of the second decimal. Very concordant determinations of density gave as a mean number 19·90.

Argon, therefore, shows no sign of association on cooling, nor of dissociation on heating, as Prof. Bevan thinks it might.

RAYLEIGH.

Terrestrial Helium (?).

PROF. PASCHEN and I have lately made a careful determination of the wave-length of the strong yellow line emitted by cleveite when heated in a Plücker tube. We owe the mineral to the kindness of Prof. Rinne. My large Rowland concave grating of 6·5 metre radius, clearly shows the yellow line to be double. Its less refrangible component is much weaker, but comes out quite bright, when the stronger one is brilliant. We photographed the two lines together with the second order of the spark spectrum of iron. There are a number of iron lines on each side that are included in Rowland's list of standard wave-lengths (*Phil. Mag.*, July 1893). From these we interpolated the wave-lengths of the yellow lines by micrometric measurement. Three different plates taken on different days gave us :

Strong component.		Weak component.
5875·894	... ...	5876·216
5875·874	... ...	5876·206
5875·880	... ...	5876·196
Mean 5875·883		Mean 5876·206

We think an error of more than 0·025 very improbable.

Now Rowland's determination of D_3 (*Phil. Mag.*, July 1893) is :—

5875·982

the result of three series of measurements which he believes to be accurate to 0·02.

The difference between this value and the wave-length of the strong component is much too large to be accounted for by an error of observation.

We do not therefore agree with the conclusion, drawn by Mr. Crookes, that the unknown element helium causing the line D_3 to appear in the solar spectrum is identical with the gas in cleveite, *unless D_3 is shown to be double.* Perhaps Prof. Rowland will tell us if this might have escaped his notice. From his note on D_3 in *Phil. Mag.*, July 1893, it appears that D_3 cannot have been so wide as to include both lines, because he would then not have considered his determination accurate to 0·02. As for dispersion, one may see in his table of solar spectrum wave-lengths that he has frequently measured three and even four lines in an interval as large as the one between the components.

Hannover Techn. Hochschule, May 16. C. RUNGE.

The Origin of the Cultivated Cineraria.

I HAD hoped that it would not be necessary for me to say anything more upon this subject. But Mr. Bateson's last letter seems to require a few remarks on my part.

I confess that I find it very difficult to follow his train of arguments. All I can do is to restate once more my original position, and endeavour to see how far Mr. Bateson has been successful in impugning it. I am sorry that Mr. Bateson thinks I have "treated" him "to some hard words," though I confess he seems to me, in that matter, quite able to take care of himself.

I asserted then (*a*) that the cultivated Cineraria only differs from the wild form, putting colour changes aside, in dimensional differences. I believe that in saying this I am expressing the deliberate opinion of the Kew staff, the members of which, such is human nature, would have no hesitation in disagreeing with their chief, if they thought otherwise. To this point I do not understand that Mr. Bateson advances any serious objection.

Secondly (*b*) I asserted that these dimensional differences had been gradually accumulated. To this I understand Mr. Bateson demurs, though I fail to see that he has brought forward a particle of evidence to prove the contrary.

Now for Mr. Bateson's own position. He asserts, in common with other authorities, that the modern Cineraria is of hybrid origin. I have arrived at an opposite conclusion. And here I may quote the support of Dr. Masters, F.R.S., the well-known editor of the *Gardeners' Chronicle*, who in that paper for January 24, 1891, p. 108, states :—"Carnations and Picotees, again, which originate from one species, vary from seed but not from buds ; and the same may be said of the Cineraria, the offspring of one species."

Mr. Bateson complains that I do not give "any specific answer" to the historical evidence. I thought I had made it sufficiently clear in my last letter that: (*a*) I doubted its value for scientific purposes ; (*b*) I set it aside as irrelevant on account of the impossibility of proving the descent of the modern Cineraria from its supposed ancestors. Both Prof. Weldon and I have shown that the historical evidence can be handled both ways. But I prefer to set it aside altogether in the face of objective facts.

Mr. Bateson's next step is one to which I most seriously demur. He transforms a proposition of mine into terms to which I could not assent, and then proceeds to attack it. He makes me say that "to improve a plant the only safe way is by selecting," &c. I absolutely never said anything of the kind. "Improve" in horticulture is a word of large connotation. I confined myself to the production of dimensional changes, and I believe that what I said was in accordance with horticultural experience.

To demolish my position, Mr. Bateson has to get over the fact, which seems to me incontestable, that there is no essential morphological difference between the cultivated Cineraria and the wild *C. cruenta*. To do this he trots out the Himalayan rabbit. I cannot but admire his courage. What possible analogy can there be in the two cases? Two "breeds" of rabbits are crossed and produce a third *different from either*. If the modern Cineraria is of hybrid origin, then it has eliminated traces of all but one of its parents. The principle of economy of hypothesis makes me slow to believe this. Anyhow the Cineraria has clearly not produced anything analogous to a Himalayan rabbit which differs from both its parents.

As to Mr. Darwin's account of the origin of the Cineraria, I must frankly take the responsibility. I have no doubt he worked with ordinary garden kinds. He wrote to me for information as to their origin. At the time I was entirely ignorant of the subject. I wrote to Mr. Thomas Moore, who was considered the best authority on such matters, and he sent me the traditional account. I passed it on to Mr. Darwin, with the opinion, no doubt, that I thought the information trustworthy. So I am afraid Mr. Bateson is only appealing in this case from Philip sober to Philip drunk ; *i.e.* from my own considered opinion to my unconsidered one.

I will now wind up all I have to say on the subject with a few miscellaneous remarks.

There can be no two opinions as to the importance of the study, from the point of view of organic evolution, of the changes which can be brought about in plants under cultivation. But it must be conducted with scientific precision. This discussion will not have been fruitless if it directs attention to the subject. A beginning has already been made. M. Bornet has worked on the genus *Cistus* at Antibes, and has reconstructed some of the forms, as to the origin of which there was only "historical evidence," described and figured by Sweet. My friend Count Solms-Laubach is engaged on the cultivated forms of *Fuchsia*, and I am quite sure that any results he arrives at may be accepted with implicit confidence. As he has asked me for species of *Cineraria*, I hope he may look into this matter also.

I must repeat my caution as to the danger of accepting horticultural evidence as to hybridity. I will give a few recent instances. I could easily give a long list with chapter and verse for each.

(*a*) *Thuya filiformis* was long considered to be a hybrid between *Juniperus virginiana* and a *Thuya*. It is now known to be a "growth-stage" of *Thuya orientalis*. The history is discussed by Sir Joseph Hooker in the *Gardeners' Chronicle* for June 22, 1861, pp. 575, 576. It affords a delightful commentary on the hybridisation fallacy and the value of "historical evidence."

(*b*) Some years ago we received at Kew bulbs of what professed to be a hybrid between *Amaryllis Belladonna* and *Brunsvigia Josephinæ*. When it flowered, it was evident that it was no hybrid at all, but only a very fine form of the former species. This is rarely propagated from seed. In this particular case seminal variation had come into play with corresponding dimensional change. The hybrid origin is recorded in the *Gardeners' Chronicle* for September 4, 1875, p. 302. It will, no doubt, be dug out hereafter as "historical evidence."

(*c*) The last number of the *Gardeners' Chronicle* (June 1, 1895,