6 Generational Divides: Rosalyn Sussman Yalow, Evelyn Fox Keller, Barbara McClintock, and Feminism after 1963

> In a science constructed around the naming of object (nature) as female and the parallel naming of subject (mind) as male, any scientist who happens to be a woman is confronted with an a priori contradiction in terms. This poses a critical problem of identity: any scientist who is not a man walks a path bounded on one side by inauthenticity and on the other by subversion. Just as surely as inauthenticity is the cost a woman suffers by joining men in misogynist jokes, so it is, equally, the cost suffered by a woman who identifies with an image of the scientist modeled on the patriarchal husband. Only if she undergoes a radical disidentification from self can she share masculine pleasure in mastering a nature cast in the image of woman as passive, inert, and blind.
>
> —Evelyn Fox Keller, *Reflections on Gender and Science*, 1985[1]

THERE WAS NO GREATER COMPLIMENT THAN THE ONE A JOURNALIST paid Rosalyn Sussman Yalow when she dubbed her "a Madam Curie from the Bronx" in 1978. After she became famous, Yalow hosted a Public Broadcasting Service series on the French scientist, for one of her greatest inspirations had been a biography written by Curie's daughter that she read as she embarked on her academic career in 1937. "For me," she reflected, "the most important part of the book was that, in spite of early rejection, she succeeded. It was in common with my background, with my being aggressive." Yalow also would see herself as a mother, a physicist, and a come-from-behind figure, one who, as a woman, had to work harder and longer to come out ahead. In graduate school she was inspired yet again by the film adaptation of Eve Curie's book: "Till my dying day, I will remember Greer Garson and Walter Pidgeon . . . coming back to the laboratory and seeing the glowing that meant they had discovered radioactivity." Yalow's later discovery of radioimmunoassay (RIA) played out in her mind in the same romantic way; depictions of Curie's life and work had been her template as well as her lens for viewing herself.[2]

Rosalyn Sussman Yalow, too, was the poor girl who made good, and had a Nobel Prize to prove it. "No one could have deflected me from my path," she later told the Nobel Foundation. Like Curie, she didn't sulk over hardship but tried harder, skipping grades until she proved she deserved scholarships to study science. She learned to read before kindergarten. When math anxiety set in for other girls, she had already decided to pursue calculus and chemistry. She came from a working-class family and contributed to the family wage by cutting out patterns for her uncle's necktie business. When there were no books in the house, she borrowed from the public library; when technical classes weren't offered at school, she sought them out elsewhere. Her parents did not have a high school education, but she never considered not going to college. Decades later she transformed her personal story into a parable about becoming the agent of one's destiny through hard work, not excuses or handouts. A sign hung prominently in her office that read: "Whatever women do, they must do twice as well as men to be thought half as good." This wasn't a complaint, but an assertion; she excelled because she had always been up to the challenge.[3]

Like other gifted daughters of New York immigrants, Rosalyn Sussman made grades that were good enough for her to enter Hunter College, where in those days tuition was free. She slipped into a lecture hall to hear guest Enrico Fermi speak about radioisotopes in 1939 and felt awed. To an ambitious girl from the Bronx, physics seemed the route to excitement and prestige, and thanks to her dogged persistence, administrators created a physics major. Sussman was the first to complete the program in 1941, but faculty members made no calls to graduate schools on her behalf. Acting on advice, she took stenography courses and a secretarial job at Columbia University's College of Physicians and Surgeons, hoping to gain access to the college labs. Then she accepted a teaching assistantship in the physics department at the University of Illinois, probably once reserved for a man who had gone to war. There were no women's restrooms in the lab buildings, and Jews were not allowed to live in on-campus housing. When Sussman appeared on campus in September 1941, she was a homeless woman among four hundred men, but she was completely undeterred.[4]

At the Sorbonne Marie Curie had met Pierre. At Illinois Rosalyn Sussman met Aaron Yalow, the son of a rabbi and also a graduate student entering the physics group. Like Pierre, Aaron didn't have the competi-

tive spirit of his wife, and friends and colleagues viewed him as emasculated in her hands. The Yalows' son believed that his dad stepped aside to let his mom's career blossom because he worshipped the ground that she walked on, but Rosalyn claimed that Aaron's Orthodox Judaism was to blame for his less illustrious career. She had no qualms about working on the Sabbath and High Holidays if that was what it took to get ahead. Nothing was going to derail her. At Illinois she persevered, seemingly impervious to criticism. When she got an A- in her optics lab, the physics chair decided that the minus was proof that women were no good at bench work. Yalow ignored him and moved on. She had a heavier teaching load than her peers and after 1943 took on the added burdens of keeping house and a kosher kitchen at the peak of wartime rationing. She and Aaron had finally married, delayed only by university nepotism policies.

Yalow had no intention of letting marriage interfere with her plans. She collected her PhD in nuclear physics in 1945 and took a job as an assistant engineer at the Federal Telecommunications Laboratory. Despite the shortage of technically trained men, she was the only woman engineer at the company, and she was replaced when qualified men returned from war. She returned to Hunter to teach physics, moonlighted in labs without pay to keep her bench skills sharp, and never doubted that her state of limbo was temporary: "I, as a small child, made up my mind that people and institutions were going to need me, and I was going to let them know." One can already detect the swagger some admired and others detested when she won the Nobel Prize thirty years later. Her determination was essential in a field as grim as postwar physics for women.[5]

Aaron introduced his wife to Edith Quimby, a pioneer in the medical use of radioisotopes; that Quimby was also a mother of four cast her further into the Curie mold. Yalow thrived in Quimby's lab, if only as a volunteer. She was put in touch with the director of radiotherapy at the Bronx Veterans Administration Hospital, who offered her a part-time consulting position in a new radioisotope service. Yalow showed her moxie when she took over the janitorial closet and built radiation-detection instruments from scratch. By 1950 she had dropped all teaching to work at the VA hospital full time.[6]

Yalow's dream of rearing children had been postponed but not extinguished. If her goal seems incompatible with her professional ambi-

tions, one may see it as a response to the "old maid" professors at Hunter who expected bookish girls to become schoolmarms and low-grade spinster scientists. Yalow wanted to be an exception, even as she, like most educated women, felt postwar pressures to keep the home fires burning. She couldn't ignore the opportunities that wartime technologies had opened up for scientists with her skills and ambitions, and thus she vowed to do it all at once.

Like her French idol, she bore two children during a frenetic period in her career—Benjamin in 1952 and Elanna in 1954. The policy of the VA hospital called for women to resign once they reached their fifth month of pregnancy. The veterinarian in the animal lab left on these grounds, yet no one dared to approach Yalow. "I was too important," she figured. "I wasn't concerned because they needed me." She worked until the day before she delivered her son and returned a week later. When she tried to do the same with Elanna, doctors forced her to stay in the hospital for eight days. On the ninth, she gave a scientific paper in Washington.[7]

Her goal was to make the transition between home and work life so seamless that it didn't look like a goal at all. The Yalows reared their children less than a mile from her lab, making it easier for her to get to the office early in the morning and to come home to prepare the children's lunch. After tucking them into bed she often returned to the hospital for a late night, only to do the same the next day. Curie's anti-natural path didn't always account for eating, and Yalow's didn't allow for sleeping. As babies her children slept by day and fed in the wee hours of the night when she was home to nurse. Although her mother and domestic workers helped her, she cooked, cleaned, and typed school papers in years when her male colleagues assumed few responsibilities at home. A seventy-hour workweek was standard, eighty to a hundred hours never out of the question; and yet the children had their vacations, their birthday parties, and their three square meals. When she had to work weekends she brought them to the lab to play with the animals—and what kids don't love animals?[8]

Yalow called her experiment in balancing work and family a huge success, but her adult children didn't always agree. Elanna believed that "emotion was lacking" and that her mother was less self-sacrificing than self-absorbed, her affection conditional, when in evidence at all. She wasn't a mother who soothed hurt feelings; she was terrible when con-

fronted. Assertiveness was necessary in Yalow's profession, but Elanna hated that, in her mother's case, it came with blinders to her other faults. In theory, having a Nobel-winning scientist for a mother creates incentive for a child to achieve. For Elanna, it confirmed only that she couldn't compete. She sympathized with Eve Curie, the "artistic" child, who felt like an outsider when the family talked physics at the dinner table. Vying for a high-powered mother's attention was futile, and so, rather than study science, Elanna studied children and wished to have her own. Maria Mayer's daughter, Marianne, reacted to her mother's science much the same way: she married a scientist but swore off working outside the home. "Mother missed a lot of fun when we were small," she later reflected. "I won't want a housekeeper to hear the excited confidences of my children when they come home from school." Ben Yalow had science in common with his mother, and yet he still found it hard to get her to take notice of him: "She was, for as long as I can remember, driven by her career. . . . She essentially had two families: there was the one she had married into and raised, the biological and marriage family, and there was the scientific family. . . In the day to day aspects it was clear that the scientific one was the one in which she spent the predominant fraction of her time."[9]

Young Irene Curie once admitted to resenting radium, Marie's "other baby." Yalow's kids also competed with a discovery their mother referred to with similar affection in her Nobel speech. The lab was Yalow's nursery and a home away from home. She took special pride in nurturing her postdocs until they left the nest and made names for themselves elsewhere. Former students recalled her acting like mom in the lab, running experiments and doing household chores simultaneously. Upon winning the Lasker in 1976, she whipped up potato salad and roasted a turkey for all the staff. "Anyone who can run a laboratory should have no trouble managing a house," she insisted. It was essential that science and domesticity look compatible—no, analogous—if she were going to be viewed as a success.[10]

We can only imagine what Aaron Yalow was feeling as his wife "kept house" in the lab. He took a teaching position at Cooper Union and assumed more responsibilities at home, even as his wife claimed the domestic work as hers. He also accepted that she spent more of her time with another man, Sol Berson, a charming Renaissance type, whom Yalow referred to as the smartest person she had ever known. He was

a resident in internal medicine at the VA hospital in 1950, when she sought him out for his clinical experience. For most of the following twenty years, they worked together day and night, desks facing, closed off in an office they shared in Building C of the hospital. Colleagues described their collaboration as a conventional marriage: they grew so accustomed to each other's thoughts that they communicated wordlessly. Miriam Berson was cordial but couldn't help but feel threatened; Yalow did little to allay her anxieties, seeming to be guilty of emotional adultery, if nothing else.[11]

Marie and Pierre Curie have been passed down to posterity as an utterly complementary couple —he as the thinker and she as the doer, "the muscle" of the pair; only in the context of an intellectual relationship is the "muscle" a mark signifying lower status. Colleagues made similar observations about Berson and Yalow. She understood the physics, he the physiology. She was deliberate and logical, he prone to "flashes of insight." She did the bench work, he the public relations, cultivating contacts with professional societies and journal editors. Colleagues assumed that Berson was the "thinker" because he represented their work eloquently at meetings and in papers. Despite her background in theoretical physics, Yalow was perceived as the experimentalist and subordinate. Although she had initiated their working relationship, it soon became clear who wore the proverbial pants. At professional conferences she accepted the mantra that women are seen, not heard: Berson represented their work and fielded questions during the Q and A, and she grudgingly mingled with medical wives. "I was living in the medical world, and I didn't have a medical degree," she rationalized. But her acceptance of the helpmeet position didn't stop at conferences. She made his lunch, poured his coffee, and organized his travel itineraries. Like a personal secretary, she made sure his manuscripts were typed and mailed. At home, Yalow never conceded an argument, but in the lab, Berson was always right.[12]

In Berson's shadow, she may have appeared the less competent scientist, but no one denied that she was one-half of an extraordinarily productive team. Together they introduced radioactive tracers into the bloodstream and detected infinitesimal amounts of antigen (often hormone) in the blood by measuring its ability to displace the binding of radioactively labeled antigen to its antibody. The technique effectively allowed them to tag red blood cells, electrolytes, iodine, and albumin to

study the distribution of globins, blood volume, serum proteins, bone and muscle metabolism, and thyroid function. By 1959 their RIA provided a tool with which to screen for diabetes and, soon, also hepatitis B and thyroid disease. It was fast, inexpensive, and ripe for any number of applications, since it measured hundreds of biological substances. When it came time to patent, Yalow followed Curie's lead, making RIA accessible rather than profitable. "Too much money is disruptive," she resolved. If altruism had been part of Curie's legend, then she, too, would develop science for science's sake.[13]

All went well until Berson died unexpectedly in 1972. Although he had moved on to become the head of medicine at Mount Sinai Hospital, Yalow still considered him her collaborator and better half. She had grown dependent on him in ways that left her compromised when he was gone. Many people wondered how she'd get on without his skills, connections, and ability to win over critics. Like her French idol after the death of Pierre, Yalow appeared the helpmeet rather than the intellectual spark of a research partnership. Most agreed that RIA deserved a Nobel Prize, but Berson's death confused matters: since a posthumous prize for Berson was impossible, would she, too, be out of the running? Yalow thought about going to medical school to get more credentials but chose simply to work harder and longer, often a hundred hours a week, to earn recognition on her merits alone. She assumed the editing and lecturing that she had once left to Berson. By 1976, some sixty papers had come out of her lab; the fruit of her labor was a Lasker Award and membership in the National Academy of Science.[14]

In 1977, when she received the early morning call announcing her Nobel selection, she had already been at work for several hours and had seen that champagne was on hand at the VA hospital. If privately she had doubted she would win, she knew she would have had fewer doubts had Berson been alive awaiting the news with her. At the Nobel festivities journalists had little to say about the outward appearance of the male laureates, all in standard black and white; yet they dedicated whole columns to Yalow's choice of a brocade vest over a blue chiffon dress. The king of Sweden escorted her to the royal banquet as Aaron Yalow followed her, arm in arm with the queen. Yalow had been selected to give the address to university students, but the usher inadvertently compromised her moment of grandeur by fetching the wrong "Dr. Yalow" to take the podium. Aaron gestured to his wife and the error

seemed quickly forgotten, but the innocence of the mistake made the moment a poignant one. Had Yalow's selection for the Nobel Prize silenced doubt that she deserved it? Had it changed the mind of men and women, boys and girls, who unconsciously assumed that great minds would always be male?

This was no time for doubt; Yalow was sure that her selection to speak was meant to inspire. This was the same year that U.S. congresswoman Bella Abzug convened the National Women's Conference in Houston to discuss glass ceilings and the Equal Rights Amendment. One month latert, it seemed fitting for Yalow to invite three female students to the Nobel ceremony—one from Hunter College, one from her high school, and one from her junior high—and to speak to her audience about sexism in science. Indeed sexism was evident, she told them, but it was an obstacle to rise above, not a problem to reform. Afterward some feminists seized on the fact that she used the platform of Nobel to make a statement about women in science. Others saw clear limits to her message; she spoke of "easing the path" for women, but not changing the path or the scientific community to which it led.[15]

The following year Yalow turned down the *Ladies' Home Journal* Woman of the Year Award because it highlighted distinctions between men and women, when excellence, as she viewed it, was in no way related to sex. After winning her Nobel Yalow grew vocal in her disdain for women's awards and affirmative action policies, which she viewed as official admissions of women's inadequacy. In 1981 she weighed in on the side of a white man who sued a medical school for reverse discrimination. He should be angry, she insisted: what's noble about women and minorities winning competitive spots regardless of their credentials? She preferred to be judged alongside the best men in the room and believed that equal consideration regardless of sex was possible. As president of the Endocrine Society she lambasted younger women who caucused within the organization: "It bothers me," she told a junior colleague, "that there are now organizations for women in science, which means they think they have to be treated differently from the men. I don't approve." Younger women told her to wake up and smell the sexism, to which she replied, "Personally, I have not been terribly bothered by it. . . . If I wasn't going to do it one way, I'd manage to do it another way." The younger physicist Mildred Dresselhaus tried to understand that Yalow's attitude was born of lived experience, not antifeminist

sentiment: "Ros was one of those visionaries who went around saying that for women to be in physics they had to be smarter than men. That shows she recognized barriers."[16]

Yalow's harsh demeanor may have been her best protection from failure, but it did not endear men or women to her. After she won her Nobel Prize colleagues thought her "arrogant," "belligerent," "awesomely full of herself." She wore a replica of her medal dangling from her neck, a constant reminder that she thought herself better than others. She had reached the pinnacle of scientific accomplishment and boasted that she had done so with "help from men, not women." If she could commiserate with less fortunate women scientists, she wouldn't admit it, for she saw herself as a loner with whom no one could compete or compare.[17] Earlier in her career when Berson was around, she had mentored women and mothered young associates, but on her own she was known to be solitary. Her intensity was of a kind that Anne Roe once heralded as the hallmark of the heroic scientist, but a generation later this scientist seemed individualistic at the expense of fruitful collaboration. Of course one may also speculate about how much these negative views emerged from those envious of her reputation or disappointed about their own limited outcomes. As a woman, had she accepted her honors gratefully or gracefully enough? Curie had fallen prey to similar judgments.

Physicists in the Age of Women's Liberation

To younger women, Rosalyn Yalow was a contradiction: a model of masculine single-mindedness and also of glorified maternity; a scientific hero and, still, a woman who had tried to balance family and career. Hers is a story of rare triumph, but also inevitable sacrifice, even if she refused to acknowledge it. It's understandable that she thought her goals reachable only as a woman who transcended expectations of domestic women and scientific men at the same time. Ever since Curie's visit to the United States in 1920, female scientists had viewed the French legend as telling them to be more intense than men in the lab and unflappably feminine outside it. A variation on this image was that of the "superwoman" in the 1970s. She occupied all professional sectors, but she was particularly salient in science, thanks to sociological studies claiming that married women, if hardworking and organized, could

be successful mothers and publishers of scientific research.[18] Yalow believed that women could have it all, and she scripted her own story as a tale of success achieved by working harder and being more focused—by taking Curie's anti-natural path.

If her contemporaries didn't choose similar paths, they often assumed her intensity and accepted her views about affirmative action and masculine science. Joan Freeman, a nuclear physicist who trained at Cambridge in the 1940s, tells a story much like Yalow's of her rise in science: both of them struggled for access to the technical training that was the birthright of boys, but this made them stronger. They watched female colleagues with less skill and determination falter around them. Yalow saw pregnant lab techs get fired, but not her; Freeman saw women deny marriages so they wouldn't lose their jobs, but she didn't. They both recalled moments in their careers when they were hard pressed to find women's bathrooms near their labs, and they remembered when they earned a fraction of the salaries of their male peers. Still, they insisted that sexism was not a factor in their careers.[19] They had internalized a masculine conception of competence; to suggest that science should be anything other than the enterprise that they had internalized was to threaten their very being.

Maria Mayer was also characteristic of this generation. Although she had suffered hardship in the androcentric atmosphere of postwar science, she had also succeeded in it and ultimately found it legitimizing. Even as she became a role model for younger scientists who embraced change, Mayer felt ambivalent about the change she unwittingly helped to create. After she had won the Nobel, the Women in Science program of the American Institute of Physics asked her to speak to teenage girls and college women about science careers. The most important piece of advice she had for her audiences was not about math or shop class; she told them that the key was to marry the right man—preferably a scientist in his own right—who would understand how it felt to be impassioned by science and thus would be accommodating of a wife's career.[20] Her advice can be read in any number of ways: Was she saying that marriage and motherhood shouldn't stop women from pursuing science? Or was she urging future scientists not to let science compromise a traditional home life? She also seemed to be suggesting that a woman's success lay in the hands of a supportive man rather than in her own efforts, that she needed his connections and permission to proceed. As much as

the image of the career woman increasingly appealed to young women, she understood its limits. In 1964 Alice Rossi confirmed the dichotomy in the minds of female college graduates about their academic futures: Four out of five women she surveyed admired women such as Mayer who received prestigious scientific awards, but few of them aspired to similar goals for themselves.[21] Mayer sensed their anxiety and refused to speak of her career as a choice made in sacrifice of other things. In reality, we know that her sacrifices were at times very painful.

Amid nascent debates about women's work and middle-class domesticity in the early 1960s, the wide coverage of Mayer in the print media suggests that Americans were ready to embrace women's accomplishments in science and in careers generally. This was also true when the press covered Curie in the 1920s, though not so twenty years later, when the popular treatment of women scientists seemed to suggest that women who "indulged" in labwork were selfishly shirking their maternal duties. When Gerty Cori won her Nobel Prize in 1947, she was compelled to shield herself from interviews of any kind.[22] Twenty years before and twenty years later, journalists heralded the accomplishments of Curie and Mayer, but also defended their womanliness. While male winners of the Nobel were called "Dr.," these women were referred to as "Mrs.," perhaps to assure audiences that their important work was done in the kitchen, not in the lab. A journalist covering Mayer's trip to Stockholm in December 1963 assured readers that, like any housewife, Mayer spent the days before and after the Nobel ceremony getting ready for the Christmas holidays. While male recipients tended to official business, Mayer disappeared "to do a little shopping."[23] A writer for *McCall's* also reassured readers that Mayer, an "intruder in masculine territory," was first and foremost a gentle woman:

> In Professor Mayer, a tiny, shy, touchingly devoted wife and mother, who speaks so softly she can barely be heard, science and femininity have achieved an astonishingly graceful union. Last winter, at fifty-seven, she received the highest honor that the man's world of atomic-age science can bestow. But on her spectacular night of professional triumph in Stockholm, when the glittering Nobel medal became hers, Maria saw everything through the starry eyes of a romantic woman: "It was a fairy tale," she says. "The King of Sweden gave me his arm after the ceremony. And my husband, Joe, looked enchanting in his white tie and tails—he had borrowed the trousers from our son." Now, months later, the magic of it still brings a special light to Maria's bright blue eyes.

Mayer's winning the Nobel had been transformed into a "Cinderella story in science," in which she was a domestic heroine—someone who gave dinner parties for which menus and crystal were "lovingly planned." Her children were her greatest joy . . . she loved growing flowers . . . she was mesmerized by the decorative accents of the royals' home . . . her favorite word was "elegant!" Though she "walk[ed] with the giants of science, she [wa]s also a sweet, diffident and an utterly feminine woman."[24]

Upon men's discoveries, we hear of pats on the back or shouts of "Eureka," the end results of masculine contest and exploration. The terms with which the press has conveyed female discovery have been no less stereotypical. Marie Curie, we were told, stood entranced over her glowing radium as a mother would stand over a sleeping child. In 1963 journalists seized on language that they claimed Mayer used to describe her shell theory to her teen-aged daughter: "Think of couples waltzing around a room," she allegedly told her. "They spin as individual couples as they also orbit the ballroom. Some couples spin in one direction, and some the other, just like the electrons that orbit a nucleus—and everybody who has danced a fast waltz knows that it's easier to dance in one direction than another."[25]

There was her theory in Disney-like packaging to add romance to the Cinderella story. The metaphor allowed the press to divorce Mayer from the masculine culture of science, to make her appear a different breed of scientist, if a scientist at all. Twenty years earlier, journalists covering Los Alamos told Americans that the physicists who developed the bomb had the power to kill; Mayer had been part of that fraternity, and yet in 1963 her atomic science was made over to appear unthreateningly dainty. She tacitly accepted the images but chastised a contributor to the *Christian Science Monitor* for claiming that she liked to bake*:* "I have never in my life baked a cake, and my husband doesn't like them anyway."[26] This writer had made similar claims in print about Lillian Gilbreth that were no less imaginary. Such creative license suggests how little cultural attitudes about science and women had changed by the early 1960s. Female scientists weren't admirable except when painted in glowing domestic shades.

Mayer's careerism was easier to defend once feminists could read her personal story of antinepotism and underemployment as a cautionary tale of sex discrimination for younger scientists. Women in the

American Physical Society and other science organizations sought her out, but Mayer was a reluctant martyr: "The fact that I worked as a volunteer professor of physics at the University of Chicago was not due to the fact that I am a woman, but due to the fact that I am a wife. My colleagues accepted me as one of theirs. I never felt any discrimination because I was a woman."[27]

Younger scientists might have thought her naive or in denial, but her dismissive tone was typical of her generation. Cecilia Payne-Gaposchkin, six years Mayer's senior, said the same when asked how being a woman had affected her scientific career. Oddly, she recanted her statements the following day and named a list of offenses against her, including the many times she had been passed over for tenure because she was female. The younger astronomer Vera Rubin thought she understood: "These indignities bothered [Gaposchkin] only at times. More likely, throughout her life she realized that she was taking steps never before open to a woman, and accepted them as part of the difficulties of being first." In 2000, Rita Levi-Montalcini, the Nobel prize-winning biologist and three years Mayer's junior, was likely of the same mind when she drew a rosy picture of her career, though her 1988 memoir leaves room for other interpretations. She admitted that, for a time, peers had virtually ignored her discovery of nerve growth factor, or at least that *she* had discovered it. Her winning of the Nobel Prize thirty years later served as proof that they eventually had come around, so she dwelled on this rather than her trials.[28]

Mayer's Nobel Prize and Gaposchkin's appointment as the first female full professor at Harvard also seemed to soften hard feelings. When younger feminists asked them to relive their professional stories, they tended to offer milder versions of them. Mayer didn't want her elongated career path scrutinized, but then reluctantly agreed to assist Joan Dash in gathering evidence of her employment history for a collection of feminist portraits called *A Life of One's Own* (1973). What may have appealed to Mayer about the project was that Dash intended also to highlight the supportive husband who had helped her succeed. With slight tweaking, Dash's rendition of Mayer's companionate marriage pleased Mayer, since it did justice to Joe. But she remained uncomfortable with Dash's account of her career as victimization vindicated. She had no ax to grind, Mayer reminded Dash, since her male colleagues had fought gallantly to shore up her success.[29]

We don't know whether Dash's corrections were acceptable in the end, for Mayer died before the book appeared. Other than a handful of celebratory juvenile biographies, there have been few accounts of her life since Dash's, suggesting that her wishes to blend into the walls of masculine science have been respected. Posthumously, feminists have made a more compelling martyr out of Rosalind Franklin, the popularly recognized victim of *The Double Helix*. They have argued that Watson's demonizing signals the sexist hostility she must have endured through her brief life. But she was also a victim of a form of cultural invisibility, since few people in the 1950s could imagine a young, attractive woman foregoing marriage for the lab. Watson couldn't either, so he represented her as caricature rather than in three dimensions.

Correcting the record, biographers have insisted that Franklin wasn't the dowdy bluestocking Watson made her out to be. Her town was not London, but Paris. She wore Chanel, Christian Dior, and lipstick and enjoyed nightlife and red wines. One author divulged her measurements: bust 34, waist 27, hips 38—physical proof of her possible sex appeal, while others emphasized her athletic body and independent attitude to make the case for her modern resonance. She's been described as cosmopolitan and grounded, decadent and virginal. Commenting on the British Broadcasting Corporation docudrama *Life Story* (1988), Francis Crick thought it no coincidence that Franklin's character was the only one portrayed with her head down doing bench work. On the television screen, she appeared as the Virgin Saint of Science because filmmakers liked that image. Certainly, she never was the Norma Rae of science, and yet she has appeared as a self-assured woman with politicized sensibilities, as if she were knowingly ahead of her time.[30]

Franklin's Birkbeck colleague Aaron Klug was amazed by the huge number of voices that emerged to right the wrongs of *The Double Helix*, but he wondered whether new caricatures would become equally constraining, for they, too, rarely delineated the living, breathing woman he knew. "I can't say she was like a man, not like that at all," he told a biographer, "but one didn't think of her being particularly like a woman; she wasn't shy, or self-effacing—but she wasn't blustering, either." When the New York Academy of Science considered awarding Franklin a posthumous prize for DNA, Klug expressed concern about the emphasis placed on her gender over her science: "If she is to be honoured, it should be not so much as a 'woman of science' but for her crucial contributions in

sorting out the A and the B forms. . . . The fact is Rosalind was never an active feminist, but simply evoked or created respect in her own right as a person, and I think she might have found some of the present attitudes somewhat distasteful."[31] It was clear to Klug that Franklin's gender and science could not be culturally reconciled into a notion of personhood, just as Maria Mayer's couldn't in 1963 or Marie Curie's in 1920.

Those women who became scientists during the war years and lived to see the beginnings of women's liberation—Maria Mayer, Rita Levi-Montalcini, Cecilia Payne-Gaposchkin, and Rosalyn Yalow—stood at a cultural crossroad, where Rosalind Franklin would have stood as well had she lived. They felt tremendous pressure to appear also as perfect wives and mothers to counterbalance their achievements as scientists. Levi-Montalcini handled the tensions by choosing a life of virtual celibacy. The Nobel winner Barbara McClintock chose this path, too, but suffered the constant scrutiny of men who thought her eccentric and unwomanly. It's not an accident that it took more than thirty years for the peers of these single women to award them Nobel Prizes for work they did in the 1950s. Married contemporaries won their Nobels more quickly, but they also paid a price. Mayer handled the tensions by underplaying the struggle between home life and science, and Yalow denied struggle altogether, boasting unabashedly about the perfection she had achieved in both worlds. For all her superiority, it's likely she dissembled pain or, as her daughter contended, blotted it out with alcohol, as Maria Mayer was said to do.[32]

In her book of interviews called *Women and Science: Then and Now* (2009), Vivian Gornick confirmed that Yalow could have been nearly any woman scientist of this generation to whom she spoke. These were wives who succumbed to nepotism rules or "de-sexed bluestockings" who chose all-encompassing careers over marriage. "There was about them a uniform remoteness of manner and expression," Gornick noted. "The pose was haughty and aristocratic, the speech guarded, the personality masked, defensive, unknowing. Each of them, in effect, said to me: 'Problem? My dear, I *had* no problem. I know what I wanted to do and I simply did it.' Surely they needed hard exteriors to withstand the environmental hazards of postwar labs, but Gornick, a younger feminist, didn't see as heroic the disavowing of struggle: "They had been treated like den mothers and mascots by their colleagues," she said of this generation, rather than as real, multifaceted women.[33]

Scientists of the following generation inherited their attitudes to some extent but were young enough to be influenced by the feminist movement. Fay Ajzenberg-Selove provides several interesting points of comparison with Yalow, since she held similar views before becoming politicized in the late 1960s. She had wanted to attend MIT as badly as Yalow wanted to attend Columbia, but her sex made it impossible, and thus she also turned to a midwestern university. She, too, didn't challenge the notion that succeeding in physics meant embracing its male rites of passage—the shop classes, the years of intense intellectual competition—to weed out the weak from the worthy. "When I began work in physics, only one in forty American physicists was a woman," she proudly recalled, and that was what appealed.[34]

Like Yalow, Selove felt that her relationships with men accounted for her success in physics; Marie Curie was practically the only woman she ever cited as a positive force in her career. "I thought that scientific work would be appraised logically and unemotionally," she explained. "I wanted to avoid emotional outbursts which I related to the behavior of artists (and of Mother), and not of scientists." Both she and Yalow claimed that their interactions with men were sexually benign until they met the scientific husbands who supported their careers. As they made professional names for themselves working with men who weren't their spouses (Yalow with Berson and Selove with physicist Tom Lauritsen), their colleagues speculated about their marital infidelities. Few around them could imagine nonmarital partnerships as viable or productive.[35]

When Selove was engaged to be married, the forty-nine-year-old Maria Mayer pulled her aside and poured her a whiskey before ranting about the plight of the married woman physicist. It left an impression, but Selove still believed that the most competent women could make family coalesce with career. She was an untenured member in the physics department at the University of Pennsylvania, operating in a hierarchical system in which power relations were readily apparent. At Argonne, the VA hospital, and Cold Spring Harbor, the Nobel laureates Maria Mayer, Rosalyn Yalow, and Barbara McClintock buried themselves in research and insulated themselves from the tribulations of other women. But at Penn, Selove was in her office whenever women students needed consoling. One graduate student came to her when her advisor had refused to help her find a job. When Selove confronted the advisor, he confirmed that the student was one of the best he had

ever had, but he wouldn't waste his time on her behalf. It was then that Selove's feminism, as she described it, was ignited like the flick of a switch.[36]

Gloria Lubkin, Selove's former graduate student and an editor of *Physics Today*, suggested that she talk with Brian Schwartz of the Forum on Physics and Society, who was organizing the first meeting of women in physics at the national American Physical Society (APS) meeting in 1971. Selove agreed that it was good timing: it was the year that Mina Rees became the first woman president of the American Association for the Advancement of Science. In previous years too, Selove had taken issue with the "ladies registration" at the annual APS meetings, since organizers assumed it was for spouses of physicists rather than for physicists themselves. Selove moderated the meeting, and more than six hundred people attended, leading physicist Vera Kistiakowsky to form the Women's Committee of APS shortly afterward.

Selove became the first female officer of APS, ironically just as she was suffering a sexist tenure review. Colleagues decided that she was not "sufficiently active in nuclear physics" and that she was too old for tenure consideration at forty-six. Being married to another department member was also an unspoken stigma. Although antinepotism policies had technically been overturned, the prevailing sentiment in the department was that spouses weren't promotable on their own merits. She filed a complaint with the Equal Employment Opportunity Commission, and became one of the first people to use the quantifiable measure of "citation counts" to make her case. The only faculty member in the department who could list more publications than she had won a Nobel Prize, making her case still more compelling. The Human Relations Commission decided unequivocally in 1973 that she had suffered discrimination, and thus she became the second female fully tenured faculty member in Penn's College of Arts and Sciences.[37] Selove's family history and career path resembled Yalow's, but her views of women's rights had been changed sharply by the women's movement. She chose to bring the law to bear on a situation that Yalow would have expected to work itself out in the allegedly Darwinian way things did.

A physicist who assisted Selove with the APS meeting was none other than Yalow's contemporary Chien-Shiung Wu. When Yalow first started at the VA hospital in the 1950s, Wu was nearby at Columbia and had moved into an apartment two blocks from her lab. Wu often left

her son with a nanny or fending for himself; graduate students admitted their discomfort when he called the lab at night to tell his mom that he was hungry and wanted her to come home, although most of the time she was too absorbed in experiments to heed him. Wu's intensity earned her a reputation as the "Madame Curie of beta decay" as well as the more sinister "Dragon Lady" of physics. She worked six long days a week, took fifteen-minute lunch breaks, and was unforgiving when graduate students took personal days. This was a woman who sent her husband to take an anniversary cruise on the *Queen Elizabeth* without her, too wrapped up in experiments to be interrupted. A decade later at the MIT Symposium on Women in Science and Engineering she continued to view science as an intense endeavor, but she rallied with women to debunk the notion that intensity was a man's trait alone, citing Marie Curie, Lise Meitner, and Maria Mayer as proof in her field of physics.[38]

Wu became an active feminist who did not seek to change the culture of the lab so much as to make women a part of it. The MIT physicist Mildred Dresselhaus, once Yalow's student at Hunter, hoped, if not to change the culture outright, then to equip women with the tools to succeed and perhaps to make changes from within. She, too, had been part of the Symposium on Women in Science and Engineering in 1964 and the APS meeting of 1971. She was tenured in 1968, but she never forgot how male colleagues joked about her late arrivals to the lab during the years she had to wait for the babysitter to arrive. When she first taught at MIT there were few women in her solid-state physics course, and those who showed up remained silent in the back row of the hall. Rather than deny inequities, she organized forums for women to talk with older scientists about the pressures of the lab and provided assistance to those applying for grants and promotions. She has seen it as her responsibility to advance the cause of women in science. "Ros didn't move in that direction," she lamented, "So I did that. . . . It had to be done."[39]

Dresselhaus thought that if women achieved "critical mass," some 10–15 percent of graduate and faculty positions in science departments, they could begin changing institutions for the better. Others, including former physicist Elisabeth Fraenkel, doubted that fundamental reform was possible. Fraenkel wrote Vera Kistiakowsky of APS in 1971:

> I am NOT interested in women like Mina Rees, any more than I am in Ralph Bunche. This is tokenism, and is the worst kind of discrimination. I am interested in the dozens and dozens of women like myself

> who idealistically went through school, hoping to be able to contribute, and who were sooner or later—usually sooner—slapped down.
>
> . . . The women in my age group almost uniformly did poorly. Some—even with Ph.D's—ended in libraries. Many, especially in microscopy, started their own businesses. We almost did. Others felt they had to take what was dished out to them—but since I am a very poor masochist, I couldn't hack this at all.
>
> . . . Today I am an active leader in the NY area in the liberation movement—every phase of it—and of course especially in Women's Liberation. I counsel women—hundreds of them—from all over the U.S. I help them be proud to be female, and not to let the male power structure suppress them. I also tell prospective women physicists, chemist[s], engineers NOT to waste their time and injure their dignity. Until things get a lot better, science is NO place for a woman in the U.S.[40]

Angry epiphanies like Fraenkel's came to women of her generation fast and furiously, as discoveries were said to have come to their male peers. "For the first time it hit me that my life had developed as it had because I was a woman, and I'd made women's choices, and ended up where women in science end up," one of Gornick's subjects divulged. It took Elga Wasserman, a chemist, twenty years to realize that her graduate advisor at Harvard had been too busy earning his Nobel Prize to take her seriously. "He had lost all interest in my career. . . . Nobody advised or encouraged my husband and me to embark on a joint job hunt, although it probably would have been futile anyhow." Her husband joined the Yale faculty and she agreed to be a research assistant in the microbiology lab. She left when her first child was born, and no one urged her to stay. She looked on admiringly at her colleague Mary Bunting, who came to the lab at night after her husband returned from the office to put their four kids to bed. His assuming of domestic chores seemed to make all the difference.[41]

Bunting became the president of Radcliffe College, and Wasserman also went on to higher education administration, at Yale, after twelve years of part-time work in industrial labs and community colleges. Meanwhile, many of their contemporaries seethed as they tried to persevere in university labs. The only tenured woman in an Ivy League chemistry department told Gornick that she hated that she had spent twenty-three years watching male colleagues pass her by. Another woman graduated first in her class and couldn't find a job; her husband, who graduated in the top third, received fourteen job offers, so

she spent a career accepting fellowships attached to his grants. Science had seduced her and then left her defiled. A woman who described her graduate mentor as a womanizer meant this literally. Another woman hated that in choosing science, she had taken a "vow of lifetime celibacy," as yet another lamented having to hide a pregnancy under a lab coat and telling no one when her child was born. Gornick's subjects didn't question that they had to appear sexless, freakishly devout to research, until women's liberation made them newly aware. As much as they wanted to point fingers at men, they admitted that their mothers were guilty of abetting: "My mother said, 'Be a scientist,' but she didn't really mean it. What she really meant was, 'Get married and teach high school biology so you can be a help and a pride to your husband.'"[42]

In the case of several of Gornick's youngest subjects, divorces brought their powerless positions into high relief; they became feminists when they had to build careers and reputations from scratch. They had learned from women before them and saw that their frustrations were caused not by personal failings but by ceilings that doomed them from the start. They understood that mechanisms of subtle discrimination led to more of them paying their way through graduate school and being assigned teaching assistantships as men were assigned to research projects full time. More of them demanded visibility and believed that they had the right to fight for it or to move on. They were now their own keepers, and science didn't define them. "It was the range of temperamental difference among them that set the young women off from the old," Gornick concluded. "Instead of concealing variety they seemed to assert it. In fact, these women seemed to reveal their individual selves through science rather than disappear into science as the earlier generation of women so often did."[43]

The Transformations of Evelyn Fox Keller and Barbara McClintock

In 1957 Evelyn Fox was a twenty-one-year-old college senior who had a mind for theoretical physics. She knew this made her an oddity. Other smart girls excelled in English, a course in which she got Cs growing up; she, on the other hand, had preferred sci-fi to classic literature, especially George Gamow novels, in which boys fell asleep in physics lectures only to dream up brilliant theorems themselves. Her dream

was to work with Richard Feynman, the subject of her senior thesis at Brandeis, and for a time she thought it possible. There had been rumors that he was taking a university chair at MIT, where she was being recruited, but he decided to stay at Caltech, which still hadn't opened its doors to women. Recruiters from Stanford and Harvard came to her with funding; indeed there seemed to be a lot of it for the taking. The National Science Foundation funneled more than ever down to university programs, physics departments in particular, to recruit the best and brightest to compete with the Soviets. In the end she decided to go to Harvard—undergraduate advisors had told her to settle for nothing less.

Looking back, she remembered feeling incredibly buoyant before arriving. "I fell in love with theoretical physics, and . . . with the image of myself striving and succeeding in an area where women had rarely ventured." "I believed not only in the possibility of clear and certain knowledge of the world, but also in the unique and privileged access to this knowledge provided by science, and scientists, as arbiters of the truth—physicists were, of course, the highest arbiters."[44]

In her idealized version of graduate school, she had seen herself ruminating on space and time as she imagined Einstein had in his younger years. But the stark reality was that "operationalism" reined supreme. "In place of wisdom, I was offered skills," she reflected. Rather than theory, instructors stressed hands-on technique. Her papers were returned with red slashes throughout: too fixated on the arithmetic, no one had bothered to notice her mastery of concepts. With each assignment male students grew bolder, and she more unsure. Only later did she understand that "in addition to the techniques of physics, they, [men], were also studying the techniques of arrogance." Her first year was an exercise in internalizing the cultural ascribing of intellectuality to maleness. In lecture halls and campus dorms she was consistently reminded that physics was hard and that she was wrong to think she understood what she thought she understood; her lack of fear proved her ignorance. No woman at Harvard had ever completed a graduate degree in theoretical physics; the prestige of the program relied on its producing only male scientists.

There were two other women among the one hundred students in the physics program, but neither was as serious as Fox about her studies. Fox later regretted that she had then believed that being taken seriously

as a physicist meant not identifying with other women. Meanwhile she was scrutinized—for her wardrobe rather than her mind. She became the object of laughter when men detected her ambition. Oral exams were a nightmare: her main examiner didn't show up and cited oversleeping as the reason, although the exam was at 2:00 p.m. She passed, but couldn't get a faculty member to answer his office door to assist in directing her thesis. She knew she was unwanted and grew lonely. She attended parties, only to be confronted with the same question of "why?"—Why stick around? Why so desirous of masculine goals? Once she had been in love with the idea of being a physicist. Now she wondered why she wanted this for herself. By the middle of her third year, she decided to give up physics. She had already stopped going to class and liked the response: professors were affable, even sympathetic.

In the summer of 1960 she found refuge in the laboratories of Cold Spring Harbor, where her brother, a biologist, had a room to spare. There she met Max Delbrück and became fascinated with the DNA code Crick and Watson had left ripe for deciphering. Her excitement for science rekindled, she returned to Harvard and worked in molecular biology, where colleagues were congenial and respected her mind. She completed her doctorate degree in 1963 and took a job at New York University teaching theoretical physics. She also married Joseph Keller, the mathematician who had supervised her, before having two children and charting new ground in mathematical biology. With colleague Lee Segel she invented equations for describing how single-celled amoebas changed into multicelled organisms. As Segel went around the country generating excitement for their findings, Keller followed her husband to Stanford and grew bored at home. She decided to collect data on a new set of organisms—ones that suffered fates not different from hers. She started "thinking about women in science."

Women often describe their years away from science rearing children as impeding their work or ambitions, but Keller turned the time into an opportunity to expand her mind in ways she couldn't when tied to the lab. She read deeply in literature and philosophy and remembered this period of "blossoming radicalism" as the most joyful in her life. In 1974 she returned to professional life with a series of lectures in mathematical biology, slipping into them her new mathematical models indicating the paucity of women in science. She had never before considered the extent to which science was bound to the idea of masculinity. "A

lifelong training had labeled that question patently absurd," she admitted, but now she told a room full of men that this was the crux of the problem of women's status in science. She became one of the first feminists to use cultural and social indicators to reveal the workings of gender in the practices, institutions, and metaphorical language of science. "The fundamental conflict—between my sense of myself as a woman and my identity as a scientist—could only be resolved by transcending all stereotypical definitions of self and success," she realized. "This took a long time, a personal analysis, and the women's movement."[45]

Keller wanted to understand how socially constructed notions of gender defined science for better or worse. This required an exploration of science through new female eyes, but whose? In the late 1970s feminists wrote books about the occasional success story, typically a female physicist with tenure or a Nobel Prize. But these biographies often were no different from those written from a male perspective. "See," they asserted, "this woman can do what men can do on men's terms." When Keller began to search for a biographical subject, Yalow had just won the Nobel Prize, but Yalow, a woman who had never questioned the masculinity of her field or her prescribed womanly duties at home, would never do as Keller's subject. Keller preferred the enigmatic geneticist Barbara McClintock, a woman who quietly kept to herself in the cornfields of Cold Spring Harbor. When Keller was there in 1960, McClintock had been tucked away, doing science on her unorthodox terms.

No one denied McClintock's brilliance, but few biographers saw a modern-day heroine in her before Keller approached her in the late 1970s. She was not the superwoman type. The work-family balance of her contemporaries was moot in her world; there was no balance, only science—at least as her early myth had been spun. Other successful women scientists had succeeded because they obeyed institutional rules. McClintock hated rules and most institutions; she never had a boss that she liked. Yalow and Mayer had been undeniably female in appearance; McClintock, on the other hand, looked androgynous. She appeared to Keller in her seventies even as she had appeared in her twenties: her wardrobe was ambiguously plain and she wore no skirts or makeup and kept her hair short; if there were any curves on her one-hundred-pound frame, she had never accentuated them. She was single

and never had children, nor had she shown pangs for them. People speculated that she was gay, but most decided she was asexual, married to the corn plants she had spent a lifetime knowing.

McClintock's corn and microscopes were hardly the stuff of titillating biography, but Keller found her fascinating, turning her story into a piece for the *New Yorker* and then into a full-scale biography. She was not simply hero or victim; Keller believed that both success and marginality characterized her career. Because the eccentricities that male scientists gloried in had served to stigmatize McClintock, her scientific innovations consequently had been overlooked. But Keller didn't see her story as a cautionary tale so much as an alternative one. "She had long ago rejected the role of 'lady scientist,'" Keller noted, "but when she had a title normally reserved for a gentlemen academic, she did not fit that role either." McClintock defied social categories of "woman" and "scientist" and in the process demystified science itself. By highlighting McClintock's relationship to nature, a relationship that Keller did not call "female" or "feminist" but that others eventually labeled as such, she hoped to show an approach to science that transcended gender and was good for science as a whole.[46]

Keller's heroine was three years older than Maria Mayer. She had studied cytology and genetics in the School of Agriculture at Cornell and had earned her PhD in 1927. During the Depression jobs were hard to come by, but this was a woman who, by the age of thirty, had revolutionized her field. As a graduate student she had identified the morphology of all ten chromosomes in corn; as a postdoctorate researcher she and graduate student Harriet Creighton demonstrated the first genetic crossover in *Zea* strains. T. H. Morgan at Caltech believed even then that the young scientist was "the best person in the world" in maize genetics, and yet men continued to overlook her for university positions. She traveled to colleagues' labs on grants from the National Research Council, and on Rockefeller and Guggenheim Fellowships, but she had no permanent position to fall back on.[47]

McClintock thought her gender was to blame but was not dejected about her nomadic status, for she hated the schedules, teaching, and committee work that came with university jobs. Nevertheless when Lewis Stadler found her an untenured position at the University of Missouri, she recognized the folly in turning it down. She stayed there until 1941, but was visibly depressed most of the time. Men in the de-

partment assumed that she was there to teach just as other women were, but she found the lectures a nuisance and complained about her insufficient research facilities. On a night that she locked herself out of her lab colleagues watched the trousered woman climb up to and through a second-story window, simply more proof of her inappropriate eccentricity. McClintock was convinced that she would be fired, and in fact there was cause, since she was known to shirk her teaching duties when they conflicted with harvesting her corn. She was contemplating a new career in meteorology when the Carnegie Institution intervened and offered her a temporary position in its genetics division at Cold Spring Harbor. She would have her own cornfields and live on-site. The salary was modest, but there was no formal teaching attached. Convinced that she'd have less freedom anywhere else, she accepted the job and became a permanent fixture there for the next fifty years.[48]

Although McClintock lived on the ground floor of a visiting women's dorm, she was known to sleep in her cornfields or on a cot in her office when absorbed in her work. She took walks on the grounds along Bungtown Road, collecting flower parts for chromosome preparations. Colleagues recalled occasionally accompanying her and listening to her explain the variable pigmentation patterns on Queen Anne's lace or the flight patterns in flocks of birds overhead. But most of the time she was physically, emotionally, and intellectually alone. McClintock told Keller that her preference for solitude "began in the cradle." As far back as she could remember she preferred to read or sit alone, "thinking about things." She never felt the need for strong personal attachments and couldn't understand marriage, for instance, since she "never went through the experience of requiring it." The admission might have given other biographers pause, but Keller thoughtfully connected her solitude and science. This was how she achieved intimacy; her closest relationships were with nature, and hence she achieved a richer, more holistic understanding of the natural world.[49]

Keller found it hard to encapsulate McClintock's science in a single term because of the encompassing and integrated perspectives she was able to achieve: "For her," Keller wrote, "the smallest details provided the keys to the larger whole. It was her conviction that the closer her focus, the greater her attention to individual detail, to the unique characteristics of a single plant, of a single kernel, of a single chromosome, the more she could learn about the general principles by which the

maize plant as a whole was organized, the better her 'feeling for the organism.'" In this sense McClintock was like Lillian Gilbreth—a "systems thinker" who saw interrelatedness wherever she looked. Rather than presume that a master control had been hardwired into genomes of corn, for instance, she believed that the interactions between genomes and their cellular environments served as the mechanism of control. It was not her practice to dissect parts of organisms and study them one by one under fluorescent lights; she looked for interactions in native settings to see nature on its terms. She didn't "press nature with leading questions," Keller explained, she "dwell[ed] patiently in the variety and complexity of organisms." It wasn't enough to amass data about a kernel of corn; she came to know its temperament, its life story—a process more intimate and creative than science-as-usual.[50]

McClintock's communion with nature was so intense that she actually lost herself to it. "I'm not there!" she explained to Keller. She was no longer cognizant of the position she was supposed to assume lording over it; she was nature's equal because she was part of the system's whole. "I found that the more I worked with [chromosomes] the bigger and bigger [they] got, and when I was really working with them I wasn't outside, I was down there. I was part of the system. . . . I actually felt as if I were right down there and these were my friends." She collapsed the distance between subject and object, inquirer and inquired, entering conversations with nature and waiting patiently to hear its utterances. Critics might call it unobjective science, but Keller saw it as science with context—"dynamic objectivity"—the purest form of scientific truth.[51]

One can compare McClintock's process to that of lovers and poets or to the masculine transcendence experienced by Einstein in his day. Indeed Keller liked to think of it as science that transcended—a science that transcended gender. It was kinder, less autocratic, but not more easily achieved by either sex, since McClintock, its practitioner, defied distinction as a gendered type. This brand of science was antihierarchical and democratic; anyone with humility could participate. Keller summoned it as proof that McClintock was not elitist or haughty, as colleagues claimed she could be toward people of lesser intelligence. Although she gravitated toward smart people, they were gentle people, often women, but not always. Her field of corn became a great social equalizer since it did not cultivate the power relationships of the lab. McClintock had no assistants to rule over or graduate students to evalu-

ate; she planted her own corn, and when she was away she found high school girls more effective surrogate caretakers than the world's leading cytogenetic experts.[52]

McClintock was never a vocal advocate for women scientists, but Keller believed that her brand of science gave women tools, perspective, and confidence to trust their intuition. It wasn't McClintock's style to tell younger researchers what to think; rather, she would guide them to clearer visions of their thoughts. A female researcher remembered sitting in the living room of the old dorm at Cold Spring Harbor asking McClintock to help her link up the facts of her most recent experiments. McClintock told her the heretical: that inspired science was to some extent an imaginative act. The young bacterial geneticist Evelyn Witkin was inspired in the 1940s when McClintock told her not to worry about what textbooks told her to find under the microscope: don't tell nature how to be; rather, let nature "tell you where to go." In masculine science, intuition and intimacy were dirty words, but McClintock feared neither and had been well served by both.[53]

Listening to McClintock describe her process, Keller could see the irony. "Things are more marvelous than the scientific method allows us to conceive," McClintock explained, and yet she felt "in closer touch with biological reality" as colleagues thought her "increasingly out of touch." She was a self-proclaimed mystic who studied Buddhism, acupuncture, biofeedback, and other means of knowing that were at odds with Western science. Her seeming fascination with the supernatural didn't endear geneticists to her ideas in the 1950s and 1960s, though in later decades many of Keller's readers thought her interests romantically "primitive" and stereotypically "feminine." Witkin insisted that McClintock didn't believe in UFOs or ESP, but believed one shouldn't disbelieve without dispositive proof. Her mysticism was simply an admission of nature's complexity, that there was no way to understand all its mysteries. As a younger colleague put it, McClintock had "the courage to say, 'I do not understand.'"[54]

These were the seeds of McClintock's feminist mythos; they germinated through Keller's telling of events, which first occurred at the infamous Cold Spring Harbor Symposium in 1951. McClintock was nervous about presenting her research, for geneticists had long taken for granted that genes lined up like pearls on a string, stable and impervious to envi-

ronmental stimuli. After years in her cornfields, McClintock was going to suggest something different, that genetic elements moved from one site to another, even between chromosomes, and that this movement was programmed by a complex system of controls influenced by their interactions with the environment. The jumping around of genetic elements, a phenomenon that came to be known as "transposition," was the discovery for which McClintock won her Nobel Prize thirty years later, but the majority of prokaryote geneticists she spoke to that day in 1951 were reluctant to see heuristic value in the controlling concept that lay at the heart of her theory. They were using radioisotopes, X-ray crystallography, and electron microscopes to study the molecular structure of simpler organisms, while McClintock used low-tech microscopes and crossbred her corn by hand. Rather than appear revolutionary, she seemed to prove that the age of maize genetics had come and gone while others passed her by.[55]

There was silence when McClintock finished her paper; she had confounded her peers, but Keller posited that she had likely also threatened them as arbiters of scientific truth. McClintock was so hurt by the response that she closed herself off from scientists in her field, refusing to publish or speak of her ideas to anyone except an inner sanctum of friends who understood. She stuck to her convictions, however, and colleagues continued to think her "mad" until the late 1960s and 1970s, when molecular biologists confirmed dynamism in the genomes of *E. coli* and other organisms. By the mid-1970s the women's movement was at its most vibrant, and other biographers were reexamining the scientific accomplishments of women obscured by history. Keller thought McClintock's story significant, less for the vindication she now enjoyed than for her unacknowledged feeling for organisms. Like feminist scholars since, Keller was interested in process more than scientific results.[56]

McClintock won the Nobel Prize in 1983, five months after Keller's biography, *A Feeling for the Organism*, was published. The prize brought readers to her story, but it also obscured the nuanced points Keller raised about gender and science. Readers who looked for a martyr of the male power structure saw one in McClintock; those who looked for a heroine among a female breed of scientists also saw what they wanted to see. Some thought her a female maverick, using male license; for others the moral of her story was that all, including men, could listen patiently—womanly—for nature to speak. In both cases, she became

a figure almost exactly in opposition to the popularly imagined Marie Curie of 1920 or the Maria Mayer heralded in 1963. McClintock was a new idealized image of a female scientist not because of her motherhood or domesticity, but because of the intuitive way she worked at science. In this respect, Keller's aim to *regende*r the portrait of a scientist had been successful.

After McClintock's death in 1992, historian Nathaniel Comfort looked through the transcripts of Keller's interviews and other archival evidence in search of the authentic McClintock. He found a woman of "intimidating intellect," "biting humor," and "fierce independence," but not necessarily a mystic or maverick or marginalized martyr. She claimed that her peers had rejected her views in 1951, and yet he discovered (and several scientists have since confirmed) that they discussed her ideas at length and generally accepted transposition in corn.[57] Comfort reiterated that McClintock was the third woman to be admitted into the American Academy of Science and the first to be president of the American Genetics Society. Honors had continued to come her way in the 1950s and 1960s, decades in which Keller claimed she had been largely ignored. By the early 1980s she had won the Thomas Hunt Morgan Medal, the Wolf Prize, and the Lasker Award, and became the first MacArthur laureate before winning the Nobel. Most scientists doubted her theories about controlling elements, and rightly so, Comfort believed, since they were never empirically confirmed. Transposition had never been the thrust of her work, and yet younger scientists generously remembered it differently. This was not a woman undermined, he determined; he couldn't see the lasting impact of sexism in her career. If her tale of victimization was a sham, so was the idea that McClintock adhered to precepts of reason that were different from her peers'. Comfort called her process "integration" and claimed that, since it had also been Einstein's and Feynman's, there was nothing mystical or female about it.[58]

So then why the myth? Comfort believes timing was a factor. McClintock was a Nobel Prize winner who needed an appealing narrative, and maverick behavior, mysticism, and a healthy dose of martyrdom would do. James Watson had once referred to the trousered, temperamental McClintock as the "Katharine Hepburn of science," but only in her eighties was the characterization a compliment. "She proved an irresistible media figure," Comfort explained, "the boyish little old

lady, dragged out of the solace of the laboratory, blinking up at the klieg lights, saying everyone had thought she was crazy, she knew all along she was right, she hated publicity, had no need for money."[59] Keller saw her differently, revealing the contested nature of the woman scientist as cultural icon. Thomas Kuhn understood that scientists unearthed socially constructed truths, but of course historians and biographers do too.

The "transformation" suggested by the title of this section was Keller's as a feminist and McClintock's as feminist legend, but it was also a singular transformation made manifest through both women. Keller's feminism changed her way of thinking about scientific inquiry, and McClintock became her vehicle for showing this scientific inquiry embodied and practiced. Where her colleagues looked for rules in nature, McClintock believed that there was value in studying anomaly for anomaly's sake. For Keller, McClintock was that anomaly, that mutation, that exception to many rules, and so she was worth getting to know on her terms. Keller's time with McClintock confirmed that everything was indeed connected. McClintock's eccentricities as a woman and a scientist gave Keller insight into whole systems of gender and science as well as insight into herself. Keller achieved her own feeling for the organism.[60]

Notes

1. Taken from the tenth-anniversary edition (Yale University Press, 1995), 174–75.

2. Elizabeth Stone, "A Mme. Curie from the Bronx," *New York Times Magazine*, April 9, 1978, 29; Sharon Bertsch McGrayne, *Nobel Prize Women in Science: Their Lives, Struggles, and Momentous Discoveries*, 2nd ed., (Washington, DC: Joseph Henry Press, 1998), 336.

3. Rosalyn Yalow, "Autobiography," in *The Nobel Prizes, 1977*, ed. William Odelberg (Stockholm: Nobel Foundation, 1978); Joan Dash, *The Triumph of Discovery: Women Scientists Who Won the Nobel Prize* (Englewood Cliffs, NJ: Julian Messner, 1991), 38–42; Olga S. Opfell, *The Lady Laureates: Women Who Have Won the Nobel Prize* (Metuchen, NJ: Scarecrow Press, 1986), 261; Diana C. Gleasner, *Breakthrough: Women in Science* (New York: Walker, 1983), 47; Nancy J. Veglahn, *Women Scientists* (New York: Facts on File, 1991), 106.

4. Adolph Friedman, "Remembrance: The Berson and Yalow Saga," *Journal of Clinical Endocrinology and Metabolism* 87, no. 5 (2002): 1925–28; Eugene Straus, *Nobel Laureate Rosalyn Yalow: Her Life and Work in Medicine* (Cambridge, MA: Perseus Books, 1998), 33–34; Yalow, "Autobiography"; McGrayne, *Nobel Prize Women*, 337–38; Veglahn, *Women Scientists*, 108.

5. Yalow, "Autobiography"; Straus, *Nobel Laureate Rosalyn Yalow*, 36–37, 56, 103, 168, 176; Dash, *Triumph of Discovery*, 44–47.

6. Yalow apparently didn't know that Quimby was a mother or perhaps forgot, since in later years she questioned her mentor's decision to remain childless. See Straus, *Nobel Laureate Rosalyn Yalow*, 68, 106; Veglahn, *Women Scientists*, 48–54; Mildred Dresselhaus, "Rosalyn Yalow," in *Out of the Shadows: Contributions of Twentieth-Century Women to Physics*, ed. Nina Byers and Gary Williams (New York: Cambridge University Press, 2006), 308–309; Dash, *Triumph of Discovery*, 48.

7. Barbara Shiels, *Winners: Women and the Nobel Prize* (Minneapolis: Dillon Press, 1985), 47, 51–52; McGrayne, *Nobel Prize Women in Science*, 336; Straus, *Nobel Laureate Rosalyn Yalow*, 83.

8. Straus, *Nobel Laureate Rosalyn Yalow*, 157–58; Dash, *Triumph of Discovery*, 51–52.

9. Straus, *Nobel Laureate Rosalyn Yalow*, 164–75; Mary Harrington Hall, "The Nobel Genius," Box 9, folder 30, Maria Goeppert Mayer Collection (MGM).

10. At the Nobel banquet, Yalow said she "[gave] birth to and nurtured through its infancy radioimmunoassay, a powerful tool for the determination of virtually any substance of biologic interest." See Straus, *Nobel Laureate Rosalyn Yalow*, 112; McGrayne, *Nobel Prize Women in Science*, 341; Gleasner, *Breakthrough*, 47, 53; Shiels, *Winners*, 63–64; Opfell, *Lady Laureates*, 256, 263; Yalow, "Autobiography."

11. Friedman, "Remembrance"; Straus, *Nobel Laureate Rosalyn Yalow*, 79–80, 170, 174–77, 223–24; McGrayne, *Nobel Prize Women in Science*, 342.

12. Helena M. Pycior, "Pierre Curie and 'His Eminent Collaborator Mme Curie'," in *Creative Couples in the Sciences*, ed. Helena M. Pycior, Nancy M. Slack, and Pnina G. Abir-Am (New Brunswick, NJ: Rutgers University Press, 1996), 39–56; Shiels, *Winners*, 54–55; Dash, *Triumph of Discovery*, 50–52; Straus, *Rosalyn Yalow*, 170, 177, 231–32; McGrayne, *Nobel Prize Women in Science*, 342; Gleasner, *Breakthrough*, 44–46.

13. Friedman, "Remembrance"; Yalow, "Autobiography"; Dash, *Triumph of Discovery*, 62.

14. McGrayne, *Nobel Prize Women in Science*, 350; Straus, *Nobel Laureate Rosalyn Yalow*, 236–37.

15. Shiels, *Winners*, 60; Straus, *Nobel Laureate Rosalyn Yalow*, 239–41; Gleasner, *Breakthrough*, 33.

16. Veglahn, *Women Scientists*, 107; Straus, *Nobel Laureate Rosalyn Yalow*, 78, 81; McGrayne, *Nobel Prize Women in Science*, 337, 354; Gleasner, *Breakthrough*, 51; Shiels, *Winners*, 63–64.

17. Straus, *Nobel Laureate Rosalyn Yalow*, 107, 171, 247–57; McGrayne, *Nobel Prize Women in Science*, 336, 340, 353.

18. Most prominent of the studies enabling the "superwoman" ideal was Jonathan Cole and Harriet Zuckerman, "Marriage, Motherhood, and Research Performance in Science" (1987), in *The Outer Circle: Women in the Scientific Community* (New Haven, CT: Yale University Press, 1992). See also Ann Gibbons, "Key Issue: Two-Career Science Marriage," *Science* 255 (March 13, 1992): 1380.

19. Joan Freeman, *A Passion for Physics: The Story of a Woman Physicist* (Bristol, U.K.: Adam Hilger, 1991). An autobiography with the same sensibilities is Rita Levi-Montalcini, *In Praise of Imperfection: My Life and Work* (New York: Basic Books, 1988).

20. William C. Kelly to Maria Mayer, July 28, 1964, Box 10, folder 19; "Can Working Woman Become Good Wife"; "Nobel Prize Winner Has Sound Advice for Women," *Mainichi Evening News*, April 9, 1965, Box 9, folder 31, MGM.

21. Alice Rossi, "Barriers to the Career Choice of Engineering, Medicine, or Science Among American Women," in *Women and the Scientific Professions: The MIT Symposium on American Women in Science and Engineering*, ed. Carol G. Van Aken and Jacquelyn A. Mattfeld (Cambridge, MA: MIT Press, 1965), 125–27.

22. Margaret W. Rossiter, *Women Scientists in America: Before Affirmative Action, 1940–1972* (Baltimore: Johns Hopkins University Press, 1995), 41–42.

23. See, for example, "Two Americans and German Get Nobel Physics Prize," *New York Times*, Western edition, November 6, 1963; "U.S. Shares Nobel Prize; California Woman Wins," *Washington Evening Star*, November 5, 1963; "12th Nobel Prize for U.C. Faculty," *San Diego Evening Tribune*, November 6, 1963, 42, Box 9, folder 28; Esther Gwynne, "Nobel Winner Home Again," *San Diego Evening Tribune*, December 24, 1963, Box 9, folder 29, MGM.

24. Mary Harrington Hall, "American Mother and the Nobel Prize—a Cinderella Story in Science," *McCall's*, July 1964; "The Nobel Genius," Box 9, folder 30, MGM.

25. Maria Mayer to Robert L. Weber, June 11, 1969, Box 2, folder 9; Hall, "Nobel Genius," MGM.

26. Maria Mayer to Mary Markley, April 20, 1964, Box 2, folder 3, MGM.

27. Vera Kistiakowsky to Maria Mayer, April 16, 1971, Box 2, folder 11; Maria Mayer to Rita Arditti and Elisa Buonaventura, December 10, 1970, Box 2, folder 10, MGM.

28. Vera Rubin, "Cecilia Payne-Gaposchkin," in *Out of the Shadows*, 167; Elga Wasserman, *The Door in the Dream: Conversations with Eminent Women in Science* (Washington, DC: Joseph Henry Press, 2002), 42; Levi-Montalcini, *In Praise of Imperfection*; Joan Dash, *Triumph of Discovery*, 99–130.

29. Maria Goeppert Mayer, "Manuscript Changes," n.d., Box 9, folder 2, MGM; Joan Dash, *A Life of One's Own: Three Gifted Women and the Men They Married* (New York: Paragon House, 1988), x.

30. Brenda Maddox, *The Dark Lady of DNA* (New York: HarperCollins, 2002), 65, 93; Anne Sayre, *Rosalind Franklin and DNA* (New York: W. W. Norton, 1975); McGrayne, *Nobel Prize Women in Science*, 303–31; Francis Crick, *What Mad Pursuit: A Personal View of Scientific Discovery* (New York: Basic Books, 1988), 88.

31. Horace Freeland Judson, *Eighth Day of Creation: The Makers of the Revolution in Biology* (New York: Simon and Schuster, 1979), 148–49; Philip Siekevitz to Dr. Aaron Klug, March 26, 1976; A. Klug to Dr. P. Siekevitz, April 14, 1976, Rosalind Franklin Papers, Churchill Archives, Churchill College, Cambridge, U.K. (letter digitized in cooperation with the U.S. National Library of Medicine, Digital Manuscripts Project, Bethesda, MD).

32. Straus, *Nobel Laureate Rosalyn Yalow*, 167.

33. Vivian Gornick, *Women in Science: Then and Now* (New York: The Feminist Press, 2009), 106; *Women in Science: Portraits from a World in Transition* (New York: Simon and Schuster, 1983), 120.

34. Fay Ajzenberg-Selove, *A Matter of Choices: Memoirs of a Female Physicist* (New Brunswick, NJ: Rutgers University Press, 1994), 3.

35. Ajzenberg-Selove, *A Matter of Choices*, 33, 49, 54, 73–74, 80, 83–84.

36. Ajzenberg-Selove, *A Matter of Choices*, 114–15, 159–61.

37. Ajzenberg-Selove, *A Matter of Choices*, 162–66.

38. Chien-Shiung Wu, "The Commitment Required of a Woman Entering a Scientific Profession," in *Women and the Scientific Professions*, 44–48; Iris Noble, *Contemporary Women Scientists of America* (New York: Julian Messner, 1979), 82; McGrayne, *Nobel Prize Women*, 266–75; Moira Reynolds, *American Women Scientists: 23 Inspiring Biographies, 1900–2000* (Jefferson, NC: McFarland, Inc., 1999), 116.

39. Mildred S. Dresselhaus, "Responsibilities of Women Faculty in Engineering Schools," in *Women in the Scientific and Engineering Professions*, ed. Violet B. Haas and Carolyn C. Perrucci (Ann Arbor: University of Michigan Press, 1984), 129–32; "Perspectives on the Presidency of the American Physical Society," *Physics Today*, July 1985, 37–44;

"Women Graduate Students," *Physics Today*, June 1986, 74–75; G. Dresselhaus, "Mildred Spiewak Dresselhaus," in *Out of the Shadows*, 355–61; Noble, *Contemporary Women Scientists*, 143–51; Straus, *Nobel Laureate Rosalyn Yalow*, 162.

40. Elisabeth J. Fraenkel to Vera Kistiakowsky, December 8, 1971, Box 2, folder 11, MGM.

41. Gornick, *Women in Science* (2009), 79–80; Wasserman, *Door in the Dream*, 4–6.

42. Gornick, *Women in Science*, 63, 80, 82, 86–87, 88–89.

43. Gornick, *Women in Science*, 61, 76, 107, 116, 128, 133–38.

44. Evelyn Fox Keller related her experience as a Harvard graduate student and her transition into the philosophy of science in Sara Ruddick and Pamela Davis, eds., *Working it Out: 23 Women Writers, Artists, Scientists, and Scholars Talk About Their Lives and Work* (New York: Pantheon Books, 1977), 78–91. See also Andrew Brown, "Fox Among the Lab Rats," *Guardian*, November 4, 2000 (online archive) (http://www.guardian.co.uk/books/2000/nov/04/books.guardianreview6).

45. Evelyn Fox Keller, *Reflections on Gender and Science* (New Haven, NJ: Yale University Press, 1995), 3–5; *Refiguring Life: Metaphors of Twentieth-Century Biology* (New York: Columbia University Press, 1995), ix; Brown, "Fox Among the Lab Rats."

46. Evelyn Fox Keller, *A Feeling for the Organism: The Life and Work of Barbara McClintock* (New York: Henry Holt, 1983), 83–84; *Reflections on Gender and Science*, 158, 173.

47. Keller, *A Feeling for the Organism*, 83–86; Dash, *Triumph of Discovery*, 75–85; Lee B. Kass, "Records and Recollections: A New Look at Barbara McClintock, Nobel Prize-Winning Geneticist," *Genetics* 164 (August 2003): 1251–60.

48. Barbara McClintock to Charles Burnham, February 20, 1930; April 2, 1935; September 16, 1940; October 9, 1940, Correspondence, Barbara McClintock Papers, National Library of Medicine, Rockville, MD.

49. Videotaped interview of Evelyn Witkin, Barbara McClintock Audio/Video Archive, Cold Spring Harbor Laboratory, Digital Archives, http://library.cshl.edu/mcclintock/mcclintock_av.html; Charles D. Laird, "The Plural of Heterochromatin," in *The Dynamic Genome: Barbara McClintock's Ideas in the Century of Genetics*, ed. Nina Federoff and David Botstein (Plainview, NY: Cold Spring Harbor Laboratory Press, 1992), 155; Keller, *A Feeling for the Organism*, 17–34, 198.

50. Keller, *A Feeling for the Organism*, 101, 207, 104; Bruce Alberts, "Please Come to My Laboratory for Better Coffee, Fresh Orange Juice, . . . Conversation," in *The Dynamic Genome*, 279.

51. Keller, *A Feeling for the Organism*, 117; Lisa Heldke, "John Dewey and Evelyn Fox Keller: A Shared Epistemological Tradition," in *Feminism and Science*, ed. Nancy Tuana (Bloomington: Indiana University Press, 1989), 112–13.

52. Barbara McClintock to Milislav Demerec, February 20, 1953, Box 1, folder 5, Barbara McClintock Digital Archive, Cold Spring Harbor Laboratory Archives (CSHL).

53. Keller, *A Feeling for the Organism*, 125, 203–4; Lilla Fano to Barbara McClintock, January 8, 1972, Box 1, folder 6, Barbara McClintock Papers, National Library of Medicine; Evelyn Witkin, "Cold Spring Harbor 1944–1955: A Minimemoir," in *The Dynamic Genome*, 116.

54. Keller, *A Feeling for the Organism*, 180, 192–93; James Shapiro, "Kernels and Colonies: The Challenge of Pattern," in *The Dynamic Genome*, 216; Nathanial C. Comfort, *The Tangled Field: Barbara McClintock's Search for the Patterns of Genetic Control* (Cambridge, MA: Harvard University Press, 2001), 153.

55. Mel Green, "Annals of Mobile DNA Elements in Drosophila: The Impact and Influence of Barbara McClintock," in *The Dynamic Genome*, 117–22; Dash, *Triumph of Discovery*, 86–87.

56. Barbara McClintock to Dr. J. R. S. Fincham, May 16, 1973, Barbara McClintock Papers, National Library of Medicine; Keller, *A Feeling for the Organism*, 137–42.

57. Green, "Annals of Mobile DNA Elements in Drosophila," 117–22; Ira Herskowitz, "Controlling Elements, Mutable Alleles, and Mating-type Interconversion," 289–97; Nina Fedoroff, "Maize Transposable Elements: A Story in Four Parts," 389–415, in *The Dynamic Genome*.

58. Comfort, *The Tangled Field*, 1–16, 32–68; Keller, *A Feeling for the Organism*, 10.

59. Comfort, *The Tangled Field*, 246–47.

60. Heldke, "John Dewey and Evelyn Fox Keller," 104–15; Keller, *A Feeling for the Organism*, xxii.