

CONTRIBUTIONS

to

WESTERN BOTANY,

NO. 13.

by

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NEW SPECIES AND NOTES.

Cheilanthes gracillima var. *aberrans* Jones is a form of *C. Fendleri* with pectinate-toothed scales and approaches *C. myriophylla*.

Gilia Wrightii. Gr. My No. 5247, from Springdale, Utah, is a low shrub having the appearance of being biennial or short-lived. The heads are subcorymbose or single, and with leaves gradually reduced above to short bracts. The leaves are mostly entire on the upper stems but with short lobes near the root, rarely all entire. Flowers smaller than in the variety described below, about 4" long, bracts wider, mostly lanceolate, otherwise as in the variety. This also includes my No. 5297n from Pahria Canon Utah, also from LaVerkin Utah and probably the material from Elk Ranch on the upper Virgin river at 7000° alt. This species would seem to belong to the section *Elaphocera* with oval anthers, but *Gilia Gunnisoni*, *pumila* and *polycladon* belong in *Heugilia*. *Gilia densifolia* is closely allied to *G. Wrightii* though with linear anthers.

Gilia Wrightii var. *pectenisecta*. n. var. This is my No. 10464 from Mack Colorado May 27 1908, growing on shelving rocks among junipers. It is a low shrub, 6-12' high, tufted with many slender and erect stems 4-12' high, simple above, minutely woolly, leafy to the top and with a single leaf subtending the mostly solitary and dense head. Leaves all pinnately divided from the base or at least with no lobes arising

above the middle, terminal leaflet the longest, lobes 6"-2' long, $\frac{1}{2}$ -1" wide, nearly smooth and green, acerose and rigid. The order of lobing is the reverse of that in *G. congesta* where the leaves are lobed above the middle. Bracts and bracteal leaves conspicuously white-woolly with long and pointed flat hairs, the edges of the nearly linear bracts seemingly pectinate because of the dense hairs along the edge, tips of the bracts and calyx lobes green and smooth and acerose. Calyx about 5" long. Corolla about 6" long, with widely spreading, entire, obovate, and water-lined lobes 3" long, white. Some specimens have leaves with lobes 1-3" long. Stems nearly equably leafy to the tip. My No. 10583 from Grand Junction May 22, 1895, is more condensed and more ashy.

***Gilia floccosa* var. *ambigua* n. var.** Slender, erect, widely branched. Heads small. Flowers blue, 5-6" long. Anthers narrowly elliptical, exserted. Leaves entire, floccose-woolly mostly throughout. Plants about a foot high. Victor, California May 17 1903, N. 10011, also No. 9917 from Bear Valley, California, July 19, 1900.

***Gilia virgata* var. *Yageri*. n. var.** Plants slender, widely and much branched. Heads small. Flowers blue, 6-9" long, with broad oval and lined lobes. Anthers broadly linear. Leaves entire or with one or two pairs of lobes at base. This is the desert form. Skull Valley Arizona No. 10250, Palm Spring California No. 10249 Wickenburg Arizona No. 10253, Franconia Arizona No. 9918, Moapa Nevada No. 9919, Hillside Arizona No. 10279, Congress Junction Arizona No. 10251. I regard the types as Nos. 10279 and 10253. Also I refer here No. 9936 from Yucca Arizona May 17 1884, La Verkin Utah No. 5189q and 5194, Springdale Utah No. 51211, Yager's north of Tucson Arizona No. 9935, Rio Tonto Basin Arizona No. 9934.

***Gilia arenicola* n. sp.** Needles California May 3 1884 No. 10447, Kelso California May 2 1906 No. 9955, growing in sand. Also gathered by Mrs. Brandegee in the Mojave region. Section *Dactylophyllum*. Lower leaves opposite, upper alternate, all about 6" long, mostly forked above the middle, sometimes 3-lobed near the base, each lobe 1-nerved, and the base 2-3-nerved according to the number of lobes, rigid, acerose, green, ciliate. Internodes short and the whole plant congested, 1-2' high, with several slender branches which have single and short-pedicelled flowers in all the axils but the lowest. Leaves not reduced above, bracteal ones the same. Flowers apparently

vespertine after the fashion of Section *Linanthus*, cream-colored, very slender corolla mostly shorter than the calyx and rarely a line longer. Calyx in fruit 3" long, lanceolate-oblong, hyaline except the 5 green ribs which end in rigid and slightly spreading teeth $\frac{3}{4}$ " long, acerose, smooth, fringed. Stamens slender, not dilated, about as long as the white and narrowly ovate anthers which do not reach the sinuses. Pistil shorter than the stamens: style shorter than the stigmas. Pods elliptical, filling the calyx, many seeded. Seeds oblong, angular, very small, much curved, reticulate-lobulate, with a close coat and without mucilage or spiracles, reniform. Embryo white, strongly curved, in copious albumen. Radicle about twice as long and about as wide as the cotyledons. This appears just intermediate between the two sections *Linanthus* and *Dactylophyllum*. I refer to the type also No. 124572 Univ. of California from Waterman, Cal., collected, I suppose, by Mrs. Brandegee.

***Gilia campanulata* Gray.** This evidently belongs to the *Dactylophyllum* section instead of *Eugilia* where Gray put it. Pedicels sometimes nearly 1' long. The species extends south to the Amargosa desert. Its relationship with *G. Parryi* and *demissa* is evident. *Gilia filiformis* also belongs between this and *Linanthus* and not in *Eugalia*.

***Phlox Douglasii* var. *salina* n. var.** Whole plant whitish but smooth, bases of leaves mostly with a few hooked prickles or very stiff hairs which are reflexed. Leaves rigid, fleshy, 3-6" long, $\frac{1}{2}$ -1" wide, with short and sharp prickles, edges a trifle revolute and so the leaf seems 3-nerved, linear or rarely some of them oblong-lanceolate, sometimes minutely roughened when young. Stems densely caespitose and with imbricated leaves. Flowers mostly single and terminal, on short and leafy branches. Calyx linear, 4" long, cleft to the middle, not at all hyaline except in a narrow line running down between the lobes which are green rigid acerose linear and erect. Corolla 9" long, with purple throat and white and nearly orbicular lobes 3" long. Stamens short-oblong. Stamens and style not exerted, style about half as long as the corolla tube. This grows in highly alkaline soil along with *Distichlis* and *Suaeda*. Cherry Creek Nevada July 13 1891 and June 19 1906, Nos. 10474 and 9982.

In the following borages the writer is not convinced as to whether the genera split from *Krynitzkia* are valid or not, it is

his opinion that they are not valid. He is equally doubtful as to the validity of the proposed species, but they seem to be good.

Krynitzkia mensana n sp. (Oreocarya). This may be a form of *Shockleyi*, Cæspitose perennial with many short stems and short and leafy-bracted racemes in a close panicle, 4-6' high, proper stems very short. The panicle begins at the end of the root leaves. Pubescence very dense on calyx, less so on the leaves, on the stems and inflorescence weakly and finely setose. Bracts 6-9'' long, linear, surpassing the flowers. Corolla white, 3-4'' long, linear, surpassing the flowers. Corolla white, 3-4'' long and equaling the calyx, limb 2-4'' wide. Fruiting calyx broadly ovate, 4'' long with deltoid-triangular lobes not reflexed, with age. Nutlets ovate-oblong, 1½'' long; back much arched and very convex; angles reduced to sharp and wavy lines; dorsal face deeply and sharply rugose in irregular transverse lines and forming pits, slightly tuberculate; ventral face very convex and deeply and bluntly rugose: groove broadly triangular-oblong deeply excavated and occupying about a quarter of the face, and bordered by a thick and white and pectinate ridge which goes to the base of the nutlet. Emery Utah No. 5445p May 16 1894.

Krynitzkia oblata n sp. (Oreocarya). Tufted perennial, 1-1½' high. Leaves oblanceolate, acute, 3-5' long, on margined petioles longer than the blades, papillate, green, shortly pubescent with some fine and some coarse short and appressed hairs, stems and inflorescence shortly hispid with widely spreading hairs. Inflorescence a narrow thyrsus with short and scorpioid racemes rarely an inch long. Flowers all bracted. Bracts lanceolate, 4-6'' long. Corolla about a quarter longer than the calyx, limb 2'' wide, tube 4-6'' long. Fruiting calyx broadly ovoid, 3-4'' long, lobes triangular, not reflexed nor opening. Nutlets abruptly arched in the middle to 80° (like those of *O. suffrutescens*), 1'' high, forming an oblate sphere, back barely convex, very tuberculate and somewhat rugose in the middle, shining, angles very sharp and thin with a rudimentary and raised wing, ventral face strongly keeled nearly to the base, where it stops a little short and drops abruptly into the transverse groove, sides concave and minutely tuberculate. No. 3759 El Paso Texas, April 23, 1884. Peach Springs, Arizona, No. 6684, May 26, 1884.

Krynitzkia multicaulis var. **setosa** n var. Pedicels stout

and about $\frac{1}{2}$ " long. Sepals 3" long. Pubescence never yellow. Setæ about 1" long and less dense. Nutlets like the type but with a very faint line of scarcely visible tubercles near each margin. Plants 6-8' high, erect. Calyx explanate. Inflorescence of very short spikes, uniserial, of 3-5 flowers. Corolla very short and small, 2-3" long, white, $1\frac{1}{2}$ " wide. Near Fort Cove Utah June 27 1901, growing under junipers.

Krynitzkia multicaulis var. **abortiva**. (Greene Pitt. 3 114.). This is a well marked variety abundantly connected with the type by intergrades from New Mexico to Arizona. The peculiar incurving of the nutlets described by Greene is characteristic of the species as well, the "peculiar stipe-like projection" of the ventral side of nutlet does not exist, it is the usual abrupt ending of the ventral keel common in the whole multicaulis group. The keel rises higher as it approaches the base of the nutlet when it drops abruptly into the transverse scar which is set in a deep recess.

Krynitzkia glomerata var. **Virginensis** n. var. Biennial, many-stemmed, erect. Pubescence of leaves very dense and compact, setose hairs very few. Thyrsoidally branched above. Nutlets broadly ovate, obtuse, nearly straight, 1" long, shiny, sparsely rugose and tuberculate, with raised central ridge somewhat transversely rugose, margin with distinct but narrow wing all around, ventral side rugose and with a raised white edge surrounding the subulate groove which ends in a transverse line a quarter of the way from the base. Pedicels 3-4" long. This is No. 5195a from La Verkin Utah May 8 1894 at 3500° alt. I would also refer to this No. 5124 from Diamond valley Utah.

Krynitzkia depressa n sp. (Oreocarya). Densely cæspitose perennial. Root leaves many, densely tomentose and appressed strigose, spatulate, 1-2' long, petioles very broad and as long as the blades. imbricated and long-hairy. Spikes nearly simple, barely surpassing the leaves, and with flowers appearing as if whorled, and on short and stout pedicels $\frac{1}{2}$ -1" long. Calyx broadly ovate, 2" long, open, with triangular lobes, densely pubescent but not evidently setose. Corolla 2" long, white, as long as the calyx, limb 2" wide. Nutlets deltoid-ovate, a little incurved, with sharp edge and obtuse apex, back concave below and slightly keeled above, closely tubercled all over, a few of the tubercles raised but rarely rugose, shining, ventral face very deeply rugose, the deltoid-triangular

scar going to the base and occupying about a third the face, and open. This plant is readily mistaken for *K. fulvocanescens* but the broad and short calyx and lobes separate it. Aurum Nevada No. 6692, June 20 1893 at 7300° alt. on slopes. Also Muncy Nevada No. 6713, Vermilion Utah June 4, 1901, also Monroe Utah and Marysvale. The latter at 9000° alt.

***Krynitzkia fulvocanescens* var. *Idahoensis* n. var.** Racemes few flowered and loose. Pedicels 2-4" long. Calyx lobes linear, 4-5" long, apparently not opening when ripe. Nutlets finely and deeply petinate-rugose but not transversely rugose, dorsal groove not at all bordered by white ridge. Corolla not longer than the calyx. Inflorescence yellow. No. 6474, near Weiser Idaho April 28 1900. These short-flowered forms are so often found alongside of the normal ones that one must assume that there is a genetic connection between them. There are many forms in western Utah and Nevada that approach this form.

***Krynitzkia mixta* n. sp.** This may be one of the multitudinous forms of *K. barbiger*. 1-2° high. Whole plant strigose with slender very long and ascending white hairs. Base of calyx densely white-hairy and shaggy as well as sparsely setose with slender setæ from a small and pustulate base. Plants paniculately branched from the base, annual, erect. Spikes dense and scorpioid in age, the fruits contiguous, spreading. Flowers very narrow, with scarcely any limb, 1½" long. Fruiting calyx 4-6" long, with ovate base and short and spreading tips closely constricted below, linear, green above. Seeds usually 2, deltoid, with an acuminate tip, shining, 1" long, coarsely muricate, truncate below, scar deltoid, excavated, groove open to tip, attached half way up, forks of scar as long as the attachment in the groove. Mescal Mts. Arizona May 24 1890. This is the type. I also refer here material from the Needles Cal. May 3 1884. St. George Utah, No. 5106 April 26 1894, Silver Reef Utah May 3 1894, Hackberry Arizona April 25 1903, No. 3832 May 6 1884, Meadow Valley Wash Nevada April 28 1904. One nutlet is apparently firmly attached to the gynobase.

***Krynitzkia decipiens* n. sp. (Cryptanthe).** This seems to be intermediate between *dumetorum* and *angustifolia*, and closely approaches *intermedia*. Slender annual with filiform bractless spikes 3-8' long in twos or threes. Leaves linear, papillose, 1-2' long. Flowers not over 1" long and minute. It has the calyx of *K. oxycarya*, 1½-2" long, lobes filiform, green,

spreading at tip, erect, appressed, oblique. Nutlet 1, ovate-lanceolate, incurved, faintly papillose but more evident above, groove open at base, attached nearly to the top, 1" long, obtuse-angled. Pubescence appressed and short. The type is my material from Yucca Arizona May 14 1884. Other material is from Hackberry Arizona, Darwin Cal., Meadow Valley Wash Nevada.

Plagiobothrys humifusa n. sp. This may be an extreme form of *tenellus*. Annual, nearly prostrate, softly pubescent throughout. Radical leaves ovate, 6" long, nearly sessile. Stem leaves the same but sessile, 4-6" long. Proper stems very many and very short, ending in simple or geminate and slender spikes 3-4' long, all leafy-bracted and loosely flowered in fruit when the flowers are 6-9" apart below. Calyx nearly hemispherical, 2" long, open in fruit, lobes triangular and calyx cleft below the middle. Nutlets cruciform, incurved to a half circle, shining, faintly rugose, sparsely set with sharp papillæ, dorsal ridge evident, angles double and denticulate with sharp papillæ, otherwise as in *P. tenellus*. Plants 3-6' long. Chat California 5000° alt. June 18, 1897. The roots secrete the usual purple dye, but the stems and leaves do not.

Lupinus prunophilus n. sp. Habit of *L. Burkei*. Densely tufted, erect, stout and coarse, 2° high. Lower petioles often a foot long, upper often only an inch long. Stems and leaves sparsely pubescent with spreading hairs. Upper side of leaflets smooth. Leaflets about 7, 2-3' long, spatulate-ob lanceolate, strongly nerved, green. Stems simple. Spikes nearly sessile, very dense, about 6' long. Flowers purple, 6" long, very broad, keel not exerted; banner smooth. Calyx short-shaggy, spurred, on a slender and shaggy pedicel 6" long. Bracts linear-lanceolate, shaggy, 6-8" long, persistent. Pods about 18" long, 6" wide, 5-6-seeded, shortly-appressed-pubescent, fleshy, narrowly oblong, triangular-acute. Seeds 2½" wide, light-colored. This grows on dry mountain slopes Middle Temperate life zone along with *Amelanchier* and *Prunus demissa*.

Astragalus lutosus n. sp. White River Utah next the Colorado line, 5200° alt. May 25 1908. This grows on very poor soil in white shale in the barest places where there is very little soil. Perennial from a thick erect and fleshy root which at an inch or so beneath the surface puts out many slender and scaly stems 1-4' long. Flowers few, white, in a head which is nearly sessile in the axils. Banner white, 4" high, arched

abruptly to $45-80^\circ$ at a place 2'' beyond the calyx teeth, thin, sides reflexed most below and there for 1'' wide, not at all at tip, and so the banner is fiddle shaped; sulcus very deep and V-shaped, little if any narrower at tip or flatter, notch deep. Wings oblong-linear, the blade 3'' long and 1'' wide, arched to 30° in a gentle arc, a trifle wider above and oblique at tip, notched a little on the lower side, rounded at tip, white, and faintly purple-veined below. Keel 2'' long and a line high, very flat, lunate, the triangular tip and base about equal, purple, 1'' shorter than the incurved wings which fold over it. Calyx hyaline, reddish, much laterally compressed, base straight, upper side convex, not oblique below, mouth a trifle contracted and a little oblique notched deeper above. Teeth triangular, $\frac{1}{2}$ '' long. Bracts triangular, as long as the short and stout pedicels, 1'' long. Pods much inflated, cross section triangular-reniform to round-reniform, ventral suture intruded and pod sulcate along the suture, the dorsal suture slightly sulcate, pod reddish but not mottled, lying flat on the ground, 1' long, tip upcurved and acute and base downwardly curved, stipitate. The above is from the material got at Dragon Utah. The specimens got in the type locality have slender stems 2-6' long, many from the thick and rather woody erect root which is branched below, the stems being underground, but not rooting, except the uppermost node or two which are above ground, forming rather dense mats a few inches wide, with the leaves, stems and pods flat on the ground, the subterranean internodes are straight and $\frac{1}{2}$ -1' long, slender, much as in the allied *A. Hookerianus*. Stipules large, rigid, spreading, reniform, 1-1 $\frac{1}{2}$ '' long. Aerial stems when present zigzag. Well developed leaves 3' long, with slender petiole about as long as the rachis. Leaflets about 12 pairs, mostly folded, but when open overlapping each other, touching when folded, oval, 3'' long, obtuse, shortly-petiolulate, thick, smooth above, hoary and densely pubescent below with appressed and very fine hairs attached by the base. Ripe peduncles stout, straight, about 1' long, with 1-4 pods which are on stout and erect pedicels a line long. Bracts hyaline, triangular, not over 1'' long, persistent, with scattered black hairs (as well as calyx). Pods 1-1 $\frac{1}{2}$ ' long, 6-8'' wide, cross section round or nearly so, much inflated, papery, on a slender stipe not as long as the calyx and not jointed to it, oblong-oval to oval-ovate, obliquely triangular-acute at tip and a little narrowed at base, sulcate about a line

deep ventrally, with ventral suture a trifle intruded, dorsally a little sulcate but without intruded suture. Calyx cylindrical, about 3" long and 1½" wide, in flower the subulate teeth are about 1" long. This blooms very early, soon after the snow leaves, in full fruit on May 25th. This is probably what Miss Eastwood has called *A. anisus* from western Colorado, but the true *anisus* has never been collected except at Pueblo so far as I know. This species is certainly related to *A. aboriginum* and *Hookerianus*.

***Astragalus detritalis* n sp.** Four miles above Theodore Utah on the Colton road, on very barren clayey detrital slopes. Allied to *A. simplicifolius*. Caespitose in small mats, perennial, from an erect and tapering taproot which at crown branches into several thick and prostrate stems 1-2' long which are densely clothed with closely imbricated hyaline nearly smooth and oval stipular scales 3-4" long, which are acute and prominently 1-nerved and adnate to the petiole. Whole plant ashy with very fine and appressed hairs. Leaves all radical, 2-3' long, with slender and grooved green petioles and 2 pairs of linear-oblancoolate sessile leaflets near the tip and which are jointed to the rachis and about 9" long and needle-tipped, some of the lowest leaves have broadly oblancoolate leaflets 3-4" long. Flowers few, capitate, purple, 7-8" long, sessile in flower and with stout pedicels ½" long in fruit, ascending, as is the fruit. Bracts very large, hyaline, green nerved, triangular, 3-4" long, about as long as calyx in flower, persistent. Calyx tube short-cylindric, 2" long, 1½" wide, triangular at base and equally inserted, oblique at tip and much deeper cleft above, the arcuate subulate teeth as long as the tube and the lower ones longer. Banner oblong-oval, emarginate, 4" long, with sides reflexed ½" wide in the middle, white spot of several bands a line below the tip, banner arched in a gentle arc from tip to base and erect. Wings narrowly oblong, 1½" wide, rounded, flat to keel and obtuse, a line shorter than the banner, 1½" longer than the straight and very obtuse keel which is lunate and boat-shaped and not abruptly erect as is usual. Pods arcuate, 1' long sessile, flat, 1" wide, flecked with small brown spots, obliquely acute at tip, 1-celled, manifestly allied to *A. simplicifolius* but differing in the broad calyx, long pod, and flowers three times as large.

***Astragalus Duchesnensis* n sp.** This may prove to be one of the extreme forms of *A. lonchocarpus*. Plants with the fili-

form and much branched underground stems of the *A. diversifolius* group, loosely tufted with mostly a single stem to a branch of the root. Stems mostly flexuous, with internodes 1-2' long. Whole plant including the pods but not the calyx ashy with short and appressed hairs, but green underneath the pubescence. Stems and peduncles fleshy and sulcate. Plants 1-2' high, racemosely branched from the base at 45° angles. Stipules rigid, hyaline below, adnate, not connate, deltoid, the lower ones 2-3" long, the upper much shorter and reflexed. Leaves 2-4' long, of about 3 pairs of fleshy, linear, distant, obtuse, petiolulate leaflets ½-1" wide and 6-9" long, the terminal one shorter or absent and represented by the prolonged rachis which is never evidently widened, the petiole 6-9" long, leaflets of the lowest leaves the widest, uppermost very narrow, drying slowly like its allies. Peduncles stout, axillary, racemosely flowered on the upper half, 3-5' long, tapering and erect or upwardly curved. Bracts minute, deltoid, persistent. Flowering pedicels very short, ascending, in fruit 1" long. Flowers set nearly at right angles to the pedicel and horizontal or in fruit pendent but pedicel still ascending. Calyx and pedicels black with appressed hairs. Calyx short-cylindric, 1½" long and ¾" wide, reddish, inserted in the middle of the base, with triangular base and oblique mouth much deeper cleft above and with very short and deltoid teeth, not compressed. Flowers red to pink-purple, arched, 4-5" long. Banner oval, about 3" long, arched to 90° in gentle arc from the calyx, sides reflexed ½" wide, most in the middle, a little black-nerved above, emarginate, a trifle longer than the wings, white spot of about 6 broad, white bands, coming within ½" of the tip and sides, sulcus shallow and very broad. Wings lunate oblanceolate, 1" wide, a little narrowed above, obtuse, arched about 45°, white, entire, flat to the leaf and about 1" longer. Keel with arched base, tip very broad, incurved more than 90°, about 1" high, black-purple appon, 2-2½" longer than the calyx. Pods white when young, pendent, arcuate rather more below, linear-oblanceolate, 18-24" long, 2-5" wide above the middle, obcompressed, broadly sulcate ventrally, often with flat cross section or triangular, 1-celled, hairy within, chartaceous, to leathery, a little inflated, apiculate to very acute, contracted below into a pseudo-stipe or nearly sessile, arched ventrally and with the present suture projecting, dorsal one impressed, red, often doubly arched like the letter S. Common on the mesas from

13 miles below Theodore Utah to Chepeta Well and White River near the Colorado line at about 5000 alt. Lower Temperate life zone.

Philadelphus argyrocalyx. Wooton. This seems to be the same as *P. Mexicanus* Schlecht. *P. serpyllifolius* Gray is only a variety of it. I have material from Hillsboro peak New Mex. and Colonia Juarez Mexico. It is characterized by the pods shorter than the hoary calyx. *P. microphyllus* has the calyx white when young, and varies considerably, Nelson having made a species out of the Utah forms from my collection.

Cogswellia rigida n. sp. This is a *Cymopterus* Panamintensis in everything but the fruit, and another very interesting link between these closely related genera. It is very anomalous as is also *C. scopulorum* which connects the *anisatus* group with *Cogswellia*. Densely caespitose from a thick, branched and woody root covered with old coarse and ribbed leaf-sheaths, acaulescent. Leaves bipinnate but the lower pair of divisions elongated so as to seem ternate, 4-6' long; segments broadly ovate, deeply and unequally 3-5-lobed and then again 2-3-toothed with coarse and triangular and rigid acerose teeth, segments 6-9" long with cuneate base; leaves with 3-5 pairs of pinnae, rigid and leathery, glaucous, smooth, on stout and angled but wingless petioles shorter than the blades. Peduncles stout, mostly declined, 6' long. Involucre none. Rays slender, about equal, 10-15, 1-1½' long. Involucels rigid, subulate-acuminate, 4-6" long, not hyaline, longer than the many flowering pedicels. Flowers apparently yellow. Fruiting pedicels few, 5-8" long, very stout. Fruit smooth, oval-ovate, with a cordate base and obtuse tip, about 5" long and 3" wide, the wings about as wide as the narrowly elliptical body, narrow at tip, thin, face with filiform and slightly raised ribs and normally 3 oil tubes in the intervals, commissure with 2 strong oil tubes on each side and 1-2 others usually reaching half way. Near Big Pine, Inyo Co. California at 4000 alt. May 30 1906, No. 7225 Hall and Chandler. Tropical life zone.

Cogswellia Chandleri n. sp. Root tuberous and with a slender foot. Stems slender, apparently single, erect, branching close to the ground and with several short internodes and with a peduncle mostly at each node except the first. Leaves bipinnate, triangular-ovate in outline, 3-4' long exclusive of the petiole; lowest pair of pinnae short-stalked, the others sessile; pinnules divided nearly to the base into several pairs

of linear-oblong and obtuse but apiculate and short-pubescent divisions. Root leaves with stout and nearly marginless petioles, 4-6' long. Stem leaves with conspicuous, very broad, clasping, hyaline-margined and nerved petioles which on all but the lowest leaves are wholly dilated and an inch long. Peduncles stout, erect, 4-6' long, with 10-15 unequal and slender rays 1-2½' long, subtended by a lanceolate, green and hyaline-margined and broad bract 6-9" long and which is lobed or toothed above. Involucels 4-6" long, broadly obovate, hyaline-margined, abruptly acuminate, longer than the many flowered pedicels. Fruit immature but evidently becoming elliptical and about 6" long, with rather broad wings truncate above and narrowed below. Oil tubes 2-3 in the intervals. Rays filiform and raised into narrow wings toward the tip. Manifestly a close ally to *C. vaginata* and the *caruifolia* group, and closest to *C. marginata* which grows on the western side of the Sierras. Nelson Range Lee District Inyo Co. California, May 23 1906, No. 7157 Hall and Chandler, Tropical life zone.

Cogswellia bicolor (Watson) Jones. It would appear that Coulter and Rose have never seen mature fruit of this species for they describe it as linear-oblong when it is narrowly elliptical. They say the wings are very narrow, when they are a millimeter wide and very conspicuous. They say that the dorsal and intermediate ribs are nearly obsolete, when they are very conspicuous and raised. This is probably due to the fact that the fruit does not mature till the leaves are dead. At first the fruit is nearly terete and broadly linear, differing but little from their genus *Leibergia*, as it matures the flesh on the fruit dries out from the face till the face is barely convex, and then the white ribs appear as strong lines on the very dark face. The inner face is then concave and the cross section of the fruit is nearly linear and very thin. These notes are taken from careful field study from the flowering time to maturity of the seeds.

Cymopterus Duchesnensis, n sp. Myton Utah among loose rocks on southern slopes of mesas. Densely caespitose, with deep and rather fleshy, coarse and branched roots. Crowns several, coarse and thick (about an inch thick), with imbricated and very thick and woody old leaf-petioles an inch long which become slightly fibrous when they rot. Nearly acaulescent. Leaves many, on long and slender petioles, 3-5' long, erect, bright-green, thick, pinnate, of one or two pairs of deltoid-cu-

neate leaflets, 1-1½' long which are varyingly 3-lobed or incised and the lobes 2-3-toothed and with a sharp apiculation. Leaves leathery and smooth; petioles not margined nor enlarged below except at very base; lower pair of leaflets distant. Peduncles erect, 6-8' long, rather slender. Involucres mostly of one linear-subulate and green bract not over 6" long, but mostly wanting. Rays 10-15 about 2' long, the inner ones sometimes shorter, all except the spreading outer ones erect and rather slender. Involucels of several green bracts, with very narrow and hyaline margins, linear-subulate and 6" long, but many of them much reduced and triangular, 1-2" long, barely united below. Pedicels 3-4" long, shorter than the fruit, stout, erect or spreading. Flowers yellow, small. Fruit about 5" long and 3-4" wide including the wings, emarginate at both ends. Seeds about a line wide and 4" long, very concave on the inner face so that the two make an oblate rounded cross section, elliptical in outline as to length; lateral wings thick and corky at very base; then quickly narrowed to a thin area which continues to the margin; dorsal wings either thickened at insertion or thin, but usually a trifle thickened beyond and then thin again, mostly as wide as the lateral ones which are twice the width of the seed, 2-2½" wide, all much wrinkled. Oil tubes very narrow, 4-6 in the intervals and 2 on each edge of the commissure and sometimes more, but very faint. Skin of the commissure very much thickened and white. This evidently belongs to the *purpureus* section but has the seeds of the *Pteryxia* section and the leaves more like the *Utahensis* section. At a distance the green tufts remind one of *Pæonia*, a genus not found in the region. This is a very peculiar species. It grows in very poor soil partly alkaline, Lower Temperate life zone.

Leptotaenia multifida and its variety *Eatoni*. For the benefit of Mr. Rose who still holds that these species are distinct, the following field notes are furnished, in order to show that they cannot be maintained as separate species. Ravalli Montana material; seeds 8 mm. long, oil tubes and corky ridge conspicuous, rays many. Alta Montana material; seeds oblong, very flat, 13-15 mm. long, oil tubes absent and also corky nerve, pedicels many, leaves finely dissected. Lambert Valley Montana material; seeds 8 mm. long, with evident oil tubes and corky lateral nerve, foliage less dissected, rays many. Blackfoot Glacier Montana material; fruit 8-10 mm. long, very

flat, rays many; oil tubes evident, foliage finely dissected. Susanville California material; fruit oblong, 15 mm. long, very flat, without lateral corky nerve, rays many, final divisions acutish, linear, 1-1½" wide, oil tubes none. Summit California material fruit 10-15 mm. long, oil tubes 4 in the intervals, corky nerve evident, final divisions short and wide, 2-3" long and 1" wide. My No. 5532 Thistle, Utah; fruit 10-12 mm. long, very flat, corky nerve evident below, commissural oil tubes sometimes evident, leaf segments like the last but blunt, 2-3" long and ½-1" wide. My No. 1699 from City Creek near Salt Lake City; fruit 14-16 mm. long, narrowly oblong, without oil tubes or corky nerve, pedicels many, leaf segments acutish and like the last, seed face very flat. Muncy Nevada material; fruit 10-14 mm. long, elliptical, without oil tubes and with lateral corky nerve, very flat, divisions of leaves ½-1" wide, 2-8" long, blunt, rays many. Fort Cove Utah material; fruit 13-14 mm. long, elliptical and acutish, rays rather few, no oil tubes, fruit thick and concave, no corky nerve, leaf segments very sharp, linear to triangular, ½-4" long, not over 1" wide. Cuddy Mts. Idaho material referred to by Mr. Rose; fruit broadly elliptical, 13-15 mm. long, oil tubes mostly present, corky lateral nerve represented by 3 lines barely at all raised and not thickened, rays many, leaf segments about ½" wide and 2-6" long. Seven Devils Mts. Idaho material; fruit narrowly oblong. 18 mm. long, rays few, oil tubes none, lateral corky nerve absent, pedicels 12" long, leaf segments linear and falcate, 3-12" long, not over 1" wide, acuminate. Robinson Utah material; fruit elliptical, 13-15 mm. long, segments short, ½-1" wide, 2-3" long, fruit flat, oil tubes evident on face but not on commissure, corky lateral nerve absent but represented by a nerve only, rays few to many; Ruby Hill Nevada material, this has large fruit and short segments; Salina Canon Utah. My No. 5176w has many rays; Silver Reef Utah. My No. 5441s, referred to by Rose is too immature to tell what it is as is true of the above; Carson City Nevada material; my No. 3878; segments obtuse and the same as in the Cuddy Mts. material, fruit 10 mm. long, concave, with oil tubes, rays many. Tenachapi California material; segments short and blunt, fruit 16-18 mm. long. Nelson's No. 4484 from Wyoming; fruit flat, 10-12 mm. long, without corky nerve, oil tubes present, rays many, leaf segments short and blunt, this would go for *Eatonii* in spite of the fruit character. Bingham Utah material:

fruit 12-16 mm. long, no corky nerve nor oil tubes, segments long and narrow and obtusish, rays many. Chat California material: fruit 9 mm. long, narrowly elliptical, oil tubes and corky nerve present, rays many, leaf segments only acutish, just like the Susanville material with large fruit. Carson City Nevada material got in 1897, same as Chat but fruit large. Spring Creek Nevada material: leaflets like the Ruby hill ones cited by Rose as *L. multifida* but immature fruit is 12 mm. long and when mature would be more. Aurum Nevada materials, cited by Rose as *L. multifida*, were in flower and with the same blunt segments less than a line wide and 4-8" long, fruit gathered later from the same plants ranges from 10-14 mm. long, elliptical, concave, with raised lateral nerve, oil tubes present, rays rather many. Rose cites my No. 3878 referred to above as *L. multifida*. The Ruby Hill Nevada material cited by Rose as *L. multifida* has the same short and blunt leaflets, and the Carson City material differs in no respect except in the smaller fruit from the others with fruit 18 mm. long, while specimens gathered alongside of the others have the fruit 18 mm. long, but identical otherwise. Now all these citations show conclusively that neither Mr. Rose nor any other person can make two distinct species out of this material as it is impossible to get any character that amounts to anything specifically.

Townsendia mensana n. sp. Densely caespitose, perennial, with heads sessile among the rosulate leaves, the mats 2-4' wide. Ashy throughout. Leaves linear-spatulate, 6-12" long, thick, acute. Heads 4" long and wide with rays $\frac{1}{2}$ " wide and about 3" long, many. Scales narrowly elliptical, shortly acuminate, with broad and hyaline margins lacerate. Pappus of ray setose and about twice the width of the akene; pappus of the disk of slender and sparsely toothed setae as long as the flowers. Akenes with conspicuously thickened and callous edges. This belongs to the Fendleri section. Benches of the Uintas from 6000-7500' alt. at Theodore Utah. I also refer material here from Silver City Utah April 4, 1896 and Mercur Utah June 6 1896 and McIntyre's ranch Utah May 18 1891.

Townsendia incana var. ***prolixa*** n. var. Winter annual with slender tap root. Root leaves $1\frac{1}{2}$ -3' long, on long petioles, obovate-spatulate to broadly oblanceolate, sparsely pubescent; stem leaves oblanceolate, 1- $1\frac{1}{2}$ ' long, triangular-acute, with short and broad petiole. Heads on short peduncles $\frac{1}{2}$ -3' long, which have 1-3 leaves, the upper one being either invo-

lucrate or near the head. Central head of plant generally twice as large as the others and on an erect and leafy peduncle. Stems with many proliferous branches below and 2-4' long and naked, ending in a cluster of 1-3 flowers, and either simple or short-branched, the heads with disk about 6" high and 6-8" wide. Rays 2" wide and 8-10" long, bright-pink. Scales with broad, lacerate and hyaline margins, acute. Pappus of ray about as long as width of akene, that of the disk as long as the flowers and plumose, the teeth being longer than the width of the hair, setae often forked at base. Plants conspicuous by being nearly green and proliferous with long rays, but there are intergrades. Common throughout the Duchesne valley Utah in exposed and gravelly places in the Lower Temperate life zone, in May. My material gathered mostly on May 20 1908.

Townsendia incana is correctly described by Torrey and Gray as being a perennial with sessile heads, short and spatulate leaves 6-12" long, strigose-ashy throughout; heads with disk about 6" wide and high, with rays a line wide and mostly shorter than the heads; pappus of ray about two to three times the width of the akene, and that of the disk as long as the flowers, with setae mostly simple and teeth shorter than the width of the scale or hair. The type and variety differ conspicuously but they have the same range and intergrade freely.

Parthenium alpinum var. **ligulatum** n. var. Ligule oval to round, 3-toothed at tip, 1-2" long, rough-pubescent below, rigid. Pappus adnate to the very thick corolla tube, green, lacerate, triangular, about as long as the tube. Akenes hoary, oblong-ovate, 2" long. Leaves ashy with minute appressed and white hairs. Hairs in the axils very long and slender, jointed, 2-4" long, nearly straight. Leaves spatulate-oblong-ovate. Plants very closely resemble *Eriogonum acaule* and grow in similar situations on nearly bare clayey and gravelly knolls on ridges at 6000 alt. forming dense mats 6-12" wide, with thick and branched roots, habit also of *Actinella acaulis*. Theodore Utah, Lower Temperate life zone.

MR. ROSE AND THE UMBELLIFERAE.

In a recent number from the National Museum Mr. Rose goes over certain genera criticised by me in my last Contributions. He corrects his error in the generic character of *Leptonia* pointed out by me, but does not correct the others. He accepts *Cogswellia* as the correct name for *Peucedanum* in his limited sense, and adds names of species that I did not care to recognize either from lack of material or because I knew they were not good species.

Mr. Rose, in commenting on my reductions, says there is no use in opposing such well-established genera as those he has separated from *Cogswellia* (*Peucedanum*), etc. Now a genus to be well established must first rest on sure natural grounds, and, second, must be properly characterized. I have shown that they do not rest on natural grounds, that they are not properly characterized and that often species referred to them cannot be found in the keys. Let us take up in order the genera recognized by Rose.

Phellopterus. This is credited to Torrey & Gray's *Flora* 1 623, but there is no such genus published there. It is, however, given as a section of *Cymopterus* by Nuttall and approved by them. Its character there given is "Calyx teeth minute; pericarp somewhat corky; commissure with 4 vittæ; carpophore none." Coulter and Rose in erecting this into a genus, p. 166, give it as of Nutt. when it should be (Nutt.) C. & R. They give the characters of the genus so far as paralleling Nuttall is concerned "Calyx teeth evident." Pericarp not mentioned but wings thick or thin. Commissure with 4 to 8 vittæ. Stylopodium wanting. Now as stylopodium is wanting in the whole genus of *Cymopterus* and its segregates according to Coulter and Rose, and as the oil tubes are equally variable in the whole genus there is not a character given by Nuttall that they recognize as binding. In addition to that neither Nuttall nor Torrey and Gray recognized it as a good genus and nobody since has done so till Coulter and Rose did. The figure given by them (p. 166) would seem to indicate that the genus was distinct but that figure does not represent

the genus as *C. Utahensis* Jones has wings as thin at insertion as in the other genera. In looking over *Pteryxia* and *Aulospermum* we do not find a single character of *Phellopterus* that is not found in one or the other. The chief character of leathery leaves and tuberous roots they do not give at all. It is correctly characterized on p. 25 of my Contributions No. 12 as a section of *Cymopterus* which, of course, intergrades with the others as is shown by *C. Utahensis*, etc. It is also made to include *Aulospermum* C. & R. which is a curious mixture containing two groups fully as distinct from each other as *Phellopterus* and *Pteryxia*, namely *A. longipes*, *glaucum*, *Watsoni*, and *Ibapense* on the one hand and *Panamintensis*, *Jonesii*, *cinerarium*, *purpureum* and *Rosei* on the other. The first group has the tuberous roots, leathery and whitish leaves not rigid nor sharply toothed, and concave seed face, and comes next to *Phellopterus*, shading into it. The second group does not have the tuberous but rather woody roots, and is really made up of two distinct parts which I have properly placed in separate sections (p. 27), one part going into *Pteryxia* and the other into *Scopulicola*. This disposes of *Aulospermum* as it is not valuable even as a section as characterized by them because of its diverse groups, part belonging to *Phellopterus*, part to *Pteryxia* and part to *Scopulicola*.

Pteryxia. This is characterized exactly in the same way by Nuttall, and on the same page as *Phellopterus* and should have been published as *Pteryxia* (Nutt.) C. & R. This is given by Torrey and Gray as "Calyx teeth distinct, lanceolate; wings of the carpels broad; commissure with 4-10 vittæ; carpophore free, 2-parted." Coulter and Rose (p. 170) give the similar characters as "Calyx teeth evident" (same as in *Phellopterus*): lateral ribs with broad wings, thin throughout (while they figure them as thick at base), dorsal and intermediate from strongly ribbed to broadly winged; oil tubes several; nothing said about the carpophore. From this we find not a single character given by Nuttall is recognized as characteristic. The only character that has any permanence is in the woody and not evidently tuberous root, and this they do not mention at all. They add a character not given by Nuttall in the much dissected and sharp-pointed leaves, but even this shades directly into the *Panamintense-purpureum* group which I place in *Scopulicola* and *Pteryxia*, and this in turn shades into the *Phellopterus* group.

Taking up *Cymopterus* proper, which I call section *Eucymopterus*, Coulter and Rose characterize the genus on *C. Fendleri* and *C. acaule* and the novice would have great difficulty in finding *C. megacephalus* and *globosus* from their generic character in this genus. Why they did not make two genera out of this is surprising for the yellow-flowered and dark-green-leaved, almost involucreless *C. Fendleri* and its var. *Newberryi* form a group very marked from the white-flowered and glaucous-leaved looking and hyaline-involucered species of the rest of the genus.

Rhysopterus. This is characterized on page 185 by Coulter and Rose. They give the generic character as with obtuse ribs while *R. corrugatus* has acute ribs, in addition they refer *Cymopterus globosus* to *Cymopterus* while it is congeneric with *C. corrugatus* which they refer to *Rhysopterus*. This shows the instability of this genus.

Oreoxis. They put this far out of its proper place in the family and near to *Thaspium*, referring half of the species that properly belong to it to *Pseudocymopterus*, while it is manifestly allied to the *Pteryxia* and *Scopulicola* groups particularly in vegetative characters. This group should embrace not only the species referred to it but also *C. anisatus*, *bipinnatus* and *Humboldtianus*, *C. anisatus* showing the intergrading toward the *Pteryxia* group.

Pseudocymopterus. This as interpreted by Coulter and Rose is untenable for it includes widely separated species, the only thing to do with it is to consider it as a distinct genus confined, as they at first made it, to *Pseudocymopterus montanus*, or to place it as a species of *Ligusticum* or to consider it as an anomalous *Cymopterus* as I have done. To refer *C. anisatus* and *bipinnatus* to it as they have done destroys it. The habit, leaf dissection, and fruit characters must keep them in separate groups. So far as *Cymopterus* and its segregates go I have shown that the segregates are untenable because they have not been and cannot be generically segregated on any character that sticks, and that they cannot find their own species in the genera that they have erected.

Cogswellia (Peucedanum). The segregates from this genus which they call *Lomatium*, *Euryptera* and *Cynomarathrum* I have retained in this genus. *Lomatium*, as I have shown, is untenable and has been abandoned by Rose.

Euryptera. This can be maintained only on the combina-

tion of rigid and not greatly dissected leaves and leptotenioid fruit, no other character holds at all, but the same kind of fruit is found in my section Campicola of Cogswellia and the same kind of leaves is found in the section Crassipedunculata of Cogswellia, so both characters fail as generic ones. The genus is manifestly congeneric with Cogswellia though forming a very good section.

Cynomarathrum. This proposed genus has less legs to stand on than any proposed by Coulter and Rose, and yet Mr. Rose speaks of this particularly as "well established." Now let us see what it stands on. This is the character given by Coulter and Rose: "Calyx teeth evident. Fruit strongly flattened dorsally, oblong. Carpel with sharp or winged dorsal and intermediate ribs and broader winged laterals. Stylopodium flat but evident. Oil tubes mostly several in the intervals or obscure. Seed dorsally flattened, with plane face." Vegetative characters are the same as in Cymopterus section Pteryxia and so are omitted. Now referring back to Pteryxia we find "Calyx teeth evident. Fruit oblong to orbicular in outline. Carpel mostly strongly flattened dorsally, with wings thin throughout, lateral ribs with broad wings, dorsal and intermediate from strongly ribbed to broadly winged, Stylopodium wanting. Oil tubes several in the intervals. Seed face plane or with a broad and shallow concavity." Now in looking over the two characters we find nothing in Cynomarathrum that is not also found in Pteryxia except that the stylopodium is wanting in the latter and present in the former, which is good if true, but it is not true. There is always a rudiment of a stylopodium in Pteryxia, mostly with a depressed center, while in Cynomarathrum scabrum whose type is in my herbarium it is a mere rudiment, no more than in Pteryxia; this is also true of my material of *C. Eastwoodæ* and *Parryi*. Hence so far as the character given by Coulter and Rose is concerned Cynomarathrum and Pteryxia are the same. The fact is they are quite similar, though no complete intergradation is yet known. The relationship to Cymopterus anisatus is also quite close. Now taking the character given by Coulter and Rose, some of the species of four genera can be placed in Cynomarathrum just as well as where they are. These species are Cymopterus lapidosus Jones, Lomatium anomalum Jones, Pteryxia species and Cynomarathrum species. Cynomarathrum therefore should be recharacterized as I have done in

Contributions No. 12, p. 32, and confined to *C. Nuttallii* and *Parryi* and placed as a section of *Cogswellia*. The rest of the species should be placed in *Eucogswellia* section and next in generic relationship to *Cymopterus* as I have placed them on page 33.

So far as the work on the Umbelliferæ criticised by me is concerned Mr. Rose has not been in the west only as far as Wyoming, and then only once, and recently in a part of Arizona, while the whole of the great west where these genera abound he has never seen, and his opinions of specific and generic limitations are therefore drawn from closet work alone. In addition Mr. Rose confesses in this work that he is rather following a fad than adding anything new to our knowledge, in splitting up genera, and to this fact my criticism has been specially directed, in order that botanists may know the reason for his many changes in names. As I have said before it is greatly to be regretted that he has been drawn so far away from the correct conception of generic and specific limitation. In view of the fact that in the same issue I criticised severely those who create so many species from purely selfish motives, it might be inferred that I also included Coulter and Rose among them, but this is an erroneous idea, for I have always thought them to be above such motives, and have therefore spent the time to go over their work critically, which I would not have done had I believed otherwise.

Mr. Rose, in speaking of my new genus *Cusickia*, is unable to see any generic character in *Cusickia minor*, stating that I have not given any. If he will take the trouble to read over his generic character for *Leptotonia* and my character for *Cusickia* he will find them differing far more than any genera he has created, or if he does not others can.

WESTERN AMERICAN BIRCHES, by B. T. Butler.

This paper in the Torrey Bulletin for August, 1909 is the result of about five days' field work at Bigfork, Montana, or portions of five days. The writer has had charge of the botanical work at the Biological station of the State University there for the last two years, and Mr. Butler was there a short time and did a little botanical work, being out with the writer on three occasions, once at Rost lake, once at Yellow Bay, and at Bigfork. In addition to this he collected a little material at Ravalli. The writer had already written a revision of *Betula* which was used on the material gathered on these trips.

At Rost Lake the writer pointed out to Mr. Butler the various forms of *B. glandulosa*, all intergrading and growing under the same ecological conditions. Out of this material he makes two new species which have no standing whatever, *B. glandulifera* and *B. Elrodiana*, while Rydberg adds another from Rost Lake under the name of *B. crenata*, all forms of *B. glandulosa*. To these Butler adds still another from Ravalli as *B. obovata*, this also is a form of the above. Butler's species amount simply to a description of individual plants to which he gives new names and from which I also gathered material (except the Ravalli specimens).

Mr. Butler then takes up *B. microphylla* describing as species some of the different forms, such as *B. fontinalis* Sargent, *B. Sandbergii* Britton, *B. Utahensis* Britton and *B. Piperi* Britton. This species is characterized by the bark not exfoliating in papery layers and not peeling off when cut through to the wood as is the case with the paper birches. It is a low shrub, rarely over 15 feet high in the type and in the larger leaved var. **Utahensis** (Britton Torr. Bull. 31 165 as species) it often is thirty feet high and six inches in diameter. It always grows in clumps.

Mr. Butler then takes up the paper birches and recognizes every name ever applied to them but one as distinct species. Among the forms of *B. alba* are *B. Alaskana* Sargent and *B. papyrifera* Marsh, which he recognizes as separate species and distinct from *alba* though they are only forms.

Betula alba* var. *pendula (Roth Ten. Fl. Germ, as species).

The forms of this variety which he recognizes as species are *B. subcordata* Rydberg, a proposed new species, *B. occidentalis* Hooker, and *B. Montanensis* another proposed species of his own gathered at Yellow Bay at my suggestion as an example of a diseased and very old tree with thicker bark and generally abnormal condition due to the inroads of the waves of Flathead Lake on the roots.

Mr. Butler is a conscientious and modest young man with great musical talent, who is taking his last year at Bronx Park and Columbia for his Ph. D. degree, and this is a part of that work. His ideas of specific limitation or rather lack of limitation he has gotten wholly from that source and so has never had any correct conception of what a species is, but regards it as simply any old form that he may wish to describe as new. The writer would be glad to subscribe several cents toward a fund for the education of the professors who are responsible for this kind of work so that they might go to some botanical kindergarten where they could learn that a plant growing in the shade or on a north slope or in a cold lake will differ from another from the same parent which grows in a warmer and drier situation, and that this condition will be reversed when the offspring of these plants occupy reversed conditions; and where they could learn that a poodle dog running around a house is not necessarily two distinct species because they see his tail only on the south side and later on his head only on the north side.

NYCTAGINACEÆ, by Standley.

This recent publication by the Department of Botany in the Smithsonian Institute is ostensibly a review of the Nyctaginaceæ, but actually it is an attempt to vindicate P. A. Rydberg, of whom the author is a protege.

The writer of this review of Standley's work published a monograph of the Nyctaginaceæ in Contributions No. 10. This work was based on nearly thirty years' observations in the field and on a very large amount of herbarium material.

Immediately after the publication of this P. A. Rydberg, piqued at the necessary criticisms of the writer on his poor work on *Astragalus*, published another review of the same family, making a great many changes in names and erecting many forms into species particularly from material that I had collected and of which Rydberg had seen but single specimens. The motive of this work was perfectly plain. It now seems that there is a doubt of Rydberg's success and so his pupil must rush to the rescue, Quixote like.

This last review by a young man with almost no field experience is characterized by a very large amount of assumption on a very small basis of fact. In addition his warped judgment is shown in all matters suggested by me where he is apparently moved by the same spirit that causes the Irishman to become Democratic when he emigrates to this country, because he is "forinst the Government." For example, he rejects *Boerhavia Thornberi* Jones, calling it a synonym of *B. hirsuta*, while it is remarkably distinct from it. He keeps up all of Rydberg's fictitious species and also *A. falsa* (a form of *A. fragrans* based on material of mine also), and adds a number of his own which are equally fictitious, though he has scarcely seen one of them growing and has for the most part but single specimens on which to base his judgment. He caps the climax of absurdity by calling *Abronia micrantha* var. *pedunculata* Jones a species while he has never seen but a single specimen of it and does not know that I have plenty of specimens of it with both sessile and peduncled heads on the same plant. I may wait in breathless suspense for him to in-

form me as to the status of these specimens. Shall I call them *Abronia micrantha-pedunculata* or *Abronia pedunculo-micrantha*, or *Abronia micrantha* x *pedunculata*. Mr. Standley now shares the high honor along with Greene of making two species of out specimens from the same plant from my collections.

I suppose that free advice is not appreciated and yet I would venture to suggest that though inspirational botany may be a great thing in a certain limited area of the east and wholly supplant reason, common sense and field experience, the people of the west are so benighted that they are as yet unable to distinguish the difference between the divine inspirational halo and New York fog, and therefore, like the people of Missouri, have to be shown.

FOREST TREES OF THE UNITED STATES,
by N. L. Britton.

This sumptuous and exceptionally well printed volume is remarkable for its cost. If there is any reason for its appearance other than a rivalry among publishers the text fails to show it. It is not to be compared with Sargent's work in accuracy, nor even with the Manual by Sargent.

GRAY'S MANUAL, SEVENTH EDITION.

The new edition of Gray's Manual has recently appeared under the joint authorship of Dr. Robinson and Mr. Fernald.

It is the same compact and handy volume as before, preserving the main features which have become so familiar to two generations of botanists, and yet is brought up to date in nomenclature and arrangement of orders. The Grayan method of description is still retained. Alterations in sequence and form are many, but the changes are more apparent than real. The natural order of families according to their genetic relationship is adopted placing the lowest families first, but for the most part the order of the genera and species in the families is reversed, placing the highest first which is a poor arrangement, and follows Gray, Watson, Britton and the rest. The keys have for the most part been entirely reconstructed and for this all botanists owe them a great debt of gratitude. Britton's 3-volume Flora copied bodily the old key of Gray's Manual and without credit. The book is remarkably free from clerical and typographical errors. A few occur here and there of minor importance, as in *Juncus* the decimal point is misplaced in giving the measurement of the seeds so that seeds seem to be longer than the pods in some species. I notice *Corallorhiza maculata* is credited to Rafinesque who so far as I can find published only a *Cladorhiza maculata*. *Betula crispa* is credited to Aiton when it should be Dryand, etc., but such blunders no book can wholly avoid, as the writer has found out too often in his own work. *Alisma Plantago* L. is published as *A. Plantago aquatica*, while Linnaeus did not publish the name. *Salicornia Europæa* is published as such while Linnæus seems to have given it as *S. Europæa herbacea*.

The most material changes or noticeable ones are in the adoption of the first name of a species instead of the first name in the genus. This as we know is done against the authors' better judgment in order to conform to the Vienna code, and though perhaps necessary is to be regretted. This alters many names of well known plants, but is reduced to a minimum by the retention of the excepted genera such *Astragalus*, etc. The adoption of such names as *Polypodium polypodioides* is indefensible and absurd.

Montia Chamissonis is given as *M. Chamisoi* (Ledeb.) Durand and Jackson instead of Greene. *Chamissonis* is the correct form. It looks like quibbling.

The worst feature of the book is the uneven recognition of species. The joint authorship of a book is never a success. Dr. Robinson's conception of species is excellent, also that of Eaton, Ames and Hitchcock, but whoever wrote the *Cyperaceæ* and some other lower orders seems to have had no conception of species at all, about as bad as that of Greene and Rydberg. Such a species as *Scirpus rebrotinctus* has no standing at all. The work on *Carex* is confusing. In comparing *Carex* with its treatment in Britton's *Flora* it suffers greatly. No one would imagine that *C. teretiuscula* in Britton's *Flora* and *C. diandra* in the *Manual* (synonyms) were the same species. The engravings in the *Manual* are much inferior, poorly etched and poorly printed. In the matter of the engravings I take direct issue with Agnes Chase in *Rhodora* 10 207 where she speaks of the engravings with unstinted praise.

One of the most commendable things in the book is the ignoring of the split genera of Britton, Rydberg and Small. For some years the Brittonians have raised a great hue and cry about the new and original work done by them in splitting up the Grayan genera. A recent repetition of this is in the *Torrey Bulletin* where the new *Manual* was reviewed through Bronx Park glasses. Anything published by Harvard is like a red flag before a bull to Bronx Park. Whenever Harvard squeezes Bronx Park has a fit, and it is a standing joke among botanists. This critique says of the work of Ames among the orchids that it fails to recognize the recent studies of Rydberg in *Habenaria*, etc., as though Rydberg had done any original work in this genus. When we come to look at Rydberg's work we find he has simply raised Gray's sections to generic rank and given new names to them and renamed all the species. The limitations to the sections were done for the most part before Rydberg was born. Rydberg tries to justify this work by saying that some of the changes have already been recognized by European botanists. Now if there is any crazy thing that some European botanist has not done in the near or remote past the writer would like to have it mentioned, and to use that as an excuse for doing some other crazy thing is no argument in its favor. Rydberg attempted to do the same thing in *Astragalus*, but he went a little farther and tried to split up some of Gray's

sections and made a miserable mess of it as I have shown in my Contributions. An amusing part of this critique is that this Manual ought to be called Britton's Manual as it is nearer Britton's Flora than Gray's Manual. Perhaps there is something to this suggestion as Britton's Flora practically copied the old Gray's Manual altering the phraseology and then adding a great deal of other matter that was not specific, but we have not as yet seen any acknowledgment from Britton of his indebtedness to Gray's Manual and are not likely to see it.

The writer had hoped that the book would discard some of the great defects of the botanies of his boyhood days. In this he has not been wholly disappointed, as there have been many improvements, but the one great defect that has run through every botany published in the last 100 years still prevails, namely see-saw descriptions. Over 25 years ago the writer pointed out this defect and in his Contributions indicated the way to remedy it and in his Ferns of the West (published in 1883) gave a practical example of how to do it. This was again exemplified in the revision of the Onions, and last year in the revision of the Willow Family. It is true that a part of the suggestion has been adopted, namely taking the same organs in the same order in related species. For example in *Betula* on which the writer is just now working the Manual takes up the leaves in the same order in *B. pendula* and *B. alba*, the twigs are taken up first, then the leaves, catkins and scales in order which is good and a very great improvement on the hit or miss way of Gray's former manual. But there the good work stops just as it does in Britton's Flora. Britton's flora practically copies Gray's old manual and then throws in about as much more stuff with no attempt at antithetical comparison in any of it, much of which would apply to any one of several related species.

Now to be specific let us take *B. pendula* and *B. alba* in the Manual. It is impossible to make a suitable comparison with the same species in Britton's Flora as he evidently knows very little about the birches, but we will take up two related species there later.

In the key we find that *B. pendula* has "strictly glabrous branches and leaves" and *B. alba* has puberulent or pubescent branches and variably hairy leaves. This is the proper antithetical arrangement if it is true. But we find at the outset that the twigs of *pendula* are very warty. Now the Manual in the glossary says

that glabrous means "smooth." If very warty is smooth then the writer must not know what smooth is. It is evident that the reviser of the Betulaceæ took glabrous to mean not hairy, which is not true. But brushing this aside it still remains that *B. pendula* has not hairy twigs and leaves while *B. alba* has hairy twigs and leaves, or at least the beginning of hairs (puberulent) which is good if true, but even this fails in the var. minor which for all we know is a form of *B. pendula*, which shows that the character of pubescence is a feeble diagnosis. Now taking the branches we find that *B. pendula* has "slender, flexuous and drooping branches" while *B. alba* has ascending branches, nothing said about the slenderness or flexuosity and so they might be either so far as the text goes, while the writer knows that the branches of *B. alba* are often drooping. This is what I call see-saw description, and is simply a waste of space and a waste of the patience of the student who tries to get anything out of it. The branchlets of *B. pendula* are "usually verrucose with resiniferous atoms" which is a long way of saying resinous-warty. In *B. alba* we find that its branchlets are often resinous-warty too but mixed with long hairs when present which is self evident if the branchlets are hairy as already stated. So briefly stated the branchlets of *B. pendula* are very resinous-warty, and of *B. alba* less so which fully covers the ground and in about a third of the space. In *B. pendula* the "leaves are glutinous when young, firm, rhombic-ovate to deltoid or broad-ovate, subcunate, truncate or subcordate at base, long-acuminate, slender-petioled." Now if the leaves are cuneate at base and long-acuminate at tip they cannot help being rhombic-ovate if they are broadly-ovate. If they are cuneate to cordate at base they run the gamut of change and there is no need of mentioning the basal shape at all. *B. alba* has ovate leaves, taper-pointed, rounded to cuneate at base, 3-6 cm. long, smooth and green above, pale, glandular-dotted, and a little hairy on the veins beneath, sharply and unequally doubly-serrate. Now comparing with *B. pendula* we find the leaves are glutinous when young; nothing said about it in *alba*, but in two varieties they are glutinous. In *pendula* the leaves are firm, nothing said about *alba*. In *pendula* the leaves are broadly ovate to deltoid through rhombic ovate; in *alba* they are ovate but they are cuneate at base and taperpointed at tip and therefore must be rhombic at times. In other words the leaves of *B. alba* are ovate and those of *B. pendula* are broader, which is a

much shorter and a fully as accurate way of stating it. The base is rounded to cuneate, but we find the var. *cordata* has cordate leaves. So the less said about the bases of the leaves the better. In *B. pendula* the leaves are long-acuminate, and in *alba* taper-pointed, if taperpointed is not long or short acuminate there is nothing in the text to show it. In *B. pendula* the leaves are slender petioled; nothing said about *B. alba*, but we know they are slender petioled. In *B. pendula* the length of the leaves is not given while in *B. alba* it is given, which is useless for comparison. In *B. pendula* the fertile catkins are 1.5-3 cm. long and 6-9mm. wide; in *B. alba* they are 1.5-4.5cm. long and 6-16mm. wide, which is another way of saying that in *B. alba* they are a little longer and wider. In *B. pendula* they are pendulous, in *B. alba* they are spreading or drooping, a distinction that amounts to very little. In *B. pendula* they are on unknown peduncles, in *B. alba* on slender peduncles, which is also useless for comparison. In *B. pendula* the scale character is given in Italics which the authors tell us is of special significance, but in *B. alba* the scale character is not given in Italics and so again we have a see-saw description of the special character. In *B. pendula* the scales are ascending, brown to straw colored, 3-5 mm. long, smooth except for the ciliate edge. In *B. alba* they are also ascending (a character that therefore does not belong in either description but belongs in the key above), mostly ciliate-edged, a character that also belongs in the key above, nothing said about the smoothness, 3-7 mm. long, in other words a trifle longer normally.

A revised description of the two species that contains all the essential points as given in the text would be about as follows:

Betula pendula. Twigs very resinous-warty, without hairs, drooping. Leaves broadly ovate to deltoid, not hairy. Fertile catkins 1.5-4.5 cm. long, 6-9-mm. wide.

Betula alba. Twigs not very resinous-warty, hairy (hairs sometimes minute), more ascending. Leaves ovate, hairy below. Fertile catkins a little longer and wider.

In Britton's Flora we will take the first two species of birch.

B. populifolia is a slender tree reaching 45' high and with 1.5" thick trunk, while *B. papyrifera* is 80' high and with 3" thick trunk, which makes it just as slender a tree as the other or more so, but this fact we have to sift out as a description of the bark is interlarded in *B. populifolia* while in the other species this is reserved for the next sentence. We are informed that the bark

of *B. papyrifera* except on the young twigs peels off in thin layers. Then we chase up the bark in the other species and find that it peels up tardily in thin sheets. What difference there is between layers and sheets Britton does not state nor does any one else know. If there is no difference then there is no use in giving it. We find that the bark of *B. populifolia* is very white and smooth but we are not informed whether it is white or black, smooth or rough in the other. In *B. populifolia* we are given some information about the twigs, but in *B. papyrifera* nothing. The leaves in *B. populifolia* are deltoid, in *B. papyrifera* ovate, which is the proper way to put it. In *B. populifolia* they are pubescent on the veins when young, nearly glabrous when old, minutely glandular, dark-green above, light-green beneath; in *B. papyrifera* we chase down several lines and finally discover by readjusting the description that they are dark-green and glabrous above, glandular and pubescent on the veins beneath, nothing said about the color of the lower side. Now what difference there is in these two characters no one would ever guess unless he intends us to think that the leaves of *B. populifolia* are a little less pubescent when old, which could be stated in a third of the space. In *B. populifolia* the leaves are long-acuminate, sharply dentate, and commonly somewhat lobed (which statement his figure of the leaves belies), obtuse to truncate at base. In *B. papyrifera* we find the leaves are acute or acuminate, dentate and denticulate, ovate to subcordate at base. Now by a process of digestion and assimilation we find that *B. populifolia* has long-acuminate leaves and *B. papyrifera* acuminate leaves, nothing said about how long or how short-acuminate, *B. populifolia* has sharply dentate leaves and *B. papyrifera* dentate leaves, nothing said about how sharp or how bluntly dentate. By referring to the figures which are always excellent and by far the best thing in Britton's Flora we find that *B. populifolia* has coarsely, and obtusely dentate leaves, while *B. papyrifera* has very obscurely and acutely dentate leaves. *B. papyrifera* has denticulate leaves and nothing said about it in *B. populifolia* but the figure shows that it also has denticulate leaves and so this character has no place in either description but should have been put in the key to both. *B. populifolia* has truncate leaves at base and *B. papyrifera* also has truncate, cordate or obtuse bases, a description without a difference except that *papyrifera* has a more variable base than the other. We find that both species have slender petioles, a character that belongs in the key to both and

not in the description of either. In *B. populifolia* we find that the leaves are 1.5-2.5' long, or in other words *B. papyrifera* has leaves normally about twice as long as *B. populifolia* but with a wider range in length, but when we come to examine the figures of the leaves we find that *B. populifolia* has leaves about a half longer than *B. papyrifera*, now which is right the figure or the description? We find the petioles channeled in *B. populifolia* and nothing said about it in the other. The male catkins of *B. populifolia* are 2-3' long, and in *B. papyrifera* 2-4' long, a distinction with scarcely a difference. The pistillate catkins of *B. populifolia* and *B. papyrifera* are cylindrical and this character therefore belongs in the key and not here, they are both slender-peduncled also,, which belongs in the key. In *B. papyrifera* the fertile catkins are described as spreading or drooping and figured as appressed and erect, nothing said about it in the other, but figured as ascending and spreading. In *B. populifolia* they are described as 9-18'' long, 3-5'' wide, and in *B. papyrifera* as 1-2' long and $\frac{1}{4}$ - $\frac{1}{2}$ ' wide. We see a use of different denominations in measurement which is always bad, it should have been in the one 9-12'' long, and the other 12-24'' long, 3-5'' wide, and 3-6'' wide, this kind of juggling with figures implies a difference when there is none worth mention. When we come to examine the figures we find *B. papyrifera* has catkins about twice as wide as the other and the same length, but the experienced botanist sees at a glance what the student will not see that the catkins of *B. populifolia* are immature and never should have been figured at all. The fruiting bracts of *B. populifolia* are puberulent, 1-2'' long, with lateral lobes divergent, larger than the middle one. Those of *B. papyrifera* are 2-3'' long, puberulent or ciliate, nothing said about the lobes. When we come to examine the figures we find that the bracts of *B. populifolia* are figured as smooth and not ciliate or puberulent, and in *B. papyrifera* as smooth and ciliate. We find that the lateral lobes of both are divergent, while the distinctive character as shown by the figures is not mentioned in the text at all, namely in *B. populifera* the lobes are deep and rounded including the central one, and in *B. papyrifera* they are very shallow and acute including the central one, which is wholly exserted from the others, while in *B. populifolia* the central lobe is but little exserted beyond the others. We find that the body of the nut is narrower than its wings in both species, a character that does not belong in the description of either.

Now out of all this botanical chaff what wheat can we find? It appears as if Gray's Manual and Britton's Flora are trying to describe the same two species.

Britton's characters for the two species simmered down and taking account of the figures would be about as follows:

Common characters. Bark white and peeling off in thin flakes. Leaves denticulate and acute, slender-petioled, large, glandular, dark-green and not hairy above. Fertile catkins cylindrical, slender-peduncled, 9-24" long. Fruiting bracts ciliate. Nut narrower than its wing.

Distinctive characters.

B. populifolia. Leaves deltoid, abruptly long-acuminate or tailed, coarsely and obtusely dentate, nearly devoid of hairs when old. Fruiting bracts with obtuse lobes, the central one little exerted beyond the others.

B. papyrifera. Leaves ovate, with more rounded base, acute to short acuminate but not tailed, obscurely and sharply dentate, pubescent. Fruiting bracts acute, the central one exerted wholly. Comparing these two sets of emasculated descriptions in the Manual and in Britton's Flora we find they agree well in a general way, and are brief and to the point if imperfect.

In spite of the great amount of chaff in Britton's Flora and the perpetually see-sawing descriptions it was the best thing ever printed on the eastern flora, because of the excellent figures, the exact citation of authorities, and the possibility of being able to sift out of the verbosity of descriptions the characters of the species. It still remains the best thing ever gotten out in the matter of figures and citations, but is much inferior to the new Gray's Manual in conciseness, and correct nomenclature, except in the Cyperaceæ which are better treated than in the Manual. Britton's Flora was a three volume affair and of larger dimensions, but except for the engravings it could as well have been condensed into a single volume without omitting any essentials. In the matter of cost Britton's Flora is not to be compared with the Manual, being almost prohibitive.

MR. HELLER AND NOMENCLATURE.

The publication of the article on nomenclature in my last Contributions seems to have given Mr. Heller (in *Muhlenbergia*) a bad attack of mental colic, recurrent colic. His capacity to appreciate the motive and scope of my criticisms reminds one of the Englishman who after traveling extensively in this country returned home and said to his admiring friends "The Americans are a very clever people but they have many uncouth expressions, for example they say Where am I at, we would say Where his my 'at.'"

Mr. Heller says he believes that the first name applied to a thing should stand. Just so. For the last fifteen years we have heard nothing else, *ad nauseam*, except once a synonym always a synonym, as though this were a panacea for all botanical ills. If so then why does he not apply it? Why do not the other Britonians? If priority is much more valuable than to accept the common usage of the botanical world and is in fact the *sine qua non* in botany why do they take the utterly inconsistent position that priority must not apply earlier than 1753 nor in any other language than Latin? What magic is there in the year 1753 any more than in 753, 53 or 3? No one is so stupid as to believe that botany began in 1753. Many plants were more accurately described by Pliny than by Linnæus; many by the old Greeks, Hindus, Chinese and even the Egyptians thousands of years ago, What magic is there in the Latin language especially the pigeon-latin that we are compelled to decipher in botanical works that would make Cicero turn in his grave to read Latin never was spoken by as many men as French which for generations was the Court language of the World, but neither of them ever was used by as many people as the English nor ever came as near to a World language as our tongue comes now. The reason most botanists take this year and this language as the starting points is not because they regard priority as valuable in the Dark Ages of botany, but because by starting there they are enabled to keep botanical names stable, and a stable nomenclature is the one great desideratum, and this can come only by common consent and not by any arbitrary rules whose application would upset the whole

fabric of accepted names as the Brittonian system has done and is doing in the hands of its adherents.

Now suppose Mr. Heller were to be consistent and really do what he says is the only thing to do, use prior names, he would naturally work the English language first. His botanical godfather Britton would allow him to follow his own example doubtless in putting a Latin tail on his English words, a la *Manihota Britton* for *Manihot L.* Suppose he takes up the gooseberries and currants to which he has given some study, his new names would pan out about as follows: *gooseberrya lacustris Heller*, *Curranta reda Heller* (it should be remembered that the great law of priority demands that we should publish the name exactly as it was first given, although Britton would allow us to Latinize the tail, apparently the most important part of it, we therefore must not say *rubra* but *red-a*). Now after he has Hellerized all the English names of these fruits and gotten the Hellerian tail properly adjusted to all, he will find that the Greek antedates the English, then he can get up another new set of names from the Sanscrit, then the Chinese, then he might take up the Egyptian and give still another batch of new names. By the time the botanical public has begun to recover from the last of these inflictions it is likely that the phonograph and the graphophone will be so perfected that they can take the ripple marks made by sound waves on prehistoric mud (now turned to stone) produced by the incoherent babblings of some of the simian ancestors of the Brittonians, and reproduce them so that they will be as lucid as some recent descriptions of plants, and will have far stronger claim to priority of publication than the Brittonian check list had at the time when it was said to have been published.

Yes, priority is a great thing—for making changes in established names.

Mr. Heller says he believes in splitting. This is really a redundant remark. For anyone who can publish the same species in three different genera at once as he did in his Catalogue of North American plants must be a splitter of splitters, outdoing even Curie the chemist, for the latter only segregated the Radium atom a split from Uranium, but Heller must have gotten far beyond that and found the botanical electrons.

He goes on to say that any name whatever applied to a plant ought to be a specific name. It makes one shudder at what he would inflict on systematic botany if he were to go through Sudworth's catalogue of trees and make specific names of all the

seedsman's varieties and forms there given. But that is not even a beginning to what he would do with the nursery names of herbaceous plants as we find them in seed catalogues. We fear his stock of good Latin would run dry long before he got through. Suppose we take some of the Hellerian possibilities under those conditions. We would have such combinations as this: *Malus Redastracanus*, *Malus pippinus*, *Malus seek-no-further-us*, a very appropriate name which he would do well to follow. Mr. Heller would soon distance Greene who now holds the record with his several hundred species of *Eschscholtzia* out of a possible valid dozen. And yet Mr. Heller says he only describes species. In view of his statements as to what constitutes a species (anything deserving a name of any kind is a Hellerian species) the botanical public will certainly demand to be shown whether he knows what a species is.

Mr. Heller says that we ought to name everything in sight because it often happens that what a person once considered trivial characters prove by further study to be specific, or some other botanist comes along and proves that the describer of a species lumped several in one. Just so. How about the hundreds of Nuttalian species that further study has shown were fictitious? The same is true of the spurious species of Small, Greene and Gadow in *Eriogonum*, Greene's species of *Eschscholtzia*, and Rydberg's *Flora of Colorado* bristling with fake species. The field botanist who resides where most of these false species grow knows that these men either know nothing about ecology or deliberately ignored fundamental and primary facts in ecology in order to make specific names to which they could attach their own names as authors. It is almost inconceivable that a botanist even with the knowledge that an ordinary boy would pick up in the fields if left to himself does not know that a plant growing in the shade will differ from another of the same species if grown in the sun and will differ in certain well known directions, will differ if grown on a hot or cold slope, a rocky or loamy soil, a well drained or poorly drained soil, and yet if the seeds of these plants are planted the peculiarities will at once disappear if put under other ecological conditions. We must give those men the credit of having ordinary sense, and if we do then the conclusion is inevitable that they made these species for purely personal reasons.

Mr. Heller claims that it is personal abuse to say that the chance of getting one's name attached to the tail of a new botanical

combination is at the bottom of so many new names in American botany. If his statement is true then there must have been some other weighty reason for the coinage of these names due to deep and exhaustive research which has put the botanical public under lasting obligation to these botanical philanthropists. Now what great and exhaustive research led Mr. Heller to publish the same species in three different genera in his Catalogue of North American plants? What deep and exhaustive reason led Mr. Heller to coin the name *Astragalus Malheurensis* for Rydberg's *Astragalus Cusickii* when he did not know enough about it to know it was already a synonym for a variety of *Astragalus arrectus*? What deep and exhaustive research leads Mr. Heller in nearly every issue of *Muhlenbergia* to make a new batch of names for recently described species which he finds are homonyms by referring to the Kew Index when he does not know whether the proposed species are valid or not, or whether the names given in the Index are nomina nuda or not? Any eighth grade school boy could do as well as that. Does Mr. Heller think that is good botany? If so he stands almost alone. No one denies that he or any other person has a legal right to publish what he likes, but no one has a moral or botanical right to make names for things that he knows nothing about. It is in violation of the fundamental laws of botanical good taste, and is everywhere condemned.

This craze for making new names for plants reminds one of a lot of school boys at a pie counter in a mad scramble for fear they will not get any pie. A sample of this foolish work is in *Muhlenbergia* 4 56 where Cockerell rushes into print because the Kew Index states that Rafinesque published a *Euphorbia montana* Am. Month. Mag I 450 (1817), and so he renames the *Euphorbia montana* of Engelman, without even referring to the magazine to see if the reference is correct. The fact is there was not only no such species published there, but no plants were mentioned there at all and Rafinesque published nothing of any kind on that page. It is perfectly safe to say that if there had been no chance to get a Cockerell tail on a new botanical name the Kew Index blunder would never have been noticed.

Mr. Heller says that I am objecting to so many new names and yet I do not hesitate to make many myself, and therefore it is a case of the pot calling the kettle black. The logic of this conclusion reminds me of the conundrum "When potatoes are worth fifty cents a bushel how many beans will it take to shingle a meet-

ing house." If Mr. Heller is not able to see the difference between such work as I have criticised above and the results published in my last Contributions he certainly will need the sympathy of the botanical public. The species published there and the suppression of species or reduction of them was the result of a lifetime spent in active work in the field and herbarium, much of the work having been held in abeyance for years after the manuscript was written in order to be sure that as few mistakes as possible were made. I am not objecting to new names, the more the better, if they are based on sound views of specific limitation and exhaustive field research, but I do object and most other botanists object to the slipshod and inexcusable work mentioned above.

Mr. Heller is right in saying that there is but one way to determine the validity of a species and that is by thorough field work. I have several times emphasized the same thing in my Contributions, saying of those who claim to be able to settle such things by appealing to their "botanical intuitions" that "no one with either inherited or acquired acumen can settle such questions off hand and that it is pure pedantry to assume it." Greene and Rydberg especially have been addicted to bolstering up fictitious species and genera by reasoning from analogy instead of resting on known facts. Such arguments always get their users into difficulty. I well remember when it was a stock argument among geologists to claim that the climate of the Arctic was Tropical in the early Quaternary because elephants (mastodons) lived there, animals which live only in Tropical climates. Many years afterwards one of these animals was found in a perfect state of preservation imbedded in the ice, and it proved to have very long hair and its stomach was full of Arctic plants which it had eaten, showing that it had its home in a frigid climate where that race of animals flourished, and not Tropical as had been dogmatically assumed. So in plants, field study has demolished many a dogma. The bases on which many species and genera were founded have been proved fictitious. For example the development of stipules in willows has proven fallacious; the drying black of the leaves is due to laziness of collectors. The development of the torus and dissection of the leaves in *Eschscholtzia* are of little taxonomic value. The development of pappus in *Townsendia* and *Chaenactis* is largely accidental and not specific always. The development of awns in *Oxytheca* has small value.

The development of leaves in parts of *Eriogonum* amounts to little. The development of wings and ribs in the Umbelliferæ has been greatly overestimated taxonomically. The whole tendency of field study has been to reduce and not multiply species. It is true that certain persons after limited field study have proposed the splitting up of many species but it is only in exceptional cases that this has been sustained by exhaustive field work. Of late the tendency to ignore ecological conditions in the making of specific names has been particularly vicious.

Mr. Heller is right when he says that no one makes or un-makes a species. They either exist in nature or they do not exist at all. The careful field botanist knows that in ninety-five percent of the cases species have well defined limitations easily recognizable as they grow. The few not easily distinguished are generally due to hybridization with allied species, while a few others such as *Astragalus lentiginosus* vary so greatly under diverse ecological conditions that nothing but long study can convince the observer that they are variations of one polymorphous species. One of the elements for confusion now is the gratuitous assumption by some botanists that genera and species must not vary beyond a certain amount arbitrarily set by themselves before they become separate species. It has become quite a fad to call these "aggregates" contemptuously with an air of superior wisdom and to proceed to "segregate" them into as many so called species as their limited material allows. Those who follow them when they come to study their own material find that they have a number of new species that do not fit these "segregates" any better than the old species and so they pile up more new names. A good illustration of this is in Greene's treatment of *Eschscholtzia*. When he first began to dissect this genus he made quite a batch of new names based on the development of the torus. After some years of quiescence he erupted again after finding that he had many more species still undescribed which were fully as good as his former ones. And so he has kept it up for nearly twenty years piling up new names till soon he will have a species for every sheet of specimens in his collection. The same course has been followed by Rydberg in his *Flora of Montana* and still more in that of Colorado. In like manner several others have made many untenable species in *Antennaria*, *Castilleja*, *Lappula*, *Eriogonum*, *Phlox*, *Scirpus*, *Carex*, *Sitanion*, and the *Compositæ*, etc. In nearly every case these specific names have been made by

people with scanty material without any or with little field experience in the cases cited. During the past season I have made special field studies of such genera as *Lupinus*, *Habenaria*, *Castilleja*, *Impatiens*, *Carex*, *Salix*, *Betula*, *Populus*, etc., and find in nearly every case the purported distinctions are fictitious. There is but one great thing that the botanist should keep in mind and that is "What are the facts?" If he starts out with the thought that species is only a name to conceal what never existed and purely a matter of the personal equation then his work has no value. If he starts out with the humble attitude of the student, ready to see what is before his eyes and ready to forego his theories when the facts compel it, then he may do valuable work and not till then. If he starts out with the idea as many have done that the more his name gets in print the greater will be his reputation especially if his name forms the tail to new botanical combinations he will land in Scylla and Carybdis where many are now. Some seem to forget that one's name attached to a fictitious combination is a perpetual advertisement of incompetence. With the most painstaking care we all have too many such advertisements already without adding to them unnecessarily. In this connection a remark by Engelmann many years ago is in point. I had been studying *Opuntia* and had collected material of *O. Missouriensis* and *rutila*. I laid it out before him and said, "Now Dr., tell me where *Missouriensis* ends and *rutila* begins." He threw up his hands in mock horror and said "I expect I shall have to suffer in Purgatory for making too many species of *Cactaceæ*," for he could not distinguish the two species. If he were to suffer so in the next world for his errors we would have to resurrect Dante to make a suitable Purgatory for some present day botanists I fear.

The fundamental law in botany which we find everywhere emphasized is that plant life must be in practical harmony with its environment, and climate produces most of this environment. We know geologically that there has been no change in the climate of the World for 10,000 years and probably a much longer time than that. We know geologically that wherever there has been a sudden change of climate the life has changed as suddenly without infinitesimal gradation. Some people seem to act under the assumption that species are changing every day, which is wholly gratuitous, for there is not a single authenticated case known. I have no reference here to the fictitious results claimed to have been produced by artificial conditions. Everyone knows that

these so-called species revert to type when restored to a state of nature, the statements of certain recent botanists to the contrary notwithstanding.

Mr. Heller intimates that the severe criticisms from certain opponents of the Brittonian system of fake species are due to personal jealousies and not to a desire for a better taxonomic system. That there are personal and institutional jealousies no one familiar with recent botany can deny, for anyone who has been at Columbia knows that at least for a time a person could not be there a day without hearing nearly everybody else disastrously dissected, and particularly Harvard men, but a person might be at Harvard a month without ever hearing anyone spoken of disparagingly. This does not prove, however, that Harvard men are not ignoramuses or that Columbia men are not angels, but men who do their own thinking as the writer does will continue to do their own thinking. Mr. Heller might explain what personal jealousy led Dr. Robinson to roast him in the *Botanical Gazette* in the most caustic terms for publishing the same species in several different genera at once as he did in his *Catalogue of North American plants*. The probabilities are that Mr. Heller never met Dr. Robinson, and it is a certainty that he never had an altercation with him. Mr. Heller might explain what personal jealousy led Professor Cowles of Chicago to write "The recent ebullitions of the taxonomic radicals have evoked in botanists in general successively dissatisfaction, contempt and rage. These things will not be endured much longer, a little more and the sinning taxonomists will be cast out into the outer darkness where there shall be wailing and gnashing of teeth." If Mr. Heller has ever done anything in a taxonomic way that might cause jealousy in others the writer has missed seeing it.

There are some people who have surrounded themselves with a halo of self adulation so dense that they can never see the difference between fighting them and fighting the truth. They are like a third rate minister the writer once knew who berated his parishioners because they did not feel like going to church to be bored by his inane harangues each Sunday, he called them irreligious, backsliders and told them they were going straight to perdition. He was so pachydermatous that he never discovered the truth and never will. However much dust the fake taxonomists may throw in the air to deceive others as to the real issue they will find that those who are ignoring the most common and well

known facts of ecology and appeal to their inner consciousness, or 'botanical intuitions' as Britton calls it, for justification of their vagaries will find that the taxonomic road is steep, thorny and devious and getting worse for them at every step, while "outer darkness' is just ahead.

So far as the writer personally is concerned he is too busy to spend any time on enemies if he has any. Life is too short to hold grudges against anyone, while the injury to oneself in harboring a grudge is worse than the offense in the first place. The writer is familiar with all the arts and trickery that the wrong side in a controversy uses to accomplish its ends and is not in the least disturbed by the hostility of any man however influential or wealthy, it would not make any difference in his attitude what personal injury might be done him. There is no doubt that most of our difficulties arise from selfish men trying to force their half-baked ideas on the botanical world, but that should not deter men who have spent their lives in searching for the truth from defending it as they know it to be. Whatever is good will stand, and whatever is bad should fall. We should be big enough to abandon any views or pet theories when the evidence is against us. The trouble with many men is that they start out in botanical research crammed with theories and try to twist the world into their little crack.

For about fifteen years the botanical world has kept silence in the matter of fake species-making while it has grown like a mushroom by the hands of its makers. Some opponents have done so under the assumption that the more rope you give a calf the quicker he will hang himself. This is all right in a forest or where there are plenty of obstructions, but on a prairie where there are none it is different. Therefore some botanists have concluded that a few snubbing posts judiciously placed on the prairie would hasten events, hence the cries from the calves.

FLORA OF WASHINGTON.

This ample and rather sumptuous volume on the Washington flora by Professor Piper shows a great deal of careful study. The skeleton keys are very helpful, and seldom poorly constructed, though the skeletonizing of the specific characters is so attenuated as to be of little value in a number of cases where the author missed a permanent character.

The compilation of data under the species is very valuable.

The best part of the work is the Grasses.

The author has shown considerable independence in the reduction of many Rydbergian species and genera, but has not carried it far enough, particularly among the orchids where Rydberg has erected a spurious genus called *Piperia* after the author.

A few clerical and typographical errors creep in here and there but no more than would be found in any well prepared volume. One page 122 we find *Danthonia thermale* instead of *thermalis*. On the same page *Danthonia Californica* is credited to "1858-62" when it was 1863. On page 127 *Melica stricta* is left out though placed in the key. On page 147 under *Agropyron biflorum* we find *Agropyron* repeated in the line below when it should be *Triticum*. On page 157 the second Volume of the Botany of California is made to appear in 1876 when it came out in 1880. On page 160 *Eleocharis obtusa* should be (Willd.) Schultes. On page 165 *Carex festucacea* var. *brevior* should be (Dew.) Fernald Proc. Am. Acad. 37 and not as given. On page 167 *Carex Douglasii* var. *laxiflora* should be Mem. Torr. Club I 21 instead of 2 20. On page 171 *Carex nigricans* should have been Mem. Pet. I 210 instead of 211. On page 183 *Juncus alpinus* var. *insignis* was published in 1868 instead of 1866. On page 184 *Juncus Mertensianus* was published in 1833 instead of 1832. On page 187 *Allium cernuum* should be Rœm. Arch. I 3 instead of I 2. On page 165 *Calochortus Lyallii*'s range is given as Eastern Washington in the Cascades, if that is true the Cascade range has moved since I was there! Etc.

His treatment of the willows does not throw much light on that tangled mess. Nor does his handling of the *Betulaceæ* give as much light. His unnamed birch is evidently *B. pendula*. But whether his *B. occidentalis* is a form of *alba* or *pendula* or micro-

phylla is uncertain though it would point to alba. He keeps up *Alnus sinuata* when it is only a form of *crispa*, and also keeps *tenuifolia* separate from *incana* with which it freely intergrades.

FLORA OF COLORADO.

This handy volume by Rydberg is on the whole the best thing that has ever been printed on the Colorado flora, and cannot fail to be helpful. It shows a great deal of study of limited herbarium material, and in many of careful field study. It is remarkable for its absence of knowledge of some of the most extensive and best collections ever made in the state, and for its imperfect knowledge of the distribution of species in the state.

Mr. Rydberg's critical work seems to be confined to collections accessible to him at Columbia and is and always has been devoid of that large generalization and critical knowledge that he could have obtained at Harvard and Washington, and of the many valuable western collections. His specific limitations of new or rare species seem to have been drawn from single specimens and are always defective.

It is very unfortunate that Mr. Rydberg has become so saturated with the desire for new species and genera that his judgment is warped and he is led to magnify trivial and evanescent characters beyond all reason. This is the great defect of his Flora of Colorado. It fairly bristles with fake species and genera, and yet his keys are remarkably well made and valuable though they are at times misleading. If he could divorce himself from the species making craze his work would be good. There is a steady deterioration in his work since he got out *Physalis* and *Potentilla*.

The writer is informed by a personal acquaintance of Mr. Rydberg that he (Rydberg) feels aggrieved that people criticise his many new species and botanical methods since he claims that he is not responsible for them but the Brittonian system requires it. This may be true, but people judge others by the results, and when those results are plainly against all the observed facts in nature the man who produces them must suffer. It is to be hoped that my informant is in error for a man who is so weak as to submit his own judgment to that of another is not liable to have any judgment worth having. We know that botanical bossism is rampant in certain quarters but we had supposed that it confined itself to nomenclature and the selection of subservient men for the work the bosses had at their disposal.

THE ORIGIN AND DISTRIBUTION

of the

FLORA OF THE GREAT PLATEAU.

An Address before the Utah Academy of Sciences.*

In determining the origin of a flora, a person immediately assumes that its geological antecedents will govern its character, which is literally true, but is not true in the broad sense generally assumed.

It is a well known fact that there has been a constant expanding and progression in the flora of the world from simplicity to complexity, and from scarcity to abundance and the reverse: but the old dea of infinitesimal gradation due to Spencerian evolution is a myth. It is true that the term evolution still persists, like the vermiform appendix in man, but it is a misnomer and is improperly used for the involution of today. The word "Spencerian" is used advisedly as he defines evolution clearly.

There is no one fact in geology better established than the sudden appearance of great numbers of new genera and species without infinitesimal gradation, all along through the ages wherever there has been a marked change of climate. The assumption that this is due to incompleteness of the record or to migration is a kind of guesswork that can hardly be called scientific.

Climatic changes are due to celestial or terrestrial agencies, particularly the latter, and the latter are either general or local.

The celestial changes are infinitesimal in gradation, such as the decreased heat from the sun and the procession of the equinoxes. The latter is assumed to have caused the alternations of ice caps at the north and south poles, and also the glacial period. This precession must have made great changes if its effects were anything like what have been attributed to it. It is very probable and practically established that alternating ice caps have been so produced, but it is almost a certainty that it did not cause the glacial period, for this period did not reach its climax one hundred and twenty thousand years ago as the precession theory demands,

*The body of this paper was written in 1895, but has been withheld pending a complete examination of the species of the Great Plateau.

for, at least, the latter part of it was still in existence less than ten thousand years ago, long after the precession theory would have required the disappearance of the ice age. So whatever effect precession has had, it has been obscured so as not to be traceable in the vegetation of today. It must be conceded, however, that though precession would have had no effect on the vegetation of the Tropics, it would have caused a frigid and temperate climate over the northern and middle portions of the northern hemisphere while the ice cap was at the north pole, and would have driven the Tropical flora far southward toward the equator, and likewise would have extended the Tropical flora southward from the equator, equally. It should also be remembered that the precessional changes noted, must have occurred in the same way every hundred and twenty thousand years since vegetation began. So these alternations from Tropic to Temperate and Frigid must have occurred through all of the formation of ice caps, at least wherever there was land.

The terrestrial changes in the flora, as I have said, are of two kinds, gradual and paroxysmal.

Gradual changes are those due to a cooling globe and fixation of carbon dioxide gas from the air, in the form of carbon from the vegetation which has been turned into coal, and in hydrocarbons and lime beds which are buried in the earth: also erosion has been gradual. These three causes have almost completely altered the vegetation of the land.

The paroxysmal changes are those due to alteration of the surface of the land, due to earth folds, and are the most important of all in relation to the vegetation of today, while the other changes noted are the most important in geological history.

It is a practical certainty that vegetable life began in the sea at the poles, for that is where the first temperature cool enough for vegetable life would be found. This life gradually extended throughout the ocean of the globe, as soon as the temperature was lowered sufficiently for life to exist, this vegetation ultimately reaching the equator and being superseded at the poles by other forms of life better adapted to a lower temperature. As the ground emerged from the sea, land forms would appear first near the poles.

The Carboniferous Age, which, compared with today, was very early in the earth's history, was remarkable for the enormous development of land vegetation in quantity which has never been

equaled since, and was remarkable for the vast coal beds deposited then, for its tropical climate, and for the enormous amount of carbon which was removed from the air at that time by the vegetation. Though this vegetation was rankest in the eastern part of our country, there were large areas in Central Utah around the Wasatch and Oquirrh mountains where great forests and swamps abounded at that time.

At the close of this age nearly half of Utah and most of Nevada was elevated above the sea to an apparently great height, but if there was a temperate climate on the highlands, we have no evidence of it, for the remains of vegetable life in those regions could have been preserved only in the sediments of inland lakes, and none of them have as yet been discovered. But we do know, that at the elevation of the sea, the climate throughout the whole of the Mesozoic was tropical in Utah, at least, till the beginning of the Tertiary and probably beyond. This condition of things also greatly reduced the carbon in the air and therefore reduced at least the size of plants and increased their capacity for development. During this long stretch of time the vegetation of the earth took on more of the form of today, though still widely different. Looking at the land areas existing at that time, we find the country more like the Grecian archipelago than the land of today, because there were everywhere vast bodies of water and arms of the sea in all directions, which must have made the climate very humid still and precluded the existence of arid vegetation. Eastern Utah itself was still under the sea and the land extended in a long ridge only a few hundred miles wide, from Arizona to Alaska, while an equally narrow strip in central Colorado, surrounded by the sea, also extended toward the pole.

During the Tertiary all was changed. Practically the whole of the Great Plateau from Mexico to the Arctic Circle, and from the plains to the Sierra Nevadas, was bodily lifted high above the sea and had its vast mountain ranges and immense valleys, but the valleys were for the most part filled with great inland lakes or seas of fresh water. But during most of the Tertiary, at least near the sea level, the climate still was tropical throughout our region. However, in the latter portion of the age, we have unmistakable evidence of a temperate climate, though warmer than that of today, and many of the genera of plants now existing abounded around the borders of the lakes, as is proved by their fossil remains in the clay beds, and even some of the willows and cotton-

woods of today fringed their shores. The climate of that age must have been radically different from ours of today, as a whole, because of the presence of the inland seas, which must have caused the humidity to be at saturation point most of the time, and therefore, must have prevented the existence of arid plants. In addition to that, it was a period of great volcanic disturbance and immense lava flows covering thousands of square miles, which must have increased the temperature of the region and also the amount of carbonic acid in the air. It was also an age of mountain making, where the structure of today as we see it, was formed in large part, and where every possible opportunity for the development of vegetable life occurred, from the frigid to the tropical. The seas formed ample means for the distribution of seeds from one mountain range to another, and thus facilitating a uniformity of the vegetation along their shores, while the freshness of the water prevented the destruction of the seeds. During this time there is no doubt that the prototypes of the vegetation of today came into existence throughout our region, so far as a humid climate was concerned.

The close of the Tertiary was marked by the beginning of the Age of Man, by the draining of the great inland seas and probably by a still greater elevation of the land. The seas were doubtless drained mostly by the erosion of their outlets to the level of their beds, so that they all disappeared with a few insignificant exceptions, giving place to small rivers in their stead. This made a great and relatively abrupt change in the climate of the entire region. The humidity rapidly decreased from saturation to very little, and the skies changed from the almost perpetual fogs to bright and sunny and almost cloudless for most of the year, and with the clouds also vanished the vast swamps around the lakes and the solid forests which clad the mountains from base almost to summit, with a mantle of perpetual green. In their places came grassy plains and rugged mountains with scattered trees, and these again gave way to a sodless land of bunch grass and sagebrush and alkali plains and lakes. Utah and Nevada at that time were made of parallel ranges of mountains with broad valleys draining down to the Colorado and the Columbia. Soon after that, volcanic disturbances formed the Great Basin and cut off the drainage to the north and south.

At this time the profound disturbances of the earth's crust toward the north cut off the warm ocean currents from the North

Pole, which had hitherto prevented the excessive cooling around the poles, and thereby made possible the beginning of the Ice Age in America. There were still vast bodies of ocean water at the north, but the lack of opportunity for the rapid intermingling of those waters with the ocean currents of the south, resulted in a rapid cooling of the water in central British America and the formation of a so-called ice cap south of the pole which spread from the Atlantic to the Pacific and which reached its maximum thickness not far north from the British boundary line and was greatly augmented by the humid air from the waters of the sea which extended far up toward Minnesota and occupied a large portion of the lower Mississippi Valley. The cooling effect of this great ice cap and the glacier streams flowing from it, produced immense glaciers throughout Montana and Idaho, and smaller ones throughout the mountains of Utah, Nevada, and Colorado.

The climatic effects of the glaciation became profound and the temperate climate and its vegetation were driven so far southward that it was only found in northern Mexico and adjacent Arizona, and in its place in the valleys, came the climate and vegetation of the Arctic Circle or the Frigid climate as we call it now, while the mountains themselves throughout the Great Plateau were covered with a mantle of perpetual snow, at least to northern Arizona. The Great Basin then filled up largely with two great lakes called Lahontan and Bonneville, of which Pyramid and Walker Lake and Carson Sink now represent the one, and Great Salt Lake the other.

As the access of the ocean currents to the North Pole was still farther cut off and the Arctic lands emerged from the sea, and the great Mississippi Valley sea retreated far to the southward, the great ice cap of British America lost its supply of moisture and ceased to grow and then gradually dwindled away till it finally disappeared. So also the snows and glaciers faded away from the mountain chains, and the ice sheet retreated northward far into Alaska, taking with it its Arctic climate and drawing after it the old Temperate climate and its vegetation, which prevailed before the time of the Ice Age. As the climate changed to temperate, so the temperate plants crept up along the valleys of Arizona, climbing the mountains and the rim of the Great Basin, and slowly displaced the dying vegetation of the Frigid invaders, till it spread out over all the valleys and possessed the land from the Colorado to the Yukon, and as the climate still grew milder, the temperate

plants climbed the mountains and possessed their lower slopes as well as the valleys.

The frigid plants which once abounded throughout the Great Plateau, were for the most part unable to endure the increasing warmth and so died out in the valleys and gradually climbed the mountains, taking the place of the melting snows; and following close at their heels came the Temperate plants. The steady increase of the Temperate climate has finally driven the snows away, and the Arctic plants have climbed the mountains and now possess only the lofty peaks of the higher ranges, which are separated many miles from one another, but still have practically the same vegetation wherever a mountain peak has a Frigid climate from Nevada to Colorado or from Arizona to British America though many of the Arctic plants have been exterminated from the high peaks to the southward. Out of all the wealth of Arctic plants which once occupied our region, Utah still holds only a hundred or so and many of them are different species though belonging to the same genera. The only remnant of the Frigid flora we now have in the valleys of the Great Plateau is found in the flowers of early spring which bloom quickly, bear fruit and die away. Of such plants are *Orogenia*, *Claytonia*, and the sand lily, some onions, the sego, most of the phloxes, the yampa, the *Fritillaria*, many of the buttercups, and some others.

As the Temperate plants displaced the Arctic plants in the Great Basin and northward, so the Tropical plants have displaced the Temperate ones in all the region south of the Great Basin. A few of the Tropical plants, particularly annuals, have even invaded the Great Basin as far north as Salt Lake City, though they are very few and only represent the hardier outliers of the Tropical life zone. As none of the Great Plateau within the Tropical life zone has a humid climate, we find no plants growing naturally which are commonly classed as Tropical. It is interesting to try to trace back the origin of this Tropical arid flora from that of the humid Tropical as commonly understood. The descendants of the former Tropical seacoast appear to the botanist more as botanical monstrosities than as real descendants of those magnificent plants. Of all the six or seven thousand vegetable forms, there is but one descendant of this flora left, namely, the California desert palm, which abounds on the eastern base of the Sierra Nevadas. Of the botanical caricatures which are remotely descended from the same stock and which dot the deserts of the South like the dead

and dying trunks in a burned over forest, there is none so interesting or peculiar as the Joshua and the Yuccas, of which there are many kinds. They stand out in the desert like scarce crows with their great bayonet leaves, sometimes three feet long, sticking out in swabs at the ends of the stems, or like needles in a pincushion from the ground, as though protesting mutely at being compelled to exist in such a region as that. These descendants of the old Tropical flora are few in proportion to the main vegetation, but are quite different from the typical desert flora.

It is a botanical axiom that the vegetation of a region is in harmony with its environment, or in other words, the plants are so constructed that they can exist and even thrive under the climatic conditions in which they are found. This leads us to the subject of life zones.

LIFE ZONES.

It has been noticed by all acute travellers, doubtless since the beginning of man, that different climates have different vegetation. This is particularly noticeable to all mountain climbers within the Tropics. When this subject first became known through any publication, no one seems to be able to state. The first definite statement of wide circulation and general acceptance was probably that of Humboldt about the beginning of the last century. From the tops of the lofty mountains of Mexico there dawned upon him with tremendous force the fact of life zones, as looking from those peaks he could see at his feet the Alpine flora, and farther down at middle elevations, the vast forests and waving plains of the Temperate flora, and far down near the sea, the palms, bananas and oranges of the Tropical flora. This led him to classify vegetation into *Tierra fria*, *Tierra temperada*, and *Tierra caliente*. This same idea was further expanded when applied to the globe as a whole, and has received universal acceptance, into the Tropical, Temperate, and Frigid life zones, the fundamental idea underlying it all being that of temperature. From a botanical point of view this classification has one element of error, as is shown by an exhaustive study of the vegetation, and that is, the Frigid does not deserve to be classed as a life zone, because its plant life differs but little from that of the Temperate, though the physical and visible effects are very noticeable. There are then but two great life zones, the Tropical and the Temperate. An exhaustive ex-

amination of the vegetation determines this fact with absolute certainty which will be shown later by diagrams. A common error into which many have fallen, has led to a serious misconception of life zones by the failure to recognize the effect of humidity on the climate. Hitherto the Tropical life zone has generally been supposed to be confined to that portion of the earth's surface bounded by the Tropics of Capricorn and Cancer and confined to the humid portion, but a moment's thought will show that the arid portion belongs equally with the humid, though its flora is almost totally different from that of the humid. This fact is also established by the knowledge that you can grow the date palm, oranges and lemons, pomegranates and figs in the arid regions by simply supplying the necessary moisture artificially. This therefore establishes the fact that the climate of Arizona and southern California is truly tropical and that we must not bound the Tropical life zone by the Tropic of Cancer, for that flora extends much farther north.

In 1883 in the *Western Galaxy*, the writer published an account of the origin of the flora of the Great Basin, giving in other words a general summary of the facts here noted, but claiming no originality for these ideas because as a schoolboy, about 1860, he had studied in Cornell's *Geography*, these same facts, though he had never had an opportunity to verify them himself till in 1878 when he had stood on the lofty mountains of Colorado and had been profoundly impressed with the same thoughts experienced by Humboldt a century before.

In 1884 a young man in the employ of the Government, Dr. C. Hart Merriam, who was studying the animal life of the San Francisco mountains in Arizona, stood on the lofty San Francisco peak and according to his own statement to me, saw for the first time this magnificent panorama before him and had impressed upon his mind also, most forcibly, the subject of life zones, and so he proceeded later on to elaborate the life zones and give them names of his own, ignoring the work of Humboldt and all his predecessors. He classifies the life of our western region as Sonoran, which he divides into Upper and Lower, Transition, and Boreal. His classification is most remarkable in the manner in which it ignores botanical facts. It was confessedly based on the distribution of animal life, but without knowing much of anything about the vegetable life, he states that the botanical and zoological life zones are identical. Whether his zoological life zones are

correct, I am unable to state from my own knowledge, but in conversation with other zoologists I find that his classification is not generally accepted as accurate. But I do know from an examination of nearly ten thousand different species of plants, which is several times the number of species of animals which he has examined, that his classification is inaccurate. His Sonoran lies half in the Tropics and half in the Temperate life zone. His Transition is a remarkably distinct life zone, showing less transition and a more distinct flora than any other life zone in his classification, and it corresponds to the central portion of the Temperate life zone. His Boreal is equally divided between the Temperate and the Frigid and cannot by any possibility be separated from the Temperate. This will be shown conclusively by my diagrams of the distribution of families, genera and species. It is a generally accepted rule that the first publication of an idea or a name shall be used among scientific men unless it shall be shown to be fundamentally erroneous or inapplicable. Therefore the classification of Humboldt, which has never been improved upon, must take the place of that of Merriam. We therefore have the life zones of the world divided into two grand divisions, Tropical and Temperate. As I have already indicated, the Tropical must be divided into Humid and Arid, and only the latter division is found in our region. The upper temperature limit of this life zone is about 60° annual mean and crosses our region in a very irregular line, following the topography from Albuquerque, New Mexico, to the Salt River Basin, Congress Junction and Kingman, Arizona, thence in a narrow tongue up to and beyond St. George, Southern Utah, and westward through Delamar to Goldfield and Owens Valley, California. All south of that line is tropical: from this tropical base there arise islands, peninsulas and other large areas reaching into the Temperate life zone, represented by isolated mountain peaks and ranges, like the Charleston, Providence, White, Catalinas, Pinals, Huachucas, Santa Ritas, Chiricahuas, Floritas, Sandias and Mogollon mountains and the Great Plateau of the San Francisco region in Arizona; in the latter, the upper division of the Temperate, namely, the Frigid, is represented on the high peaks. The Tropical life zone is chiefly characterized by large numbers of cactus plants, Yuccas, thorny shrubs, and annuals. In our region north of Mexico this life zone is represented by twenty-two families which are found only in that zone, and by seventy-three families which also extend into other zones. The total number of

families existing in our region is 136 and therefore the exclusive families are 16.2% of the whole and the non-exclusive, 50% of the whole. The remarkable distinctness of this life zone shows that it has five times as many exclusive families as any other. The total species in this life zone which are found nowhere else, are 1460, while the species which are found in other life zones are only 250 out of a total of 5910 species in all the life zones. The genera which are found in this life zone and nowhere else are 257, while those which are found in other life zones are 282. The total genera in all the life zones are 1040. The varieties found in this life zone exclusively are 188 and the non-exclusive ones are 28 out of a total number of varieties in all the life zones of 1102.

Comparing the Tropical with the Temperate life zone, we find that the Tropical contains 22 exclusive families, while the Temperate has but 12. The Tropical has 257 exclusive genera and the Temperate 327. The Tropical has 1460 exclusive species and the Temperate, 3461. The Tropical has 188 exclusive varieties and the Temperate, 746.

Of the genera in the Tropical life zone we find that 92 range to the Lower Temperate, 104 to the Middle Temperate, 46 to the Upper Temperate and 28 to the Frigid, out of a total genera of 1040. It should be borne in mind that as a general rule, where a genus ranges from one life zone to the other, the species occurring in the other zone are different, though belonging to the same genus. These species in the Tropical life zone which also occur in the Lower Temperate, are 170, those which range to the Middle Temperate are 31, those which range to the Upper Temperate are 4 and belong only to the lower orders, while none range to the Frigid, out of a total of 5910 species. The varieties which range from the Tropical to the Lower Temperate are 1 and none to the Middle Temperate, 1 to the Upper and none to the Frigid, out of a total of 1102 varieties. The predominant genera which are not exclusive, are 91 in the Tropical and 226 in the Temperate.

The Temperate life zone, of course, occupies all of the line north of the Tropical. It is conveniently divided up into four divisions, which were first published by the writer about 1898 in his Schedules of Zonal Distribution, which were not intended to represent the general conditions throughout the world, but local only. The names to these life zones were given for the characteristic plants which abound in most of those regions. The Tropical was called the Larrea, and the Temperate was divided into four

zones, the Juniper, Deciduous Oak, Spruce and Frigid. In my *Geography of Utah*, published in 1902, I published the permanent divisions of the Temperate as applicable to the world at large, namely, Lower Temperate, which corresponds to our Juniper, Middle Temperate, corresponding to our Deciduous Oak, Upper Temperate, corresponding to our Spruce, and Frigid, which includes not only the Arctic plants but the Alpine plants as well. The upper temperature limits of the Lower Temperate life zone are approximately 52° annual mean. The upper Middle Temperate limit is approximately 46° annual temperature. The Upper Temperate life zone limit is not much above 32° and the growing period is therefore very short, belonging to the zone above it, the Frigid.

It is practically impossible for the novice and decidedly difficult for the skilled botanist to determine the limits of the life zones experimentally in the field without having some characteristic plant as his guide. This is why both Merriam and myself have used certain plants which are characteristic of certain life zones, as zonal plants, though neither of us originated the idea. Merriam uses the pinon as the zonal plant of his Upper Sonoran, which corresponds to my Lower Temperate; but the pinon is not a good zonal plant for the reason that it is relatively scarce compared with the juniper. I therefore use the juniper as my zonal plant. Merriam has used the *Larrea* as the zonal plant for his Lower Sonoran, which as I have said is the true Tropical life zone in our region. No better selection than this could be made. In the selection of a zonal plant for the Middle Temperate (Transition of Merriam) the deciduous oak is the most conspicuous, but unfortunately disappears both west and north of the Oquirrh mountains. The spruce and firs are good zonal plants of the Upper Temperate. The Frigid or Alpine life zone includes the subalpine and treeless regions and meadows of the high peaks and Arctic and needs no characteristic zonal plant.

Beginning then with our zonal plants, we find the *Larrea* or creosote bush abounds throughout our Tropical region and comes to a sudden stop in a region where the annual temperature is less than 60° Fahrenheit. There will be, of course, mountain ranges whose southern and hot slopes will have the *Larrea* growing high up on that slope and low down on the northern slope. There will also be places where streams flow down through canons far into the Tropical life zone where temperate plants only grow. A mo-

ment's thought will show even the novice that plants thrive on the particular spots on which they grow according to the climate at that particular spot, and therefore the *Larrea* must grow high up on a hot slope and will not grow along a stream where the climate is more humid and where cool breezes from above follow down the canon. So these life zone tongues will be found to dovetail into one another wherever there are mountains and canons or large streams. But when their origin is once understood, they need not cause confusion.

It almost never occurs that any zonal plant will stop off suddenly at the upper and lower limit of its own zone. And we must therefore recognize this fact in the use of zonal plants.

The juniper makes an excellent zonal plant for the majority of the Great Plateau, since it begins suddenly near the limit of the *Larrea*. A better plant still than this so far as the southern limit of the zone is concerned, is the sagebrush, which begins at the upper limit of the *Larrea*. Whenever either of these two plants is seen growing, the traveller may know that he is entering the Lower Temperate life zone. In southern Arizona, however, the juniper does not occur sufficiently to be of value, but its place is taken by the live oaks, which are as characteristic of the Lower Temperate life zone there as the juniper is in the Great Basin. The upper limit of the Lower Temperate life zone is immediately recognized by the disappearance of the live oaks in Arizona and by the disappearance of the juniper and the appearance of the deciduous oak from Colorado to California. So that wherever the deciduous oak is absent, the limit of the juniper is a good index of the limit of the life zone. The two junipers which must be used as the guide for this zone are the Utah juniper and the California juniper, and they must not be confounded with the red cedar, which is a juniper, or the thick-barked juniper of the San Francisco mountain region in Arizona, which belong to the next upper zone. The Utah juniper is also called the white cedar. The upper limits of the Lower Temperate life zone form a very irregular line skirting around the base of the Rocky Mountains from Cheyenne, Wyoming, to New Mexico, westward to southwestern Colorado and northward to the Book Cliffs and westward to Castle Gate, southward to Fish Lake and the Colob, and northward through the Sevier valley to Salt Lake City and Logan, and northward westward to the Snake River and eastern Oregon, the mountains of western Utah and Nevada forming islands on the Lower Tem-

perate life floor.

The Middle Temperate life zone for the most part forms belts around all our mountain ranges and does not come down into the valleys until we reach Logan and southern Idaho. From there northward into Central Montana, it occupies most of the valleys only and swings around to the northwestward in western Idaho. The deciduous oak disappears generally very irregularly before we reach the upper limit of this zone, but its upper limits can generally be well made out by the beginning of the spruces and firs and by the disappearance of the yellow pine and the sagebrush, which extends from the limit of the Tropical to the beginning of the Upper Temperate life zone.

As I have said, the beginning of the Upper Temperate life zone is represented by the appearance of the spruces and firs and the disappearance of the yellow pine and sagebrush. Throughout the lower portion of the Great Plateau the most conspicuous and valuable tree representing this life zone is the aspen, but in the Montana and Idaho region it is valueless as a zonal plant because it grows low down in the valleys in the Middle Temperate life zone. The upper limit of the Upper Temperate life zone from a botanical point of view, is very poorly defined, but in general it can be used to apply to the limit of trees where the elevation is high, but care should be used in remembering that trees are absent from all ridges on the higher mountains, often a thousand feet or more below the real timber line, while the Alpine meadows and meadows of the Upper Temperate life zone intermingle in a most confusing way so that only those meadows which are manifestly extensions of the treeless region above, should be called Alpine or Frigid.

PLANT FORMATIONS.

Now having given a running summary of the life zones, we naturally come to the subject of plant formations, which are subdivisions of the different life zones caused by local conditions of climate and soil, and modified by barriers. The word plant formation is supposed to apply to a restricted area in which the plant life is substantially the same. This may be due to the condition of the soil, such as the various alkaline formations of which the Salt Lake formation is the most conspicuous, but it is generally due to differences of climate and isolation. For example

isolation is brought about by a flora being separated from another similar one by an entirely different botanical condition. Take for example the densely forested area of the Appalachian Mountains, particularly at the south. That contains doubtless quite a number of trees which would grow in portions of our mountains, and it contains many other plants among herbs which would thrive here if it were not for our isolation. This is brought about by the vast treeless region west of the Missouri river, whose climate is sufficiently different from that of the Appalachians to make it impossible for those plants to grow there. This then becomes a vast barrier over five hundred miles in width, extending from the Great Lakes to the sea, which will effectually prevent plants from going from the Appalachians to the Rocky Mountains unless carried there by animals, which in a state of nature would scarcely be possible, or whose seeds might be carried there by the wind, which is almost an impossibility for anything but the most minute seeds, and an absolute impossibility for seeds of any size. We therefore do not find any plants in the Great Plateau which have come directly from the Appalachians, at least not since the glacial period.

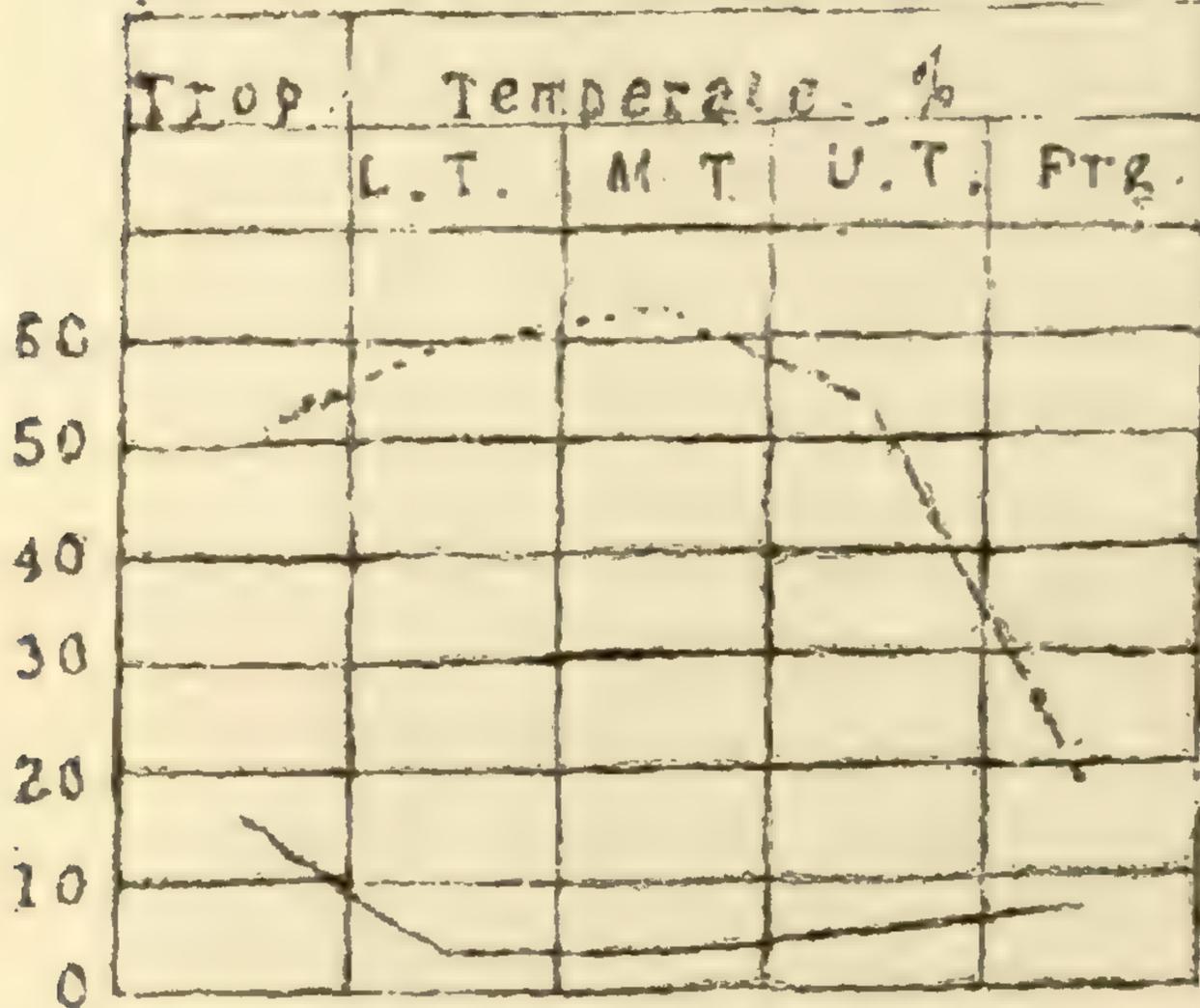
Another kind of barriers is found in the mountain ranges. It is almost a uniform fact that the mountain ranges of the Great Plateau run north and south and parallel to each other and have wide valleys between. These mountains therefore act as barriers to easy progress of plants from east to west or the reverse, because the climate of the mountains is different from the valleys below and therefore valley plants must be carried by the winds or animal agencies, through their seeds, from one valley to the other before they can appear in the valley beyond, and consequently it would be long periods of time before all the valleys of the Great Plateau could in this way intermingle their vegetation till they would have a uniform life, which has never occurred to the present time. The vegetation of the different life zones on the mountains would have to have the seeds blown over the mountains and over the intervening valleys before they would find a congenial soil and climate in the range beyond. This would be less difficult than the transportation of the valley plants from one valley to another, but it still would be fraught with many drawbacks. In this manner we expect to find and do find a decided difference in the vegetation in the valleys as we progress from the plains to the Pacific. For example the vegetation of the Columbia Basin is decidedly different from that of the rest of the Great Plateau. The

vegetation of the Carson Lake region is devoid of many plants growing in the Salt Lake region, and in addition it contains many plants from the Californian region. As we go westward from Salt Lake City we meet the California immigrants one by one. For example *Madia sativa* has traveled from the Pacific Coast to Salt Lake City and central Montana, but has not yet reached Colorado. *Astragalus tetrapterus* has traveled from southern Utah and adjacent Arizona northwestward as far as Coöre, Nevada. *Astragalus scopulorum* has traveled from central Colorado to Thistle, Utah. Many illustrations of the like, both cultivated and native plants, might be given to show the effect of barriers. The influence of barriers is very much obscured by slight differences of climate which have an infinitely greater effect on the plant formation than the barriers. This is well illustrated in the Columbia Basin, whose peculiar flora is due more to an increased humidity than to barriers. Another illustration is the difference in climate between that portion of the Great Plateau east of the Rocky Mountains and that which is west of them. The Colorado climate up to the crest of the mountains, is that of the Mississippi valley, which is characterized by a slight rain and snow fall in the winter and a relatively excessive rainfall during the growing season. The climate of the region west of the Colorado Rocky Mountains is characterized by an excessive rainfall during the winter or resting season and a great deficiency of the rainfall during the growing season, and therefore sections having the same rainfall are strict-arid in the Great Plateau, while in the central Colorado region they are only semi-arid. This is noticeable at once by the fact that you can divide these two areas into the sodded and sodless regions. There is practically no sod west of the Rocky Mountains, while east of the Rocky Mountains the whole region is sodded to the Atlantic Ocean. The plains are characterized by a grassy sod in every direction, while the Great Plateau for the most part has only little tufts of grass here and there scattered among the sagebrush and similar shrubs which cover the Great Basin. This results in a very great difference in many ways. There are very many more annuals and bulbous plants in our region; it is a region of omnipresent dust and in the hotter portions, of dust and sand. There is also an enormous amount of alkali in almost every portion of the Great Plateau, which affects the vegetation in the valleys particularly. It makes many of the valleys deserts, whose centers contain absolutely no vegetation and whose margins con-

tain only plants suitable for growing in highly alkaline soil. Under these conditions we would expect to find an excessive number of new species and genera on these alkaline plains, but as a matter of fact, we find very few. In all the ages which have passed since the glacial period, but two genera have been produced from Salicornia, which are *Spirostachys* and *Sarcobatus*, while the new species are very, very rare. But among the other plants, we find quite a number of new genera which have developed in these isolated regions, and many new species. But it is not in the Great Basin proper where there has been the most production of new species and genera. It is in the hot regions of Arizona and northern New Mexico where the enormous differences in climate of the plains and mountains and the absence of great water courses, and the multitudes of scattered springs or water holes have produced the isolation necessary for the production of new vegetable forms. To go over all the plant formations which have been produced by these conditions, would consume entirely too much time. Practically all these formations were published by me in my *Schedule of Zonal Distribution*, published 1898. I will only mention a few of the more important formations. Among the alkaline formations the most important are the Salt Lake which covers the old bed of the lake in the Bonneville area, and the Pyramid Lake formation, which extends from Walker Basin on the south to the Malheur on the north, occupying the old Lahontan Basin. Probably the most semi-alkaline formation and the most peculiar of them all is what I have called the Navajo Basin, which occupies that great desert area, whose soil is a peculiarly tenacious clay and is remarkably devoid of vegetation, which extends from the Uinta Mountains to Arizona. Another area but little less interesting is the similar Green River Basin north of the Uintas. A very unique and remarkable region is the lava bed formation extending from Glenn's Ferry to St. Anthony. The lower Snake river formation is also attractive, which extends from Glenn's Ferry to Huntington. These are all interesting valley formations. The Lower Colorado formation is a vast area with a very uniform flora, which embraces Death Valley, the Amargosa Desert, the St. George region, and extends to the Gila and the mouth of the Colorado. Beginning in the vicinity of Tucson, Arizona, and extending eastward to and beyond El Paso and northward to Albuquerque, is what I call the El Paso formation, which covers all those valleys and is characterized by ex-

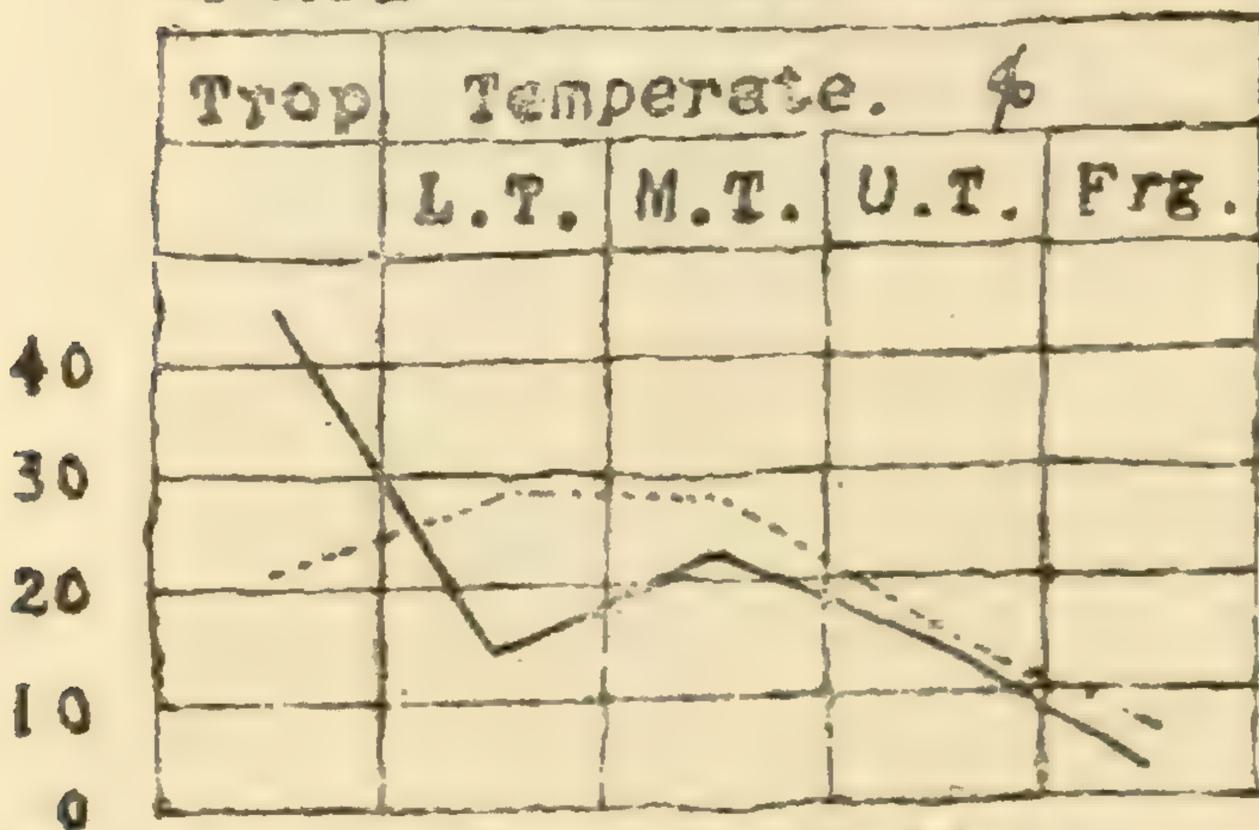
cessive summer rains and a gorgeous vegetation, which is desert until July, but at the first touch of the summer rains, starts into life and reaches its climax in September. These latter are tropical formations. The most important mountain formations are the Colorado formation, the Uinta, the Wasatch, the East Humboldt, the Malheur and the Sierra Nevada formations. The southern formations of most interest are the Kaibab, the Henry Mountains, Funeral Mountains, Panamint Mountains and White Mountains, all belonging to the northern portion, while the central portion is represented by the San Francisco Mountains and the Mogollons and Sandias, while the southern portion is represented by the Piñals, Catalinas, Huachucas, Santa Ritas, Floritas, Chiricahuas, and the Sierra Madres of Northern Mexico.

Families of Great Plateau



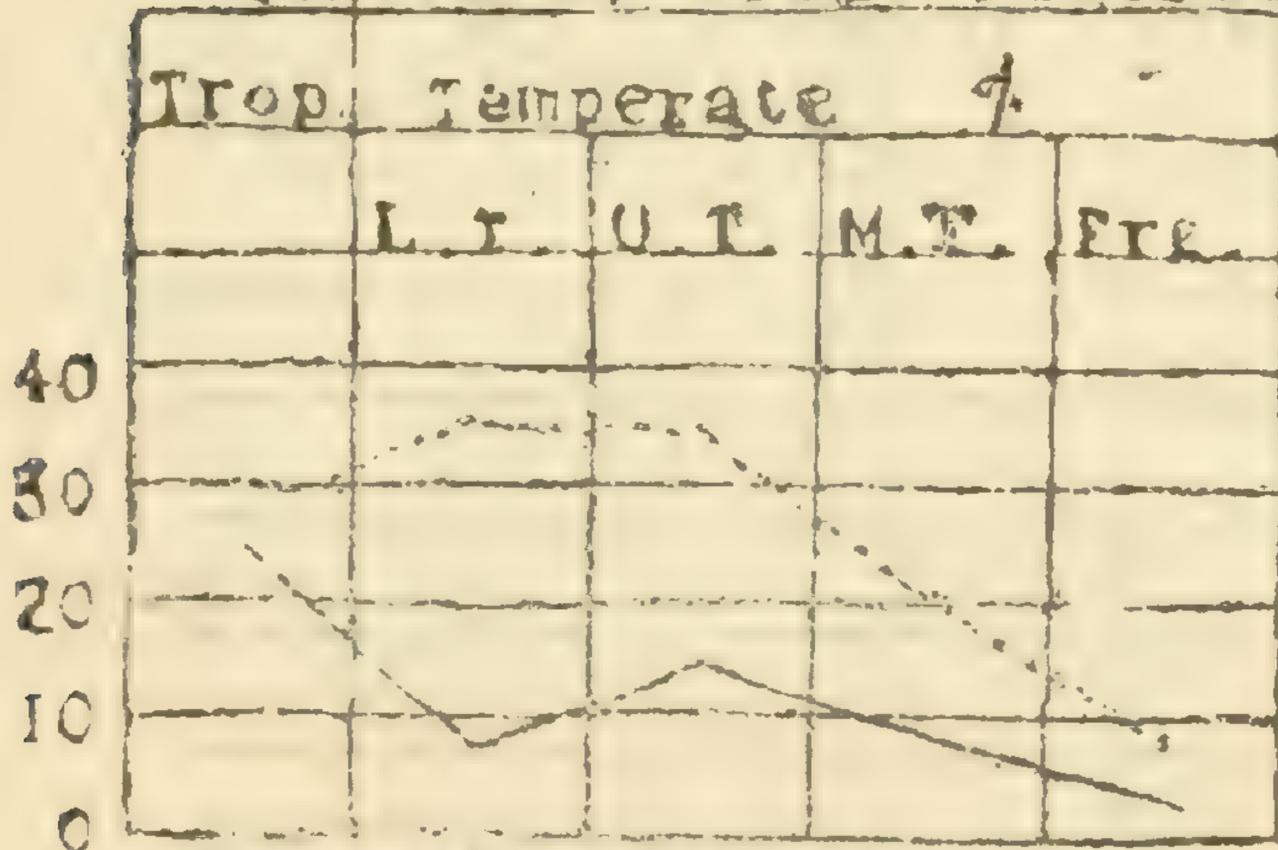
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Genera of Dicotyledons.



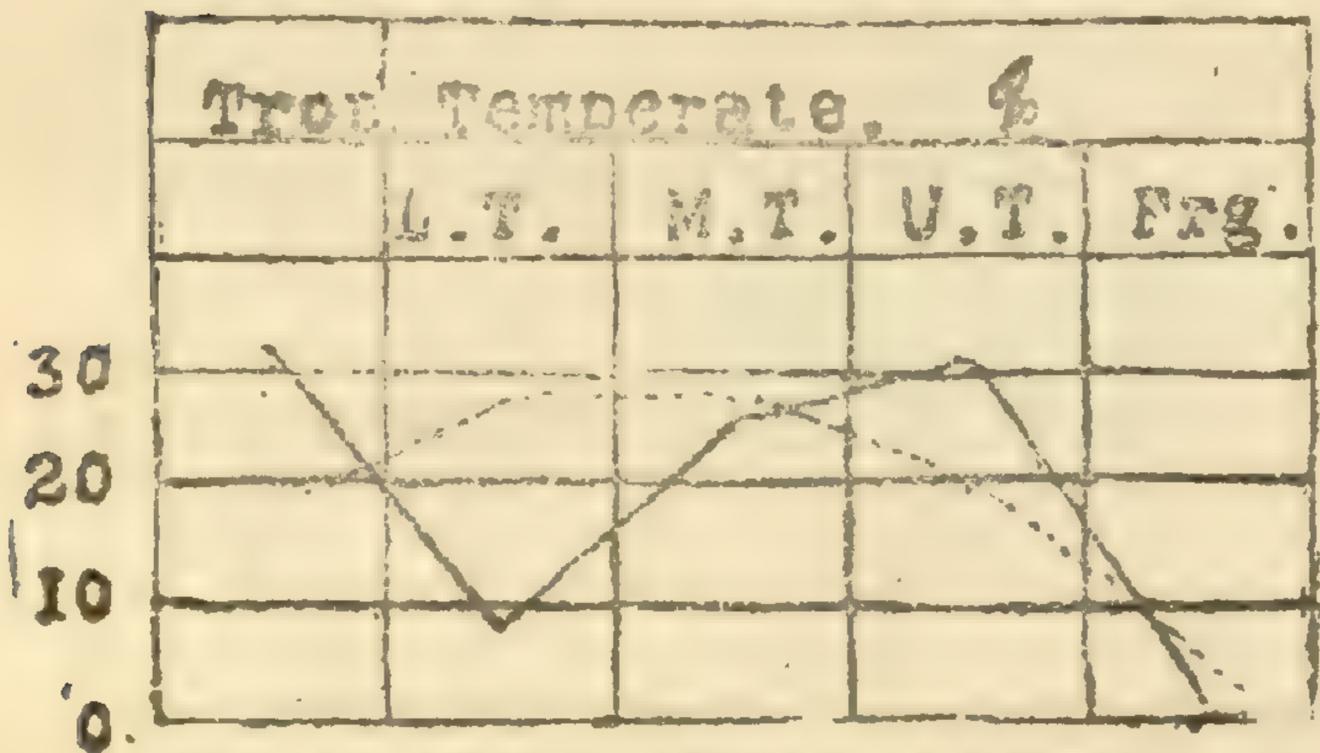
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Genera of Great Plateau.



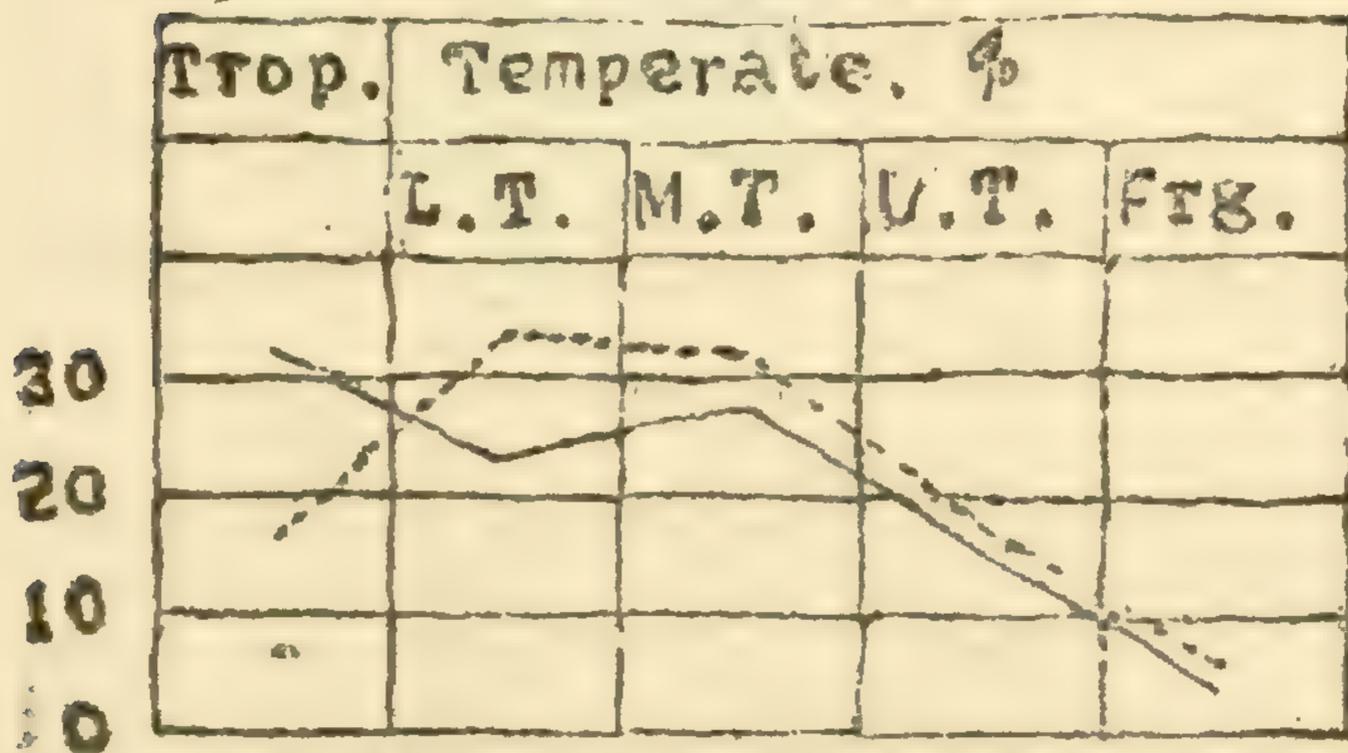
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Genera of Monocotyledons.



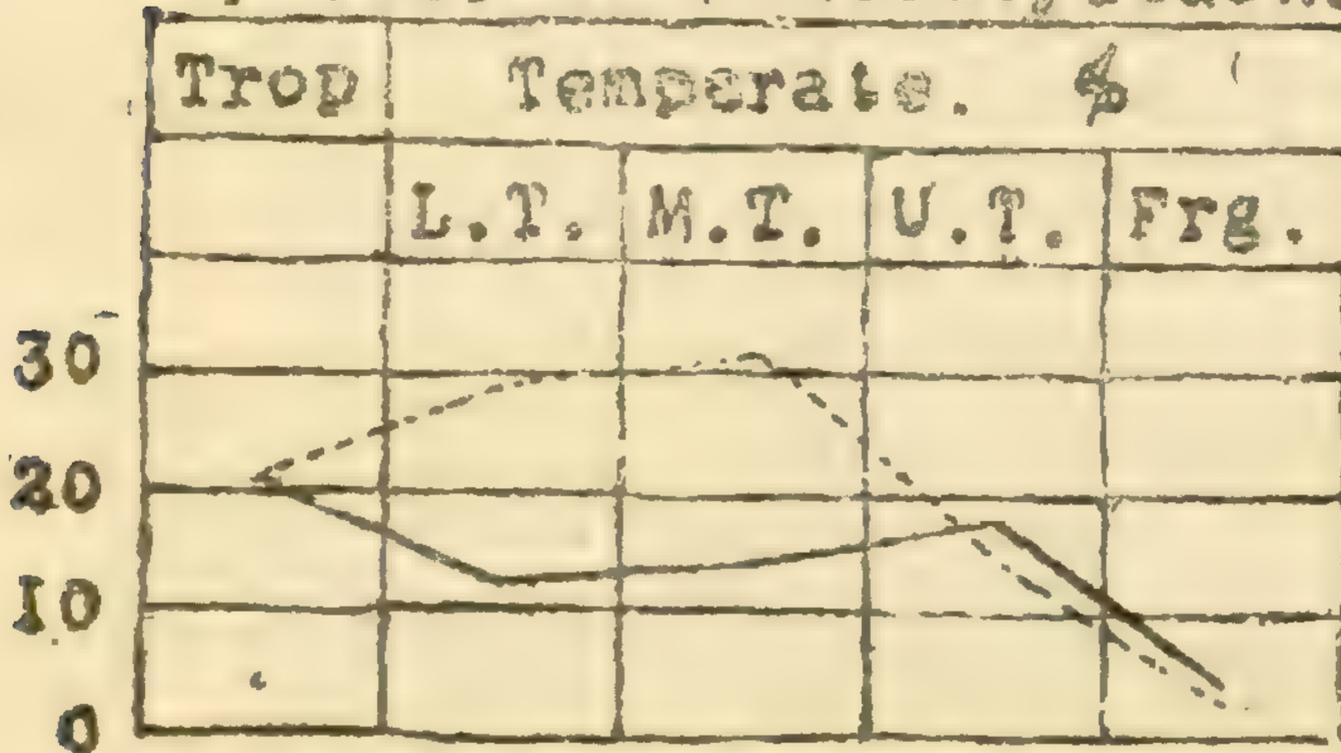
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Species of Dicotyledons.



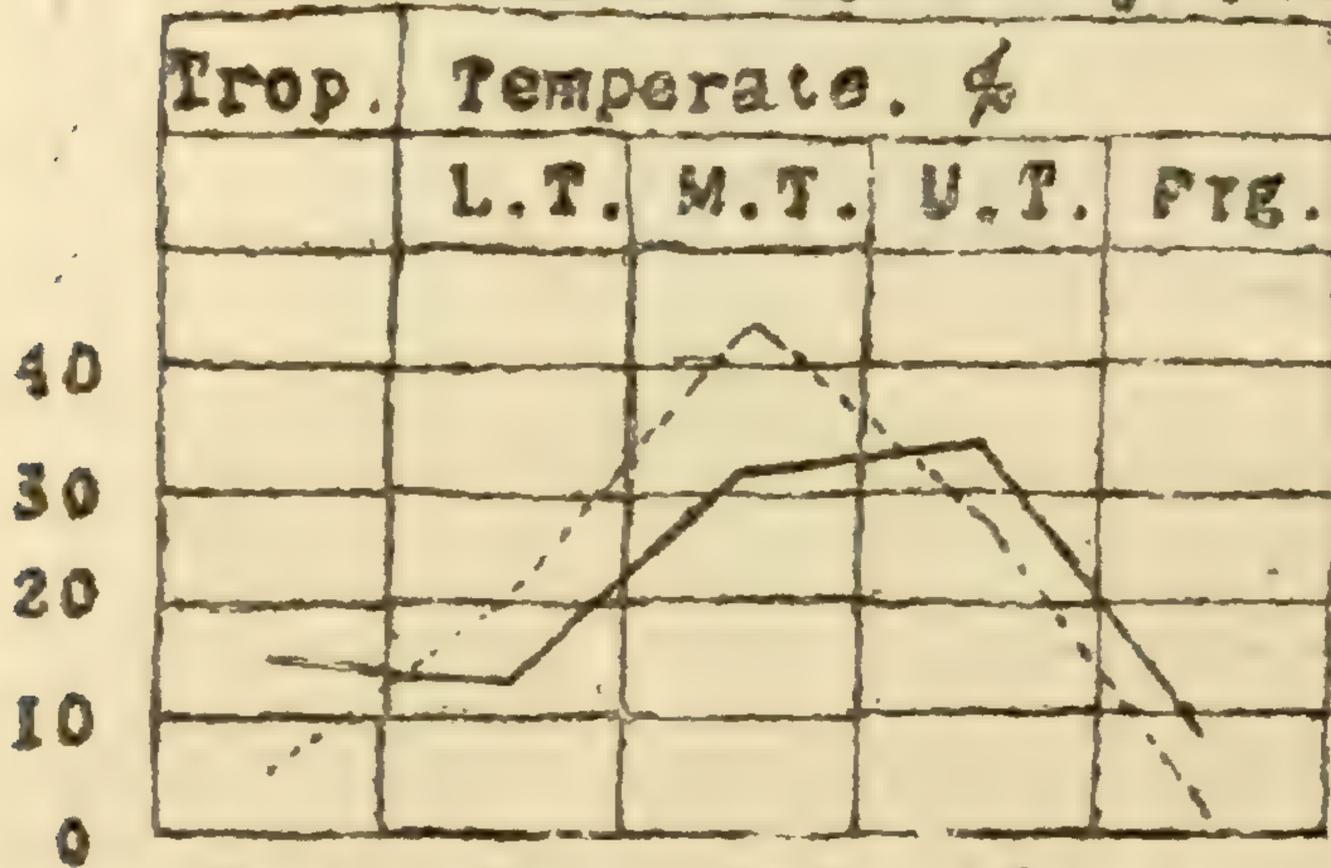
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Species of Monocotyledons.



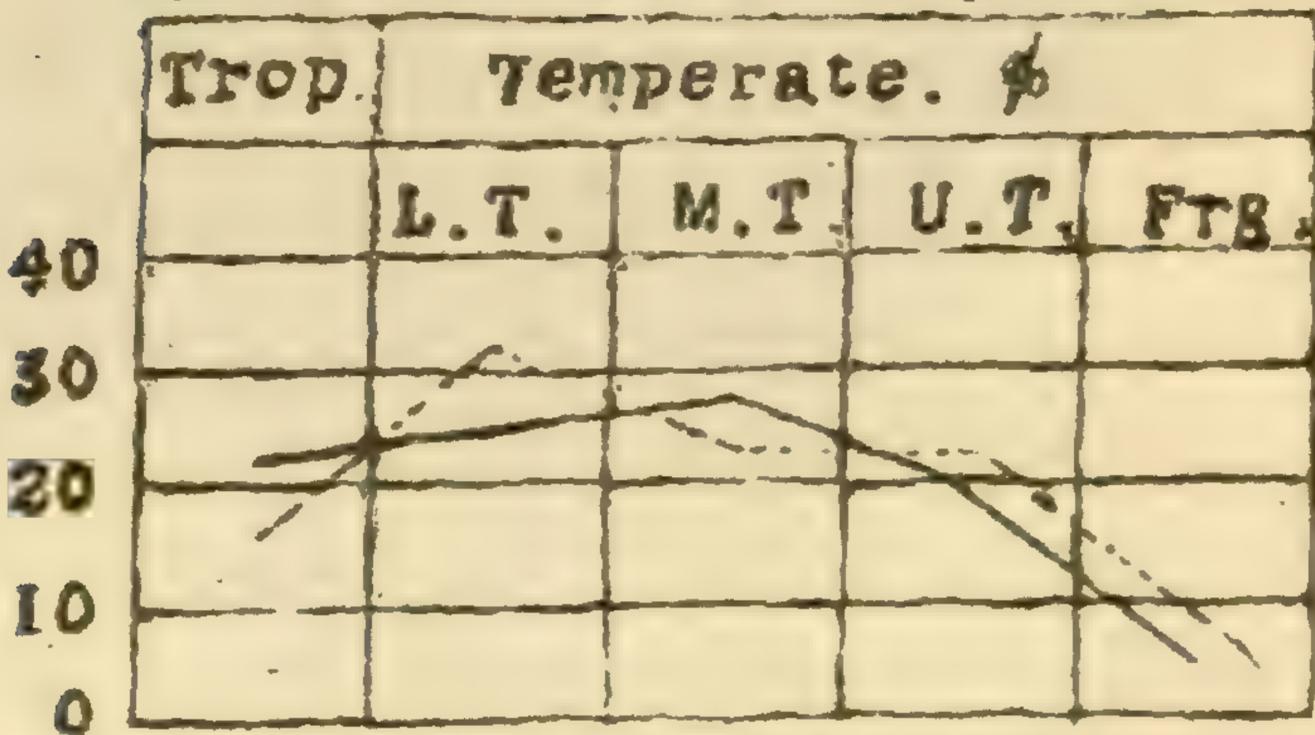
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Varieties of Monocotyledons



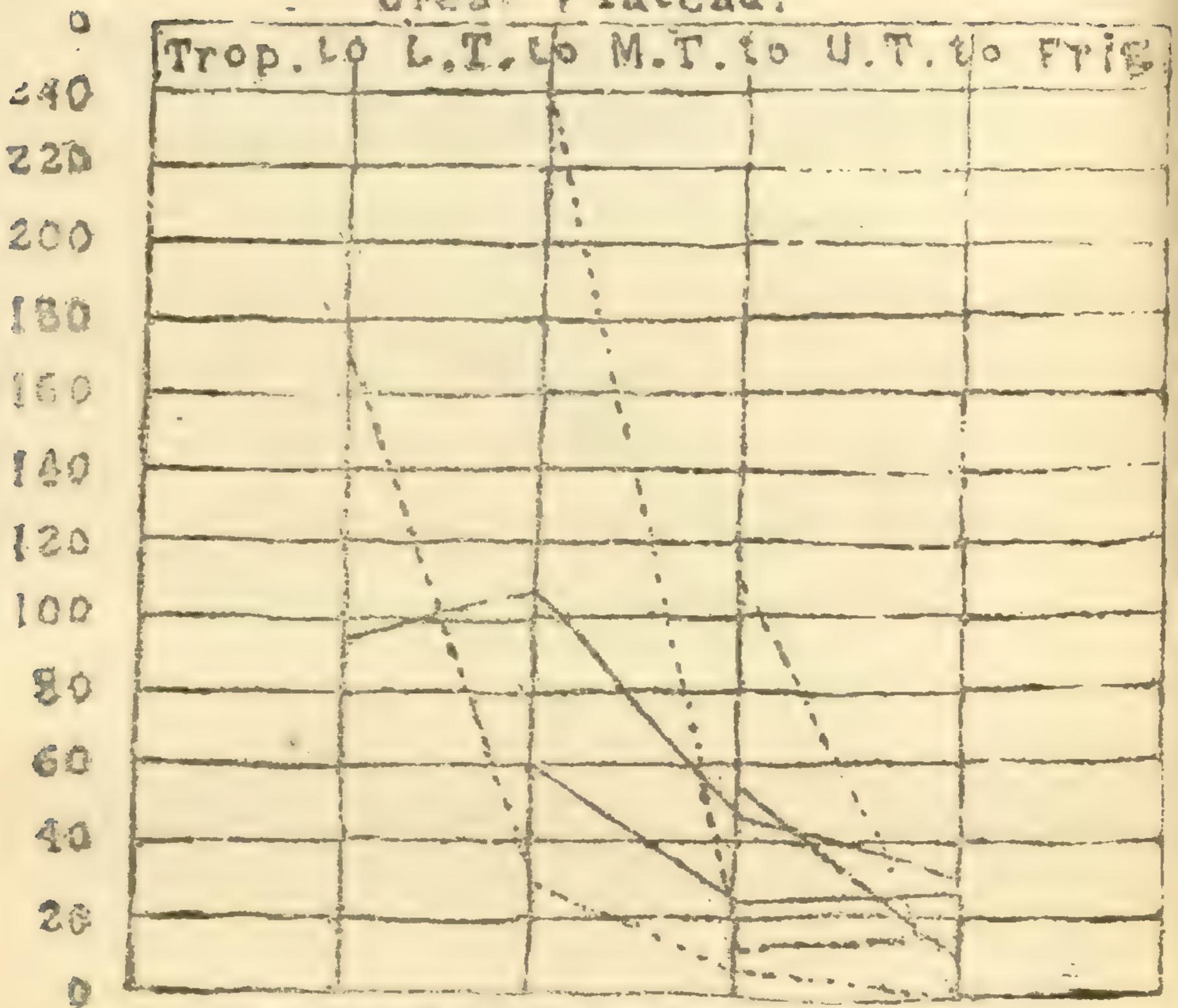
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Varieties of Dicotyledons.



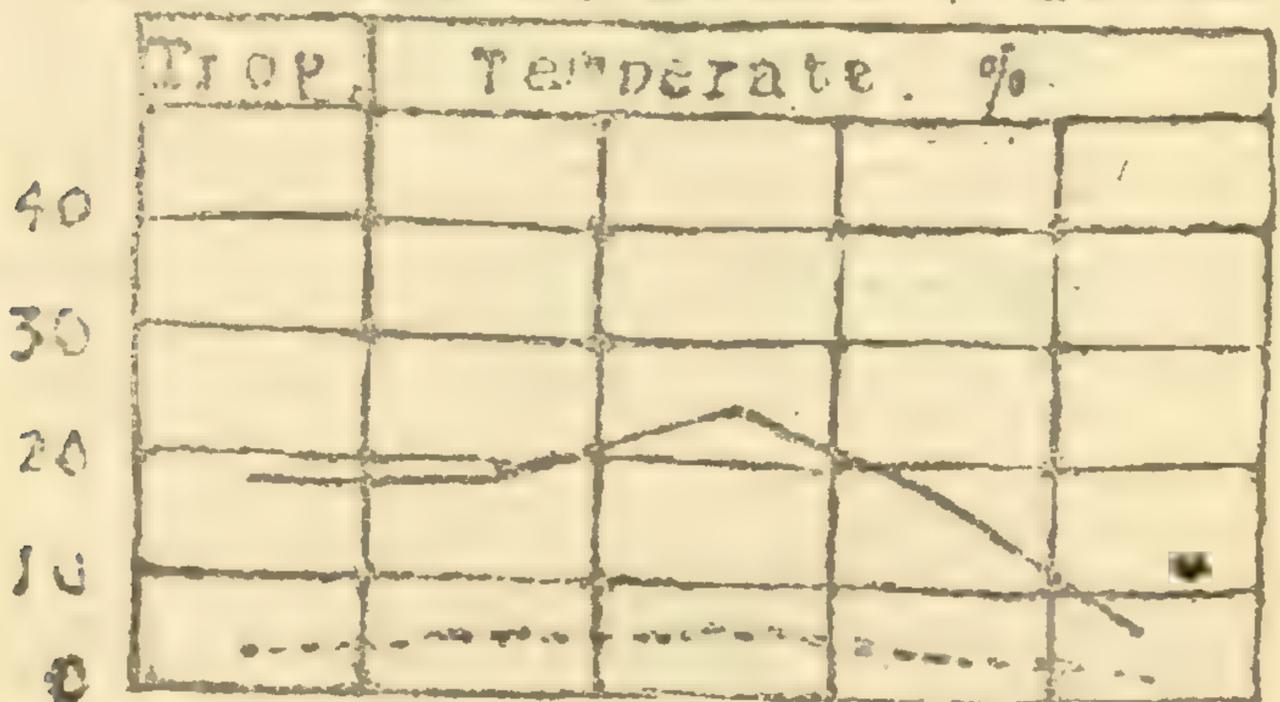
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Range of Plants
Great Plateau.



Genera are full lines.
Species are dotted lines.

Varieties of Great Plateau.



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LUPINUS.

In two recent numbers of *Muhlenbergia* Mr. Heller begins his onslaught on this inoffensive genus, in his characteristic Hellerian way by trying to name everything in sight, apparently following his old statement that any name (varietal, subvarietal, form or otherwise) for a plant should be a specific one, and yet he assumes that his proposed species are comparable with reputable species. He takes up characters long proven fallacious and uses them as specific ones such as size and color of flower, length of peduncle, variation in depth of sinus in calyx, variability in shape of bracts, shape of banner, etc. One would suppose that a person having spent even one season in the arid region would know that the development of flowers and peduncles is governed by the humidity and sunshine during the period of blooming, and that dry seasons make material differences from wet seasons. But all these things go for naught with Mr. Heller. Even plants from the same seed growing together are different species to him, if one has a slight variation from the other. *Lupinus brevicaulis* is the chief center of his attack this time, though he also does a little with *L. malacophyllus* without apparently mentioning its chief characteristic. His work shows a particular lack of material and fully warrants the refusal of some botanists to allow the use of their herbaria for such crude work.

Pentstemon Eatoni var *undosus* Jones. 1895.

P. coccinatus Rydberg, Dec. 1909.

The writer fails to see how Mr. Rydberg can escape the guilt of deliberate and intentional injustice and botanical piracy in publishing his species as he quotes my variety as a synonym of his species and he knows that it is the oldest name and not preoccupied. Heretofore he has been a great stickler for priority and has made many changes of specific names because of previous varieties antedating them. For twenty-five years it has been the rule in America to adopt varietal names when raised to specific rank. It would take a Philadelphia lawyer to keep track of the kaleidoscopic changes of front of certain botanists on nomenclature. If

the Brittonian nomenclature has been at last interred for good would it not be a bright idea to send out invitations to the obsequies, or is the funeral to be strictly private?

It is a fitting end to a system (professedly founded on rectifying the injustice to botanists who first described varieties or species and being robbed of the credit by the transfer to a species or other genus according to the Candollean rules) that it should degenerate to bald botanical piracy. Rydberg repudiates the very system under which he has trained so long (after making about as many changes as were possible under it) and seems to adopt such part of the Vienna rules as suits him in order to make a new name. If anyone were seeking evidence of the personal character of Rydberg's work that it is governed more by petty spite than scientific knowledge this would be ample evidence.

In the same number (Torrey Bulletin for December) which came to hand after the rest of this paper was printed Rydberg illustrates the point made by me above that when you pass the true specific limit in any case and make species out of variations the only limit thereafter is chaos, for intergrades of all sorts abound and to be consistent the maker of these fake species must continue to make species out of every possible variation between his so-called species till they are legion, and every new batch of species requires still more, ad infinitum. As the writer has already shown in previous Contributions Rydberg's and Greene's work on *Mertensia* is untenable, and so in the latest Bulletin Rydberg is compelled to make a new batch of *Mertensia* intergrades between the former species and of course has to give them new names for they are as good species as any he has described, and none of them are any good. Fortunately some of these are from material collected by myself and my boys (F. E. Leonard, etc.) of the old Agassiz club, when we were on our botanical trips about Salt Lake City in the later eighties.

A FLORA OF CALIFORNIA,
by W. L. Jepson.

This beginning of the California Flora is out in two parts, dealing with the Coniferæ and willow family, birches, oaks, walnuts, wax myrtles and nettles.

From the manner in which they are gotten out it would seem that they are intended more as advertisers, advance sheets, than as parts of a flora. There is nothing to tell how many parts there are to be or what is the ultimate cost of the completed flora. This follows the bad precedent of the German books, and most foreign works that run up so high as to be almost prohibitive in the end. In order to get at the approximate final cost of this work if completed as planned we find that the two numbers issued cost \$1.70 for 64 pp. These 64 pp. are equal to 41 pp. of Watson's Flora of California and therefore the Jepson Flora when complete will cover about 1735 pp. which cannot be contained in less than two volumes and without being bound will cost \$47.25 or bound in proper form about \$49 to \$52 a cost out of all proportion to the value of the work and out of the reach of a majority of botanists. How Mr. Jepson can finish this work as planned must be an enigma to all except himself, and how he can ever be reimbursed for the outlay is also an enigma. It is, in the judgment of the writer, wrong to publish a work in this way for many people unable to buy will subscribe for it without counting the final cost and it will work great hardship on them.

The book is evidently planned on the style of Watson's superb work that will ever stand as a monument to Watson's genius. It is an improvement on that work in many particulars. There are many engravings that can hardly be improved upon in fineness of execution and printing. The heliotype reproductions of photographs are artistically selected. The typographical work is of the highest order and the proof reading excellent.

In the matter of nomenclature the author follows the sensible method approved by the best botanists. He rejects the silly Brittonian dictum "once a synonym always a synonym," and appears to approve of the Vienna rules as a whole.

His conception of specific limitation is sound and in accordance with the results of field study. His conception of generic

limitation is equally good, so far as one can judge by the limited genera given. He adds his peculiar method of inserting forms which has many advantages and is harmless even when not beneficial.

His citations of original place and time of publication and type localities are excellent, and he follows a sensible rule in omitting a multitude of citations, giving only the most essential.

His work is devoid of the stilted half-breed English so prevalent in the works of Rydberg and Small. Technical terms are seldom used and then only the simplest, the English language is good enough for him and ought to be for any scientific man, the constant interlarding with uncouth Latin and Greek derivatives in the works of men who never had a classical education but came mostly from Agricultural Colleges or scientific courses where the Classics were never studied, always makes one suspicious that these names were culled from some obsolete dictionary and that their users would get tongue tied if they tried to pronounce them. They always remind one of the little three-year-old trying to chop with his grandpa's ax.

His method of single citation follows the old rule and has the advantage of simplicity but in my judgment is a retrogression as it emphasizes the name of the last author to the exclusion of the original author who in fact deserves the most credit, but no system of citation possible would do justice to all who have been responsible for the correct names of all plants.

The chief defect in the work is the total lack of antithetical arrangement and condensation. There is a skeleton key at the beginning which is good but outside of that there is nothing. The student has to wade through line after line of matter that is the same in related species. If the work were properly keyed and grouped so that not over three species were ever kept in the same group and all common characters were put in the keys and nothing in the description of the species except what was peculiar to it there would be no duplication and there would be great condensation without the loss of any essential.

In the recognition of varieties there is great variation. *Salix longifolia* is given with only a single variety while they are legion, while *S. Sitchensis* has a number of forms, all but one of Jepson's creation. *S. Sitchensis* var. *angustifolia* is not properly placed and probably grades into *S. pellita*. *Salix flavescens* is *S. Scouler-*

iana an older name. *Salix Lemmoni* is credited to the Wasatch mountains Utah when it does not grow within two hundred miles of them. Mr. Jepson evidently does not know that the Wasatch mountains end at Nephi, Utah. *Betula occidentalis* is given as a good species and *B. fontinalis* is referred to it, while *occidentalis* is almost certainly a variety of *alba* and *fontinalis* is a form of *microphylla*.

Salix Monica Watson is kept up though it is not considered a species. *Salix Nevadensis* is ignored though it reaches the region he covers.

He omits all mention of *Populus Fremonti* var. *Wislizeni* (which may be *P. Mexicana*) though it is the common form of Southern California.

In the matter of specific description there is not great evidence of extensive field work, the descriptions are for the most part copied. In *Salix longifolia* the most remarkable character is omitted, namely blooming throughout the season, which is not characteristic of any other willow. In *Pinus monticola* the chief character of the bark is not given at all, namely the oak-like method of cracking into rectangular and small areas. It is only the young trees or small ones that have the smooth bark. In the keys to the *monticola* group no mention is made of the peculiar character of the cones, their lightness and softness, and the same is true of the *Coulteri* group where the cones are immense and rigid and excessively heavy. The peculiar glaucous and airy foliage of *monticola* is not given, nor the soft and punk-like wood and the very resinous bark of the group. Many other characteristic things among the pines are omitted in like manner that would identify them at once which shows lack of keen field study. In the description of *Pinus monophylla* he gives the needles as one in a place when they are frequently two in a place. He repeats the stock blunder about *Tsuga Mertensiana* being in Montana and which Blankenship, Rydberg and Sargent have also made, not knowing that it is not found there at all but the cones of *Picea Columbiana* have been mistaken for it by some one who did not know the difference between a hemlock and a spruce. The writer doubts the correctness of his treatment of *Cupressus*.

Cupressus species found about San Diego are not mentioned. He does not mention the fact that the writer of this regards *Pinus monophylla* as a variety of *P. edulis*, and that other botanists have gone a step farther and united both with *P. cembroides*

which is probably better still.

Among the oaks one new species is *Q. durata* which is Emgelmann's *Q. dumosa* var. *bullata*. *Quercus MacDougali* and its variety *elegans* are not mentioned. *Quercus agrifolia* var. *frutescens* is common about San Francisco and Tamalpais and is not mentioned. *Quercus Palmeri* was never described as a species but as a subspecies in the varietal sense and yet he recognizes it as a species and ignores the older name *Q. Dunnii* Kellogg. The familiar chestnut oak is put into *Pasania* a separate genus though all but the *Pflanzenfamilien* still keep it in *Quercus*. The author seems to be a little muddled on pp. 357 and 359 where he says (under *Q. Sadleriana*) "teste spm. in Herb. Royal Botanic Garden Edinburgh, W. L. B," and (under *Q. chrysolepis*) "while none of Robert Brown's oak specimens were found by the writer either at Edinburgh or Kew herbaria, Brown ticketed a Jeffrey specimen of *Q. chrysolepis* at Edinburgh as *oblongifolia* Torr." Now did he or didn't he see *Q. Sadleriana* and *Q. chrysolepis* at Edinburgh?

The author makes nearly twenty new "formas," several new varieties, and three new species, all of them with parenthetical Latin descriptions which are assumed to be his own manufacture but internal evidence shows that they were written by some one who was not familiar with botanical terms.

There is one thing that the writer very much regrets to see and that in his reference, on page 342, to Miss Eastwood, who has done many times the amount of unselfish work on the Californian flora that Mr. Jepson has done. The writer most emphatically disagrees with Miss Eastwood's conception of species, but she is honest and a lady always, and this spiteful, jealous slur is too characteristic of Mr. Jepson's treatment of people who have the misfortune to work in the same field. When one is reviewing the work of another it is proper to analyze his motives and methods and reliability but in a work intended to be standard if the work of another is ragged or poor it should be ignored.

Another bad feature of his work is that he refuses permission to see the types of his new species.

Mr. Jepson is not as well equipped for such a work in field experience as Mrs. Brandegee, Miss Eastwood, Prof. Hall, or the scholarly Parish. The writer had hoped that Mrs. Brandegee could be prevailed upon to crown her Californian work by publishing a suitable flora of the state such as

no other botanist could do, for she is in every way best fitted to do such a work justice, but failing in that the field is open for others. The writer hopes that Mr. Jepson may successfully complete the herculean task he has assumed, and that before it is too late he may modify his methods and improve the quality of his work and let others labor alongside of him without friction. The work can never become a school manual because of its bulk and cost. There will be a clear field for Prof. Hall or some other botanist to supply that crying need of a handy and compact school book on Californian botany.

The writer is puzzled by two things, the constant reference to the collections of the author and the almost complete ignoring of those of others especially Miss Eastwood, Mrs. Brandegee, etc., and the constant reference to Jepson's little Flora and the almost constant ignoring of the great Flora by Watson. Whether the author is using his own collections to the exclusion of others in his class work and his own little Flora to the exclusion of that of Watson in his teaching or whether he considers them paramount is not stated, but one would suppose that a great work costing \$49 to \$52 would have space to refer to the collections of all botanists and to the publications of all his predecessors. If this is his real plan for the whole book and the enormous amount of botanical work done in the state by others is to be ignored then the book might as well be thrown in the scrap heap at once. The writer of this has himself collected nearly two thousand species in the state alone, and his collections contain several score of types of new species. Mrs. Brandegee and Miss Eastwood have doubtless collected still more, also Parish, Hall, Bolander, Lemmon, etc. In addition to the ignoring of Watson's Flora we find no mention of Greene's Flora Fran. and Botany of the Bay region, Abram's Flora of Los Angeles, and Ball's work on the willows.

The work is of course not free from typographical errors such as *Salix glauca* L. (1853) instead of (1753).

The author's conception of the meaning of botanical terms might be improved both in this and his little Flora. Samples are "circumscissile, splitting at the middle;" "glandular, bearing glands or having a surface which exudes a sticky or viscid liquid;" "habit, general aspect or hue of a plant;" "lenticular, shaped like a lens;" "loculicidal, a capsule splitting longitudinally into the backs of the cells;" "indument, with a close pubescence or coat of hairs;" "nate, a termination meaning divided;" "nigrescent, be-

coming blackened;" "pedicellate, having or possessing a small or short pedicel;" "personate, when the bilabiate has a very prominent palate or elevation in the throat;" "posterior, the side behind;" "pustulate, dilated;" "repand, with slightly uneven margin;" "revolute, rolled backward from each side;" "rostrate, with a beak or spur;" "septicidal, a capsule splitting between the partitions of the cells;" "series, successive rows;" "spadix, a spike with a fleshy axis;" "silique, a 2-celled capsule;" "terete, round;" "turgid, distended or inflated;" "umbel, branches nearly equal and proceeding from the same point, so as to form a flat-topped cluster;" these are only a few samples. Many others leave out some necessary point, or include something that narrows the meaning too much. There are many terms in the text like albumen, anatropous, cucullate, epicarp, induplicate, retrorse, testa, etc., which he does not define at all but he goes to considerable length to explain such terms as commonly, mostly, Coast Range, interior, etc.

The figures in the text have one defect in that they are either photographs whose value is more ecological than taxonomic, or they are copies from other works and are therefore well known already.

The announcement on the cover that the papers are for sale naturally leads to the conclusion that he intends to make the pupils attending his classes purchase them, a course which made Jepson's little Flora financially successful.

GENERA AND SPECIES.

In the foregoing pages the writer has incidentally referred to the conception of species and genera exemplified in certain recent works. Under this head he proposes to speak of the subject from two points of view, bibliographical and taxonomic.

Names of plants were originally given to groups of plants like the plums, cherries, strawberries, raspberries, etc., in the vernacular, and each publication had a different name according to the language in which it was published. When the Latin became the language of the Courts and later of the scientific world its names were commonly accepted by scientists as common means of identification. It soon became necessary to separate still farther which was at first done by short descriptions which in the end simmered down to the first word of the description or some prominent word in it, this was some time before the work of Linnæus and about the time of the general adoption of binomials for persons. Linnæus who was called by his contemporaries the botanical pirate of his time published his monumental work on genera and species and adopted or rejected the names given by others to plants just as the whim suited him. As time went on names got into a greater and greater tangle from trying to do justice to prelinnæan botanists. It was then agreed by the botanists of the world that absolute priority was an ignis fatuus and that some basis of compromise must be adopted to settle nomenclature. The basis of settlement was that all species published by Linnæus in 1753 should stand as the correct names, and that where two names were published in the same genus the first should stand irrespective of any previous name applied to a plant in some other genus which should be transferred to that genus later, and that should a name prove to be a synonym of some older name that fact should not invalidate a later name like it that might have been applied to a species in the genus. In other words the older name having gone out of existence as a valid one the same name can now be used for another species. This same rule also applied to genera.

In the matter of genera Linnæus's *Genera Plantarum* was considered the beginning point, or if that did not contain the genus then the *Systema* was used. In addition no names for genera or species should be recognized except Latin ones. In other words

priority applied up to the year 1753 only. This was a common sense basis of settlement.

In recent years a small coterie of American botanists apparently moved more by institutional jealousy than by any desire to benefit botany have attempted by hook and by crook to devise some method by which to unsettle names and get a chance to rename many species. One of these schemes was trying to adopt priority of a name out of the genus. For example, *Astragalus Smithii* though it may be the oldest name in the genus must be displaced if there happens to be an *Oxytropis Smithii* that was published before it in case that the latter proves to be not an *Oxytropis* but an *Astragalus*. This gives a chance to make a new name for *Astragalus Smithii* even if it has been used and is well known all over the world and for many years has been the accepted name. Another scheme for making new names is in the dictum "once a synonym always a synonym" This is simply a device of a lazy bibliographer to get out of keeping track of names, and shows how utterly inconsistent the framers of these schemes are, for if a name for a plant must give way when some other name like it is transferred from some other genus, one would suppose that a name that never was a synonym in its correct genus should stand. Another scheme for making new names is in trying to make out that a genus never was published unless it had a species attached to it. This would invalidate all the Linnæan genera of the *Systema* and *Genera Plantarum* not found in the *Species Plantarum*. It would be just as sensible to invalidate all the Linnæan genera of the *Species Plantarum* on the ground that no genera were published there. The attempt to adopt these rules has enabled certain men to coin thousands of new names not only without benefitting the botanical world but actually doing it incalculable harm. Another rule has been tried, that of requiring all botanical descriptions to be published in Latin, which is a fearful hardship on our graduates of agricultural and scientific colleges, and is incidentally a still greater infliction on those who have to get sense out of their pigeon-latin. The writer does not favor this rule though it would not be any hardship on him, nor would it be on any of the older men who have had the benefit of a classical education.

Taking up the subject from a taxonomic point of view we find that the Linnæan conception of species was nonintergradation, and the conception of genus was a group of species (or in

some cases single species) that could be better comprehended as a separate entity whose parts were more nearly related than to any others, or sets of groups that formed a composite whole clearly separable from the nearest related groups. We need not consider here the Linnæan conception of the origin of species.

In approaching the conception of species and genus there are at least two points of view, one is the selfish one to see how many groups one can make, or how different he can make things from the accepted names, in order to get his name attached to as many combinations as possible; and the other is how can he best subserve the interests of those who are trying to solve the great problems of genetic relationship and co-ordination for the purpose of finding the ultimate practical value of plants to human life, and for understanding the methods used by the great Force behind it all in adapting plants to their environment of today and throughout geologic time. The former method is unscientific and vicious, the latter is scientific. Another point is one that all taxonomists must recognize, namely, the limitations of the human mind, they must so group their species and genera that they can be readily grasped and held by the average mind. The average mind is not capable of holding more than three things at a time for comparison, very rarely can any botanist hold five and it is almost none at all who can hold seven. This fact has led our best systematists to devise elaborate keys for tracing up genera and species and even families, and this has advanced the study of botany more than anything else. Certain misguided botanists of today have tried to carry this still farther and make one-character keys. This is particularly true of Rydberg's *Flora of Colorado*, Piper's *Flora of Washington* and Jepson's *Flora of California*. This is all well enough when there is one prominent character, but many times a species or genus does not rest on any one character but on a combination of several, and therefore the key is not only worthless but worse than worthless, and wholly misleading from a genetic point of view. For if the key fails then there is no way but to wade through all the species of the genus in order to find the one desired. On the other hand with the keys elaborated as Gray and Watson did them and placed in the body of the genus and not at the beginning, finding the one desired is easy and at the same time the genetic relationship is indicated by the position. Even this is not always possible in a linear arrangement where there are two groups, co-ordinate, branching

off from a common stock, or more, as in *Astragalus* and most of the larger genera. But this can be allowed for by primary and secondary keys. Not enough attention has been paid to this in the recent books on botany. Gray and Watson made great advances in this direction but their followers have not. Britton copied Gray's keys made out on the basis of putting the highest groups first, then he arranged the families on the basis of the lowest first (following the *Pflanzenfamilien*), and kept the genera and species in the families on the basis of the highest first! This was about as inconsistent a method as could possibly be devised. The new Gray's Manual has an improvement on this by making a new key and arranging the families in order from the lowest up, but puts at least the species in the reverse order, which is also inconsistent. The new Manual is the only one that does not follow the unwise method of having only skeleton keys and at the beginning.

Taking up the conception of genus as worked out by Torrey, Gray, Watson, Bentham and Hooker, the *Pflanzenfamilien*, and most modern botanists we find them grouping the species in genera and subgenera in the natural way, the scientific and sensible way, and avoiding the creation of new generic names as far as possible. In some cases they went too far and in others not far enough as some put certain species in one genus and some in another such as shown by thorough research. In scattered cases, debatable ones, species of *Aster* in *Erigeron* and the reverse, certain species of *Bigelovia* in *Aplopappus* and the reverse, certain in *Solidago* etc. all due to one systematist emphasizing certain characters to the exclusion of others and other systematists following a different method and getting other results, but in the main they agreed, the policy followed by most of them being to keep all problematical species and subgenera confined to the groups to which they showed the nearest approach. This was carried to the extreme limit in *Aplopappus*, it being considered far better to hold these loosely aggregated groups in a composite whole than to let them loose in the family to wander unplaced among the *Solidaginoid* genera without anything to indicate their relationship as Greene and others have done. Under the Grayan method we know where to find them, under Greene's all is chaos. The same is true of changes in other genera such as *Astragalus*, *Oenothera*, *Ranunculus*, the *Umbelliferae*, and many others. Most of these changes were made with a great flare of trumpets and claims of superior wis-

dom when in fact none of the men had either the knowledge or acumen or even a small part of that of Gray, but their claims were pure botanical buncomb, to deceive the minds of the uninitiated. When we come to examine the proposed new genera we find them in nearly every case clearly defined a generation ago by others as groups or subgenera, or in some of the recent discoveries were so recognized by Gray or Watson. We know the motive in many cases is not at all taxonomic but purely the result of institutional or personal jealousy or pique. The most prominent of all these men has openly stated that he would devote the rest of his life to undoing the work of Gray, and all on account of offended personal vanity. There is no doubt about the undoing but it has not been the work of Gray that has been undone. Now when we know that the men who are the loudest in their demonstrations against the Grayan conception of genus and species are the very ones at the bottom of the greatest institutional and personal jealousies and who have used unworthy and improper means in the propagation of their ideas the botanical public must be excused if it fails to take such people at their face value, and demands other and sounder views on generic and specific limitation. The revolt against this is wide spread and increasing daily, for every unbiased man knows that it is unscientific. In the matter of priority in nomenclature many a well meaning man has been misled by this specious plea and yet I have shown that if consistently followed it ends in chaos. If we are to have stable names we must adopt a certain date and then make liberal exceptions to even that so as to keep up large genera that might be affected, and this is the method of the Vienna Congress. But even then too many concessions were made to the noisy ones in the adoption of priority out of the genus and the use of the Latin language as a means of publication.

If we take up certain genera we find our attitude varied according to our interpretation of the importance of certain characters, and this variability is due to ignorance of the conditions under which the plants grow, particularly aridity, alkalinity, and high temperatures. This is more noticeable among species. Let us take up a few cases of genera. Among the Coniferæ we find Engelmann, Small and Rydberg working. Engelmann was a man of ripe experience, wide travel, and keen judgment who had studied all the plants from germination to maturity and in their habitat. Opposed to him are two young men with very little ex-

perience, little travel, and poor judgment but who do not hesitate to throw Engelmann's work in the ash heap. *Pinus*, a well defined genus remarkably distinct from any other they split up into *Pinus*, *Caryopitys*, and *Apinus*, and on what grounds? Do we find any of the great characteristics of *Pinus* given? None at all. *Pinus* according to them rests on a long wing to the seed and adnate to it, and a prickle to the scales. *Caryopitys* rests on a small wing adnate to the scale and a prickle (nothing said about the few and small cones with only a rudiment of a prickle, large seeds, stunted habit and habitat which are the only grounds on which the genus could possibly be founded). *Apinus* is founded on a small or rudimentary wing and no prickle (nothing said about the long, soft cones with large seeds). Now suppose we were to apply this "scientific method" to the common cat. In the beginning he would be pure cat, but if he met a dog which bit off a piece of his tail he would be *catera*, if the blood oozing out of his tail happened to stick it to his leg then he would be *cat-erella*, and if he got his tail all chewed off he would be *catacauda*. Let us take up Rydberg's handling of *Juniperus*. He confines it to *J. communis* which he splits up into two species, and he makes the genus *Sabina* out of the rest. He of course cannot rest the genus on the well known berry and leaf character, but instead of that raises this character to the rank of a family which he calls the *Juniperaceæ*! So he rests *Juniperus* on the channeled and awl-shaped and not appressed leaves and sessile berry, and *Sabina* on the pediceled berry and scale-like and appressed leaves. Now whenever the Virginian juniper grows in moist soil and shaded places and develops the awl-shaped leaves it is a *Juniperus*, but when it grows in drier soil and has only the scale-like leaves it is no longer a juniper but *Sabina*, but Rydberg does not say what it is when it has both kinds of leaves on the same plant as is commonly the case, and has pedicels varying from nothing to evident. When our Utah juniper which he calls *Sabina* first starts and for some years after has only the awl-shaped leaves it is forsooth a *Juniperus*, but when it is mature and has only scale-like leaves it changes its name and becomes *Sabina* like the girl that gets married. This is just a sample of Rydberg's treatment of genera all through his *Flora of Colorado*. In previous papers I have shown how absurd is Rydberg's attempt to split up *Astragalus*.

Now let us take up another class of genus splitters, whose

work consists almost entirely in raising Grayan subdivisions to generic rank. In my review of the treatment of the Umbelliferæ in Contributions No. 12 and above in this paper I have shown the fallacy of splitting up *Cymopterus* and *Cogswellia* and the resultant confusion therefrom. In the borages Gray began the work himself by splitting off *Plagiobothrys* on the one end and *Omphalodes* on the other from the old *Eritrichium*, and he followed this up with a too attenuated subdivision of *Krynitzkia* based on what are now proving to be too trivial fruit characters. This was the signal for Greene to run riot through the genus tossing the fragments right and left and making new species galore, in addition some of the rest of us have not been guiltless, the writer probably among them. Each year that the writer studies this group in the field the more convinced he becomes of the folly of the segregations, for there is no genus character that seems to hold in any of them, there seeming to be a complete transtion, while a clear conception of the family would seem to require the keeping together of all the parts in a composite whole, about as Gray first put it. Another case in point is the treatment of *Claytonia*. No one ever seems to make any stir about putting annuals and perennials in the same genus, or tuberous and nontuberous rooted plants together, but when it comes to *Claytonia* it is different though we do not hesitate to keep even shrubby portulacas beside annuals in the same genus. Even Robinson has accepted the division into *Oreobroma* and *Montia* and *Claytonia*. But Rydberg of course carries it to an absurdity by making *Crunocallis* and *Erocallis* in addition. The treatment of *Ranunculus* and *Clematis* is another case in point, which is adopted by Greene, Rydberg, etc. What for example is gained by making three or four genera out of *Clematis* by putting the climbers with small white flowers in one genus, the large-flowered lavender-colored ones in another (semiclimbers), and the nonclimbers in another when by keeping them all in one genus we have a well defined unit clearly separable from all others except *Pulsatilla* which along with *Anemone occidentalis* might just about as well be put in *Clematis*, connecting the *Douglasii* group with *Anemone*, or be kept up as a connecting link. Every one knows that there is a question as to what we shall do with *Pulsatilla*, but it only befogs the whole issue and multiplies the confusion to disintegrate *Clematis*, just as it does only harm to disintegrate *Cymopterus* and *Cogswellia*. In *Ranunculus* the buttercups are well defined, and even a novice

would have no difficulty in placing them, but when the splitters get at them we find the floating *aquaticus* in *Batrachium* though it is often creeping late in the season, and the creeping *multifidus* which is often floating early in the season is placed in *Ranunculus*. We find the stoloniferous *Cymbalaria* in *Halerpestes* and the semi-stoloniferous or creeping *repens* in *Ranunculus*. We find the pretty Nuttall's buttercup in *Cyrtorrhyncha* and its close relatives of the *adoneus* group in *Ranunculus*, and so even the expert comes out of it all with his mind befogged so long as he assumes that the splitters have any botanical judgment worth following. In the treatment of *Sedum* and *Cotyledon* Britton and Rose have gone to the limit of absurdity, basing their generic characters on things like the leaf-shape and varying adhesion of the carpels which are known to have no permanence. *Saxifraga*, a loosely aggregated genus though well definable on the whole, is split up beyond all possible recognition and on trivial characters. We find the heucheroid James's saxifrage in *Telesonix*, the delicate *adscendens* in *Muscaria*, the Virginian one in *Micranthes* and the very different *arguta* in the same genus, and the yellow saxifrage in *Leptasea* while the very different *bronchialis* is in the same genus. All this shows the utter lack of all system and consistency in the subdivision of *Saxifraga*. Time and space would prevent giving all the unscientific work of this kind from the ferns to the *Compositæ*.

Taking up the species proposed as segregates from other well known ones we find that here is where the ignorance of some of our eastern botanists is monumental, especially in matters of ecology, and bearing on the effects of aridity, alkalinity and high temperatures combined with low humidity. We find them gravely describing as new species forms of annuals which extend into warmer regions where they survive over several seasons and even at times become woody below. We find them making new species out of thin-leaved plants which happen to extend down into places which have a little alkali in the soil which makes the leaves thicker, or which happen to grow in a more arid region and so develop thicker and smaller leaves like *Prunus Virginiana* which becomes *demissa* in a drier climate, or which becomes *Prunus melanocarpa* when its fruit ripens in the normal way. When *Ribes lacustre* ripens too hastily by being nearly frostbitten and its fruit does not blacken but remains red then it is *R. lentum*. When the service-berry grows in southern Utah on exposed rocks and is scraggly

and short branched and rigid and with small glaucous and slightly toothed leaves and has a small berry or two it is *Amelanchier Utahensis*, when it grows in a better soil and develops larger leaves and more berries it has half a dozen names according to whether the leaves have more or less teeth and the petals are larger or smaller, and when it grows still farther north where it has a chance to grow normally it has a new crop of names according to the size of the berries, the shape of the leaves and the petals, till it has more names than a German crown prince, even though every possible transitional form can readily be found between all the species. The handling of this species by botanists is like the treatment a man received from a newspaper that announced his death. He called at the office and demanded a retraction. The editor refused to retract saying that they never made any mistakes, and that all he could do for him was to put him in among the births. This seems to be Greene's method, the intergrading forms come in among the births. Whatever may be our conception of the origin of species it seems to the writer that the only scientific way is to keep recent derivatives in the same group, nothing is gained by raising them to specific rank (except a temporary notoriety of which the author will be heartily ashamed later). Take for example *Astragalus lentiginosus*. This is the most variable of all *Astragali*, it has both geographical and ecological varieties which have been described as species to the number of a score or more. There is complete transition between all the forms and the extreme ones are remarkably different from each other. The common form in central Utah Gray called *diphysus*. It grows in gravelly soil somewhat alkaline, is ascending, rather stout, with narrow flowers, chartaceous 2-celled pods varying from ovate to round and often has a falcate tip. The climate is not hot. As we go northward into Idaho the pods become stiffer but never horny, smaller, more falcate, but the plant has the same axillary and subterminal racemes longer than the leaves. As we go westward into Nevada where the influence of the humidity of the Wasatch is not felt and the rainfall is below eight inches per annum and the humidity less, two forms develop: one with the pods of *diphysus* but more papery but the same ascending habit and racemes, these forms inhabit the alkaline valleys and gravelly slopes. Around springs in the canons where the soil is moist and somewhat alkaline the plants are prostrate with all the racemes appearing as if axillary by the abortion of the upper ones, and all

shorter than the leaves. The pods are chartaceous and globose, this is my var. *ovalis*. As we go into southern Utah in the Tropical life zone where it is much hotter we find the *diphysus* forms with thin pods with a tendency to being narrower and intergarding with the remarkably narrow linear pods of what I once called *A. palans* and later put as a var. of *lentiginosus*. When we get into the Lahontan basin commonly known as the Carson sink and its adjacent low areas that run from southern Oregon along the eastern base of the Sierras to beyond Walker lake we find the thin podded *diphysus* forms on the mesas, the *ovalis* forms in the canons and another prostrate papery podded form along the alkaline places in the valleys, all apparently intergarding. On the higher mountains westward we find the closely related *A. Bolanderi* with stipitate pods whose genetic relationship the writer has not yet made out, but which is also related to the *arrectus* group through *A. Weiserensis*. When we get on the Death valley slope and particularly in the Owen's valley region and Mojave desert and southward we find other variations, due to the intense heat and greater aridity, to protect the plants from radiation. The prostrate forms are absent except the var. *aridus*. The pods are mostly chartaceous but when glabrous are papery mostly. The flowers are often very dark-purple while they are normally light-colored; and two forms occur, one with the normal sized ones and one with very small flowers but intergarding. There are two forms as to length of racemes, one has long ones and very conspicuous, the other has short ones and condensed, this latter mostly with smooth pods. The long racemes have two forms one with smooth pods and one with almost woolly pods, this is the var. *Fremonti*. In nearly all these forms the pubescence of the leaves is conspicuous, in one form it is silvery and the leaves are mostly rather leathery. When we get into the Needles region along the Colorado river these forms are nearly all absent and are replaced by robust forms with large smooth and papery pods in dense racemes. Now to recognize all these multitudes of forms as species will finally land us where Greene and his five hundred species of *Eschscholtzia* are, in chaos. To consider them all as variations, due to climate and soil, of one polymorphous species makes of them the most interesting and instructive study in ecology and in addition keeps them all together where they belong, and does not confuse either the novice or the expert. This is the writer's view of the way other species should be treated, and it removes the temptation from sel-

fish men to make new names, for there is no glory in reducing species to mere varieties. This method of treatment would reduce by nearly a half the species of *Antennaria*, *Castilleia*, *Salvia*, *Lupinus*, *Senecio*, *Arabis*, *Draba*, *Lepidium*, *Thlaspi*, *Erysimum*, *Delphinium*, *Aquilegia*, *Eriogonum*, *Quercus*, *Habenaria*, *Zygadenus*, *Poa*, *Festuca*, *Agropyron*, *Elymus*, *Viola* and hundreds of others. Geographical varieties would then be the most interesting plants to study, and the tracing out of the causes of these variations would result in the greatest practical good to students of agriculture and horticulture, and to those who are furnishing the food supply of the world. It would also bring systematic botany back to a point of respectability and standing instead of being the scientific laughing stock of the world, as it is now.