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Edward C. Carter, II Librarian

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Dr. Rose Scott-Moncrieff
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Dear Dr. Rose Scott-Moncrieff:

Some while ago I received from Dr. Joshua Lederberg, president of the Rockefeller University, a copy of your letter to him dated November 20, 1981, together with a note suggesting that, in view of my work on the history of genetics at this Library, I might wish to communicate with you about your work on the inheritance and biochemistry of flower pigments prior to World War II. I am delighted to do so, and to be informed of your paper in the Notes and Records of the Royal Society published last year.

Your work, together with that of the pioneer in the field, Muriel Onslow Wheldale, as well as that of J. B. S. Haldane, is probably better known among American geneticists than you have thought. I recall, as though yesterday, that when I was in my first year as a graduate student at the University of Texas, in 1929, the first seminar I was asked to present to the staff and graduate students of the Department of Zoology was a review of the monograph on the anthocyanins and their inheritance written by Muriel Onslow. It was quite a task for me, as at that time I had a very limited background in biochemistry. I was elated when, after my presentation was over, both J. T. Patterson and H. J. Muller (the latter my major mentor in genetics) complimented me highly on my presentation and analysis of the implications of the work. I was later to summarize those and later findings in my book Genes and the Man, published in 1942, pp. 183-5, where I stated that of 35 genes in some 14 species or genera of plants studied, "in every instance the biochemical action of the gene is a simple affair," adding or subtracting hydroxyl or methyl groups to the molecule, or an organic acid group, or altering the pH of the cell sap. "Perhaps," I continued, "there is here only a single step, or very few, between gene and known biochemical effect." I did not, however, anticipate the "one gene--one enyme" relationship that Beadle and Tatum demonstrated in Neurospora in 1941, even in the form made explicit by Grüneberg in 1938, the "one gene--one primary function" relationship. That insight, as Beadle himself has noted, was implicit in the writings of "Garrod, Scott-Moncrieff, Wright, Haldane, /and/ Wheldale."

The studies on the anthocyanins were also summarized for students in the leading American genetical textbooks of the 1930s, for example, in

Sturtevant and Beadle's Introduction to Genetics (1939), pp. 355-56, and in Sinnott and Dunn's Principles of Genetics (1932; 1939), both of which I used for many years in teaching college courses in genetics. Richard Goldschmidt, in his Physiological Genetics (1938), analyzed what was known about the genetics and biochemistry of flower pigments. Wagner and Mitchell, in their highly esteemed book Genetics and Metabolism (1955), also summarized the subject. True, more recent books seem to jump from Garrod to Beadle and Tatum, but that is the fate, (is it not?) of much substantial work in the history of every scientific field--to be passed over by the younger generations for lack of space in text treatments and because of the exponentially increasing volume of significant scientific work. Only we historians of science are likely, after a time, to dig back into the step-by-step origins of even our most significant concepts as we try to give a true account of the history of such ideas.

One thing I feel I must say, in all candor. It has always surprised me and created some wonder that J. B. S. Haldane, who was by far the best-trained of modern geneticists in biochemistry, came so close to a valid conception of the relation between gene and first evident phenotypic biochemical effect, and yet missed a realization of the gene-enzyme tie. Do you think it was because he was bemused by the apparently equally close relationship between gene and antigen in the immunogenetic studies? or because in the anthocyanin work the effect via pH seemed as significant as the effect upon adding or removing hydroxyl, methyl, or acetyl groups? I have searched rather carefully in both New Paths in Genetics and in Biochemistry of Genetics and failed to discover why he, rather than Beadle and Tatum, wasn't the discoverer of the gene-enzyme sequence. It is all the more disconcerting when one remembers that it was mainly through Haldane's discussions that the contributions of Garrod were kept alive. I should be very grateful to learn whatever you may have to say on this subject.

The History of Genetics Project at this Library is principally an undertaking to index and prepare a guide to the rich collections of papers of deceased geneticists which are in our archives: Dunn, Dobzhansky, Demerec, Davenport, Pearl, Jennings, Caspari, Curt Stern, and others. I have been publishing some interesting matters that turn up as my assistant and I proceed with the more routine task. Last summer, when on the occasion of the joint meeting in London of the Royal Society and the American Philosophical Society I was able to visit Oxford and make the personal acquaintance of the Contemporary Scientific Archives Centre (Professor Margaret Gowing and Mrs. Jeannine Alton) I asked why no attention was being given by them to geneticists or other biological scientists. It seems to be a matter of money and limited staff. Perhaps you might be able to do something to see that the papers of Bateson, Punnett, Darlington, Ford, Penrose, J. Huxley, Haldane, Hogben, Wheldale, and yourself are preserved for study. If the British centre is unable to do so, I think our Library might be of assistance as an archive.

Very sincerely yours,

Bentley Glass

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