Reminiscences of B. O. Dodge and the Beginnings of Neurospora Genetics

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Dr. Dodge had begun the study of Neurospora while he was working in the U. S. D. A. at Washington under the supervision of Dr. C. L. Shear. At that time, Dr. Thom was also working under Shear, on "Monilia sitophila," the imperfect stage of Neurospora. Thom had committed himself deeply in print on the proposition that Monilia had no sexual phase. While Dr. Shear was looking over Thom's cultures, he found a plate containing perithecia. He took it away from Thom and turned it over to a woman working in the Department. But Shear was not satisfied with her progress and finally turned the cultures over to Dodge. Dodge soon established the existence of a perfect stage and proceeded at a great rate with both the cytological and genetical analysis. When he began to talk about "sex" in the ascomycetes, his supervisors felt that his hard work had affected his mind and arranged for a field trip to California, to release him from the tension of his work. Eventually he returned to New York and continued the Neurospora work in close contact with Professor Harper, who had been his senior professor.

Harper was the leading anti-Mendelist at Columbia. Harper's relation with Mendelists, including the Drosophilogists, had not been good. In addition to his spectacular cytological work on the ascomycetes, Harper had presented data on the genetics of maize that were inexplicable on Mendelian theory. These complex aspects of inheritance of pigmentation in maize were eventually clarified and yielded much new light on Mendelian and cytoplasmic heredity. In this atmosphere at Columbia, the Drosophila group was relatively unpopular. When Millikan (who had shown that electricity was particulate) became enamoured with the idea that life, or at least heredity, was likewise particulate, he invited the Morgan group to Cal Tech.

During 1928 we lived in Pasadena where I was convalescing from a long illness. I had enrolled as a part time student in organic chemistry at Cal Tech, hoping to continue my graduate study. When Dr. Morgan and his group arrived to set up their laboratory, I visited Dr. Morgan and told him that I had a master of science degree in plant pathology from the

University of Wisconsin and was interested in biological research. He had some cultures of Neurospora that Dr. Dodge had given him and he was trying to isolate the ascospores on agar petri dishes with dissection needles. In the fly laboratory, the dense contamination of penicillium made this effort very difficult. He gave me several of Dr. Dodge's reprints and asked me to read them and come back to see him later. When I returned, I told him that I understood everything in the reprint except "haplont" which was new to me. He said if I worked with them, the word would soon become clear. He offered me a stipend of $75.00 a month and gave me the cultures to study.

Dr. Morgan was always an economical man and especially with regard to laboratory expenses. I built a micromanipulator in the carpenter shop at Cal Tech for which I needed some metal rods and tubing that cost 43 cents from petty cash. This pleased Dr. Morgan very much.

After I had worked a year or two with the cultures, Dr. Morgan told me that the members of his group felt that there was no place at Cal Tech for this project, but he would be glad to recommend me to some other institution where I could continue the work under more favorable circumstances. We were in the process of buying a house in Pasadena and my health had not been too good, so Jerry (Gertrude) asked me to persuade Dr. Morgan to keep me on and suggested that I give a seminar on the work thus far accomplished. He agreed and I reported on the constancy of the ratio of first to second division segregation of the mating type markers. I showed that constancy was independent of the segregations occurring in the parental clones. The pedigrees were extensive and the staff was favorably impressed. Eventually E. G. Anderson suggested that the markers were relatively near a centromere.

In the summer of 1930, Dr. Morgan suggested that I spend a few months in New York studying with Dr. Dodge. He gave me $300 for my expenses and Dr. Beadle told me subsequently that he strongly suspected Dr. Morgan had taken the money from his own pocket.

Dr. Dodge was very pleased to have me come and told me about giving Dr. Morgan the cultures and demonstrating his technique of isolating ascospores. He was pleased with Dr. Morgan's enthusiasm for the subject and glad to help me. He spent a great deal of time showing his cytological work and I struggled for several years later with the triple stain. Dr. Dodge was always helpful and pleasant, but I had great difficulty in convincing him that crossing-over explained second-division segregation. I published the first papers on this subject in the Bulletin of the Torrey Botanical Club that he edited in 1932, but he did not accept the idea until 1939 when he gave a lecture to the Botanical Society in New York on the subject.

I remember one thing that has always impressed me and made me envious of Dodge. When he finished a manuscript and had it published, he burned all the data and made a bonfire of all of his notebooks.

I gave a seminar at Columbia on the work that I had done on Neurospora and was pleased with the friendly reaction of Harper. Dodge was very pleased with my work and especially flattered by the interest of Morgan and the Cal Tech group.

I returned to Cal Tech and continued to work on crossing-over and demonstrated to my satisfaction that it was non-random with two-strand doubles exceeding either threes or fours. (This inference has been abundantly confirmed in the study of yeast genetics.) At this time Beadle and Emerson, as well as Mrs. Morgan, were deeply involved in the study of four-strand crossing-over using attached X's. I insisted that their data were faulty because the distances were too great and the results had been randomized by undetected multiple cross overs. In Sturtevant and Beadle's book, dealing almost exclusively with 4-strand crossing-over, they suggest that the Neurospora data were faulty because the markers were too difficult to read.

I was often surprised to discover, when I demonstrated the segregation in Neurospora asci to Drosophila people, who were so experienced in detecting genetic markers in their material, to find that they could not distinguish differences in the mycelia growth that were obvious to me. So they may be excused for thinking that our markers were inadequate.

I worked about six years on Neurospora at USC. We then spent one year with Lewis Stadler at the University of Missouri. He was interested in direct information on the effect of radiation on cells as opposed to statistical information on this subject. He also wanted to know about the possibility of studying the genetics of smut since smut is a common corn disease. To get direct information on radiation, we planted mononucleate microconidia on agar, radiated them and transferred them to nutrient media. The results were quite interesting and we produced a great variety of mutants. This was reported in the Journal of Heredity. While we were with Stadler, Beadle wrote of his interest in Neurospora and we sent him cultures. A few years later, when we were at Washington University studying yeast genetics, Ed Tatum spent some time with us and showed us the technique for using biochemical markers and this help expanded our mapping program.

During the years that we worked on Neurospora at USC and CT and the subsequent long period that we spent on yeast genetics, Jerry worked as a full time collaborator and did all the microdissection and all the biochemical discrimination. Our records in the yeast work show more than 100 thousand spores were isolated and analyzed and over 100 thousand spores of Neurospora were dissected and tested. All this work was done without any compensation, which is some kind of a record for this day and age.