

Down Bromley Kent

Sept. 5th

My dear Gray

I forget the exact words which I used in my former letter, but I daresay I said that I thought you would utterly despise me, when I told you what views I had arrived at, which I did because I thought I was bound as an honest man to do so.

I shd have been a strange mortal, seeing how much I owe to your quite extraordinary kindness, if in saying this I had meant to attribute the least bad feeling to you. Permit me to tell you, that before I had ever corresponded with you, Hooker had shown me several of your letters (not of a private nature) & these gave me the warmest feeling of respect to you; & I shd indeed be ungrateful if your letters to me & all I have heard of you, had not strongly enhanced this feeling. But I did not feel in the least sure that when you knew whither I was tending, that you might not think me so wild & foolish in my views (God knows arrived at slowly enough, & I hope conscientiously) that you would think me worth no more notice or assistance. To give one example, the last time I saw my dear old friend Falconer, he attacked me most vigorously, but quite kindly, & told me "you will do more harm than any ten naturalists will do good"—"I can see that you have already corrupted & half-spoiled Hooker"(!!). Now when I see such strong feeling in my oldest friends, you need not wonder that I always expect my views to be received with contempt. But enough & too much of this.—

I thank you most truly for the kind spirit of your last letter. I agree to every word in it; & think I go as far as almost anyone in seeing the grave difficulties against my doctrine. With respect to the extent to which I go, all arguments in favour of my notions fall rapidly away the greater the scope of forms considered. But in animals, embryology leads me to an enormous & frightful range. The facts which kept me longest scientifically orthodox are those of adaptation—the pollen-masses in *Asclepias*—the misseltoe with its pollen carried by insects & seed by Birds the woodpecker with its feet & tail beak & tongue to climb trees & secure insects. To talk of climate or Lamarckian habit producing such adaptations to other organic beings is futile. This difficulty, I believe I have surmounted. As you seem interested in subject, & as it is an immense advantage to me to write to you & to hear ever so briefly, what you think, I will enclose (copied so as to save you trouble in reading) the briefest abstract of my notions on the means by which nature makes her species. Why I think that species have really changed depends on general facts in the affinities, embryology, rudimentary organs, geological history & geographical distribution of organic beings. In regard to my abstract you must take immensely on trust; each paragraph occupying one or two chapters in my Book. You will, perhaps, think it paltry in me, when I ask you not to mention my doctrine; the reason is, if anyone, like the Author of the *Vestiges*, were to hear of them, he might easily work them in, & then I shd have to quote from a work perhaps despised by naturalists & this would greatly injure any chance of my views being received by those alone whose opinion I value.—

I have been lately at work on a point which interests me much; namely dividing the species of several Floras into two as nearly as equal cohorts as possible—one with all those forming large genera, & the other with the small genera. Thus in your U. States Flora, I make (with omissions of naturalised & of a

few protean genera & *Carex* from its unusual size) 1005 sp. in genera of 5 & upwards, & 917 in genera with 4 & downwards; & the large genera have 881000 varieties & the small genera only 501000. This rule seems to be general. & Hooker is going to work out some Floras on same plan.— But to my disgust your vars. marked by big-type are only in proportion 481000 to 461000.

Several things have made me confidently believe that “close” species occurred most frequently in the larger genera; & you may remember that you made me the enclosed list. Now to my utter disgust, I find that the case is somewhat the reverse of what I had so confidently expected, the close species hugging the smaller genera. Hence I have enclosed the list. & beg you kindly to run your eye over it, & see whether, not understanding my motive, you cd have attended more to the small than to the large genera: but I can see that this is not probable. And do not think that I want you to “cook” the results for me.— Are the close species very generally geographical representative species: this might make some difference?

Lately I examined buds of Kidney Bean with pollen shed, but I was led to believe that the pollen cd hardly get on stigma by wind or otherwise, except by Bees visiting & moving the wing petals: hence I included a small bunch of flowers in two Bottles, in everyway treated the same: the flowers in one I daily just momentarily moved as if by a Bee; these set 3 fine pods, the other not one. Of course this little experiment must be tried again, & this year in England it is too late, as the flowers seem now seldom to set. If Bees are necessary to this flower's self-fertilisation, Bees must almost cross them, as their dusted right-side of head & right legs constantly touch the stigma.

I have, also, lately been reobserving daily *Lobelia fulgens*— this in my garden is never visited by insects & never sets seeds, without pollen be put on stigma. (whereas the small blue *Lobelia* is visited by Bees & does set seed); I mention this because these are such beautiful contrivances to prevent the stigma ever getting its own pollen; which seems only explicable on the doctrine of the advantage of crosses.

I forget whether I ever said I had received safely Mr Watson's papers. & your Lesson in Botany, for which very many thanks & which I am now reading. But I have never had the last part of your paper on Naturalised Plants. If you have a spare copy (which is not likely) I shd be very glad of it: otherwise I will borrow Hooker's. I ought to feel ashamed of the length of this letter, knowing how busy you are.

My dear Dr Gray
Believe me with much
sincerity Your's truly
C. Darwin

I will try if I can anyhow get seed of the *Adlumia cirrhosa* & observe it next summer. Perhaps they have it at Kew.



Darwin, Charles. 1857. "Darwin, Charles Sept. 5, 1857 [transcript]." *Charles Darwin letters to Asa Gray*

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