspicion as to the guilty persons. Nothing but a thorough investiga-
tion of government departments will right matters. Roosevelt went in with
the determination to correct things, but his efforts were dissipated in
fighting the money barons & politicians. There never has been to
this day a man in the Pres. chair pledged to a real house cleaning.
(Just as near Rydberg [illegible] after Britton) At Pomona college, where I am now in head of the botany dept.
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in Crookshank Hall, forms a kind of patriarchal family,
in which we cultivate individuality and the growth & development of
personality and frown on the subterfuges of society such as false dignity, etc.
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I have had enough experience as a teacher to know that any attempt to
dominate people is fatal to progress. It is not quantity but quality of
work that counts. There is much waste in getting quality, but there is
waste in all progress, the scrapping of the effete. Probably this is the reason
for the death of the aged. We get too hidebound by our traditions and environment.

[illegible]

It has been my pleasure to meet in recent years many of the younger generation who are coming along slowly picking their way to efficienvy in the school of hard knocks. Among them are Johnston of Harvard, a former Pomona man, Prof. Cory of Texas, Prof. Cottam of Utah, Miss Mathias of the Mo. Bot. Gard. and others. I have also found much pleasure in association with the members of the Los Angeles Nature Study Club, and with members of the Cactus society which issues Desert, one of the best publications on succulents. In fact I have been amazed at the number of capable women in southern California who are doing botanical work. So in spite of the systematic eruption in recent years in botany there is much hope for the future. I ~~have omitted names of~~ there are many younger botanists coming along with, some of whom I have worked on intimate terms in the last few years, whose places are yet to be made. Among them are Vesta Newsom, Martha Hilend, Elizabeth Brow, Helen Sweet, Tom Craig and Ray Fosberg.

Insert after "inadequate system".

. If I were trying to be giving a fair estimate of the botanical work done during my time I would have to go into the work of Trelease, Miss Eastwood, Piper, Thornber, Conard, Nelson, Rydberg, Payson, Pennell, Munz, Johnston Hillman, Dewey, Holm, Wood and possibly others, work which I never intend to do.

typed

Conte. #17 6-7

So far there has been but one discordant note in comments on this article by other botanists. A woman, whose name I suppress, for the moment, speaks of it as "mean" things" that I have written about men. Spiteful things are to be expected from women when you tread on the toes of their pets, but even they have a right to their opinions, even if they think the moon is made of green cheese. If telling the truth about people is "Mean" then I plead guilty. I feel that it is absolutely necessaty to call a spade a spade if we are to get anywhere with the "Push". They would shut our mouths if they could. So I am duly squelched, like the Irhsiman's turtle after its head was cut off still wiggling its tail, I am "dead but not sinsible of it". to attack them by ridicule, for they are impervious to argument. When people make so much fuss about priority as they have and then deliberately adopt once a synonym always a synonym, and brazenly relegate to oblivion all the names of families like Coniferae, Leguminosae, and Compositae because they are not founded on a genus, nothing but ridi cule will get under their skins.

The only objection to my criticism of the botanical dictators which I have received has come from a woman who did not like me to write "mean things"; I should be duly squelched but I do not mind to be spiteful at all, but I approve of frankness, honest criticism, + independence. I have chosen to attack the dictators by ridicule because such domineering intolerance in scientific work is not necessary at all. Here at Pomona College — etc.

push" in forcing out men with individual opinions, and creating an atmosphere of pussy-foot-ism. This spirit has also been used to crush the spirit of investigation in denominational schools, under the guise of Fundamentalism. As a result throughout our schools there is a timidity about propagating any new ideas without first getting the sanction of the higher ups. The professional staffs of most of our colleges are made up of moral cowards, who in the very nature of things can never become investigators whose results are worth the paper on which they are written. These men call themselves diplomatic, and suave, and other euphemous names, but at last analysis are simply cowards. There never can be any real freedom till men are relieved from the threatening domination of the higher ups, and this is what I have been fighting for. We see the sinister results of this spirit in the somersault of L. R. Abrams in his second edition of the Flora of Los Angeles. No man with a particle of self respect could ever have done such a thing. This thing is also evident in the monographs put out by students for the masters degree and Ph.D. degrees, which monographs represent not the opinions of their ostensible authors, but those of the professors under whom the students worked. I have often asked such persons why they do not take independent stands and express up-to-date ideas. Their reply is always the same. "I would not get the credits I am working for if I antagonized the professor." So everywhere we find scientific servility to the detriment of research. This I consider the most vicious tendency of today. J. N. Rose told me in 1895 that he considered it more important to have a chance to work than to stick for any special form of nomenclature. This was when the fight was on against the beginning of Brittonianism. So as the years went by he became more and more a slave, till he lost all sense of proportion and his critical judgment was impaired.

Typed

Early Botanists.

Madrona published recently an article on this subject by Jepson with illustrations. I think this is a most commendable thing. Dr. Nevin and Shockley are particularly mentioned, and cuts of them are given. One can see in a phto of a man very much more about his personality than he will ever get in any other way. It has been my intention for many years to print the photos of all these men with whom I have ever been associated botanically, so far as possible.

I met Shockley at the railrosnd station at Candelaria in 1883. In the brief conversation with him then he told me of Hecastocleis a new genus which I had just collected near the track, and of other finds there. He was then a tall and slender young man, very active, and if I remember right had light brown hair and blue eyes. His photo given in Madrona shows him with the form taken by men on approaching old age. I had some correspondence with Shockley later on.

It is to be hoped that those in a position to know will give reviews of the lives of such men as Dr. Hasse of Santa Monica.

Photos of Dr. Gray seem hard to find. At least those giving proper views of his face. No doubt there is extand a good photo of Torrey, for I see that there is a very poor cut of a statue of him in Torreya.

There is a fine engraving of Engelmann as I knew him in the frontispiece of his works. Though this does not show him as porpulent as he was.

In the Bulletin of the Southern California Acad. Sci. Vol. 30 Part 2 page 55 and following, Ray Fosberg, a Pomona College graduate proposes a rearrangement of certain Cactaceae. He should have consulted a Latin scholar before printing his work. Errors are as follows. spinae raro adunci, semini nigri, semini spadici.

For Dr. Engelmann See Desert Cont. 16 Card

Printing.

For many years I have had difficulty in getting my printing done right. There has always been the cry that my stuff was techinca and required extra care and time to set. and I heve been charged as much as four times the usual price for setting it., particularly after the advent of the machi The typesetres were always glad to get my ztuff since it gave them a chance to learn to set difficult mattes. but they wre always glad to charge me plen ty. for giving the chance to learn to set my stuff. This became so exasperating that I vowed I would not submit to it any longer, amd so I bought a press and type to set my own mattes., and my Astragalus was practically all set and gotten out by myself.

In 1930 when I got out Contributions No. I7 I had to get it out in a hurry in order to get off to Mexico, So I told the employing printer who got it out for me that I knew he would hold me up and that I would take my medicine but that in the future it might be different and that it was not a personal matter at all, and that I knew they were all ham-strung and hog-tied by th the typothetae and did not dare to charge what the work really cost according to the usual scale of prives. Since the Los Angeles holocaust the public seems to have settled back into the consciousness that the great fight of the Times against this guild has resulted in the independence of the employing printere., but the real facts are that the public is being victimized more than ever by the typothetae who ought to be called typodestrae They are the most reactionary guild in America and thaough all other prices have declined since the world war theirs have not., but they are still exacting the pound of flesh as usual, It is an outrage that should not be tosserated When my printer and myself were discussing the subject he tried to show me that he was not asking more for my work than any othet printer would do. I replied that it was not a personal mater and that I knew they are all ham-strung and hog-tied, and had to do it. and had no independence about it but did as they were told. and that I would submit to it only as long as I had to.

and ever since have been steadily overcharged for both and with this.

The end of this suit was a sell out by the Doctors employed by the farmers. The jury directed by the referee.

I had previously refused to have anything to do with the suit because one of the farmer's wives I considered a crook but in a critical part of the trial conditions arose requiring consulting me and I was urgently requested to join the case but I did go out and visit the Coompin ranch with their lawyers one day and tell them what I thought certain damage was that was done one night, my report was that it was frost and not smelter smoke and I was again importuned to join the staff, but refused unless I could be paid what the referee got that the damage was not smoke as testified to previously. This was the only known case of frost damage to farms in the smelter area. The people won the case, but by the double dealing of the lawyers their victory has been largely nullified, and during the past winter there has been a great outcry against the smelter smoke. but the farmers Attorneys are doing nothing to compel adherence to the injunction.. and now that Ledyard is gone the trouble is liable to come to a head, for there is now no respondent in the case.

Ledyard's attitude seems to have been that since he was the plant expert of his company he was obligated to to minimize the plant damage done to the vegetation by the smelter and to lay on insects the damage done to plants so far as that was possible, but he never seemed to care to go into the insidious damage done to vegetation by smelter smoke. This whole attitude of some scientific men is unscientific and immoral, and savors of commercialism. I can see no excuse for it. In the courts experts are supposed to be employees of the court to inform it of the real facts in the case and that is the only reason for the use of experts in courts, but to use us as moral prostitutes for the establishment of wrong is infamous. and it is inafmous for any scientific man to take employment for any such purpose.. a concrete example of it is the case of Ruth Judd murder case at Phoenix Arizona where a woman was to be hung for the confessed murder of her associates where certain doctors testified as to her insanity. There is no doubt that she was a dope fiend and killed her friends in a fury of jealogy over Halloran a cattle man from Galt

Lake City. The foul laws of the stateon insanity were at fault. for there is no doubt that the woman knew just what she was doing when she murdered the them and did it intentionally and was as guilty as any drunk would be in the same case., and so she escaped the gallows which she richly deserved.. it is a common practise to employ sicnetific men to dirtort and misrepresent eh tru truth in lawsuits. and it is the general opinion of lawyers that scientific m men are all moral prostitutes.. ,Ihad an illustration of this inmy first case in the courtsI tstified that I was a mining expert . The oposing attorney Arthur Brown the most experienced lawyer in the Citywho afterwards wa shot by Mrs Bradley a paramour of his.. He said you are a mining expert are you ? There are liars admned liars and mining experts. The Court A carpet bagger fr from the south Kentucky Smith. alughed at the sally. This made me very angry and I made up my mind I would roast the attorney for it if I ever had a chanc but the chance never came,for he was sent to the happy hunting gronuds of th Devil before I could get to him, which I very much regretted.

I am not making these aspersions on scientific men to besmirch Ledyard whp was a personal friend of mine, but toregister my hatred for this atti tude of mind that some scientific men entertain. A certain acting head of a great university who was on the montana smelter case with me and who was recommended by me as referee in the great Utah case, entered the employ of the American Smelting co. and in the case in point laid all the blame for smoke damage on the U.S. Smelter Co. which company was the only one that really tr tried to abate the smoke damage, and did reduce it to a minimum and he conce cealed the damage done to the farms by his own company and considered it a smart pieve of testimony. Whether he was at that time under secret employ by the company I do not know, but he has since been in its employ. I made it a rule early inmy life not to go on the morally wrong side of any case at law. nor to let any person lead me to distort my testimony., and I have never regretted it. I felt that the approval of my own conscience was more to me than any money I could get from employment.

Many years ago the country passed laws against perjury, but scientific men are so adept at distorting the truth that they have escaped punishment.

friend
A firned of tedestrom's in tryignto defend hima gainst some of my critcisms sates that he is not a young man, and that he was once an asistant of Greene and that Mrs Chase was the author of the Grasses in his Flora of Utah and Nevada, it is no excuse for a persin getting otu a flora that he has left part of it to be done by others, for he should edit all su ch work with car and see that they don not loaf on the job. tha blunder of leaving out Eremochloeis still more criciable because King's report of the 4oth par allel must be consulted constantly in getting out such a flora and it is th there that the genus Eremochloe was proposed by Watson. All men who have come on the field after 1890 I consider young men though they be gray haired now. it is no recommendation for a person that he once worled with Greene,

Wooth and Standley also left out of their Flora of New mexico all mention of Eremochloe Bigelovii which was discovered in new Mexico,

There has been a tendecy to ignore the work of Watson on the 4oth. Parallel by the Dept of Agriculture since 1893, and this may be the excuse in ignoring Eremochloe , but it is not to the credit of anyone working in the West to do such a thing. Watson was preeminently the greatest ecological botamist of his time and no one can ignore him without besmirching himself.

The botanical world does not care anything about any particular university nor system so long as it appeals to reason abd plays the botanical game on the square, No doubt people had some reason to resent the dominance of Harvard when Gray was alive but I see no reason to carry this on after his death as institutional jealosy. I never was a Grayan partisan and never would follow blindly the lead of anyone.. I would like to see Harvard complete the work of Gray but I would not tolerate any attempt to interfere with my rights in anything. I have lent material to Harvard for study, but I would not lend material to anyone who was a confirmed splitter. I did lend Aquilegia material to Nelson for Payson which act I have always regretted. for Payson never had any proper conception of specific relationship in the genus. and he made and recognized a lot of fictitious species. Hairsplitte may object to my use of the term species. by saying that species is the same thing as it exists in nature. No doubt this is true but the botanical world

does not use the term in this way nor do I.

F M. Ledyard.

In the recent death by pneumonia of Prof. Ledyard in Salt Lake City I feel that I have lost a sincere friend. My first acquaintance with him was when I was an expert witness in the Bonanza smelter smoke suit against the American Co I noticed a man in the audience following the testimony closely and showing in his face his approbabtion of the hits made against the opposing counsel in the Cross-examination. After the session was over he came up to me and i introduced himself as the expert of the U.S. Smelting Co. and he warmly approved of my position in the case, and advised me to prosecute one of the o opposing firm who attacked me physically after the trial, my reply was "No, violence is a confession of defeat." Some time later he came to me and asked me as a personal favor to go out with him to his smelter and look over the field and tell him as a professional courtesy what my real opinion of the situation was. To this I agreed, and I went to some dear friend s and told them of his request. Their advice was not to go for they had some kind of a trick up their sleeves to get me in bad., My reply was that I feared no man getting me in bad on anything and that I was going. I later on found out that he had told the head men of the smelter of his plan to take me out and they advised against it saying that I was a very resourceful man and would get the better of him. I had then been fighting the smelters for 15 years and had beaten them in every suit, and shut up one of them at a cost to the of half a million dollars. When the time came to go I ws ready and he came and we had pleasant visit on the way and went all over the field and saw no sign of smoke damage till we were passing a wheat field where I suspected damage and asked to stop and look. Sure enough there was damage and I sh showed him the effects. Whether he reallu thought his company was doing no damage I cannot say but it looked that way., I took him at his word that it was a professional matetr between us. Near the close of the visit he told me that he usually turned in his car and sold it in the fall and if I wanted to buy he would sell it to me cheap for $200. my reply was that I always wanted a car but could not afford to buy one but would be glad to take hi him up in the fall if I could spare the money. When the fall came I went to

him and said I would take the car if he would be easy with me on the terms. So we went out for a ride in the car and he told me he would have some work done on the car and would sell it to me for 150 dollars, Indue time he came with the car and put it in my barn, so I paid him $75 down on it and he sa said I could have all the time I wanted to pay for it.. Later on he asked m me to do some professional work for him for which I would make no charge but he insisted on paying me and charged off the entire debt to his company for the car. When I had paid him in full and received a receipt for it I told h him I considered myself greatly in debt to him for selling me the car at so low a price but that I considered myself under no obligation to his compan for Howat their lawyer had done me thousands of dollars of damage for which i held his company responsible., his face fell but he said nothing. I wanted it understood between us that I did not consider the selling of the car as a bribe to shut my mouth. In the winter of 1931 I was visiting my son in Sa Salt Lake and I called on Tedyard as usual. We had a pleasant visit and in the course of the conversation he asked me to furnish him an article on the early days in Utah. I refused for lack of time to write the article, but he offeered to furnish an steno to take down my dictation, and so it was arranged and the young Mormon girl and I had a session in his office the next day which was Saturday and I was going back the following Monday to California. I was not satisfied with the article and wanted to revise the M MS. before publication. He would not agree to it but agreed to go over the text and fix it up right where there might have been an error. He was then getting out a company magazine caled Ax-ident a very high classone. and was wished to print the article init. Sunday he called and wanted me to go out w with him and take my photo for illustration, and he gave me the copy of my diactated article saying that he had had the girl get it out at once and do special time on it. In due time I received the magazine and with it the printed copy of my dictation. In looking over it I found he had edited it pretty strongly in fact had actually distorted my statement in one particular and had made me say just the opposite of what I really did say. where I took occasion to criticise the work of the founder of his company Bert Hold

Bert Holden a former pupil of mine whose geological theories of ore deposits were not those accepted by geologists., but created to help his company ~~ho~~ hold on the lands they were not enttled to own. I wrote Ledyard that he was a peach as an editor. He replied acknowledging my compliment as though it was not a sarcasm and thanking me for it as a geat appreciation for which he was grateful. He knew well what I referred to. but took this way of sidetracking me.

In a late issue of Ax-Ident is an appreciation of Ledyard anonymously written in which they say he would never compromise a conviction or a principle to gain an ally or effect cooperation. My experience with him shows that he would distort the truth to prevent its coming out. evidently on the ground that it was all right to create an impression not intended. to shield his employers. I have always held that the truth is the truth and must be uphwld at any cost. and that you must not sheild your employer to help his case. This has led me to be sidetracked in trials at law where I would not warp my testimony to suit the law. It is no mamtter to a scientist what the law is, the main question is what is the truth. Ledyard did the farming public a great good by inventing his weed killers in using the arsenic waste of his smelter. , but he oversetimated the damage done by insects and fungi to vegetation. and in his testimony in the big injunction suit against the Salt Lake smelters he took an untenable position as to smoke damage. He once told me his comapny would never acknowledge doing any damage to crops in Utah . He was always softpedaling when it caome to smoke damage. It is my opinion that he considered himself the guardian of his company and that his softpedaling was a little too soft at times. though he may have compromised his conscience with a little too much emphasis on other things. He was a sincere friend and a tireless seeker after facts.

His company was the only one that in the early days made any sincere attempt to do away with the smole damage and they reduced it to a minimum [illegible] the great injunction suit should have received the greatest commendation instead of having all the publicity of future [illegible] that in the decision rendered by the [illegible] court.

Cockerell in Torreya for January 1932 suugests two reforms in nomenclature, one of them being the dropping of parenthetical names, and using only the name of the person who last got the species under the "correct" genus. In most case the one who first described the species did so in the "correct" genus at that time. Then came along a lot of egregious splitters, among whom Cockerell is on of the worst, and they proposed new names for old genera, and therefore the transfer of the old names to the new genus with Cockerell on the tail of each. He speaks of the keeping up of the name of the original describer as "personal advertisements" omitting the pertinent fact that the preservation of the name of the last juggler of the species names is a still worse form of personal advertisement. It is true that the use of the name of the describer is more a matter of botanical history, and could be dispensed with by all but the professional botanist. I have always advocated the omission of the author's name on species and genera, and have practised it largely till now. BUt I am confronted with a a new angle to the case. During the last year some unknown botanist attempts to review my Contributions No. 15. I never send my publications to any journal for review, for the reason that these reviews are mostly perfunctory hack performances by some one with an ax to grind, and cannot be trusted either for accuracy or fairness. He find fault because he cannot determine whether I am giving new names or using old ones for certain species on which I publish note though the suffix n. sp. put to every new species proposed ought to be enough for a botanist of any intelligence. SO I guess as a matter of self protection I shall have to revert to the old method of putting the name of the autho on any name I use, but to me it is an intolerable nuisance.

X

The Agassiz Association. Paper marked X each word printing

The recent death of Prof. Arthur G. Leonard at Grand Forks N. Dak. brings b back a flood of reminiscences. In ehe early eighties he and his brother and Walter Nichols a grandson of of Mr Hammond a wralthy busnness man of Chica go, the creator of the famous New West Commission, came to me one day and said they planned forming an Agassiz association chapter in Salt Lake and wanted me as advisor. The father Rev D. L Leonard head of the Home Missiona ry socirty of the Cong . church in Utah was the father of the boys. So they met at my house every other week and I put them through Roberts rules of order to ena ble them to preside at meetings. Fred Leonard took up botany and later on became Prof of athletics at Ober lin Ohio, Arthur took up ge- ology. I took the boys through the mountains of Utah afoot, and we had many fine campfire meetings together. There were eight boys in the chapter and among them Fred and Wesley Browning. Fred was a cartoonist, and Wesley an artist. They prnted a monthly paper and illustrated it with their own cartoons.. We sailed all over Great Salt Lake taking soundingsand explorin the bird islands.. We were caught in a great storm in a frail boat not made for the heavy seas of the water, and the rigging was almost blown out of the boat. I was the skipper and found the breaking of waves over my back at the helm a great discomfort. All but two of us were seasick, and we had great fun picking fun at the sivk ones. Later on we were in a great calm a the heaving swells did not reduce the sivkness. as we rowed to shore.

This experience led to a discussion between me and one of the boys abou the orper kind of a boat for the heavy waters of the lake. I held that no boat worth while could navigate the lake well without being sharp- e the bow. One of the noys held that a spoon shaped boatwas the thing in to slip over the waves, and he offered to pay bor the material if I wou bulid a boat according to my ideas and he would make one of his own. So we built our boats, mine was so ahrp that it had to carry ballast or outrigger, and si I used an outrigger. At last the boats were built and Walter took his out on the lake in a storm and could get nowhere with it. Later on I took mine all over the lake and made a great trip.

Ecologivcal Terminology

Some years ago there was launched on the botanical public a series of ecological terms that has passed into common use by teachers of ecology So Far as I know these have never been subjected to any serious inspection by linguists. It was assumed by botanists that the authors of these terms were linguists or had consulted linguists about the vomposition of the terms suggested, but it seems thatthey were invented by incompetent men assuming knowledge that they did not have. Recently I have been asked to lecture on this subject and it has been my lot to look up the origin of the terminology., This was brought about by the foolish terminology of the Britton & Rose terminology in the Cactaceae of North America.. It is a well known fact that Rose was no latin nor Greek scholar. But it was assumed tha Britton had some literary training, which assumption seems to be erroneous The latins and Greeks had an ear to harmony of sounds, and they used to so construct their new words and combinations so that two vowels would not com together in a word, or if they were alowed to do so it was as a diphthong. It was also the practise in the formatio of new words to take the root of adjectives and nouns which is best shown by the genitive ending after dropping the genitive sign,. This was the universal usage in both languages.

An illustration of this would be in any combinations of the word pes foot The gentive ofped is pedis. and the genitive ending is is this leaves ped as the root. of thw rod which must be used in all combinations. so So the word expedition which means literally walking out was made by adding ex out and ition a term for the state of or act of walking out. and from this we have in many English terms derived names. such as expeditious, etc. When the root ended ina vowel this was combined with the word added to it t 5o form a new combination of was dropped alto gether

Nowbtaking up ecological terminations. we findZerophytes halophytes and calcophytes. The term Xerophyte is taken from Xeras and Phutos. the fo former meaning a piece of rocks and the latter a woody plant. The root of Xeras is xerad and not xer as asssumed by the one who created the name xerophyte therefore any combination of Xeras would be Xeradophyte.

taking uo the name halophyte we find that the Greek word for salt is alas witht the rough breathing and the root is alat so any combination such as

halophyte would have to be halatophyte or alatophyte according to thr whim of the creator. The latin term for Salt is Sal, evidently borrowed from the Greek by using the sibilant S in place of the rough breathing and omitting the last letter of the root., and from this we have a host of derivatives s such as salinesaleratus, etc. Therefore the creators of the terms Xerophyt were in competent grammarians, just as was Linnaeus in the gender of Erige ron and followed in his error by Gray. Gray and Watson also blundered in th genus of Cotyledon, but Linaeus had it right ijn making it feminine.

taking the term for the flat opuntias negelmann who invented the name gave it correctly as Platopuntia, but Britton and Rose come along and try to improve it by calling it Platyopuntia, Engelmann was more nearly right in omitting the Upisolon in the combitation, for there were many examples in the Greek for his use , and few for the other. towns

it is one of the rules of Latin that the names of countries , islands, and trees are feminine, andsoso the name Pinus is feminine and all names of Pin us should be feminine.. It is also true that the name cola meaning inhabita tant is masculine and must not be changed for difrenent genera to wgich it is attached. Following the rule against hybrid names pinus monticola is unallowable because it is a hybrid name, of mons and cola, but the latin were not particular about such combinations. of ten using hybrid combinat tions

Insular Floras.

It has been the recreatipn of certain young botanists and amateur geologists to guess that the life on the islands off the coast of California is due to a survival of a floraand fauna migrating from the mainland by a land connection in recent geological times. This guess is the result of finding mammal bones particularly elephant on the islands. and from the assumption that such animals would have to walk to get there. There is one factor in the matter that has been overlooked by the guessers. and that is stratigraphy. it is a fact we well known that when the sea is at any time washing a shore that it breaks d down the shores into sea cliffs of varying magnitude according to the time ta ken in the washing.. It is also well known that the emergence of soild mase ses from the Ocean partivularly sedimentary ones is slow , and that the formation of sea cliffs is an attendant of such emergence. If at any time such cl cliffs were produced the soundings in the channel would show itin submerged beaches. if there has been any marked subsidence of the islands since then. I have searched in vain for any evidence oc sea cliffs past or present on the islands enough to account for such a state of affairs, Ther are no ancient beaches to amount to anything on the islands nor is there any evidence of erosion in the soundings as there would be if there had been a land connection in the past. It is a well knwn fact that theCoast range is an offshoot from th ancient the seashore caused by the buckling of the sediments from a lateral thrust which fact has caused all the great upheavals of the past. An attandamt phenom enonhas ben the outnurst of coast volcanoes. and the flow of igneous magmas The geological history of our coast shows both pehnomena were in evidence in the production of our islands. particularly the volcanic, in subsea flows. Now in the face of lack of stratigraphical evidence we must hunt for some oth other sause for the existence of mammals on the islands. InUtah in the early years There was an island in Great Salt Lake that was cut off at times from the mainland by a rise in the lake At such times it was proven that deer swam the water connection to get on the islands . It is the remark of old time Mexicans that Deer have been found swimming from Natividad island to Ce

Cedrus island where deer are now found ~~on the island~~ at Venice. Very recently some boys found a deer out in the sea, that had been frightened away from the land by dogs. It is a well knwon fact that the saber topthed tiger was a mortal enemy of the elephant and killed them in large quantities. and it is no great stretch of the iagination to assume that the elephants were driven to dea by the tigers. and since the islands were in plain sight of the mainland they became the refuge of such as were able to swim across. and that they also then swam acros to San Miguel ~~also~~ from Santa Cruz. That there never were many such animals reached the islands is shown by the gradual extinction by interbreeding of th the herd on Santa Cruz. The evidence of a suitable forage for elephants was s shown by the contents of the great elephant found in the ice of Siberia where leaves of evergreen trees were found in the stomach. It is not necessary to assume the presence of a Tropical flora in California at that time. nor is there evidence that it differed much from that of today.

Had there been a land connection with the islnds at any time the islan flora would have been the same as the mainland, and the fauna likewise. But o on the islands we do not find the native squirrels nor other mammals particul larly the carnivora. though there is some evidemce of deer living on the islands.

At a meeting of the Sierra Club one evening on Santa Cruz island stated my belief of the falsity of the prevailing theory about the islands origin and a local geologist got off a sneering remark about theories of new comers but so far a I can find he has no evidence of any value about the mainland connection. It is easy to guess the origin of things but these theories must conform to the facts of geology. and though guessing is ht privilege of the palaeontologist it is no evidence worth considering. in the face of certain indubitable fats .

Citrus fruits.

It is a well known fact in California that lemons ripen all the year and are in bloom all the time, and that we have two crops of oranges according to the kind grown. The navel orange ripens in Decem ber and the Valencia in June. The navel was introducedfrom South America whose seasons are reversed in comparison with this country. Under normal conditions the navels bloom in the spring , bht this year there was little bloom at the proper season and now in the fall the trees are in full bloom. it was tought that it might be due to neglect of proper irrigation, but the condition is almost universal and it is a question whether the plants will ever get back to normal again. It seems to be a rule of nature that plants shall bloom and fruit by seasons but where growing conditions prevail all the year nature seems to slip a cog with some species. In centralMexico we often see corn in fruit at all seasons of the year, but in this regiom where we live introduced plants seem to hold their ancestral conditions well. The American elm persists in shedding its leaves first of all plants and going to rest early in the fall anf then comes out the first of all plants to bloom in the sprimg.

Other intrpduced plants seem to try to keep on schedulesuch as the maple, maples and cottonwoods but the sycamore does not seem to know whether to shed its leaves or not. though our native species has the same fault. The pear and cherry are natural evergreens and hold their leaves late. In fact we have a native cherry, Prunus ilicifolia, that is here an evergreen.

Growth rings. In the New England states growth rings are the same as annual rings, and by them it is easy to tell the growths of Trees many years past and to estimate the normal rainfall by the size of the rings. in fast growing trees. This holds good til one gets down into theregions with Tropical climates where growth is a mater of rainfall and not temperature as it is at the north., In the arid Tropical where there is more or less rain in the winter, and where there is heavy rain in the summer growth rings are formed at the beginning of each resting period and the size of the rings is determined by the lenght of time elapsed since the beginning of of a wet pwriod.. Over zealous archaeologists have begun cal ling these growth rings "annual rings" and have computed the time elapsed by these rings. It is a well known fact that fast growing trees like the cottonwoods make large rin rings annually, or rather seasonally but ehs lower growing oaks and juniper make narrower rings.. It is an easy matter to count the rings , but it isa matter of great difficu ty to masure the width of such rings, and even afte that is done one must allow for the individual equation in each tree for every tree has its period of youth maturity and old age and the rings vary with time. If a person were to make a cross section of atree andmake a graph of the various rings according to the width of the section and make an accurate representation of the measurements on the graph he could not take any other section from the same tree and make th two graphs track Then a n injury to any park of the tree will slow up the formation of a ring at that point. Then it is a difficult matter to tel exactly where one ring ends and the next begins and is usually an estimate, and where the com piler has an ax to grind he isliable to err in his judgment. Suppose al/ person takes engineers profile paper and makes a graph on a scale of ten to one his measurements must be correct to within I/I00 of an inch a millimeter a thing that is practically impossible

Then when the graph is made he must compare it with a graph made from th known rainfall records of the locality. The generaar contour of the two graphs should track, but when without records with which to compare his gra graph he has no means of telling what his graph signifies. with accuracy.

For where ther are two growth rings formed in a year

For where there ertwo growth rings formed in a yearht will count them as one year and there there is but one ring formed due to lack of rainfall during the year or of rainfall he will be at fault .

The best way to make the sections is to cut thin ones with a band saw and then sandpaper them down by clamping sand paperon a circular saw and then star starting up the saw and pushing up against the saw till smooth It is a tedio tedious job to sand paper them down by hand.

Fog

I have noticed the various behavior of trees in the hravy fogs we have had this year. The pepper tree drips water so much as to be wet underneath on the the groundthose less so but also dripping some are the royal palm and Washing tonia, peach trees and black acacia, and sycamore. deciduous trees not at all t to speak of. also conifers.

Olive Growing.

Olive growing like avacado growing for a profit in California seems to be a frost, since the trees bear very little fruit as a rule, but this year the trees were a mass of bloom in the late spring, and many fruits set so that th the trees are loaded with fruit this year, bearing much more than in the last ten years on the relics of former cultivation, since the tres were mostly upr rooted for citus fruits.

Greek Names.

When one looks over botanical history he finds that the craze for outlandish Greek and Latin names is not over 200 years old. Just why and how it started would be hard to find just now. But it undoubtedly started in the fad used by Linnaeus and his time to give names to plants that would be universally recognized. At that time French was the court language of the world, and Latin the one used by scholars. Greek never was considered a universal language, though at the time of Alexander it had a chance to become so. In the last thousand years there never has been a really dominant nationality, but today the English language is by far the most used of any on Earth. Scholars are notoriously laggards in social progress. The grammarians of all races have been the executioners of their own tongues. The riff-raff and hoodlums have always been the makers of language.. When a language gets so inflected and loaded up with fine distinctions by the grammarians it begins to die. The Sanscrit was the most complete language ever invented, and the one most capable of the finest distinctions , and one of the first to die. The tendency of all language is toward monosyllabism in the ordinary affairs of life, the Chinese being the one that has gone farthest in that direction.

It was some two generations ago that men of science began to give extensive names to plant and animal life. This was the climax of classical scholarship, and a time when a man showed to a gaping world the fact of his education by the facility with which he could sling Latin and Greek. For some reason, probably not to appear as tagging along after the botanists, the zoologists, began to show their great (?) scholarship by inventing new names culled from a English-Greek, and Greek-English dictionary. in order to name new genera and species. The two most glaring exemplars of this fad were O.C.March and E.D.Cope. There is no evidence worth considering to show that either man could read the simplest Greek with any facility, but the less they knew the more they inflicted atrocious names on the scientific public. They spared us the additional affliction of making their descriptions in Latin, probably because they could not write Latin.

No douth the present revolt from this asinine terminology in using common English names for birds is in part due to this foolish attempt to air what they did not know of Greek..

But we are not out of the woods yet, for there is a considerable following in Europe still to force young botainsts to write their descriptions in Latin. And our American botanists have not had the courage to stand out against it, even in the last botanical congress. To me it is a self evident fact that English has become the world language, and it would be far better for us English speaking people to hold out for it than to tag along behind like a flock of sheep. It is a convenience to have descriptions in Latin instead of Irish, Norwegian, Danish, Russian etc. languages which most of us cannot read, but this handicap could easily be remedied by the botanical abstracts translating into English such descriptions. and printing them thus as a part of the abstract. It is a notorious fact that few botainsts of today can write Latin at all, and have to enlist teachers of Latin to help them out, men who have no sympathy with botanical reaserach, and who often do not know the fine points of Latin themselves. An example of this is in Jepson's flora of California where he invents impossible Latin names and informs us that they were supervised by a professor of Botany there.

In a previous Contributions I referred to an egregious blunder in Coulter's Flora of Texas, tending to show that Coulter never wrote the MS. of his book. For the benefit of anyone desiring to look the matter up refer to page 434 Of his Flora; He states there that Allium bisceptrum Watson grows in the "Trinity to East Humboldt mountains along mountain streams". Just below he makes the same remark about Allium Nevadense. Now the fact is that neither species ever was found within 1000 miles of Texas. The person who wrote this part simply copied Watson's description of these two species as given on page 351 of the report of the botany of King's Expedition, as to locality of occurrence, omitting the remark about its growing in Utah. The writer of the Flora did not know that there is a mountain range in Nevada called Trinity and this was the one referred to by Watson in his monograph and not the Trinity mountains of Texas.

Financing.

For the sake of giving due credit and correcting misinformation I will say that my trip to Mexico in 1926 was financed in part by the Los Angeles Museum, the rest by myself. The reason for my taking up work in Lower California was due to a suggestion of Mr. Bryan of the Mseum who sponsered this trip. He also had ordered the trip of 1927, but a changes in the financing of affairs of the Museum made it necessary ti countermand this order at the last minute. Pomona college then stepped in and half-financed the trip, in the place of the Museum and I put up the rest as usual. The trip of 1928 was financed in part by the U.S.Dept. of Agri but after the year's work was done I decided that the terms of work were too onerous and refused to continue the work. The trip of 1930 was half-financed by by Pomona college (Miss Scripps), and the rest by myself. The college also aided the trip to Texas in the spring on the same basis. To date the trips to Texas in 1931, and the trip north along the Coast and to Oregon were financed by myself entirely. Most of the financing till 1931 was accomplished through Prof. Munz who was instrumental in getting the money.

The fact that I sold my great herbarium to the college in 1923, and the Faculty of the college having put me on the Faculty as "Honorary CUrator of the Herbarium", have led others to think that Iwas connected with the college. But I have never had any official connection with the school at any time, and do not expect to have. The college has benefited by my work in the last eight years by my having deposited my types in the herbarium, but this has been a free-will affair on my part so far, since I have had no intention of starting another hebarium. As to what I may do in the future is another matter. I am under no strings to anybody.

Changed + condensed, See 15: 131

The Cosmos.

Many years ago in Salt Lake I was asked to deliver a series of lectures on geology at Salt Lake College. At that time I was working on the specific gr gravity theory of the origin of the Earth form, and in the course of the talks gave out some revolutionary ideas of Earth formations, which caused the principal of the College to fail to notify me when the next lecture was to come and so the course was interrupted. Since that date this matter has been taken up by various men and much that is contained here has been anticipate

I held that the earth formation was a matter of specific gravity largely and that the heavier and slow chemical metals were gathered in the center and that the reason for the relative absence of gold and paltinum was a question of specific gravity and slow chemical action and that these two elements formed the center of the Earth., and that the relative presence of lead and Mercury was a matter of volatility. though they were also heavy el elements.. I might also have put Uranium in this class.

Recapitulating these ideas I willbring them out in due time as the discussion progresses.

Recent electrical experiments are leading to the assumption of a primordial substance, self actuating and self propagating which the ancients hsve called matter and the actuating element God, and which has been eternal. There has grown up an idea of God, as the creative element in nature. Omnipresent omnipotent and omniscient., directing all phenomena.. This is the Christian theory of the Universe. We find in this primordial element tw two selective agencies one constructive and the other destructive. These two are called by religious people God and the Devil, and around them have been built all the theories of humanity from the Egyptians and Mayas to now. This was polytheism. Then came the Jews with their monotheistic God.and malevole lent Devil and their inspired Bible full of scientific and social absurditi es. on which has been built Christianity. leaving the religions for the present let us return to the primordial matter. research seems to have post ulated the creation of all substances from this matter by selectivity.

Cosmos.

whether internal or external matters not. but all religion hangs on it. we do know

whether self perpetuating or not science does not say but all observed fact indicate that our universe is running down, and will ultimately die.. The primordial mist bears out the Christian view expressed in the Vible " and t the Earth was without form and voidand darkness covered the deep". Then th this primordial mist began to disintegrate, or rather to integrate into other forms and this produced heat and concentric motion.all the elements of which were in a gaseous state during the production of the various elements and remained so till the loss of heat by radiation had caused condensation of the elements into other forms. of liquid or solid matter. where specific gravity brought the heavy elements together, (those not affected by chemical combination such as oxidation) such as gold and platinum. while the other elements oxidizable and therefore lighter formed the surface all in a fluid condition at first. and affected by the force of gravity such as tidal changes caused by attractiond of heavenly bodies. and the consequential disturbances of equilibrium causing the invasion of the lighter surface by the heavier elements. and the formation of mineral veins.particularly of silver and gold. platinum, in combination with sulphur and chlorine and bromine. and he acids formed by the union of oxygen and hydrogen and sulphur. nitrogen, chlorine, That the formation of the acids was subsequent to that of of the elements seems assured. and possible only on a cooling globe.

It is evident that the sun is today a glowing globe enveloped in an atmosphere of glowing metallic gases which condense into drops and fall on th the surfaceas rain and are again expelled by the terrific heat into gases a again andform the terrific cyclonic storms that disturb the surface that is visible to us, That any of the acids could exist as such is doubtful except where the temperature available temperature of the external envelope is low, All the evidence ava available to usleads to the conclusion that acid action was not possibletil till long after the metalic gaes had condenzed itno solids bu the loss of h heat by radiation forming a solid surface.. Chamberlain's theory of the Ear formation by planestesmal accretions does not seem to fit the known struc ture of the Earth, nor is there sufficient evidence of external accretions in fact there is no evidence of such iin the s surface rocks of today.

hat the surface of the moon is pock-marked by craters of impacts with heav enly bodies. is manifest to any observer, but we have no evidence on the arth of such impacts. The theory of some that the moon is the result of a collision of the Earth with a heavenly body cutting a slice out of it and forming the Pacific ocean seems a pipe dream. If such had been the case hydrostatic pressure would long ago have obliteredted any evodence of such a collision. That there has been a collision of a satellite of the Sun with a heavenly body which formed the leonid meteors seems assured, for the collision blew it into fragments and forced them into the orbit of theEarth. where we how pass through them in November.

Goinfg back to the precipitation of the metals on the Sun we find them taking the form of a globe through rotational forces before and after condensation till the surface had cooled below the vapor point of water. when the combination of Oxygen and hydrogen into water produced the atmospheric conditions of today. and when oxydation and erosion began as we find it to day. Then there followed a condition favorable to life as we know it. and life began. How no one knows,but the fact is evident. The rocks show that animal and vegetal life came in about simultaneously.. In the course of eve events Man finds himself among the various forms of life, and in his struggle to live he finds elements favorable and unfavorable to his existence, the favorable he calls good and the unfavorable bad, or evil. Then he begins to try to adjust himself to these influences by assuming them to have minds like himself. and he idealizes them, he finds the Sun the great good and he makes it his supreme God.. Then he finds cold and heat wind and water harmful or beneficial and assigns to them attributes human both good and evil, Then he idealizes them still more by making images of them and worshipping themas gods. This is polytheism. His leading minds evolve a system of worhsip according to their capacity to influence their brothers. thinking, and then the system of priesthood comes into being. through medicine men who fabricate all sorts of theories to enable them to hold the faith of their brothers in their influence over the forces worshiped.. The selfishneess of these menled to human sacrifice and the propitiation of the gods by sacrifi

Then the Jews come along with their theory of one God and one devil and th they adopt such part of the ritual of the nations as seems best established among the nations. They adopt the morals of the world of their day. The ~~ca~~ idea of sex relations and of sacrifice and of priesthood. and invent special the theories of their own. such as the ten commandments . They inculcate the worship of the one God and his sanctity by the laws against profanity, and sacrilege and the propitiation for evil by scrifice and the shedding of ~~the~~ blood for the sins of the people. which is the basis for the atonement a very stupid religious conception. and they bulwarked their Jews by the assumption of the inspiration of their religious books. and special revelation from on high. and by their selection by the Supreme God as his chosen people, and buttressed this by repeated revelations from their priests or prophets as they called them.

They also adopted the pagan ideas of a world of future punishment fo for violators of their regulations. and moral codes. which inhibitions have persisted to the present day.

There never has been anythign so vicious as the atrocities of the va ~~rious~~ religious institutions, and no wars so severe as these against each other. The great isolation of the various sections of the human race has led to the formations of different languages and ~~and~~ races, of men, and ~~their~~ their conflicts.. The gradual elevation of races ~~xxxx~~ of men by eduvation has led them to a better conception of the relation of the human race to i its environment and the elimination of the vicious religions practices. and beliefs but none of them without a struggle. abd today the various religio gions are evaporating before the sun of enlightenment. as men begin to realize that they are only man-made. and without spiritual authority. and the churches are being abandoned and ministry is pussyfooting between the classes of education and religion. and trying to hold on to their jobs. but not knowing where they are going, though they are on their way. Now various states are trying to regulate the religions of their people such a as Russia and Germany, and Mexico. and invading the sacred rigjts of the poe think as they please

Dec. 12

and Modern
Early Botanizing.

Good if have space
Contrib 18 p 147

Botanists of today wonder how men did their work in the early days. To understand how work was done in those days we must consider the conditions of travel. One could not carry a mass of materials or equipment to make fine specimens, and in addition the traveler was handicapped by lack of means. In the Lewis and Clark expedition they had nothing but their axes for making canoes by hollowing out logs, and when not boating they had to carry their loads on their backs as packs at the portages. ~~and there was much danger of splashing water in going through rapids on the way.~~ There was much danger in going through rapids. In those days they did not even have old magazines to ~~carry along and use~~ use for a temporary press, as the modern woman traveler ~~does, to a Temporary press~~ uses to preserve her specimens. About the only paper available was the rough brown wrapping paper we used to call butcher paper, and which was about the consistency of the coarse modern paper towels ~~now used in laboratories.~~ This paper was about 3 feet wide and was folded four- double to about the size of an ordinary magazine and tied up in packs. Then the botanist gathered snips like the modern school girl, and slipped them in the pack as he had a chance, between the leaves, and left them tied up till they either dried or molded and rotted. ~~There was not the solicitude shown specimens those days as shown now and the necessity for drying them properly was not regarded as necessary.~~ In the Wyeth expedition when Nuttall made his memorable trip to the western coast he started out with a pack on his back intending to foot it through, ~~but he came upon Wyeth's party on the way and between the two~~ and joined them, and his specimens show the inadequacy of the equipment; for all that I have seen were blackened in the drying and became almost worthless; but they are all that is left of his many types.. These specimens were mounted and arranged by Thomas Meehan and I was enabled to examine the Astragalus material in 1897 on my visit there. Whether Nuttall ever made decent specimens I do not know, but his trips were made mostly afoot or horseback, and his facilities were inadequate. No doubt that Fremont's trips across the conti

nent were made horseback and with limited space for specimens. This was in the early forties.. Practically all the types of the various expeditions made on Government surveys were made in the same way. Wagon roads were not available those days, for buffalo trails were the only roads.. My own experience with making specimens began in the early seventies, some thirty years after Nuttall's time, when wagon roads had been made by emigrants all over the west, and when transcontinental railroads had just been built.

When I began to get ready for my first trip to Colorado I had the benefit of advice from all the old ladies who never had made any such trip, and from all the old grannies among them men, who knew as much about theories as spring chickens. I was told to wear the heavy cowhide shoes that the soldiers of the Rebellion wore in the war and to have the soles filled with hob nails, to enable me to climb the mountains, and to take along with me tissue paper to put the specimens in., all of which I did. Following their advice I took and which I found it to be a nusisance. For driers I bought butcher paper, and folded it double, making driers a foot wide and a foot and a half long, and sewed the edges together, about four sheets to a drier. At that time some wise guy in the east had evolved a theoretical drier out of felt cut to the regular size and I/4 inch thick, and sold then at a reasonable price which was altogether out of my reach. Then another wise guy in the east had evolved a wire press with clips on the corners and wire chains to tighten up as the specimens permitted, the two parts to the press were made of band iron riveted together at the corners and wire passed thro' holes punched along the sides, This press I used as a portfolio to carry along with me in the field. and used boards for press , tying them up with ropes. When I was not moving I would put a heavy board on the top and a big stone on it to keep it tight. This was my equipment in 1878. Since I walked about 15 miles a day botanizing, those days my cowhide shoes became a nuisance, although they stood the wear and tear of walking all right, and lasted me all summer, but the exertion of lifting them at each step took so much energy that I was tired out with the walk every day, but being young, I did not realize what was the matter. When the weather was cold or wet they were later.

(1) (2) It was Futtall's habit to tear off a little piece of paper and put it in with the specimen showing the locality and date. I used this method in the first year of my botanizing but discarded it for specimen books in order not to duplicate numbers. On these pages of the book I wrote in pencil the number of each specimen and the date and locality and any notes I wanted to go with the specimen. These books were made by taking a few sheets of specimen paper and folding them double and cutting the edges, and punching holes in the folds through which I could pass strings to tie them up. These books usually held 400 pages and I put an oil cloth cover on them, and I put a fragment of the plant, whose number the sheet bore, in with the data and later on I referred to this specimen to see if my memory had failed me in getting the species of plant placed there. Then on each sheet of paper containing specimens I wrote the number in pencil. It often happened that I had specimens of other plants in with those bearing the number, and this made some confusion in dates later on, requiring looking up the number to determine the date for that specimen. This was when I was making sets for distribution. But in later years when I was not making sets I wrote the data on each sheet separately to avoid errors. This is my method now. I do not favor carrying note books with numbers for each specimen gathered, for it takes too much work in the field; and the less field work the better.

In my work in the Grand Canon for the Government in 1894, I was instructed by Coville to use the notebook method, but I disregarded it for my own way, and when I got to Washington I had a row with him about it, and in one discussion with me he snapped out "You and Greene are the two most conceited men I ever saw." I did not reply to this but could have said that I did not believe him for he must have looked in a glass some time in his life..

In commenting on this in a letter to Rose some years afterwards I told him I was puzzled for a long time to understand how Coville could make such a palpably false remark till I concluded that he must have been raised in the backwoods of New Jawk where he did not have an opportunity to look in a mirror..

I had had twenty times as much field experience as he had and did not care to adopt a method already proved inferior in the field. There is too much danger of losing your notebook in the field (as I did once in Mexico) leaving it and then to be all at sea.

In the Death Valley region I wore my old cowhide shoes one year, when I found the hob nails fell out of the blackened holes in the leather as though burned out. The heat of the ground was such that in walking from Ballarat to the Panamint mountains one day I actually had to sit down on the ground and hold my feet up while the soles cooled off. (1 & 2)

When tennis shoes came into vogue, I found that their lightness was a great advantage, and used them altogether. A heavy soled pair will last six weeks, cost less than half as much as leather shoes & weigh only a fourth as much. They are unsurpassed for climbing rocks and are made durable only in rock slides or swamps.

As I began to use my reason I discarded a lot of foolish equipment & evolved what was usable & indispensable. In place of a trowel or butcher knife to dig up plants, I had a blacksmith make me a light pick with one end a blade 1½ in. wide, each end being only 6 in. long. Into this I inserted a pick handle 3ft. long, so that I could reverse it & use it for a cane in climbing. I had early learned that to make good specimens I must dig up the whole plant, fold it skilfully to fit the sheets & still look lifelike. With this pick I could easily get any ordinary plant with one blow, and could use it to kill snakes. I drew my

9m 80's

for driers we now use double faced corrugated cardboard

In the eighties when I was botanizing in Arizona I had the old time driers and often had two piles of drying plants four feet high, and To change these, and spreading out the wet driers and put on new took four hours of the most strenuous work, sitting in the hot sun and shifting the sheets from one place to another. Though it took but a few minutes for the wet driers to become dry in the baking sun it had to be done daily to prevent specimens from molding, or turning black, a fire, which is a long and tiresome job. *

Never had I had to dry my driers over a fire but three times in all my botanizing.

board from a Bottom P.T.

it with oilcloth cut 4 in & 8 in. wider all around. I cover

VI

MODERN AND EARLY BOTANIZING

Botanists of today wonder how men did their work in the early days. Insufficient funds and the conditions of travel were great handicaps to the collector. One could never carry enough materials or equipment to make fine specimens. The Lewis and Clark expedition had to use canoes made by hollowing out logs with axes. There was much danger in going through rapids and always the risk of having everything splashed with water. When not boating they had to carry their loads on their backs at the portages. In those days they did not even have old magazines to use for a temporary press, as the modern woman traveler does. About the only paper available was the tough brown wrapping paper that we used to call butcher paper, which was of about the consistency of the coarse modern paper towels. This paper was about three feet wide and was folded four-double to about the size of an ordinary magazine and tied up in packs. Then the botanist gathered snips like the modern school girl's, slipping them into the pack as he had a chance and leaving them tied up till they either dried or molded and rotted. The necessity for drying them properly was not even considered. Whether Nuttall ever made any decent specimens I do not know. All of his that I have seen showed the inadequacy of his equipment, for they were blackened in the drying and almost worthless. But they are all that are left of his many types . They have been mounted and arranged by Thomas Meehan. I was able to examine his Astragalus material in 1897.

No doubt Fremont's trips across the continent were made on horseback and with limited space for specimens. This was in the early forties. Practically all the types taken on Government surveys were collected in the same way. Wagon roads were not available in those days, for buffalo trails were the only roads.

My own experience with making specimens began in the early seventies, some thirty years after Nuttall's time, when wagon roads had been made by emigrants all over the west, and when transcontinental railroads had just been built.

just been built.

When I began to get ready for my first trip to Colorado, I had the benefit of advice from all the old ladies and old men who never had made any such trip. I was told to wear the heavy cowhide shoes that the soldiers of the Rebellion wore in the war and to have the soles filled with hobnails to enable me to climb the mountains. Also following their advice, I took tissue paper to put my specimens in but found it a nuisance For driers I bought butcher paper and folded it in about four layers to make driers a foot wide and a foot and a half long, sewing the layers together around the edges. At that time some wise guy in the east had evolved a theoretical drier out of felt cut to the regular size and one quarter inch thick, and sold them at a reasonable price, which was, however, altogether out of my reach. Then another wise guy had evolved a wire press with clips on the corners and wire chains to tighten up as the specimens permitted. The top and bottom of the press were made of band iron riveted together at the corners and with wire laced through holes punched along the sides. This press I used as a portfolio to carry with me in the field. For a press I used boards, tying them up with ropes. When in camp I put a heavy board and a stone on top to keep it tight. This was my equipment in 1878.

Since I walked about 15 miles a day botanizing, my cowhide shoes became a nuisance, although they stood the wear and tear of walking all right, and lasted me all summer. The exertion of lifting them at each step took so much energy that I was tired out every night, but, being young, I did not realize what was the matter. When the weather was wet or cold they were very uncomfortable. In the Death Valley region I wore my old cowhide shoes one year but found that the hobnails fell out of the blackened hole in the leather as though burned out. The heat of the ground was such that in walking from Ballarat to the Panamint Mountains one day I actuall had to sit down on the ground and hold my feet up while the soles cooled off.

It was Nuttall's habit to tear off a little piece of paper and put it in with the specimen showing the locality and date. I used this method in the first year of my botanizing but discarded it for specimen books in which I placed a fragment of the plant together with all data written in pencil and the specimen sheet number. These books I made of sheets of specimen paper folded double with edges cut and holes punched in the folds through which I passed strings to tie up about 400 pages together. I covered each book with oilcloth. This was my method when I was making sets for distribution. If my memory failed me, I could look up the number to determine the data for each specimen. In later years when I was not making sets, I wrote the data on each sheet separately to avoid errors. I do not favor carrying note-books with numbers for each specimen gathered, for it takes too much time and work in the field, and the less field work the better.

In my work in the Grand Canon for the Government in 1894, I was instructed by Coville to use the note-book method, but I disregarded it for my own way, and when I got to Washington I had a row with him about it. In one discussion with me he snapped out, "You and Greene are the two most conceited men I ever saw!" I did not reply to this, but wanted to ask, "How about the man you see in your mirror?" I had had about twenty times as much field experience as he, and did not care to adopt a method already proved inferior in the field. There is too much danger of losing your note-book in the field (as I did once in Mexico) and being left completely at sea, all your work worthless without your data.

When tennis shoes came into vogue , I found that their lightness was a great advantage, so used them altogether. A heavy-soled pair will last six weeks, cost less than half as much as leather shoes and weigh only a fourth as much. They are unsurpassed for climbing rocks and are undesirable only in rock slides or swamps.

As I began to use my reason in the light of my own experience, I discarded a lot of foolish equipment and evolved what was usable and

inexpensive. In place of a trowel or butcher knife to dig up plants, I had a blacksmith make me a light pick with one end made into a blade one and a half inches wide, each end being only six inches long. Into this I inserted a small pick handle three feet long, so that I could reverse it and use it for a cane in climbing. I had early learned that to make good specimens I must dig up the whole plant, fold it skillfully to fit the sheets and still look life-like. With this pick I could easily get any ordinary plant with one blow, or I could use it to kill snakes, if I saw any.

In the eighties when I was botanizing in Arizona, I had the old time driers and often had two piles of drying plants four feet high. To change these, spreading wet driers out in the sun and putting in new ones, took four hours of the most strenuous work, sitting in the hot sun at noon and shifting the sheets from one place to another. Although it took but a few minutes for the driers to dry in the baking sun, it had to be done daily to prevent specimens from molding or turning black.

For driers we now use double-faced corrugated cardboard cut into sheets 12 by 18 inches. When tightly strapped down, they seldom need changing in the usual weather of the deserts, if the bundles are kept out in the sun and wind. I have gone for weeks without changing these driers. It is best to keep fleshy plants (such as succulents) in a separate press with extra driers and for frequent changing. I usually take along 300-400 driers for a trip. When the weather is wet there is nothing to do but change driers even if they have to be dried over a fire, which is a long and tiresome job. But in all my botanizing I have had to dry my driers over a fire only three times.

I found that any rigid press lets the edges wrinkle in drying, while the center gets too much pressure. I make the finest press imaginable as follows: I buy #14 tar-board [now called binder-board] from a book-binder, have it cut 12 by 18 inches and cover it with oilcloth cut 4 to 8 inches wider all around. I cover the cloth side with Le Page's Glue, lay a tar-board ~~in the center~~

in the center of the cloth, pressing it down tight and folding the edges of the oilcloth over (corners cut out) on the back of the tar-board. I keep them under pressure until dry. I take 4 to 6 of these heavy covers along for presses and I use straps made of sole-leather cut six feet long and two inches wide with holes punched two inches apart and with a heavy buckle on one end. A bundle of driers with a tar-board cover on each side and one of these straps around each end of the bundle ~~of driers~~ make a good press. I sit or kneel on the bundles and pull up the straps tight daily. For several years I carried a collecting-can with a tight lid, but now I use two of the prepared tar-board covers for a portfolio to carry along in a shawl-strap with plenty of paper sheets to put specimens in. For specimen paper I buy unprinted newspaper cut 23 by 18 and folded in quires to fit the driers. I take along 2 to 6 quires for a trip and 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 and write on the lower left hand corner the date, locality, and any other notes about color of 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 until all are in. If specimens are put in fresh, the flowers and leaves usually lie flat and lifelike, with only the occasional special ~~arrange~~ arrangement of a leaf. I pile the empty quires of paper on the filled sheets, strap up my portfolio, and 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 three 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 and strapped up in an empty press. But be sure the stems are dry. If not, put the few damp sheets, tied up tight, out in the sun for an hour or so.

My sixty years of experience have taught me that to do the best botanizing, you must travel light, with the least possible toggery. So I take a first-class post-card camera, a strong tripod, a color screen, a fine field-glass, a barometer, a pick with long handle, a pocket folding Coddington lens, a heavy pair of overalls with plenty of pockets, a wool shirt, a cork hat, heavy socks, and tennis shoes, and I let the dudes have the rest. If it is very dry and the foot-trip long, I carry a small canteen.

Dont let anyone fool you by saying that any one way is the only way of doing things. Use your own horse-sense. If you can make good specimens standing on your head or dragging heavy boots, then do that. Results are what count. I find it best to do my routine work with the least possible effort, saving my energies for more important things, and so at over 80 years of age I am still climbing the mountains with the young folks.