a master's candidate. Creighton was agreeable, ready to take McClintock's advice.

Had McClintock singled her out because she was a woman? "It could have had to do with being a woman," according to Harriet Creighton; "I don't know. We didn't think so much about it in those days, or at least didn't verbalize it." More to the point, she thought, was that McClintock had already begun to anticipate leaving Cornell and was looking for someone to groom as Sharp's next assistant. She recalled that at the time there was an active organization of graduate women in science—Sigma Delta Epsilon—and that McClintock had urged her to join, even though she herself seems not to have been a member.

Sigma Delta Epsilon, later to grow into a nationwide organization, began at Cornell in the early 1920s with about forty members at any one time. "Everyone joined." At first, its ambition was to provide a living center for the graduate women in science, but, by Harriet Creighton's time, they had abandoned the idea of maintaining a residential house. The group continued to serve as a kind of social and intellectual community, meeting now and then for dinners. At the very least, it provided an opportunity to get to know women from other disciplines. But for most of them, the principal focus of intellectual and social life was the lab, and Creighton learned more about being a woman in science from looking around her own laboratory than she did from the meetings of Sigma Delta Epsilon. She could see that Ezra Cornell's liberal vision of founding a university where "any student could receive instruction in any subject" did not extend to the faculty level. There, for example, was Miss Minns (Lua A. Minns, Department of Horticulture), who, though held in high regard, in her fifties was still an instructor. Seeing Miss Minns made an impression on Creighton that would loom large over the next few years as she sorted out what she would do.

In the meantime, the main business at hand was the learning of science itself. The subtle and difficult techniques of cytological analysis required a great deal of attention. But Creighton found that she was also learning a technique that may have been even more valuable; she was learning a method of following McClintock's discourse—which, even then, she recalls, many found dense and "hard to follow." Her discovery was that what sometimes seemed like non sequiturs in McClintock's line of thought were in fact a response "to the question you should have been asking at the moment"; she was addressing the doubt "you should have had." This discovery served Creighton well; it helped her, too, in building the confidence she felt was necessary to deal with the high standards McClintock imposed. "She was very quick to see things, and someone who wasn't quick had a hard time."

Toward the end of that year, in the spring of 1930, McClintock suggested a problem for Creighton to work on. She thought that it ought to be possible, by using the corn stocks that displayed the deep-staining knob she had observed on chromosome 9, to finally establish the correlation between genetic and chromosomal crossover that geneticists took for granted, but that had not been proven. (Genetic crossover is observed when an organism combines the traits of both its parents corresponding to two genes that are normally linked. "Linked" genes are normally inherited together and hence assumed to be on the same chromosome. It had been assumed by most geneticists that the physical basis of this event was an actual physical crossing over of segments of the paired chromosomes in question, resulting in the inheritance of a chromosome that derives partly from one parent and partly from the other.)

McClintock had already determined the location of a particular group of linked genes on the same chromosome and was in the midst of working out the morphology of additional cytological markers. All that was needed were two cytological markers on the same chromosome, located near two distinct genetic markers. By simultaneously following these two sets of markers through genetic crosses (matings) with plants that did not have these markers, it would be a simple matter to resolve whether or not both kinds of crossover take place concurrently. Creighton's initiation into the art of corn genetics began with the seeds (or kernels) that displayed the genetic and cytological markers that McClintock had isolated.
The life cycle of corn. Mating in corn occurs midway through the growing season, when the green plant is large and fully developed, but the ears just barely formed. The pollen matures in the tassel at the top of the stalk, while the embryo sacs are buried in the ears—one embryo sac in each future kernel. Pollination, normally subject to the vicissitudes of wind and air, takes place when the pollen grain falls from the tassel and settles on a silk extending out from the embryo sac to the outer end of the young ear. In order to control mating, extreme care must be taken to ensure that the only pollen that comes into contact with the female flower is pollen from the designated paternal plant. This is normally accomplished by tying a paper bag over the ear shoot before the silks appear. The pollen to be used in the cross is collected by enclosing the appropriate tassel in a different paper bag. Fertilization takes place when the pollen grain (containing two sperm nuclei) comes into contact with the silk, whereupon it sends out a long tube that grows down through the silk into the embryo sac. One sperm nucleus fuses with the egg nucleus, and the other (identical to the first) fuses with the two polar nuclei (genetically identical to the egg) in the embryo sac. This fused nucleus (containing three sets of chromosomes, 3n, one paternal and two maternal) gives rise, through subsequent mitotic divisions, to the nutritive tissue that surrounds the embryo and comprises the endosperm, the bulk of the mature kernel. Both embryo and endosperm carry the same genetic alleles; both are products of the same fertilization. The great advantage of corn genetics is that the endosperm matures with the parent plant, thereby providing the investigator with a preview of the genetic character of the plant that will grow from the embryo in the following season. Cytological examination can be performed both on the endosperm tissue and, later, on the tissues of the new plant.
It was his policy, she recalls, to give a new student “the best and most promising problem you have.” Young Creighton herself hardly realized its significance and, by Rhoades’s recollection, required constant prodding from McClintock to get it done. It was not until well into the summer that it dawned on her that no one had actually solved this problem before, that it was not just an educational exercise but a substantial contribution to the field. To this day, McClintock shrugs it off—“It was such an obvious thing to do”—but she also acknowledges that it needed to be done.

Without question, it was a timely piece of work. Across the Atlantic, Curt Stem’s work with Drosophila was progressing to the point where he, too, could anticipate doing the experiment that would put “the final link in the chain” of classical genetics. Microscopic examination of the meiotic stages of Drosophila had, throughout the 1920s, continued to prove intractable. Finally, Stem was able to delineate cytological markers that were sufficiently clear to follow through the process of crossing over. The experiment that would also establish the correlation between genetic and chromosomal crossover in Drosophila was well under way. Stem would surely, according to Creighton, have beaten them to publication if T. H. Morgan had not intervened. During the spring of 1931, Morgan came to Cornell to deliver the annual Messinger lectures. Once the lecture series was over, the great geneticist emerged from seclusion and made the rounds of the laboratories; he wanted to know what everyone was doing. When he came to the little office that Creighton and McClintock were then sharing, Creighton told Morgan of her project and showed him the preliminary results they had from the past summer’s crop. Immediately, he asked if they had yet written this up for publication. No, they were waiting for the next crop of corn to confirm their initial data. Morgan demurred. He thought they had quite enough already; they should publish their results now. Overriding everyone’s hesitations (Sharp, for example, pointed out that this was Creighton’s Ph.D. dissertation, and she had three years of residency to fulfill), Morgan asked for pen and paper. On the spot, he wrote a letter to the editor of the Proceedings of the National Academy of Sciences telling him to expect the article in two weeks. The paper arrived on July 7 and appeared in August 1931.

Curt Stern, whose parallel work was by then well on its way, was scooped. His paper was based on more extensive data, but it did not appear for several more months. Stern was visibly perturbed. Late in his life, Stern reminisced about the day he publicly presented his work:

I gave my paper with the enthusiasm of a successful youth. Soon after, one of my colleagues from the Kaiser Wilhelm Institute came to me and said: “I didn’t want to spoil your fun but while you were on vacation a paper came out written by Harriet Creighton and Barbara McClintock who did experiments in maize equivalent to what you just announced as unique.” May I confess... that I am still grateful to my colleague for permitting me the feeling of triumph for half an hour longer than I would have had it if he had told me about the Creighton-McClintock paper before my talk.

According to Creighton, Morgan later confessed that he had known about Stern’s work at the time. But, as he explained (about a year after his intervention), he was also aware of the fact that, even though Creighton and McClintock had begun the summer before, it would have been a simple matter for Stern to overtake them. With Drosophila, one need not wait an entire growing season to learn the results of genetic crosses; one can get a new generation every ten days. Creighton recalls Morgan’s saying, “I thought it was about time that corn got a chance to beat Drosophila!”

All of the principal characters of this story had a chance to meet the following summer when the Sixth International Congress of Genetics convened in Ithaca, New York. It had been five years since the last occasion on which the Congress, with 836 members from thirty-six countries, had had the opportu-
nity to meet at all, and thirty years since it had met in the United States. The meeting opened on August 24, 1932, with 536 geneticists registered (many of the European delegates had been unable to attend). All the luminaries of the field were present. T. H. Morgan was President, and Rollins Emerson Vice President. Richard B. Goldschmidt, Director of the Kaiser Wilhelm Institute in Berlin, acted as spokesman for the European contingent.

In his opening address, Morgan undertook to review the history of genetics and to assess the current state of the art. By way of summary, he concluded with a list of the five most important problems for geneticists in the immediate future. First was an understanding of "the physical and physiological processes involved in the growth of genes and their duplication"; second, "an interpretation in physical terms of the changes that take place during and after the conjugation of the chromosomes"; third, "the relation of genes to characters"; fourth, "the nature of the mutation process"; and fifth, "the application of genetics to horticulture and to animal husbandry."

General papers were given in the morning. They focused primarily on the areas of Mendelianism, evolution, chromosome mechanics, and mutation. In one of these, Emerson reported on the current status of maize genetics; in this he referred extensively to the work of McClintock on the determination of the chromosomal location of known genetic linkage groups. Other aspects of her cytological investigations were reported in papers given by H. J. Muller and Lewis Stadler, and the Creighton and McClintock work received special mention in the papers of Karl Sax and (of course) Curt Stern. Stern had been invited to review the genetics and cytology of crossing over (which he did in German). The afternoons were devoted to sectional meetings, running five or six at a time, and covering a somewhat broader range of issues. McClintock delivered a paper on the occasional pairing of nonhomologous parts of chromosomes—a subject on which she would subsequently elaborate further—and served as Vice Chairman of another of the afternoon sections. In addition, she and Creighton prepared an exhibit illustrating their cytological evidence for four-strand crossover.

A group photograph taken at the meeting shows McClintock and Creighton in the second row on the far right—two out of seventy women in a group of 389 attendees. Her colleagues of that time describe this as probably the high point of Harriet Creighton’s career as a research scientist. Two years later she would leave Cornell for a teaching job at a women’s college. For Barbara McClintock it was only the beginning.

After the Congress was over, a chance and especially felicitous encounter occurred on a transatlantic steamship. Rhoades remembers McClintock’s great delight and pleasure when she heard about it. It seems that Dr. and Mrs. McClintock were embarked for a holiday in Europe when they struck up a conversation with one F. A. E. Crew, a Scottish geneticist who was on his way home from the Congress. When it emerged that these were Barbara McClintock’s parents, Crew had the opportunity of being the first to inform them of their daughter’s great scientific success. In a moment, years of misgiving, disapproval, and worry about their youngest daughter’s “odd” choices gave way to pride.