

they stretch out their short stumps of wings in order to assist in their attempts to escape. But all their efforts to acquire by such means the additional power of flight have been unavailing, and the type of the species remains as it was in this respect 3000 years ago. And in the case of other analogous species of birds, such as the *Dinornis* and the Dodo, we know that the actual destruction of the species has taken place, whilst that of the Kivi of New Zealand is equally certain in a very short time. I purposely avoid referring to geological evidences, believing that—1st, if the permanence of a species can be proved for such a length of time as 3000 years; 2ndly, if it be admitted that varieties exhibit a tendency to revert to the original type; and 3rdly, if cases can be shown in which modifications beneficial to a species have not taken place in wild animals, even when the creature has made efforts in that direction, we are in each of these cases furnished with an answer to Mr. Darwin's theory. As regards the second of these points, it seems inevitable that a theory which supposes the principle of development to be inherent in the works of the creation cannot be maintained if it be admitted that the antagonistic principle of reversion be also inherent in individuals. Mr. Darwin builds his theory that species are only intensified varieties, and that generic groups are only intensified species, mainly on the modifications which man has effected in domestic animals. For his theory, however, to work, it is necessary to suppose that the modified individuals possess such powers of discrimination as well as of exclusiveness as not to allow of their intermingling with their less favoured brethren, whereby they would keep their improvements to themselves. Thus, supposing the large and small common white Cabbage-butterflies to be modifications of one species, we must allow to them (as they never pair together, although frequenting the same garden and feeding on the same cabbage) a power of selection for breeding purposes which the improved breeds of domestic animals do not possess. The terrier and spaniel, or the pouter and tumbler pigeons will, under similar circumstances, breed together, although they apparently differ much more from each other than these two species of butterflies. It will at once be seen that the idea of such a power of selection becomes more and more untenable—the nearer we ascend to the supposed origin of the modification.—*Gardeners' Chronicle*, Feb. 11, 1860.

Mr. Darwin on the Origin of Species. By W. H. HARVEY, M.D.

IN a recent number of the '*Gardeners' Chronicle*' you figure a monstrous many-headed Cauliflower; and, in making some editorial remarks upon it, you suggest that it possibly throws some light upon the way in which species, according to Mr. Darwin's theory, originate in Nature. I am not quite sure that, as respects this particular Cauliflower, Mr. Darwin would agree with you, for it hardly comes within his principle, which denies to natural selection any power to act, unless the variation acted on be "favourable to the variety," in battling with its neighbours in "the struggle for life." Now, though the many heads may be very advantageous to the cook or the market gardener, it is doubtful whether, in a crowded society,

they would help a plant that had them in pushing itself forward toward the light. For, in a struggle, the lateral heads would become etiolated and abortive by the close contact of neighbouring plants, and the terminal head would alone have a chance of pushing forward and forming seed. Meantime the new variety would be spending its strength (like a Protectionist) in favouring a non-paying "manufactory." Clearly, therefore, the old, one-headed cauliflowerers, unburdened with unprofitable speculations and concentrating all their energies on one result, would stand the better chance of turning their "crown into a pound." But, be this as it may, I wish now to call the attention of your readers to another monster, which, by a curious coincidence, appeared at Kew about the same time that Mr. Darwin's book appeared in Albemarle Street, and which, if I interpret it aright, speaks much more forcibly against the truth of Mr. Darwin's hypothesis than your cauliflowerer, on the most favourable interpretation, says in its favour. I allude to a monstrosity in *Begonia frigida*, figured in the 'Botanical Magazine,' t. 5160. fig. 4, and thus described by Sir William Hooker:—"Our artist, Mr. Fitch, while making the drawing, detected a curious morphological structure in the fact of one of the flowers having an *inferior* perianth of four very unequal sepals (such as are indicative of a male flower); and above their point of insertion are four stamens (apparently perfect), alternating with four *superior*, free, ovate ovaries, each with a short style, and two downy linear stigmas. It is to be regretted that no section was made of these ovaries, which, from situation and in form, so little resemble the three-celled inferior fruit of *Begonia*." To this account I may add that Dr. Hooker assures me that the ovules appeared to be normal, such as might have been fertilized. Let us suppose that they were perfect, and had been allowed to seed; every gardener would anticipate, I presume, that some of the progeny at least, if not all, would have borne similar flowers. Now, had this occurred in a state of nature, and had a botanist collected a plant with such flowers, he would not only have placed it in a distinct genus from *Begonia*, but would probably have considered it as the type of a new natural order. Can it be possible, then, that genera and even natural orders spring up like mushrooms in this sudden manner? According to Mr. Darwin's hypothesis, the thing is impossible; for it would have required hundreds, perhaps thousands of successive generations to have enabled "natural selection" to convert an inferior ovary and unisexual flowers into a superior ovary and bisexual flowers. If there be one thing more frequently iterated than another in Mr. Darwin's book, it is this: that "it is fatal to my theory" if changes be not slowly progressive, by the accumulation of small increments from generation to generation; increments which, at first, may be only obvious to a breeder, but which, "bred up to" continuously, are sufficient, through "natural selection" alone (as we are told p. 186), to change the eye-speck of a Medusa into the human eye (if not to transform a slave-making ant into a Southern States-man). If time be only long enough, and generations and divarications of form many enough, according to the theory, not only such things may be done, but they have been done!

But a sudden change, like that hinted at by our *Begonia*, was not contemplated by Mr. Darwin's hypothesis; and if such should ever be established; if seeds should ever be raised from such a flower, and should breed true, then the theory would receive a serious damage, and a few such cases would overthrow it altogether. For, says Mr. Darwin, at page 206, "on the theory of natural selection we can clearly understand the full meaning of that old canon in natural history, '*Natura non facit saltum*.'" This canon, if we look only to the present inhabitants of the world, is not strictly correct; but if we include all those of past times, it must by my theory be strictly true." It might be easily shown, by quoting other passages, that the theory, strictly taken, denies not only a "saltus" but a "gradus," and proceeds by a sliding-scale. But let us confine ourselves to the "saltus." Is it not a "saltus" for a plant, at one bound, to change an inferior ovary and unisexual flowers to a superior ovary and bisexual? Would not such a fact, if fairly established in the vegetable world, be almost as wonderful as if a rhinoceros were born of an elephant? And are we quite sure that such a fact has not occurred in Nature? I merely throw out as a hint—not as asserting a truth, or even a probability, but merely as a hint, hypothetically put—that there are two natural orders of plants which have so many indications of common affinity that they were placed near together by Mr. Brown, but which differ from each other nearly by the very same characters as those by which our monstrous *Begonia* differs from its normal parent. The orders I allude to are Aristolochiaceæ and Nepenthaceæ. Aristolochiaceæ, like *Begonia*, has an inferior ovary of 3–6 carpels; Nepenthaceæ, like our monster, a superior ovary of 4 carpels. On theoretic principles, it is probable that Nepenthaceæ is the newest type; for it is not, as yet, generically diversified, its flowers are 4-merous, its embryo more fully organized, and its geographical range more limited; and, as we are supposing, we may further guess that if *Nepenthes* were born "*per saltum*" from an Aristolochioid, it was some such genus as *Trichopodium* or *Asiphonia* that performed the part of cuckoo-parent. I use the term "cuckoo-parent" advisedly, for I should consider such an origin to be as true and as miraculous a creation (not "manufacture") of a new type as if it had pleased the Divine Creator to call up, without seed, from the dust of the ground, a new organism, by the power of his omnipotent word.—*Gardener's Chronicle*, Feb. 25, 1860.

The Monstrous Begonia frigida at Kew, in relation to Mr. Darwin's Theory of Natural Selection. By J. D. HOOKER, M.D.

YOUR ingenious correspondent, Dr. Harvey of Dublin, has noticed this remarkable plant in your last Number, and described the singular modifications of the floral organs as presenting a most decided "saltus." He proceeds to speculate on the importance of this case as affecting Mr. Darwin's theory, and, by what appears to me to be reasoning "*per saltum*," he arrives at the conclusion that "a few such cases would overthrow Mr. Darwin's hypothesis altogether!" Now I venture, on the contrary, to think that the "saltus" of this *Begonia frigida* has not the importance which Dr. Harvey imagines; and



Harvey, W H. 1860. "Mr. Darwin on the origin of species." *The Annals and magazine of natural history; zoology, botany, and geology* 5, 348–350.

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