

EXPLANATION OF PLATE XII.

Figs. 1, 2, and 3. Female flowers from different parts of a spike of *Carex acuta* (March 15).

Fig. 4. Anterior view of the same (March 26).

5. Ditto ditto (April 14).

6. Posterior ditto (April 14).

7. Anterior ditto more advanced (April 14).

8. View, partly anterior, partly lateral, of a female flower of *Carex acuta* with a second rudimentary flower within the perigynium.

9. Anterior view of the female flower of *Carex pulicaris* (April 14). The perigynium has been opened, and the branched seta bearing rudimentary flowers drawn forward.

10. Posterior view of the apex of the seta from another flower (April 14).

On the Genus *Cinchona*. By JOHN ELIOT HOWARD, F.L.S. &c.

[Read May 1, 1873.]

“La manière de distinguer les espèces de *Cinchona* présente des difficultés qui ont souvent arrêté les botanistes. Ici, en effet, comme en d'autres associations très-naturelles de plantes, les caractères distinctifs *semblent osciller sur une échelle de variabilité*, dont les limites extrêmes ne sont point encore fixées avec une rigoureuse précision. Ces difficultés disparaîtront *quand on sera d'accord sur les signes généraux qui constituent l'espèce*, et sur les degrés de variabilité organique qui séparent les individus. Mais, dans l'état actuel de la science, la nomenclature et le classement des espèces sont une affaire d'appréciation personnelle, généralement abandonnée au courant des opinions particulières.”—*TRIANA, Nouvelles Études sur les Quinquinas*, p. 26.

THE genus *Cinchona* has been celebrated for the difficulty of classifying its species; and, notwithstanding all the amount of research recently bestowed, this desirable object does not yet seem to have been accomplished.

Two works, recently published on the subject, present the most striking contrasts. The first is by Dr. Weddell, the eminent Quinologist, who has before added so much to our knowledge of the species, and gives us in his ‘Notes sur les Quinquinas’ a contribution to the further elucidation of the subject, fraught with all the matured results of his long and personal acquaintance with the plants in question. It was first given forth in the ‘Annales des Sciences Naturelles,’ 5^{me} Série, vols. xi. and xii., and afterwards published at Paris in 1870. The second work is entitled ‘Nouvelles Etudes sur les Quinquinas,’ par J. Triana, &c. &c., himself a native of one of the richest *Cinchona*-growing districts of South America, and a

gentleman to whose superior botanical knowledge it were superfluous for me to offer any testimony. Suffice it to say that I think all who are interested in the genus are much indebted to him for the many and very valuable observations contained in his work, as also for the representation to us of the elaborate Plates of Mutis, now for the first time brought before the scientific world. This work also bears date 1870, and was published at Paris. The state of political affairs had prevented any conference or comparison between the two distinguished authors*. Each treatise must be valued for its own merits. I shall have occasion in this paper to refer to and quote from both these publications, and to explain how far they support the views of classification which present themselves to my own mind.

To show what these are, I must refer to some "Observations on the present state of our knowledge of the genus *Cinchona*," which I presented to the Botanical Congress held in London in 1866.

In that paper I approached the subject "from a practical rather than from an abstractedly botanical point of view ;" and I confess that it is a gratification to me to find that the views then put forth appear to have been not altogether uninfluential on the subsequent arrangements of at least one of the authors I have referred to. I am encouraged to hope that whatever may be true in these "Observations" will not be lost sight of in the future.

I there urged the study of the whole plant in its living state, not disregarding either the microscopical or chemical examination of the bark. This sentiment finds a full response in the notes of Dr. Weddell, but scarcely so in the work of M. Triana, who seems to me to rely too much on typical specimens as found in a dried state in our Museums. I believe that the number of real species of *Cinchona* is very much more limited than is supposed—and that between these species there exist a very large number of intermediate forms, which ought not to be considered so many different species, inasmuch as extreme inconvenience would thereby be caused, even if botanical distinctions existed between them.

In the conclusion of that paper I expressed "my opinion that every well-defined region of the Andes has its own prevalent and

* M. Triana's allusion to these events is as touching as it is simple, leading us to admire his perseverance in the work under such unfavourable circumstances.

characteristic *Cinchonæ*, generally found in varied aspects and *incapable of being reduced to any one typical form*. I do not think that any species has been clearly proved to prevail unchanged from end to end of the Cinchonaceous region, and I believe the plants which resemble each other in distant parts will be found analogous rather than identical."

I am confirmed in the correctness of this view of the genus both by seven years' additional observation, and by the testimony I have had of its correctness from those who could observe on the spot. But if proved correct, it must be highly influential on subsequent classification. Many so-called typical species will have to resign such exaltation, and to be classed as well-defined *forms* (varieties or, rather, races ?); and the whole arrangement will become more natural and less artificial*.

I must establish some of these statements by reference to the works in question.

M. Triana arranges his *Cinchonæ* under the head of thirty-six typical species. Dr. Weddell divides (with more truthfulness to nature, as I think) the genus into five grand divisions, to which he attaches the appellations Stirps I. *C. officinalis*; Stirps II. *C. rugosa*; Stirps III. *C. micrantha*; Stirps IV. *C. Calisaya*; Stirps V. *C. ovata*. Then follow the different branches, then the species, the subspecies, and varieties, with, again, varieties of these varieties.

Behind all this arises the important question whether the supposed typical species were really such or not. We have to do

* Dr. Weddell in subsequent observations confirmed this view of the subject, saying :—"I perfectly concur with Mr. Howard as to the great variability of the species of the very natural genus now before us—so much so, that, allowing for exaggeration, it might almost be said that all those described are but varieties or races sprung from one typical form. There is, in fact, no single one of them that can be distinguished from its neighbour by one absolute character; they can only be so by a certain *ensemble* which the eye may be unable to collect in an herbarium specimen. In these respects I believe a parallel may very well be established between *Cinchona* and many of our European genera. It is, then, to be expected that, as the number of specimens augments in herbaria, quino-logists will have to contend with more than one difficulty arising from their peculiar tendency either to extend or restrict. And it must be evident to any one who has handled these polymorphous plants, that if the multiplying or splitting system be adopted, quinoLOGY must fall at last into an inextricable chaos." (See Report of Proceedings of the International Horticultural Exhibition and Botanical Congress, London, 1866, p. 222.)

with a genus which throws itself into a vast variety of permanent forms, as in the case of the willows and the roses; and if every one of these is to be called a species, there will soon be an end of all power of classification; for the different kinds would have to be counted by hundreds at the very least. Amongst these also it would be difficult to say which is the really typical form.

I think that I shall best be able to explain my own views, and my accordance or otherwise with those of these eminent botanists, by examining the arrangement of some of the more marked forms, and those respecting which we have the largest amount of information.

In the first place I will take the "*Quina primitiva*" or *C. OFFICINALIS* with its subdivisions.

In the 'Histoire Nat. des Quinquinas' Dr. Weddell ranged under the head *Cinchona Condaminea* :—

α. *CONDAMINEA VERA.*

β. *Candollii.*

γ. *lucumæfolia.*

δ. *lancifolia.*

ε. *pitayensis.*

In the 'Report of the Botanical Congress,' I proposed to class the *Loja* or Crown barks as

CINCHONA OFFICINALIS.

α. *Uritusinga.*

β. *Condaminea.*

γ. *Bonplandiana colorata.**

—— *lutea.*

δ. *crispa.*

omitting of course the *lucumæfolia*, *lancifolia*, and *pitayensis*, which would not fall in with my arrangement.

If these views are, as I think, correct, the barks of Loja may be fairly classed together thus as different forms of *Cinchona officinalis*; and this arrangement has been followed by Dr. Weddell in his most recent classification.

* Both the *macho* and *hembra* varieties are flowering with me whilst this paper is in press.

CINCHONA.

Stirps I. CINCHONA OFFICINALIS.

Ramus A. *Euofficinales*.

C. OFFICINALIS, Linn., &c.

α. *Uritusinga*.β. *Condaminea*.γ. *Bonplandiana*.α. *colorata*.b. *lutea*.c. *angustifolia*.

*C. CRISPA, Tafalla.

This table is sufficiently elastic to take in some additional varieties, if, as is likely, such should be found to occur either in India † or in South America; and it leaves in a certain measure of obscurity, corresponding to our present state of knowledge, the relation of the *C. crispa* as a *subspecies*. I have published, from a MS. in the British Museum, the diagnosis of M. Tafalla, who makes the *C. crispa* a distinct *species* *. I must return to this question of *species* and *subspecies* presently.

If it should eventually prove that the *C. crispa* does not differ specifically from the other forms of the *Quina primitiva*, we shall have the Loja barks ranged very satisfactorily, to my mind, under the one head, as I proposed.

It will be seen that I have much reason to be satisfied with the evident approximation to the arrangement I submitted. It would not do justice to Dr. Weddell were I to omit his own statement of the case, to which I must now refer.

Dr. Weddell says (Ann. des Sc. Nat. ser. 5, vol. xii. p. 24):—"M. G. Planchon, qui a pu étudier la belle série d'échantillons de *Cinchona* de l'herbier Boissier, propose un autre arrangement des formes rapportées à ce type, et j'avoue que tout d'abord je me

* See 'Ill. Nueva Quinologia,' sub voce *C. Chahuarguera*, p. 3.

† I receive (whilst this is passing through the press) a letter from Mr. McIvor, with specimens of the bark (very promising in appearance) of "a hairy-leaved variety of the *C. officinalis*," respecting which he adds:—"Dr. de Vry found the bark of this species to yield 10.67 of total alkaloids with 4.92 per cent. of crystallized sulphate of quinine. If under all conditions this bark is found to yield this amount of alkaloids, and especially quinine, it is certainly the best plant we can grow, being hardy and of rapid growth and perfectly free from canker and other diseases to which the Cinchonæ, especially the *Calisaya*, are liable." I should call this var. *δ pubescens*, H.

suis senti assez disposé à l'adopter. Si j'ai donné la préférence à celui de M. Howard, c'est qu'il m'a paru offrir des avantages pratiques qui compensent les quelques défauts qu'on pourrait lui reprocher."

I must be pardoned for observing that I have not yet any evidence to show that the *C. crisper* really merits the dignity of a subspecies; nor am I sure that the *C. angustifolia* is any other than an accidental variety wanting in those attributes of permanence which would be necessary to constitute it a race. This seems to be the opinion of Mr. M'Ivor. I find amongst the specimens from India two or more varieties, hitherto undescribed, which confirm me in this way of looking at the matter.

Tendency to reproduce the exact form in all its minutest particulars, and, on the other hand, tendency to diverge into remarkable varieties from the same seed, are both to be noted in this ramus A of the *euofficinales* of Dr. Weddell.

I now come to the arrangement of M. Triana, who, in the first place separates the *C. lancifolia* as a distinct species*. Then follows, as No. 2, *Cinchona crisper* with synonyms; and M. Triana remarks†:—

"On a cru voir dans le *Cinchona crisper* de Tafalla d'après les manuscrits de Pavon et la publication qu'en a faite M. Howard un synonyme ou une variété du *Cinchona officinalis*.

"Il est vrai qu'au premier aspect ces deux plantes se ressemblent beaucoup, mais nous croyons qu'il s'agit de deux espèces distinctes. Nous avons pu observer, en effet, dans les serres de M. Howard plusieurs jeunes plantes du *Cinchona crisper* placées à côté de celles du *C. officinalis*, et il nous a paru que même en l'absence de tous autres caractères, la texture des feuilles du *C. crisper*, le développement remarquable de ses scrobicules &c. suffisait à le distinguer du *C. officinalis* et des espèces voisines."

With these observations of M. Triana I agree up to a certain

* Though not without difficulty; for he says (pp. 30, 31), "Malgré les nombreuses difficultés de cette tâche, une grande partie des espèces de *Cinchona* peut être facilement caractérisée. L'hésitation ne se fait sentir qu'en présence de quelques espèces de certains groupes dont les formes affectent des ressemblances plus marquées. Nous signalons ailleurs les rapports intimes qui rapprochent, par exemple, le *C. lancifolia* et le *crisper* du *C. officinalis*. Nous y ajouterons le *C. chahuarguera*, comme étant l'espèce la plus voisine du *C. officinalis*, avec lequel elle risquerait aisément de se confondre par ses caractères distinctifs moins saillants.

† Page 53.

point; but I do not see that they prove the *C. crispera* to be a different species. My reasons for this will appear presently.

I have also to remark that the peculiar tissue (*contexture*) of the leaf is one of those diagnostic marks which disappear in dried specimens. It is well hinted at in the drawing of this bark in icon vii. of the 'Quinologie of Mutis.' The conspicuous scrobiculi are represented in my 'Quinologia of Pavon.' They are correctly drawn and not exaggerated by Fitch.

Then follows, in M. Triana's classification, the *Cinchona officinalis* with its synonyms as No. 3 species. Then No. 4. *Cinchona chahuarguera* with the synonyms. M. Triana remarks:—

"Suivant une tradition ancienne, l'arbre nommé *Chahuarguera* par les Indiens de Loja aurait eu le privilège de fournir l'écorce fébrifuge dont l'emploi opéra la guérison de la Comtesse de Chinchon. Il existe d'ailleurs entre le *Cinchona officinalis* et le *C. chahuarguera* des ressemblances des plus grandes, mais on peut l'en distinguer par ses inflorescences moins lâches, par ses pédicelles plus courts, par ses fruits plus corsés, par ses feuilles elliptiques et plus consistantes, par les lobes du calice plus allongés," &c.

This is all very correct, I have no doubt; but if no difference or incompatibility exists in the organs of reproduction, I must still look upon them as one species.

I have studied this particular ramus rather more closely than most other branches, and see no reason to look upon one form more than another as the *typical* form or really *Quina primitiva*. I entirely agree with Dr. Hooker, and think that he has done good service to botanical science in restoring the name *C. officinalis* of Linnæus to the *Uritusinga* form—as this was unquestionably the "*Quinquina*" of La Condamine, and thus the first described. But the β . *Condaminea* of Weddell, or *Chahuarguera* of Pavon, is pretty certainly the sort which cured the Countess Chinchon, and, if I may judge from a specimen in my possession (and also from the icon xiv. of Mutis, the *Chahuarguera*, compared with icon xi., which is the *Uritusinga*), perhaps the finer plant of the two; whilst I might be inclined to prefer to both the var. γ . *Bonplandiana*, with its long-valued varieties the *colorata* and *lutea*, and with the recently found variety, the *angustifolia*, producing 10 per cent. of quinine from its bark—a truly noble kind, which was called in India *Cinchona mirabilis*. Perhaps Planchon is right, that in the *Chahuarguera*, and again in the *crispera*, the species verges towards *macrocalyx*. Nature, in fact, will not lie

in the Procrustean beds of our systems ; and thus we have not a perfect isolation for our species of *C. officinalis* : but on the whole we have attained something by being able to say that the *C. officinalis* (in varied forms) is *the species* of Loja, whilst the *C. macrocalyx*, as we are told by Weddell, is the tree of Cuenca.

The Ramus B of Dr. Weddell, the *Macrocalycinæ*, are well separated from the *Euofficinales* by the character indicated in the floral organs ; but I am unable to say whether the order of the branch will be confirmed or not. But little is known of several of the divisions, though in a commercial point of view they assume an increasing importance—especially the *C. Palton*, the description of which in the ‘Nueva Quinologia’ is, I understand, very correct ; but I should be at a loss to distinguish any points in my *dried specimen* from Pavon to separate it from *C. macrocalyx*. Nevertheless the bark is wholly different, and, I doubt not, the tree also.

Ramus B. *Macrocalycinæ*.

C. MACROCALYX &c.

**C. Palton*.

**C. suberosa*.

**C. coccinea*.

**C. heterophylla*.

C. LUCUMÆFOLIA.

β. *stupea*.

C. LANCEOLATA.

This very nearly meets the views I expressed in 1866.

In Ramus C. of Dr. Weddell's ‘Notes’ I remark with great satisfaction that the *lancifoliæ* occupy a place by themselves. We have thus the special species of New Granada at length placed apart from that of Loja. I have not the opportunity of studying the living plants ; but, as first described by Mutis and recently by Karsten, it must certainly seem to take its place among the first and most prominent species of *Cinchona* ; and it will be observed that this, like the *officinalis*, presents itself before us in a number of variant forms.

Ramus C. *Lancifoliæ*.

C. LANCIFOLIA, Mutis &c.

α. *vera*.

β. *rubra*.

γ. *obtusata*.

δ. *Calisaya*.

ε. *discolor*.

From these must be abstracted the var. δ , if the information of M. Triana be correct, which I do not doubt; and I am inclined to think that some further forms must in time be added*. I can say nothing as to the inclusion of the *C. Forbesiana* and *C. amygdalifolia* under this branch. I suspect that the inflorescence of the former and its chemical contents may place it under the *micranthæ*.

As I have remarked above, M. Triana makes the *Cinchona lancifolia* his first species, and observes:—"On a souvent discuté au sujet du *C. lancifolia*, Mut. (restreint au Quinquina Tunita de la Nouvelle Grenade) soit comme espèce, soit comme variété du *Cinchona officinalis*. Nous avons donc longtemps hésité sur l'importance qu'il convient d'attacher à ces affinités et à ces différences, et dans notre catalogue de l'exposition de 1867, nous avons incliné à les considérer comme des variétés d'une même espèce. Il existe certainement de très-intimes affinités entre le *Cinchona officinalis* et le *Cinchona lancifolia*, Humb. Ces deux plantes se ressemblent à tel point qu'il est difficile de préciser leur distinction. Néanmoins, l'examen comparatif de documents plus complets nous permet de reconnaître qu'elles ne sont nullement identiques, et que les caractères tirés des feuilles, du fruit, de l'inflorescence, de l'écorce et de l'habitat, peuvent nous aider à les distinguer. En outre, au point de vue pratique, il est préférable de les mentionner sous les deux noms spécifiques qui leur sont attribués" †.

I have much satisfaction in considering the new aspect of the Stirps *C. rugosæ*. The first branch, the *eurugosæ*, comprehends the Pitayo sorts now made into the species *C. pitayensis* (and the subspecies *C. corymbosa* ? ‡), together with varieties. Dr. Weddell says (*Annales des Sc. Nat.* 5^e série, tome xii. pp. 37, 38):—"Les renseignements que je possédais sur cette plante, lorsque je publiai ma Monographie étaient fort incomplets; je soupçonnais néanmoins, déjà, qu'elle devait constituer une espèce distincte (voy. *loc. cit.* p. 42). Les nombreux matériaux réunis depuis lors permettent à peu près aujourd'hui de compléter son histoire,

* Whilst this paper is passing through the press I am enabled to describe (to the Société Botanique of France), from specimens just received, a *varietas oblonga*, forming the "soft bark" of New Granada.

† I cannot despair of Quinology when I find three observers arriving by such different roads at one and the same conclusion, and agreeing in so *natural* a classification.

‡ Whilst correcting the press, I have a letter from Popayan, identifying the *C. corymbosa* (?) with the *C. lancifolia*, var. *discolor*, of Mutis.

et de lui assigner la place qui lui appartient dans la série de formes qui constituent le genre.

“ Dans l'ouvrage cité, j'ai appelé l'attention (et pour la première fois, si je ne me trompe) sur l'existence des poils qui tapissent, chez certaines espèces, une étendue plus ou moins grande de la face interne du tube de la corolle, et je regrettais alors que le petit nombre de ces espèces ne permit pas de faire de ce caractère un moyen de sectionnement de genre. Eh bien ! ce desideratum a presque cessé d'en être un, car M. Karsten a remarqué cette même particularité dans ses *CC. Trianae* et *corymbosa*, et, tout récemment, je l'ai trouvée également dans le *C. pitayensis*, dont les deux plantes de M. Karsten paraissent être des formes.”

Thus far Dr. Weddell, to whose testimony I am happy to add that of M. Triana, who includes all these forms under one head, that of *C. pitayensis*, the eighth species in his list.

In thus recognizing only one species in the barks of Pitayo, I think that he is right. I do not exactly see the connexion between the Pitayo plant and some of the others in Dr. Weddell's list. In *C. pahudiana* (Ramus B) I do not detect the above peculiarity in the interior of the corollas ; in other respects there seems a considerable analogy to the *C. rugosa* and even to the *C. parabolica* in one or two of the divergent forms of this plant now growing with me from the same seed given me by the late Dr. Anderson.

It seems to me that the measure of separation from other species granted to the Pitayo barks, and their being almost all gathered into one sort, is a decided step forwards in Quinology.

It is not needful to my argument in this paper to notice each species or even each division of the Notes of Dr. Weddell. I adhere to what I have before said about the Huanuco barks ; and passing over Dr. Weddell's Stirps *Cinc. micranthæ*, I venture to criticise the *C. Calisaya*, the special species of this eminent botanist, from whose “ Notes ” I take the following observations (Ann. d. Sc. Nat. ser. 5, vol. x. p. 353):—“ Les difficultés qui se présentent dès l'abord à celui qui tente de grouper entre elles les formes déjà très-nombreuses de ce genre, résultent, à ce qu'il semble, des circonstances suivantes.

“ 1. Il n'y a aucun caractère botanique qui permette de sectionner le genre d'une manière utile.

“ 2. Sauf dans un très-petit nombre de cas, il est impossible de

distinguer nettement une espèce des espèces voisines, au moyen d'un seul caractère. Cette distinction ne peut être établie que par un ensemble de signes diagnostiques."

It is quite in accordance with these general remarks that the new classification of this branch should be more elaborate than the former. As given in the 'Histoire' we have only

CINCHONA CALISAYA.

a. *vera*.

β. *Josephiana*.

whilst the *C. boliviana* constitutes a distinct species.

I prefer the present arrangement:—

STIRPS IV. CINCH. CALISAYÆ.

C. CALISAYA.

a. *vera*.

a. *glabra*.

b. *pubera*.

β. *microcarpa*.

γ. *boliviana*.

a. *glabra*.

b. *pubescens*.

δ. *oblongifolia*.

ε. *pallida*.

*C. JOSEPHIANA.

a. *glabra*.

b. *pubescens*.

c. *discolor*.

*C. ELLIPTICA.

I should, however, be more entirely satisfied if I could see all the forms of *Calisaya* brought under the head of one species. I suspect, however, that the *C. elliptica* stands nearest to *C. purpurea*; and I do not see that the *C. Josephiana* differs more from the normal form than does the *C. boliviana*, both of which flourish with me.

It will not answer, practically, to confound the above two forms *C. Calisaya* and *C. Josephiana* together. The latter plant is worthless for cultivation, and could not be improved (so far as I

know) by mossaing or any other expedient. It is, then, desirable and necessary that the botanist should lend his aid in discrimination; but how shall this be done?

In carefully considering Dr. Weddell's remarks on the *C. Josephiana*, I am led to conclude that the position of his *subspecies* is very nearly if not quite identical with that which I assign to race, he says (Anf. d. Sc. Nat. ser. 5, vol. xii. pp. 58, 59):—"La constance de la forme du *C. Calisaya* à laquelle j'ai donné le nom de *Josephiana* et en particulier son aptitude à se reproduire de semis, en conservant ses caractères (au moins pendant la première génération) m'ont engagé à la considérer plutôt comme une race ou une sous-espèce que comme une simple variété. M. Markham qui l'a étudiée dans la province péruvienne de Carabaya, pense que les caractères qui l'éloignent du *Calisaya* type sont dus plutôt à la hauteur à laquelle elle croît qu'aux autres conditions auxquelles elle est soumise. Il est en effet peu douteux que cette cause n'ait sa part d'action; mais ce que j'ai dit ailleurs de la configuration générale de quelques uns des districts où j'ai pu l'étudier, prouve au moins qu'elle n'agit pas seule.

"Les graines du *C. Josephiana* étant infiniment plus faciles à trouver que celles de la race forestière du *C. Calisaya*, la rusticité de la forme frutescente étant d'ailleurs beaucoup plus grande, il paraît en être résulté non-seulement que c'est cette dernière qu'on a soumis tout d'abord à la culture* mais, que c'est également sur elle qu'on porte les premiers essais de multiplication. L'expérience démontrera au bout de combien de générations les caractères de race s'effaceront: mais en attendant, on agira sagement en choisissant les éléments de multiplication du *C. Calisaya* sur des types de meilleur aloi, et de donner la préférence au bouturage sur le semis, afin d'éviter les perturbations qui pourraient résulter de l'hybridation."

With all this I agree entirely, with this exception, that I am not aware of any reason for supposing that any number of generations would efface the characteristics of this race; on the contrary, my impression is that it would be found recurrent amidst the produce of the seed of the purest varieties.

I have strong reasons for believing that I obtained the true *C. Josephiana* (as I have mentioned) amongst the produce of seed

* "C'est du moins ce que semblent démontrer les échantillons de provenances indienne et javanaise que M. Howard et moi nous avons eu occasion d'examiner."

from a genuine *Calisaya*. If so, I presume that it cannot be a distinct *species*. The cultivator of bark plantations is learning to avoid the plant, and will not be influenced by any value the botanist may assign to it to give it a higher place in his esteem.

But it is not this sort only which has to be avoided, as will be seen by the subjoined list of dried botanical specimens attributed to the *C. Calisaya*, which I happen to possess, and of which only a minority belong to the genuine plant.

1. A specimen, from Dr. Weddell, of the typical *C. Calisaya* in fruit.
2. Ditto, ditto, of *C. Josephiana*, in which the differences noted in leaf and fruit from No. 1 are apparent.
3. A specimen from Don Pedro Rada of the leaves of *Calisaya Zamba*, identified (as we shall see presently) with the *C. Calisaya*, var. *microcarpa*, of Weddell.
4. A specimen from Hasskarl of the same *C. Calisaya*, var. *microcarpa*. (India.)
5. Ditto?
6. A specimen of flowering branch of *C. Calisaya*, with pink flowers.
7. Ditto, with white flowers.
8. A glabrous specimen in flower marked *C. Calisaya*, bark of inferior quality (*C. Josephiana*.) (India.)
[None of these Indian specimens correspond to the type No. 1.]
9. A specimen sent me by Mr. Broughton as a hybrid between *C. succirubra* and *C. officinalis*, but marked by M. Triana as the *Calisaya* of Weddell. The leaves do not coincide with the typical *C. Calisaya*.
10. A specimen from Dr. Hasskarl of the kind called by Miquel *C. Calisaya*, var. *rugosa* ("der Sendung Hasskarl's"). It has not the slightest resemblance to the typical *Calisaya*, but is remarkably like the *C. undata* of Karsten, of which I have a specimen. Still I do not affirm their identity. It is accompanied by bark having similar characteristics to some of that sent from Java as "*Calisaya*."

We have thus far only three specimens (Nos. 1, 3, 4) on which I can look with any confidence as representing the species as described by Dr. Weddell.

The following possess a separate place :—

1. A specimen of the *C. boliviana* in leaf (typical) from Dr. Weddell.
2. A specimen of the leaves of the *Cascarilla morada*, given me by Don Pedro Rada. Query the species ? and see the plate and description in the Journal of Botany, vol. vii. (1869).

These produce very fine *Calisaya* bark ; but how far they are related to each other, or how nearly allied to *C. Calisaya*, I am unable to say. I should perhaps have thought both these quite as distinct from the typical *C. Calisaya* as the *C. Josephiana*.

I must not omit mention of another specimen, although it has no pretension to a place in the genus *Cinchona*. This I received from Dr. Hasskarl with the inscription :—

“ *C. Calisaya* indigenorum Uchubambi verisimiliter erronee dicta arbor. In montibus altioribus prope Uchubambi, mense Julii 1853.”

I am informed by Mr. Markham that this plant is called the “ *Comadre de Calisaya*,” because the Cascarilleros believe that where this is found they shall soon meet with the true *Calisaya*.

In order to show the difficulty of the subject, and to confirm the accuracy of that which I have written, I shall copy the statement of my friend Dr. Weddell himself, who says (Ann. d. Sc. Nat. ser. 5, vol. xii. p. 52) :—“ Depuis la publication de cette espèce dans ma Monographie, quelques renseignements nouveaux sont venus s'ajouter à ce que nous avons déjà sur son compte, et doivent trouver place ici. Les uns sont le résultat d'observations faites dans le cours de mon second voyage en Bolivie, en 1851 ; les autres sont dus aux voyageurs qui ont visité plus récemment les districts habités par ce type. Mais nonobstant ces additions à nos connaissances, il est présumable que certains points de l'histoire du *C. Calisaya* seront, pendant longtemps encore, entourés de quelque obscurité, aussi bien par suite des difficultés mêmes que présente leur étude qu'à cause du peu de confiance que l'on peut avoir dans les documents de source indigène quand ils ne sont pas appuyés d'échantillons authentiques. Cette remarque est surtout applicable aux noms, une même désignation étant donnée quelquefois, dans les localités différentes, à des variétés ou même à des espèces

très-dissemblables, et *vice versa*. Ce que j'ai dit ailleurs sur la difficulté de caractériser les espèces du genre *Cinchona* d'une manière absolue trouve parfaitement son application ici, aucun des caractères diagnostiques du *C. Calisaya* n'étant tout-à fait constant. Est-ce, par exemple, le port que l'on envisage? On a, en regard du *C. Calisaya* type, qui est un arbre de haute futaie, la race ou la sous-espèce *Josephiana*, dont la taille ne dépasse pas celle d'un arbuste. Vient-on à comparer les feuilles, dont la forme typique est un obovale-oblong très-obtus? On en trouve qui sont oblongues-lancéolées et aiguës, ou bien ovales ou même elliptiques, et de consistance et de couleur variables. Étudie-t-on enfin les fruits, typiquement petits et à contour ovale? Il y en a dont la grosseur atteint la moyenne, et dont la forme tend à devenir lancéolée. Mais il n'y a peut-être aucun caractère qui offre plus d'inconstance que celui tiré de la présence des scrobicules; et comme il arrive que ce sont, aussi souvent, des formes ou variétés riches en alcaloïdes qui en sont privées, on a, par cet exemple, la mesure de la confiance que mérite ce caractère, en tant que significatif de telle ou telle constitution chimique de l'écorce."

I direct particular attention to what is said here in reference to the very varying aspect of the leaves in different kinds, corresponding exactly to the divergences which I find in my seedlings.

It appears, then, to have been an unfortunate bias which induced the early botanical labourers in this field to name many of the species after the shape of the leaves, as *cordifolia*, *lanceifolia*, &c., since even on the same plant may be found at times much diversity.

Stirps 5. *C. OVATA*. Ramus A (Wedd.).

I now approach the consideration of the *Cinchona succirubra*, which is, equally with the species before considered, a kind presenting itself under a variety of distinct forms. These are well represented in the consignment of botanical specimens which I received in 1858 from America, and have described in my 'Nueva Quinologia.'

This is specially a species defined, as Dr. Weddell observes in another case, by characteristics drawn from the bark. In a note respecting *C. pitayensis*, var. *almaguerensis*, Dr. Weddell says (Ann. d. Sc. Nat. ser. 5, vol. xii. p. 41) :—"Voici, comme on voit, une variété établie sur des caractères tirés de l'écorce, et l'on a pu

remarquer que plusieurs autres membres des séries que renferme mon tableau ont une origine semblable. Il n'y a rien là qui doive étonner. Quand on songe, en effet, au rôle considérable de l'écorce dans le développement de la plante, pendant les diverses phases de son existence ; quand on a vu les nombreuses formes qu'elle est susceptible de revêtir ; quand, enfin, on prend en considération l'attention toute particulière donnée à cette partie par les quinquilogistes, on comprend qu'il y ait d'aussi bonnes raisons pour fonder des distinctions de rang sur son examen, que sur celui des feuilles ou de tout autre organe ; bien mieux encore, quand on est habitué à voir les caractères physionomiques et organiques appuyés par des différences correspondantes dans la constitution chimique. De nombreux précédents rendent, du reste, ces remarques presque superflues. Il suffirait de rappeler l'exemple de l'*Ulmus suberosa*, Ehrh., qui n'a été constitué en variété distincte de l'*U. campestris* qu'en raison du caractère particulier présenté par la partie en question."

M. Triana dwells on the therapeutic value of the Red Bark, and observes :—

" Il était donc naturel que la provenance d'une écorce si bien-faisante fût soigneusement recherchée. Mais la plante qui produit le Quinquina rouge demeura longtemps mystérieuse. Ce n'est que tout récemment que son origine s'est révélée, et c'est au zèle ardent de M. Howard que nous devons enfin des données positives sur ce point obscur de l'histoire naturelle des Quinquinas. Il publia, au mois d'Octobre 1856, dans le 'Pharmaceutical Journal,' une notice sur cette plante, et donna un dessin de ses feuilles d'après les exemplaires transmis par un habitant de l'Equateur qui exploitait précisément le Quinquina rouge. La localité où avaient été récoltés ces exemplaires correspondait aux indications fournies par M. Weddell sur la patrie probable de l'espèce, dans son 'Voyage au nord de la Bolivie,' publié en 1853, et concordait également avec les renseignements de Lambert sur le même sujet. Précédemment M. Howard avait déjà cru découvrir, dans l'herbier de Pavon, au Musée britannique, un véritable arbre de Quinquina rouge, désigné sous le nom de *Cascarilla colorada de Huaranda* ; puis à deux années de distance, à l'Université de Berlin, il trouva aussi dans l'herbier de Pavon un meilleur échantillon, d'après lequel M. Klotzsch fit alors une description soignée de l'espèce.

" Enfin, l'espèce reconnue distincte a été exactement identifiée avec une plante décrite dans les manuscrits de Pavon, sous le nom

de *Cinchona succirubra*. Elle vient d'être clairement exposée dans la splendide publication de M. Howard, intitulée *la Nueva Quinologia de Pavon*."

On this quotation from M. Triana, which is historically correct, I have to remark that, though the plant which I figured in 1856 was true red bark, as shown by the wood and bark transmitted, at the same time it happened not to be of the typical form of leaf, but one verging more towards the *C. erythroderma* of Weddell. It was therefore but a step towards the solution of the question. There was also another difficulty in the way of tracing out the *C. succirubra*, owing to the great resemblance existing between the specimens of *C. colorada de Huaranda* and those of *C. cordifolia*, var. *rotundifolia*. Mutis represents it as a variety (only) of *C. cordifolia*.

I do not think it necessary to dwell longer on this branch, as I have given every information in previous papers published at different times, and also in Reports to the Indian Government, which are printed in the 'Returns.' I have only to express my regret that the cultivation of this Red Bark has been so widely extended. *It is not capable of becoming the quinine-producing tree of the future.* I have described its idiosyncrasy in my 'Quinology of the East-Indian Plantations,' and find by a recent letter from Mr. Broughton that I am right in my anticipations. Neither the plan of covering the branches with moss, nor that of stripping off the bark, will prevail to cause the *C. succirubra* to change its inveterate habit of producing Cinchonidine.

This does not diminish the value of the bark for the purposes of the druggist (as distinguished from the quinine-manufacturer); but in a commercial point of view it is different, as this particular alkaloid is so abundantly produced by inferior barks that no sale could be found for the quantity that could be prepared—unless, indeed, the Indian Government could be induced to substitute Cinchonidine for part of their supplies of Quinine, or the same course of action were followed by other nations. The Government of India have already rendered an essential service to medicinal science by the Commission which they appointed to test the relative value of the different alkaloids. It would seem but a very obvious sequence to such a Commission to encourage the consumption of that therapeutic agent which the plantations of India can now so abundantly produce.

I must here refer to the *C. rosulenta*, which I described in the

'Journal de la Société Botanique de Paris,' and which I take to be the source of the *Quinquina rose d'Ocana*, a tree the abundant produce of which in cinchonidine renders the bark of less value in the market. I take this to be the Red Bark of Ocana—and at present must regard it as a distinct species, though very nearly allied to *C. cordifolia*. The Red Bark which M. Triana describes, and of which he has seen specimens in the possession of Mr. Rampon, is (from the description) a wholly different bark, being the red variety of the *C. lancifolia*.

I cannot quit the subject of the Red Bark without expressing the gratification which I feel in the fresh light which M. Triana has thrown on the subject of the false Red Barks of New Granada, the source of the *Quina nova* or *Quina roja* of Mutis; which for a time supplanted the true kind in English medical practice, to the great detriment of the patients.

Here also we have, in an allied genus, the same tendency to present itself in several different forms, all of which may probably be reduced within the bounds of one species, the *Cinchona* (?) *oblongifolia* of Mutis, now classed under the genus *Buena* of Weddell. It is well described and figured by Karsten as *C. bogotensis*.

The genus *Cinchona*, in fine, appears to me to possess the characteristics both of great stability of organization, as shown in the different sorts which have been most studied, and also a remarkable tendency to variation in the product of the seed from the same capsules. Both these facts I have ascertained from actual observation, confirmed by the researches of others. For instance, as to the first mentioned, I possess carefully gathered specimens from the time of the earliest observers of the barks of different species. These also exist abundantly in the museums of Europe, and are known by all who have studied them to present the most exact resemblance to the barks grown in the present day, not only in outward features, but in minute and well-marked microscopical organization*, and in definite chemical contents, whether the place of growth be in their native mountains or in India. I was prepared to expect some evident and marked influence from the great alteration of climate; but, though liable in

* See Berg's 'Atlas' and my 'Quinologia.' I have interesting confirmation of this in a recent examination of some renewed bark of *C. succirubra* sent me by Mr. M'Ivor, reproducing the peculiar radial structure of the lax cellular tissue which is represented in my 'Quinology E. I. Plants,' and therewith the like rich contents in alkaloids.

both regions to singularly precise and well-defined changes from drought or moisture, from sunshine or shade, from difference in elevation above the sea-level, still on the whole the East-Indian plants appear to be the exact reproduction of those in South America. The character of the inflorescence and that of the general habit of the plant are equally permanent. I must refer to my published descriptions of three generations of the *C. officinalis*, one in South America, one partly in my stoves and partly in India, and one altogether in India, as one proof of the persistence of characteristics which I have been remarking. Many others might be given; but I think the truth of the observation will be recognized by all who have studied the *Cinchona*. As to what might be effected by a longer period of time, we seem to be without any thing to guide us. The practical deduction for those who are interested in the plantations is, that they may safely rely on this characteristic of permanence, and need not fear any mutability of structure or loss of their peculiar virtue in well-selected sorts. I think that this may be affirmed with safety.

But then comes into consideration the strange and, to me, inexplicable tendency to vary in the seed produced by the same plant, which is exhibited in the product of the capsules of the same botanical specimen sent as No. 6 from India. From this I raised one or more plants resembling the parent, but also another which appears to be exactly the No. 11 of the same consignment, this latter being a variety to which great importance has been attached on account of its yielding by far the largest amount of quinine of any yet known*. I do not think this can be regarded as a hybrid sort, but as a simple variety of the *C. officinalis*, var. *Bonplandiana*. The inflorescence, it seems, does not differ; and Mr. M'Ivor informs us that this admirable variety cannot be depended upon to come true from seed; so that the only certain mode of propagation is by cuttings. The seed of No. 11 might not improbably reproduce the No. 6. In other and quite different kinds of *Cinchona* I find the same tendency to variation; so that it would seem that each species must present itself under a variety of distinct but closely allied forms. I have found this to be the case with almost every kind which I have yet grown from seed, but with none more notably than with the produce of a particularly good form of the *Cinchona Calisaya* (var. *Ledgeriana*), from which

* *Subsequent Note* (Dec. 1873).—With exception of the var. *Ledgeriana*, which has yielded M. Moens equal to more than 12 per cent. of quinine. The numbers 6 and 11 refer to specimens in the Museum at Kew, of which mine are duplicates.

Mr. Broughton obtained the largest percentage of quinine met with in the bark of any form of the *Cinchona* in question. Of this he was good enough to send me by post a small packet of seed in April of last year. These soon germinated and gave me between 800 and 1000 seedlings. From amongst these I have reserved those which presented the greatest variation and which would certainly have been in old times accounted different species. Are these all, or any of them, hybrids? I scarcely think so, since it is pretty evident that a similar state of things prevails in their native woods, where (as pointed out by Dr. Weddell) cross-breeding between really different species must be of rare occurrence. If any one will look carefully into the researches of Pavon or any of the old botanists, or even of distinguished modern observers, such as Dr. Weddell, they will see that, however carefully the genus is divided into species, yet each of these species, when better known, is found under several forms.

The inquiry will of course arise whether this variation may not be due to the interference of the pollen of other species. If the peculiarity were at all isolated, I should perhaps incline to the opinion; but this seems to be set aside by facts. For instance, in this very species I have another example of similar variation, where hybridity seems improbable. An English traveller (Mr. Charles Ledger), whilst engaged in the project of transferring the Alpaca from Peru to Australia, obtained the information from his native shepherds respecting the best kind of *Cinchona*. After nearly four years' fruitless efforts, he learned that the trees of "Calisaya red bark" were in magnificent condition and promising for seed. An Indian and his son were sent as collectors into Caupolican and returned thence with the precious seed, which on July 22nd, 1865, was shipped from Arica to London. The export of these seeds was at that time prohibited, as this is one of the finest Calisaya-growing districts in Bolivia. The bag was nevertheless sent to Mr. C. Ledger in London; and I assisted at the negotiations which ended in its transference to India. The aspect of the capsules was that of the var. *microcarpa* of the *C. Calisaya*—that is to say, one of the best kinds. I recommended that the payment should be made dependent on the produce of at least 10,000 plants in India. The number was reached and exceeded; and since commencing this paper I have had the opportunity of learning from the purchaser himself (Mr. Money), who happens to be in the country, that the produce is as varied as that in my

stoves. I myself took a small handful as the reward of my assistance in the bargain, and found a like result in the produce. In the autumn of the year 1865 Mr. G. Ledger sold some portion of the seed to the Dutch Government; and the result has been most favourable to the prospects of the plantations in Java.

I have now growing variant forms of the *C. Pahudiana*, of the *C. succirubra*, and of the *C. officinalis* which bear no appearance of hybridity; but the extent of variability, though much insisted on by Mr. Broughton, I could scarcely credit till brought in so many different ways under my own observation.

In concluding this paper I wish to suggest as a legitimate inquiry whether, in reference to this family of plants, there is not a possibility of arriving at greater certainty as to the question of *species*, *subspecies*, or *race*. In other departments of scientific research, as in chemistry, we are accustomed to put questions to Nature and to extract answers with greater or less difficulty. I do not see why we should not do the same in this genus. I suppose that all naturalists are practically agreed, to a considerable extent, in the view which must force itself on the mind, that it is in the sexual organs we must find the boundaries which have been established by an all-wise Creator, and which the creatures cannot overstep. In the animal creation we have the phenomenon of sexual repugnance. In the vegetable kingdom we find incompatibility. Thus the varied features of the beautiful Cosmos around us are prevented from intermingling and reducing the aspect of things to a frightful chaos. The genera being thus kept apart, the species may, I suppose, be characterized by an inferior and yet marked amount of incompatibility; so that if two forms of a plant are so far incompatible in their floral organization that the result of the mixture of their essences would be an unstable (and therefore, I must presume, unnatural) hybrid progeny, there I think that I should be justified in terming them distinct species. In calling the progeny unnatural, I imply that they are not intended to continue, but to revert to one or other of the parent lines, or else to become extinct. I derived these views at first from the botanist Dr. Klotzsch, at Berlin, and subsequently find them so well expressed by M. Quatrefages, the President of the French Institute, that I may be permitted to copy his definition* in order to illustrate my meaning:—

“ Pour moi, *l'espèce* est l'ensemble des individus plus ou moins

* ‘Charles Darwin et ses Précurseurs,’ p. 227.

semblables entre eux qui sont descendus ou qui peuvent être regardés comme descendus d'une paire primitive unique par une succession ininterrompue et naturelle de familles.

"La *variété*, ai-je dit, est un individu, ou un ensemble d'individus appartenant à la même génération sexuelle, qui se distingue des autres représentants de la même espèce par un ou plusieurs caractères exceptionnels. La *race* est l'ensemble des individus semblables appartenant à une même espèce, ayant reçu et transmettant par voie de génération les caractères d'une variété primitive.

"Ainsi l'*espèce* est le point de départ ; au milieu des *individus* qui composent l'espèce apparaît la variété ; quand les caractères de cette variété deviennent héréditaires, il se forme une *race*. Tels sont les rapports qui, pour tous les naturalistes, règnent entre ces trois termes, et qu'on doit constamment avoir présents à l'esprit dans l'étude des questions qui nous occupent.

"De là résulte premièrement que la notion de ressemblance, très-amoindrie dans l'espèce, reprend dans la race une importance absolue. De là il suit également qu'une espèce peut ne comprendre que des individus assez semblables pour qu'on ne distingue pas même chez eux des variétés, qu'elle peut présenter des variétés individuelles dont les descendants rentrent dans le type spécifique commun, mais qu'elle peut aussi comprendre un nombre indéfini des races."

The arrangement of the genus proposed by Dr. Weddell accords well with the above views of M. Quatrefages*, in accordance with which he has chosen the term *stirps*, which perhaps would be nearly equivalent to my *species*. His observations, which I now quote, seem to show this. He says (Ann. d. Sc. Nat. ser. 5, vol. xi. p. 354):—

"..... le nombre des formes composant le genre *Cinchona* paraît avoir constamment tendu à augmenter, et les chaînons de la série à se relier entre eux d'une manière si intime qu'on a bien pu se demander si tous n'étaient pas le résultat du développement

* "On peut se figurer les espèces dont le premier type n'a pas varié comme un de ces végétaux dont la tige est tout d'une venue et ne présente aucune branche, et les espèces à races plus ou moins nombreuses comme un arbre dont les branches mères se subdivisent en branches secondaires, un rameau en ramuscules plus ou moins multipliés. À travers quelques différences de langage, il est facile de reconnaître que tous les naturalistes s'accordent encore sur les points que je viens d'indiquer."

et de la variation d'une très-petit nombre de formes primitives* ou typiques. Telle était vraisemblablement l'opinion de Mutis; et c'est sans doute sous l'empire de cette idée que dans sa 'Quinologie,' il a admis deux types seulement parmi les espèces fébrifuges, les *C. lancifolia* et *C. cordifolia*, auxquels il rattache, comme variétés, toutes les autres formes qui étaient parvenues à sa connaissance."

I willingly give preference to the opinion of this distinguished botanist, and think the probability of several centres is greater than that of one such for the whole genus. In fact, the species do not lend themselves so easily as I once thought to the latter arrangement. I cannot find any probable centre from which the genus should spring; nor am I quite so certain as my friend Dr. Weddell as to the absolute definition of the circumference. The intermingling of characteristics seems to me more like the crossing of circles, some of which have their centre *outside* the genus.

I suppose that the above difficulties do not belong only to this genus, but beset us in many other directions when we seek to grasp (in a logical sense) the origin of species. It may be more satisfactory to recur to that which is before us, and to occupy ourselves rather with that which now exists than with some imagined previous state of things. For, as far as I see, there is nothing like the confusion which would result from indefinite and endless variation, nothing, again, like the survival of the best and the selection of favoured forms; but varieties spring up from seed without any assignable cause; and these, if they *happen* to be permanent, form definite races; so that the whole constitutes a highly variable unity, if I may call unity that which is but a part of the great whole, held in its appointed course by the agency specially of those two opposing principles called by Dr. Lucas† the law of imitation, which is "hérédité," and the law of *invention*, which this author calls "l'innéité‡" or *innate disposition to change*.

* In a note in the same page Dr. Weddell adds:—"Ou même d'une seule qui serait alors nommé, à juste titre, *Quina primitiva* 'The occurrence of these intermediate forms,' dit M. Howard dans l'introduction du bel ouvrage cité plus haut (p. vi), 'suggests the inquiry whether all the species of *Cinchona* have had but one and the same origin, and have varied by the influence of climate, soil, &c. into the many sorts which we now behold.' J'ai moi-même exprimé il n'y a pas longtemps (in 'Report of International Hort. and Bot. Congr.' p. 222) la même manière de voir; mais je crois que l'hypothèse de plusieurs types offre plus d'avantage au point de vue pratique."

† 'Traité philosophique et physiologique de l'Hérédité naturelle,' par le Dr. Prosper Lucas. Paris: 1847.

‡ Ibid.

Whatever may be the state of the case, it seems to me that we have at the present moment an opportunity which should not be lost for investigating and, if possible, settling some of the questions which have arisen. Mr. Broughton and the Indian cultivators are confident that hybrid varieties have arisen there, specially between *C. succirubra* and *C. officinalis*. Dr. De Vrij is equally confident that there has been a cross between *C. Calisaya* and *C. Pahudiana*. M. von Gorkom and others concerned participate (I believe) in this opinion; but I specially mention the first-named gentleman, because I think his analysis of the barks presents some added confirmation of the view.

The Indian Government have at their command the requisite means for putting this question to the proof, and for ascertaining which of the now numerous forms in its possession would yield real hybrids, and what are the laws which prevail in the reproduction of the races or varieties, and amongst them the variety *angustifolia*, so famed for its excellence*. It is impossible to say what might not arise in the way of practical suggestion, as well as of theoretic clearness of classification, from this new line of research; and if the Linnean Society were understood to be favourable to the investigation, I can scarcely doubt that the subject would be other than favourably entertained and beneficially acted upon. I am informed by a person practically conversant with the subject that the needed precautions might in his opinion be taken, and results that could be depended upon obtained. At all events we might expect to have the question decided whether or not the suspected hybrids in the Indian plantations are or are not such in reality.

* *Subsequent Note*.—Also of the *C. officinalis*, var. *pubescens*, and of the *C. Calisaya*, var. *Ledgeriana*, which may even surpass the one above mentioned. The var. *Ledgeriana* seems to be distinguished from the normal *C. Calisaya* both by its small and pure white flowers, and by its small capsules.

I learn from Mr. Broughton that the seed he sent me, and which produced so many varieties, "was gathered from two trees of the same red-under-leaved variety of *Calisaya*."
