

p. 692) affords a striking instance. Hybrid Cypripediums are of considerable pecuniary value. One recently exhibited at the Royal Horticultural Society was at once denounced as no hybrid at all, but a merely seminal variation. The possessor has fears that it will "add one more to the long list of doubtful crosses by which auctioneer and purchaser are alike misled."

Notwithstanding the Himalayan rabbit, I am afraid botanists will continue to refuse to accept hybrid origin on historical evidence unless there is palpable objective proof of the fact.

There are two additional bits of evidence, to which, however, I do not attach great weight, but which may be recorded to complete the story. It is, at any rate, agreed that the *Cineraria* originated from the Canaries. I have already pointed out that De Candolle divided the wild Canarian species into shrubby and herbaceous. I do not believe that they are mixed in the modern *Cineraria*, which remains entirely herbaceous. Now, Schultz-Bipontinus, who described the Canarian species for Webb and Berthelot, relegates the shrubby species to *Senecio*, and the herbaceous to *Doronicum*. Though this is not now sustained, it shows that the two groups are not very closely related, and diminishes the probability of their freely intercrossing.

On the other hand, *Cineraria cruenta* and the modern *Cineraria* cross with the greatest facility. In fact, if you grow the two together it is almost impossible to keep the wild species true. I have no doubt that in a short time we shall be able to combine the pleasing habit of the wild plant with the fine colour of the modern strains. All this does not surprise one, as to me they are all essentially the same thing.

I must add one word more. I cannot but think that there is a growing danger nowadays of a pseudo-biology growing up for the especial use of evolutionists. This is not the first time by many that I have been so unlucky as to come into collision with it. Long ago I pointed out in these pages that biology is not a deductive science, and for the present, at any rate, theory must be adjusted to facts, not facts to theory.

W. T. THISELTON-DYER.

Royal Gardens, Kew, June 1.

MR. BATESON now admits that *some* named varieties of *Cineraria* may have arisen from pure-bred *C. cruenta*, or from plants believed to be pure-bred. He holds that these have become extinct, while Mr. Dyer believes the hybrids to have disappeared. I have never attempted to discuss this question, and shall not do so now. I wish only to justify my interpretation of the passages I quoted against Mr. Bateson:—

(1) Mrs. Loudon begins the article quoted by both of us with these words: "Most of the purple *Cinerarias* are varieties, or hybrids, of *C. cruenta*." She then goes on to say that in or about 1827 (the year in which he recommended the growth of pure-bred *C. cruenta* "for the production of fine double and single varieties"), Drummond, of Cork, produced certain hybrids; while since his time other hybrids had been made. She then, in a new paragraph, says: "Some of the most beautiful *Cinerarias* now in our greenhouses have been raised by Messrs. Henderson . . . particularly *C. Hendersonii* and the King, both raised from seeds of *C. cruenta*"; and a line or two further: "Two new ones have lately been raised, of remarkably clear and brilliant colours, apparently from *C. cruenta*, named Queen Victoria and Prince Albert," &c.

It will be seen that the general statement, with which the article begins, declares "*most purple Cinerarias*" to be "*either varieties or hybrids*" of *C. cruenta*. Of others, and of those *Cinerarias* (such as "the King") which are not purple, nothing is said. This general statement is illustrated by examples, first of hybrids, next of pure-bred varieties.

In discussing the examples of pure-bred forms, Mr. Bateson omits to notice "Queen Victoria" and "Prince Albert," and discusses only *Hendersonii* and "the King." He believes Mrs. Loudon, in saying that these were "raised from seeds of *C. cruenta*," to mean simply that *C. cruenta* was the female parent, the male being unknown, or unnamed. I do not know what degree of inaccuracy Mr. Bateson is willing to attribute to Mrs. Loudon; but in the writings of serious botanists a "seed" means the fertilised product of two elements, the ovule and the pollen grain; and therefore the "seed" of *C. cruenta* means the product of two parents, both of which belonged to this species.

Mr. Bateson says that six or seven years after writing the passage in question, Mrs. Loudon speaks of *C. Hendersonii* and the King as "hybrids." This simply shows that she

changed her mind; and although it may affect the value of her opinion as evidence, it does not alter the plain meaning of her words in 1842.

(2) The only author whom I quoted as asserting the pure-bred origin of *C. Hendersonii* and the King was Mrs. Loudon. It is true that in two other articles quoted by Mr. Bateson these plants are called hybrids. I did not allude to this matter in my first letter, because I hoped Mr. Bateson would himself see the folly of attributing to these articles any definite meaning whatever. It will suffice to consider one of them.

In the earlier article, describing *C. Waterhousiana* (*Paxton's Mag. Bot.* iv. p. 219), that plant itself is called a "variety," although it is said to be the offspring of specifically distinct parents. On p. 221, *C. Hendersonii* is alluded to in these words: "The following are the names of some of the hybrids raised and cultivated by Messrs. Henderson . . . *C. cruenta* var. *Hendersonii*, *formosa*, &c." Both these passages are meaningless, if the words "hybrid" and "variety" are construed strictly. If they are not to be so construed, and they evidently cannot be, then I was justified in ignoring the passages, for they prove nothing but the incompetence of their author.

On the other hand, the passage which I did quote from this article is at least intelligible; and it asserts that *C. cruenta* "may be regarded as the parent"—which means, if it means anything, the *only* parent—"of many of those beautiful varieties so successfully cultivated by Messrs. Henderson," &c. This passage Mr. Bateson does not consider in his reply to me.

The second article (*Paxton's Mag.*, 1842, p. 125) in which the King is called a hybrid, uses the word in the same loose fashion, and it would be as easy as unprofitable to quote other passages in which the same plants are called now "varieties" and now "hybrids."

Enough has been said to show that Mr. Bateson's original evidence does in fact bear the interpretation I put upon it; and further, that the words "variety" and "hybrid" are so loosely employed by early writers that their records are often of little value. Stories of hybridism and sporting are frequently brought forward on such evidence; so that I have thought it worth while to examine the case for one such story, as stated by its advocate. Having done this, my interest in the matter ends, and I do not propose to speak further upon it.

W. F. R. WELDON.

University College, London, May 31.

Some Bibliological Discoveries in Terrestrial Magnetism.

IN a letter on the above subject, by Dr. L. G. Bauer, published in *NATURE* of May 23 last, I read as follows: "I find it asserted that the Frenchman, L. J. Duperrey, was the first (1836) to construct 'magnetic meridians' for the whole earth, *i.e.* those lines on the earth's surface marking out the path described by following the direction pointed out by a compass needle." The writer then remarks that the honour of first introducing this method is due to Thomas Yeates, an Englishman, in 1817.

This is hardly correct, as I possess a coloured map of the Northern Hemisphere with the "magnetic meridians" as described shown upon it of an earlier date. The title of the map is:

"To George Washington,

"President of the United States of America,

"This Magnetic Atlas or Variation Chart is humbly inscribed by John Churchman."

As Washington died in December 1799, it is evident that John Churchman has a prior claim to being the first to construct "magnetic meridians."

ETTRICK W. CREAK.
London, May 31.

Effects of Earthquake in Sumatra.

ON May 17, 1892, an unusually severe earthquake was felt through nearly the whole of North Sumatra; most severely shaken was the district between the Dolok Lubuk Raja and the Gunung Talamau (Ophir). Serious landslips occurred in many parts of the mountains, especially near the summit and along the slopes of the Gunung Merapi, a volcano 2145 metres high in the residency Tapanuli. On inspection it was found that the safety of a brick pillar, erected on its most elevated point by the triangulation service, was endangered by part of the crater having been