In habit and size our seedling is not at all like *lanata*, but might be taken for a poor specimen of the common Cinerarias. In several characters it is intermediate between *lanata* and the latter. The stem is rather woody, less so than in *lanata*, but it is thick like those of garden kinds: petioles like *lanata* in having no auricles: leaves, nevertheless, large like those of garden kinds, the backs very woolly, but largely purplish, as in many cultivated sorts. Now this plant must be either (1) a sport from *lanata* in the direction of the garden forms, or (2) an accidental hybrid between *lanata* and one of the cultivated kinds growing in the same house with it (we have no others). The latter seems more likely—an opinion in which Mr. Lynch fully concurs.

Similarly Bouché (*Wittm. Monatss.* xxii. p. 298, orig. not seen, quoted from Focke, *Pfl. Mischlinge*, 1881, p. 201) says that a hybrid between *C. Webbii* (Schlz. Bipont.) and *cruenta* arose in the Berlin Botanic Garden as the spontaneous product of these species growing side by side.

It was, I think, on evidence like this that the parentage of the older hybrids was conjectured; but that Drummond and Henderson certainly—and possibly others—did make definite efforts to hybridise, cannot on the evidence be doubted. That these efforts went no further than the brushing of pollen of some species upon the flowers of others, I fully believe, and that on such evidence the *precise* parentage cannot be assigned is obvious. Nevertheless distinct seedlings resulted. In a few years, as the writer in *Paxt. Mag.*, 1842, p. 125, says (in an article urging to fresh efforts in crossing), this hybridisation "was the means of creating quite a novel and superior race." There were the new plants: how had they arisen? Those who doubt that these new kinds were hybrids must choose the other horn of the dilemma, and accept them as sports pure and simple.

That the historical records may contain errors, I am fully aware; but if they cannot be accepted in detail, should they be altogether rejected? We might perhaps reserve a doubt whether the King came precisely from pure *cruenta* fertilised by *lanata*; whether *cruenta* var. *lactea* was a hybrid between *cruenta* and *populifolia* (as de Candolle surmises); whether *Waterhousiana* was the offspring of true *cruenta* and true *tussilaginis*; whether Mrs. Loudon's statement that the species used were *cruenta*, *lanata*, *aurita*, *tussilaginis*, and *populifolia*, or Moore's belief that *cruenta* and *tussilaginis*, with perhaps *Héritieri* (= *lanata*), *maderensis* (= *aurita*), and *populifolia* ("Cross and Self-Fert.," p. 335, *note*), or Otto's similar declaration (Regel's *Gartenflora*, 1857, p. 66), or that of the *Jour. d'hort. Gand*, 1846, already given, should each be taken without hesitation as full and complete statements of the whole truth, but that they contain a substance of truth is hardly in question.

Against this Mr. Dyer offers nothing but an opinion derived from an inspection of certain modern plants. He who has confidence in the results of this method must suppose our knowledge of the laws of inheritance to be much more complete than I believe it to be. It is not the method Darwin used. Take a well-ascertained case. Who would know from inspection of the Himalaya rabbit that it came directly from the Silver-greys or Chinchillas? (See *Animals and Plts.*, i. p. 113.) It is unlike them, is of sudden origin, and yet breeds true.[1] To suppose that in cross-bred offspring given characteristics of the parents must be found, is to assume the precise question which in a discussion of organic stability is at issue. Let it be remembered that on the hypothesis of hybrid origin for our Cinerarias it is supposed that they result from several species and varieties, crossed not once only, but many times, in wholly irregular ways. Can it be seriously expected that any special resemblance to a given ancestor should be still traceable?[2]

My position then is this. We heard Mr. Dyer's statement; turning to the literature I found an entirely different account, borne out by copious and on the whole fairly attested evidence, pointing irresistibly to the conclusion that the Cinerarias are species which hybridise freely, and that our modern forms have arisen through such hybrid unions.

[1] To Mr. James, of Farnham Royal, a celebrated grower, and to his foreman, I am indebted for several interesting points, and especially for the following: Formerly blue self-coloured Cinerarias were scarce in his strain, but some years ago he introduced some plants of a French strain. After this, and presumably as a result of the cross between his own and the French kinds, there appeared a strain of blue selfs. These, though shy seeders themselves, transmit their peculiarity so strongly that they have to be kept in a house apart, for fear that their character should assert itself to the exclusion of everything else.

[2] In order to meet Mr. Dyer on his own ground, I have assumed, what I cannot admit, that in none of the various modern strains traces of the different parent-species appear.

Mr. Dyer has well said that "if you take any statement that Mr. Darwin has put forward, you may feel assured that behind it is a formidable body of carefully considered evidence not likely to be upset." By the courtesy of an opponent I have been directed to a passage in "Cross and Self-Fertilization," 1876, p. 335, where (before describing experiments showing considerable self-sterility in the garden Cineraria) Darwin gives this definition of his material, "*Senecio cruentus* (*greenhouse varieties, commonly called Cinerarias, probably derived from several fruticose or herbaceous species much intercrossed*"). It seems, therefore, that in this matter also Mr. Darwin has, to use Mr. Dyer's words, "squeezed out" of the evidence "all that it would profitably yield."

Here I would fain leave the subject. But perhaps it may be suggested that though Darwin's Cinerarias were probably hybrids, our Cinerarias may not be their descendants. Such a suggestion involves the supposition that in some hidden place there was a thin red line of pure *cruenta* waiting for the moment when it should oust the hybrids. If this be seriously suggested, I shall ask where such a strain was kept, and what steps were taken to preserve its purity. In view of the evidence that chance blendings occur freely, to keep a pure strain would require some care. Until this has been proved, we shall not, I think, be wrong in supposing that each grower worked on the material his predecessors had created, and that our Cinerarias are the lineal descendants of the hybrids raised in the first half of this century,

In the course of this discussion, Mr. Dyer has treated me to some hard words, which I do not particularly resent. Whether I have deserved them is not, perhaps, for me to judge. But I will ask Mr. Dyer to point out when, on being asked for the facts upon which I have based a view, I have replied that that was a "matter for future collection." The facts I have been able to collect may be few, but by a study of the writings of my antagonists, I have not been able to add materially to their number.[1] W. BATESON.

St. John's College, Cambridge, May 26.

IT has been pointed out to me that my remarks on Mr. Bateson's account of the Cineraria have been interpreted in a sense of which I did not dream when I wrote them.

I wish, therefore, to say that, although I do not believe Mr. Bateson's reading of the passages I quoted to be the true one, yet I have never questioned his sincerity in suggesting it, and I am pained to find that I have seemed to do so.

May 24. W. F. R. WELDON.

Boltzmann's Minimum Function.

I GATHER from Mr. Culverwell's last letter (NATURE, April 18), and Mr. Bryan's (May 9), that we may regard the following conclusion as established, namely, the proof of the H theorem, for any system depends on a certain condition (A) being fulfilled among the coordinates and momenta of the molecules forming the system. Considering these as elastic spheres, and using Dr. Watson's notation, $fdp_1 \ldots dq_3$ is the chance that a sphere shall have for coordinates and momenta $p_1 \ldots p_1+ap_1$, &c., and $FdP_1 \ldots dQ_3$ the chance that another sphere shall have $P_1 \ldots P_1+dP_1$, &c. The condition required is that f and F are independent, even for two spheres on the point of collision.

Otherwise we may express it. Let there be n spheres in space S. Let us suppose Mr. Culverwell to assign to each its position at time $t=0$, and Mr. Bryan to assign independently to each its component velocities. Then the condition A is fulfilled when $t=0$.

We can then prove that when $t=0$ $\frac{dH}{dt}$ is negative, or, as Herr Boltzmann would have us say, is more likely to be negative than positive.

Now arises a question which seems to me to deserve consideration. Assuming our system to be finite, and to be left to itself unaffected by external disturbances, does it necessarily

[1] It has been impossible for me to incorporate in this letter all the mass of information which has been most generously sent me by correspondents since this controversy began. It is suggested that I should point out that Mr. Dyer's use of the word "feral" to mean "wild" is not usual. A correspondent tells me that it was probably first used in the special sense of "run wild" by Hamilton Smith, *Nat. Libr.*, *Mammalia*, 1839, ix. p. 92. It has since been so used by many authors, especially Darwin, *An. and Plts.*, i. p. 117, &c.