

Letter from Charles Darwin to Asa Gray, 1856, discussing his developing ideas concerning the origin and mutation of species.

1857
Down Bromley Kent July 20 (1856)

My dear Dr. Gray:

What you say about extinction, in regard to such genre and local disjunction, being hypothetical seems very just. Something direct however, can be advanced on this head from fossil shells; but hypothetical such notions must remain. It is not a little egotistical, but I should like to tell you, (and I do not think I have) how I view my work. Nineteen years (!) ago it occurred to me that whilst attention employed on Natural History I might perhaps do good if I noted any sort of facts bearing on the question on the origin of species; and this I have since been doing. Either species have been independently created, or they have descended from other species, like varieties from one species. I think it can be shown to be probable that man gets his most distinct varieties by preserving such as arise best worth keeping and destroying the others, but I should fill a quire if I were to go on. To be brief I assume that species arise like our domestic varieties with much extinction; and then test this hypothesis by comparison with as many general and pretty well established propositions as I can find made out, in geographic distribution, geological history, affinities etc. etc. etc. And it seems to me, that supposing that such hypothesis were to explain such general propositions, we ought in accordance with common way of following all sciences, to admit it, till some better hypothesis be found out. For to my mind to say the species were created so and so is no scientific explanation but a prescient(ific) way of saying it is so and so. But it is not sensible trying to show how I try to proceed in compass of a note. But as an honest man I must tell you that I have come to the relentless conclusion that there are no such things as independently created species, the species are only strongly defined varieties. I know that this will make you despise me. I do not much under-rate the many huge difficulties on this view, but yet it seems to me to explain too much, otherwise inexplicable, to be false. Just to allude to one point in your last note, viz about species of the same genus generally having a common or continuous area: if they are actual lineal descendents of one species, this of course would be the case; and the sadly too many exceptions (for me) have to be explained by climactic and geological changes. A fortiori on this view (but on exactly same grounds) all the individuals of the same species should have a continuous distribution. On this latter kind of subject I have put a chapter together and Hooker kindly read it over: I thought the exception(s) and difficulties were so great that on the whole the balance weighed against my notions, but I was much pleased to find that it seemed to have considerable weight with Hooker, who said he had never been so

much staggered about the permanence of species. I must say one word more in justification (for I feel sure that your tendency will be to dispare over my contents) that all my notions about how species change are derived from long continued study of the works of (and concern with) agriculturalists and horticulturalists; and I believe I see my way pretty clearly on the means used by nature to change the species and adapt them to the conditions and exquisitely beautiful contingencies to which every living being is exposed.

I thank you much for what you say about variability and crossing of the grasses: I have been often astounded at what Botanists say on fertilization in the bud: I have seen Cincifera mentioned as instances, which every gardener knows how difficult it is to protect from cuping! What you say on Popilionaceous flowers is very true; and I have no facts to show the varieties are cuped; but yet (and the same remark is applicable in a beautiful way to Frumaria and Dielytia as I noticed many years ago) I must believe that the flowers are constructed partly in direct relation to insects' visits; and how insects can avoid bringing pollen from other individuals I cannot understand. It is really pretty to watch the action of a Humble-Bee on the scarlet Kidney Bean, and in this genus (and in Lathrus Grand. flowers) the honey is so placed that the Bee invariably alights on the side of the flower towards which the pistol is pointed (bringing out with it pollen) and by the depression of the wing-petal is forced against the Bees' side all dusted with pollen. In the Broom the pistol is rubbed on centre of back of Bee. I suspect there is something to be made out about the Leguminosae which will bring the case within our theory: though I have failed to do so. For theory will explain why in vegetable and animal Kingdoms the act of fertilization even in hermaphrodites usually takes place sub-jove, though thus exposed to the great injury from damp and rain. In animals in which the semen cannot, like pollen be occasionally carried by insects or wind: there is no case of Land-animals being hermaphrodite without the concourse of two individuals. But my letter has been horribly egotistical: but your letters always so greatly interest me; and what is more they have in simple truth, been of the utmost value to me.

Yours most sincerely and gratefully

C. Darwin



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