

## 5 Maria Goeppert Mayer and Rosalind Franklin: The Politics of Partners and Prizes in the Heroic Age of Science

One may ask as a last question whether the fact that she was a woman had any influence on her career as a scientist. This does not appear to be the case. She possessed a unique combination of feminine charm and a keen analytical mind. She was happily married with a man of similar inclinations and tastes with whom she could collaborate very closely. Finally, she was sought out as a co-equal discussion partner and friend by such greats as Max Born, Eugene Wigner, Edward Teller, and Enrico Fermi. She was cast from a similar mold.

—Eulogy by physicists Willard Libby, Bernd T. Matthias, Lothar Nordheim, and Harold Urey upon the death of Maria Mayer, 1972<sup>1</sup>

Our efforts have been largely complementary, and one without the other would not have gone as far as in combination.

—Carl Cori on his work with wife and fellow Nobelists Gerty Cori, Nobel Banquet Speech, 1947

If she had had someone to talk to, chances are she would have gotten to DNA first; it was all there in her notes and photographs . . . in no sense was she in the club, anyone's club . . . Franklin was something of a permanent freak in their midst.

—Vivian Gornick, *Women in Science: Then and Now*, 2009

It was 1948 at Argonne Laboratories, just outside Chicago. For two years the theoretical physicist Maria Goeppert Mayer had been working with her colleague and friend Edward Teller on one of the most basic questions in the physical universe: the origins of elements, starting with the nature of neutronic matter after the Big Bang. Teller hated working alone; he had recruited Mayer because he knew no other person with equal mathematical skills and a tolerance for his musings. But to his irritation she had become sidetracked by the peripheral problem of “magic numbers.” Back in 1933 Walter Elsasser had noted them but hadn't figured out their significance. Years later, when she took up the question again, Teller recalled her fascination:

In examining the facts our theory had to explain, Maria noticed that those nuclei that had either 2, 6, 14, 28, 50, 82, or 126 protons or neutrons were far more abundant than nuclei with not very different proton or neutron numbers. . . . If both the neutrons and protons were of a “magic” number, the isotope was particularly abundant.

That seemed like a detail to me, but Maria thought that the regular repetitious appearance of these abundances must have an interesting explanation in itself; whether it was connected with the origin of elements was not the issue. I persisted in disparaging her interest until finally she lost her temper.<sup>2</sup>

Teller left for Los Alamos, leaving Mayer to ponder the magic numbers on her own. Helium, argon, and xenon were all gases that didn’t readily change into other elements, and they were highly stable, with magic numbers of electrons tightly bound to the nucleus. Abundance was a factor in these numbers, to be sure, but so were elements’ spins, binding energies, and magnetic moments. Were beta-decay properties or quadropole moments also factors? Mayer was mulling over her numbers in her office with Enrico Fermi, when he got up to take a long-distance phone call. He turned in the doorway as he was leaving: “Is there any indication of spin-orbit coupling?” Mayer chewed on his suggestion, even though it ran counter to the commonly accepted “liquid drop” model of the atomic nucleus. Most scientists assumed that coupling was very weak, but Mayer had lived with the data so long that she could immediately see how much Fermi’s suggestion explained. She scribbled equations madly. When Fermi returned she explained her epiphany in rapid fire: In the nucleus the intrinsic spin of every nucleon is strongly coupled to the angular momentum of its own orbit. The actual nucleus was that in which the protons and neutrons were at the level of lowest energy permitted by the exclusion principle. Fermi was skeptical and left her to her numerology.<sup>3</sup>

That night the children eagerly awaited their mother, but Mayer was too consumed to talk with them. She and her husband, chemist Joe Mayer, poured cocktails and went through the notes she had amassed on her magic numbers. Cigarette after cigarette burned as they plugged one element after the next into equations that confirmed she was on to something, and days later she returned to Argonne with the write-up of her shell-model theory of the nucleus. Now with more time to look at her ideas, carefully laid out, Fermi agreed that her theory was too el-

egant to be wrong and started teaching the model to graduate students as if it had long been accepted.<sup>4</sup> Fourteen years later that model won a Nobel Prize for Maria Mayer.

When Mayer was an instructor at Columbia, she had a colleague named Chien-Shiung Wu, an experimental physicist, who seemed well positioned to win a Nobel Prize of her own. She had come from China in 1936 to work with Ernest Lawrence at Berkeley and had become a leading expert on fission and beta decay by the time she submitted her PhD thesis in 1940. Administrators at Berkeley hesitated to offer her a position, not only because she was a woman, but also because of strong anti-Asian sentiment on the West Coast. She moved east to teach women at Smith College, just as her expertise in nuclear physics opened up unprecedented opportunities for research. She was offered replacement positions for faculty at Princeton and MIT who had left for war work, and in 1944 she joined Manhattan Project scientists, including Mayer, at Columbia. Mayer left for Chicago in 1946, but Wu stayed on and became a full-time, albeit untenured, faculty member in 1952.<sup>5</sup>

Junior colleagues moved up the ranks more quickly, but Wu never complained. Tsung-Dao Lee was fourteen years her junior and tenured when he walked into her office in 1956 to seek her expertise on weak interactions, for he wanted to prove that parity was not conserved in them. Wu was singularly able to produce experiments requiring conditions at absolute vacuum and near absolute zero, and she agreed to take up his problem. For the following six months she worked tirelessly in New York as well as Washington, since the equipment and staff she needed for low-temperature spin polarization was at the Bureau of Standards. Her experiments were elegant in their simplicity, beautiful in their definitiveness. Observation of beta particles given off by cobalt-60 made it clear that there was a preferred direction of emission; parity was not conserved for this weak interaction.<sup>6</sup>

Word of her findings spread quickly; she posed with Lee and Yang on the cover of the *New York Times* and was featured in *Newsweek* and *Time*. The Nobel Prize became a foregone conclusion ten months later, but she did not join her colleagues in Stockholm. By the time her group had written up results, others had closed in; apparently it was easier to differentiate the originators of the *idea* to disprove parity than to tease out the first and best experimentalist on the case. Physicist Noemie

Koller echoed the sentiments of many who believed that Wu deserved the prize and that sex discrimination was to blame. She “straightened up a big mess in physics quite elegantly,” Koller concluded, but Nobels are not awarded for cleanup; they go to the conspicuous parties once all the dust has cleared.<sup>7</sup>

If there is truth to the lore of how Wu’s mentor, Ernest Lawrence, came up with the cyclotron that won him his Nobel Prize (1939), it may provide insight into the dynamics that worked against her. Reportedly Lawrence sat in a Berkeley library reading a paper on accelerating charged particles when the idea of the cyclotron burst forth. He was so sure that he had hit the mother lode that he boasted he was going to be famous—and he wasn’t wrong. The pure ideas that came to mind like a flash of light—the theory of relativity, nuclear fission—weren’t supposed to come to the plodding and deliberate minds of women. Scientific virtuosity would not describe Marie Curie’s years of burning off pitchblende or Wu’s meticulous building of apparatus. Nobel Prizes went to the men who theorized the ideas women conscientiously confirmed at the bench.<sup>8</sup>

Of course none of this explains what happened to Lise Meitner, the theoretical physicist who did indeed come up with the concept of fission. She had been one-half of a thirty-year partnership with chemist Otto Hahn, but she was a Jew and forced to flee Berlin in 1938, leaving her uranium experiments behind for Hahn and his assistant, Fritz Strassman, to run. She settled in Stockholm and, from a distance, remained the “intellectual head” of the group, at least as Strassman described it. That winter Hahn sent her a letter about the results of his latest experiments. He had bombarded uranium with neutrons and produced barium fragments that he couldn’t explain. Meitner figured that the barium was a product of an unknown process and stewed over the problem until she conceived what’s known as fission. Hahn won the Nobel Prize for the discovery in 1944.

In dismissing Meitner’s intellectual contributions to his Nobel-winning work, Hahn also dismissed her part in all the knowledge he had acquired to that point. Together they had discovered thorium in 1908 and protactinium in 1917, and together they had conducted studies on nuclear isomerism. When Hahn was conscripted into the German army during World War I, Meitner was left to isolate protactinium on her own, yet she listed him as first author when she reported the discov-

ery. In the case of fission, he did not return the same courtesy, despite the fact that she had been running uranium experiments for four years before fleeing to Sweden. When peers raised the question of her Nobel-deservedness, Hahn called fission a problem of experimental chemistry, not theoretical physics. His scientific arguments were flimsy, but other factors probably contributed to his minimizing her work. Hahn was an iconic figure, a former war hero, whose successful science improved the collective psyche of Germans during World War II. To credit Meitner for his discovery not only would have tarnished his mythic status, but also would have necessitated confronting the persecution of a Jewish woman, thus opening a political Pandora's box. A recent biographer believes that Hahn and the German establishment thus colluded to keep the box closed.<sup>9</sup>

Nevertheless, the real rub for Meitner was not losing the Nobel, but gaining, in references to her thereafter, the label of Hahn's *Mitarbeiterin*, a German term whose meaning is closer to the English "assistant" or "subordinate" than to the "partner" she was. Marie Curie suffered a similar reduction in status in relation to Pierre, as did Rosalind Franklin in relation to her King's College colleague Maurice Wilkins. Whatever the realities of the relationships between these pairs of scientists, history has tended to obscure the contributions of women, who have rarely been seen as in charge. Collaborative science is the norm, yet Nobel's policy of rewarding a prize to no more than three individuals in any field continues to obscure the contributions that men—and especially women—make to discovery. The inability to tease out one scientist's contributions from another's isn't inherently sexist, and yet the idealization of certain individuals has made it possible to overlook those who have not had comparable titles or salaries and thus have worked invisibly. Historically, women have suffered such anonymity. As late as 1974 the Nobel Prize for the discovery of pulsars went to astrophysicist Anthony Hewish, though Jocelyn Bell, his graduate student, first noted the stellar radio source he later identified. Cosmologists wondered whether she too should have received the award, but Bell humbly dropped the issue. Students don't win Nobel Prizes, she insisted, though male graduate students have proved otherwise.<sup>10</sup>

Back at the turn of the twentieth century, biologist Nettie Stevens (1861–1912) proved that the x and y chromosomes in beetle sperm determined sex in a fertilized egg. Her observations were duly noted, but over

decades and with mounting studies by male biologists, her pioneering insights have been omitted from textbooks. Stevens didn't begin studying science until she was thirty-five, and she died nine years after earning her PhD, well before establishing an esteemed reputation. Most of the credit for her work thus went to lab director E. B. Wilson and her graduate mentor, Thomas Hunt Morgan, who won a Nobel Prize for pioneering the field of genetics.<sup>11</sup>

This is not to say that men haven't been snubbed: the Nobel Committee passed over Oswald Avery and Erwin Chargaff before overlooking Rosalind Franklin in her same area of DNA research, and examples abound in other fields. The point is not that women have been cheated exclusively but that their recognition has been disproportionately omitted, even when sex discrimination has been inadvertent. The Nobel Committee's preference for discrete discoveries rather than contributions to knowledge leading up to discoveries has also limited the recognition of women, for we know more cases of men's harvesting what women have sown than the other way around. When Maria Mayer came up with her shell model, she, too, could have fallen prey to men eager to usurp her ideas. She likely would have let them, since she was self-effacing to a fault; in seminars she stopped talking no matter how rude a man's interruption. "She felt she had to be a 'lady,'" her daughter, Marianne, explained, "... if someone else did some work, she always let them publish it first and then she published her additions afterward. She was always very conscious that she had to behave properly if she was not to be accused of being a conniving, abrasive woman."<sup>12</sup>

When Mayer saw the unpublished paper of German men that suggested that they, too, independently had worked out the shell-orbit model, her impulse was to defer to them. She wrote the group leader, J. Hans Jensen: "Wigner, who does not seem to like the shell model, made the comment 'If Jensen has done it too it is probably right.'" Jensen responded: "You have convinced Fermi, and I have convinced Heisenberg. What more do we want?" Still, she delayed submitting her findings until she and Jensen could publish together in the *Physical Review* in 1950. Over the next several years they corresponded back and forth, fine-tuning their ideas for a definitive book on the shell-orbit model, but the division of work was hardly 50-50. Jensen had an "aversion against writing manuscripts" and procrastinated badly. When *Elementary Theory of Nuclear Shell Structure* was finally in print in 1955, Maria was named

first author; Jensen conceded the courtesy for a work that was largely hers.<sup>13</sup>

Eight years later the greatest honor bestowed in all of Western science was Jensen's as well as Mayer's; they made independent discoveries and came together to reap the rewards as a team. As the first American woman to win a Nobel Prize in physics (she had become a U.S. citizen in 1933), Mayer begs the question, If she was so deferential to men in her field, why was her fate different from Meitner's, Wu's, and who knows how many other women physicists'? Why did *she* win the Nobel Prize and not the others?

A pattern emerges that may provide a clue: when we look at the women who won Nobel Prizes before 1970—the Curies, Maria Mayer, Gerty Cori, and Dorothy Crowfoot Hodgkin—we see that their close proximity to elite male scientists was a factor in their science-winning distinction.

Their experiences run counter to those of most women scientists. In general, single women, seen by employers as unburdened by a husband's relocation or children's needs, have fared better in science than married women. Harriet Brooks, an expert on radioactivity who worked with Ernest Rutherford at Cambridge, took a job, as so many women did early in the twentieth century, teaching at Barnard, a women's college, and found that even there women were dismissed upon marriage. In 1923 all the women listed in *American Men of Science* were single. Marriage and scientific career were mutually exclusive choices for women but not for men, and statistically this has remained the case. In 2009, studies confirmed that 70 percent of male science faculty were married with children versus only 44 percent of women at the same ranks. Twelve years after receiving their doctorates women were more than twice as likely to be single and significantly more were likely to be divorced. Such statistics cannot be blamed on lifestyle preferences. Forty percent of the women surveyed regretted that they hadn't had more children; in their minds, it was still not possible for them to have it all.<sup>14</sup>

And yet at the very highest echelons of science, where Nobels are won, marriage has seemed to be an asset. Connections to men have been crucial—not because the work of women laureates has not been worthy in its own right, but because without such connections, sexism would have prevented these women's access to the channels of visibility that their husbands provided. Margaret Rossiter confirmed that, while

there were proportionately more single women in science in the 1920s and 1930s than today, a disproportionately high percentage of married women listed in *American Men of Science* were “starred” as exceptional. In such successful careers as Maria Mayer’s, marriage served as a convenient social cover for professional interactions. Mayer benefited from a husband who was a scientist, but also from the social permissibility to form meaningful relationships with married male scientists who could not have worked as closely with an unmarried woman. That’s not to say that her relationships with elite men were *always* enabling. Those whom Mayer knew intimately protected and propelled her, but the male establishment also found ways to exploit her. Such married scientists as Esther Lederberg, Isabella Karle, and Ruth Hubbard watched their husbands win Nobel Prizes for their joint research. Indeed, their experience suggests that marriage could in fact obscure women’s contributions, unless they established a bit of distance from their mates. To understand Mayer’s success is to understand how she worked out this balance, but also to qualify it and to view her failures and frustrations along the way.<sup>15</sup>

### Part 1: Maria Goeppert Mayer: A Tale of Proximity and Patronage in Elite Science

Maria Goeppert’s road to the prize started back in Göttingen, Germany, where her father was a pediatrician and university professor, and her mother a faculty wife. The Goeppert men had been university professors for six generations, and because Maria was an only child, Friedrich Goeppert expected her to continue the tradition of excellence. “There never was any question of my being able to go on to the university and study whatever interested me,” she recalled, though this was taking a lot for granted at the time. Very few schools in Göttingen prepared girls for higher education, and so she moved from one private institute to another until her parents enrolled her in a suffragist-run *Frauenstudium* that trained women for college. When it closed before she had completed her studies, she took the entrance exam for the university a year earlier than boys her age.<sup>16</sup>

Goeppert’s contemporaries Chien-Shiung Wu and Elizabeth Rona tell similar stories about their formative years as physicists-in-training, all of them attributing their early education to the unconventional



thinking of their fathers. A leitmotif in the early recollections of elite women scientists is the dad who ran outdoor experiments and tinkered with gadgets with the daughter he treated like a son. He encouraged college training, even when Mom worried that her daughter's marriage prospects would suffer.<sup>17</sup> Sociologist Alice Rossi noted the strong father-daughter bond among world-class mathematicians, and certainly women physicists have echoed Mayer's perception that professional fathers had more to do with their scientific proclivities than had their domestic mothers. Anne Roe noted the same in *The Making of a Scientist* but went one further, blaming intervening mothers for failed scientific careers, nullified before they had begun. In this she didn't sound different from Philip Wylie or other social scientists who blamed reproducing women for postwar social ills; this time Mom was the one who prevented the potential Nobel Prize winner from saving the world.<sup>18</sup>

There is truth to the studies, insofar as the majority of fathers of Nobel Prize-winning scientists through the early 1970s were physicians, engineers, science teachers, or researchers in their own right. One study in 1954 confirmed male scientists' social and emotional distance from their mothers and noted that, in the rare instance of female scientists, one could find an even greater alienation from female parents. Even female science majors at Ivy League schools at the end of the twentieth century typically had fathers, not mothers, in their respective fields. But more important perhaps are women's *perceptions* that scientific talents are paternally influenced and acquired, for they reveal a culturally pervasive tendency to associate scientific ability with masculinity. Goeppert felt more affection for her father than for her socially graceful mother. "My father was more interesting," she told a biographer. "He was after all a scientist." In this she echoed her father's belief that domesticity dulled the mind and stifled the intellect. Friedrich told the parents of patients in his pediatric practice that mothers' coddling harmed children, for their fear of the larger world inhibited curiosity and risk-taking, traits essential to great leaders and thinkers. He vowed he would be a liberating influence in his daughter's life. Instead of buying baby dolls, he designed special lenses so that she might observe a solar eclipse. "Never become a woman!" he implored. A life of marriage and motherhood was not enough in his eyes. She needed interests and the ability to think as men could.<sup>19</sup>

Maria Goeppert internalized the message thoroughly and couldn't

be bothered ingratiating herself with female classmates, none of whom went on to the doctoral degree. She socialized almost exclusively with male students, not an uncommon practice of Nobel Prize-winning women. Barbara McClintock, Rita Levi-Montalcini, and Rosalyn Yalow recalled that from early childhood they also preferred the company of males. Mayer thought that men provided stimulating conversations without the time-consuming intimacy women expected afterward. In later years she justified her preference as one simply born of convenience. "I've never had time for science, my family *and* kaffe klatsches . . . Large groups of women tend to get shrill." The more interesting challenge was engaging men: "to keep up with them was wonderful."<sup>20</sup>

While Goeppert didn't view her mother as a primary influence, in the end she was not a liability either, for Mrs. Goeppert hosted lavish dinner parties for Göttingen faculty that her daughter eventually matched, despite herself. Such social entertaining as well as family relationships were helpful to Maria's success. The Goeppert family was connected to some of the most revered men in Göttingen science; Max Born and James Franck were her parents' friends, as was the esteemed mathematician David Hilbert, who was mentor to Emmy Noether and who invited fifteen-year-old Maria to his Saturday lectures.

Maria Goeppert's early introduction to campus life made it less shocking to people when she entered the university formally as a student in 1924. At the time the ratio of men to women studying in any field of the German university was ten to one, a ratio far below that in the United States. In the sciences and especially in physics, women were even more rare. And yet Goeppert became friends with Arthur Compton, Wolfgang Pauli, Werner Heisenberg, Leo Szilard, Linus Pauling, and Edward Teller—all at Göttingen. In Max Born's advanced theory seminar she sat with Paul Dirac; Eugene Wigner; Enrico Fermi; John von Neumann; and Robert Oppenheimer, the insuppressible American. Cecilia Payne-Gaposchkin and Marie Curie never forgot the embarrassment of sitting in a reserved section of the physics lecture halls of the Cavendish and the Sorbonne, but Goeppert never felt the same sting of ostracism. After Born's seminar she joined the men for a walk in the hills and dined with them at rustic inns. Born swore that she got through his courses "with great industry and conscientiousness, yet remained a gay and witty member of 'Göttingen society.'"<sup>21</sup>

The responses that Maria Goeppert elicited from male scientists

were very different from those provoked by prominent female contemporaries. Barbara McClintock exuded a pixielike androgyny in the genetics lab at Cornell, and Annie Cannon presented herself as the hospitable matron of the Harvard Observatory. Lise Meitner, too, was devoid of sexual allure and remained single all her life—"married" to her research, as male colleagues saw it. But men in Göttingen thought Goeppert radiated youth and femininity, with her slim figure, strawberry hair, and lively blue eyes. She laughed, she danced, and she fraternized. Victor Weisskopf and Robert Mulliken admitted to being seduced by her charms. "For most of these scientists, they'd never met a woman who was as intelligent as they were," her daughter explained, adding to her myth, "and then to have a woman who was very definitely a woman—it was an unbeatable combination." Some wondered why Born took so much interest in the young, attractive pupil, but their relationship was like so many she cultivated throughout her life. Her doctorate examination committee consisted of Born and future Nobel laureates James Franck and Adolf Windaus, men whose reputations intimidated those not already made timid by the intense *Doktorvater-Studentin* apprenticeship of the German university. Graduate students deferred to their masters to the point of worship, yet Goeppert won their affection and respect.<sup>22</sup>

In 1927 Goeppert met an American, a Rockefeller Foundation fellow named Joseph Mayer, who had come to study quantum mechanics under Franck. The son of a bridge engineer, he had gone to Caltech and Berkeley and had cultivated an easy manner that defied the stereotype of the cloistered lab rat. To his German peers he was a man of the Jazz Age: his dress was stylish and he indulged in the local nightlife. He came to the Goeppert home in 1927 to inquire about a room: "I went and rang the doorbell, and a pretty little snip of a girl came to the door, and wouldn't talk German to me. Her English was perfect." She was "a terrible flirt," he recalled, "—but lovely, and brighter than any girl that I had ever met." Before long they were out dancing almost every night.<sup>23</sup>

Like Goeppert's father, her future husband had little tolerance for uninspired women; he loved that she needed the stimulation of science. When he asked her to marry him in 1929 Joe Mayer didn't go on about how happy she would make him. He would make *her* happy, he promised, since he wouldn't make her give up physics. Maria admitted years later that she likely would not have continued toward the doctorate de-

gree without Joe's prodding her. On Christmas Eve, as she worked in the kitchen to prepare a holiday meal for friends, she realized that she desperately missed the maid. "If you go on in science," he told her, "I will always keep a maid for you." When she procrastinated about writing her dissertation, Joe took her to the Netherlands to talk the thesis through with the great Paul Ehrenfest, whose solution was to lock her in his guest room and force her to write. Goeppert sat in the room alone, admiring the walls, which had been signed by famous scientists who had stayed there. Einstein's signature was prominent as one of Ehrenfest's closest friends. Having grown up in Göttingen, Goeppert had taken for granted the privilege of watching the quantum revolution emerge before her eyes. Her proximity to luminaries was unique, and she had the natural gifts to move among them. "I solved my problem within an hour," she remembered. "It was the basis for my doctoral thesis." Eugene Wigner called the work "a masterpiece of clarity and concreteness." Decades later, with the greater light intensity of lasers and the development of nonlinear optics, physicists confirmed much of what she had hypothesized in 1929.<sup>24</sup>

Maria married Joe in January 1930 and got her PhD two months later. She was twenty-four years old, and, like other women, couldn't get a job in the academy that had trained her. Margaret Maltby, the first woman to receive a PhD at Göttingen, or any German university, ended up leaving Germany to head up the physics department at Barnard, where she wasn't promoted to the rank of associate professor until the age of fifty-two. Emmy Noether took an unpaid appointment at Göttingen at the age of forty-one but then was fired and forced to leave for the United States, where she secured a job at Bryn Mawr, a women's college. Lise Meitner and Hertha Spöner, Göttingen's second privatdozent after Noether, also went to the United States to secure academic positions. Mayer followed precedent: she and her husband emigrated to Baltimore, where he assumed a tenure-track position at Johns Hopkins.<sup>25</sup>

No one there had her expertise in quantum mechanics, but the university was no different from nearly all coeducational colleges throughout the United States, particularly during the Depression, in its refusal to hire both husband and wife into full faculty positions. Antinepotism policies were ostensibly implemented to prevent the hiring of unqualified spouses into positions better occupied by other applicants, yet they invariably quashed the careers of qualified women married to similarly

or less-credentialed men, rather than the other way around. Faculty wives typically fell into unpaid assistantships or “volunteer” positions that gave them access to facilities in exchange for heavy teaching loads. They worked in labs that resembled male fiefdoms and felt fortunate to get credit on publications or assurances of continued employment. Mayer was grateful to get workspace in the attic of the physics building, where she took on graduate students, Robert Sachs being her devoted first. Unofficially, she was paid a few hundred dollars a year to help faculty with their German correspondence, and over time she gave graduate lectures on a “voluntary” basis. Her courses were listed in the university catalog under “G,” a single initial for her maiden name.<sup>26</sup>

At Hopkins, Mayer had the professional status of a candy stripper, but she chose to look on the bright side. It was a rare opportunity to be a fly on the wall, taking in all she could about as many areas of physics as possible. Again, she was in the presence of stimulating men. James Franck had come to Hopkins, and Edward Teller and George Gamow were in Washington, convening seminars she attended. She teamed up with experimentalists and men of the math department. Karl Herzfeld, a theoretician of kinetics and thermodynamics, helped Joe Mayer move her into physical chemistry. She collaborated with Herzfeld’s graduate student Alfred Sklar, applying methods of group theory and matrix mechanics to the structure of organic compounds. Soon she was publishing with Robert Sachs on meson exchange.<sup>27</sup>

Her dealings with students reveal her unique status as a woman physicist. All were young men and in awe of her command of theoretical concepts, but they also found her more approachable than they did the great men in their field. Sklar admitted that he felt more comfortable asking her for letters of recommendation or tips on real estate than he did the male faculty; she wasn’t the father figure evaluating his every move. He wrote her in 1944: “Mrs.’ doesn’t fit; ‘Dr. Mayer’ is not you; Dr. Goeppert Mayer is too formal for words. Most people I know solve the dilemma by resorting to Maria.” He couldn’t imagine, however, calling her husband anything but “Dr. Mayer.”<sup>28</sup>

The volunteer physicist had proved her indispensability in the department and had found advocates among her colleagues. Herzfeld thought her so ruthlessly exploited by the university that he paid her out of pocket a modest compensation. When he requested her name be added to the departmental letterhead, the dean of physical sciences

took offense and removed all names but his own. Mayer paid no mind to the politics; without a formal appointment or voting privileges she felt free to leave campus when the need arose. She spent her first three summers away from Hopkins in Göttingen to resume work with Max Born, a collaboration that resulted in a groundbreaking paper in the *Handbuch der Physik* in 1933. She developed theories of double beta-decay based on Fermi's formulation of nucleon-neutrino interaction and published on the excited electronic states of the benzene molecule. Colleagues were struck by her intellectual range.<sup>29</sup>

Life in Baltimore was not all science; she hosted a steady stream of anti-Nazi defectors in her home and had a baby in 1933. Mayer morphed into the doting first-time mother her father had vilified. She took time off from science during her daughter's infancy to be the full-time parent her mother had been for her, content to keep up with the literature at home. But during her second pregnancy with Peter in 1937, she spent her final trimester at home and was struck with the same epiphany she had had making Christmas dinner in 1929: that she really had little tolerance for domesticity. She learned to loathe coming up with daily menus and humoring neighborhood mothers with empty chitchat. Her frustration mounted whenever she had to leave campus to nurse sick babies at home. When Marianne was a newborn, Mayer had gradually worked back up to a one-third-time teaching schedule. Immediately after Peter was born, however, she went back to work "with a vengeance."<sup>30</sup>

Joe Mayer made sure during the second pregnancy that she had the stimulation of a project to occupy her—a textbook on statistical mechanics that he worked on with her at night. Benefiting from the editorial eyes of both a chemist and physicist, *Statistical Mechanics* became an instant classic, the most comprehensive work in the field then and for decades afterward. It also had the unintended effect of catapulting Joe's career and doing little for Maria's, for those who didn't know better assumed that she had been an editorial assistant rather than a coauthor. Joe accepted an associate professorship at Columbia in 1939 at twice his Hopkins salary, hoping the change would also benefit his wife. Harold Urey, the chair of Joe's new department, rustled up the title of "lecturer in chemistry" for Maria, since the publisher of *Statistical Mechanics* couldn't stomach printing a book cover with no academic title following her name. The unpaid position was all she could get at Columbia for the first two years. She applied for a full-time job in the physics department,

but when the chair refused her, she settled for office space and unpaid teaching assignments.<sup>31</sup>

The move to New York seemed to be unrewarding for her, but then Max Born assured her from overseas that her underemployment wouldn't lead to boredom, since Urey, Teller, and Fermi were there. Mayer enjoyed these men on campus, but they were also her neighbors in Leonia, New Jersey, the suburb where she and Joe settled with the kids. On any given weekend Teller shot spitballs with Peter Mayer, while Fermi talked history with Marianne; everyone's kids played together. With the help of caterers, Mayer extended hospitality to colleagues who convened in New York for science meetings. Graduate students who couldn't afford hotels found it most invigorating to talk physics at the Mayers' breakfast table with luminaries in the field. In offices on campus or on sofas in suburban dens, Maria Mayer chain-smoked with men and ruminated on the mysteries of atoms.<sup>32</sup>

Some of their American-born spouses were unimpressed. Laura Fermi noted the tensions when Maria Mayer talked "technical" with the men, and couldn't understand why Americans separated the sexes in social settings. Husbands went to stag parties and left "poor young wives to mope at home" and then expected their women to plan luncheons with faculty wives who were virtual strangers. Maria Mayer avoided the Tupperware parties and became the focus of envy and disgust. Joe wondered which women had intervened to have her excluded from the dinner that followed his department's weekly seminar. Maria urged Joe to say nothing about it and happily accompanied Edward Teller to the opera instead.<sup>33</sup>

In 1941 Mayer was honored with election into the American Physical Society, but her status at Columbia didn't change. She accepted a part-time salary at Sarah Lawrence College to teach math and physics courses to nonscience majors and soon taught double the load of introductory courses and chaired the entire science department. For all the responsibility, she was unable to secure a permanent contract or full pay. Max Born remained encouraging: "Your influence will lift the standard of science amongst the wealthy girls of Sarah Lawrence skyhigh." By all accounts it did. Administrators worried that her highbrow theory wouldn't prepare students for their more likely need to regulate the flue on a home furnace. Mayer asked them if women learned English only to read cookbooks. It was the beginning of the atomic age, and she had

every intention of teaching the same lessons in nuclear physics that she taught to men at Columbia.<sup>34</sup>

Mayer's teaching was interrupted when the United States declared war on the Axis powers and physicists were called to defense work, regardless of their sex. As a Manhattan Project manager, Urey carved out for Mayer a position in Columbia's Substitute Alloy Materials (SAM) Laboratory. The appointment had the potential to thrust her into a position of visibility, as war work did for young male physicists. She was better compensated than ever before and had a team of researchers who answered to her. But the war's timing coincided with teaching and the rearing of small children. She arranged for the English nursemaid to work longer days but insisted on being home to read to the kids before bedtime and on being with them on the weekends. Urey readily agreed, assigning her the more peripheral work of isotope separation by photochemical reactions. Soon Edward Teller asked Mayer to join his Opacity Project, and she agreed to take it on. Looking back, her daughter thought it a good thing, since her dad was at Aberdeen Proving Ground doing weapons research, and her mom was "actively miserable without him," her only solace to be completely immersed in work.<sup>35</sup>

After the war Mayer returned to teaching at Sarah Lawrence and spent time tutoring Marianne in German and addressing Peter's poor performance at school. Her days were full, but she wanted more meaningful science. Joe had just received word that he had been elected to the National Academy of Science and offered a position at the University of Chicago; again, he hoped for his wife that the change would be for the better. Everyone who was anyone in atomic physics was moving or had moved to Chicago, including most of their Leonia neighbors. But nepotism policies proved equally stringent there. Mayer was given faculty status, but again no pay, even though she worked in a separate department from Joe's. She was a "volunteer associate," which meant that she handled committee work, teaching, and advising duties in the physics department for free. Nevertheless, Robert Sachs, now the head of the physics division at Argonne, offered her simultaneously a part-time position as a senior physicist. Mayer was pleased with the spacious office and "nice consulting salary," but more important, she no longer felt like an appendage. She enjoyed unprecedented access to resources and took solace in knowing that faculty friends were fighting for her to earn a full-time salary.<sup>36</sup>



Mayer's life in Chicago was nothing like Marie Curie's anti-natural path. She threw the grandest of parties in their three-story South Side mansion, decorating lavishly and entertaining hundreds of people. Younger scholars looked up to the Mayers as the epitome of sophistication. For Maria the hospitality was effortless, and she was also the easiest of guests. Before a symposium on high polymers, colleagues convened at a Kyoto inn and drank sake as geisha girls danced around them. Women's designated work at the event was as entertainers, for female chaperones escorted the scientists to the public baths. And yet Mayer was, as always, too graceful to appear out of place. When colleagues threw stag parties, she wore slacks and joined them. Male scientists consequently decided she was one of them, electing her to the Akademie der Wissenschaften in Heidelberg and the National Academy of Science. She was the fifth woman to receive the latter honor—a decade after Joe had been elected. She collaborated with Jacob Bigeleisen on isotopic exchange reactions and plugged calculations into ENIAC, the first electronic computer, to figure out criticality for a liquid metal breeder reactor. Some of her fondest memories were of the seminar at the Institute for Nuclear Studies, where Teller sparred with Fermi, Urey talked of the moon, and Willard Libby announced his discovery of carbon dating. She also fortified personal and professional bonds with the quantum theorist Gregor Wentzel, soon to be the father-in-law of her daughter, Marianne.<sup>37</sup>

This was the milieu in which Mayer conceived her shell orbit theory. Men liked her; some loved her, and most were protective of her. Joe Mayer, Fermi, and Teller were three who insisted that she publish alone and immediately, so that her ownership of the theory would never be questioned. Their instinct on her behalf was to strike while the iron was hot. Joe grew so exasperated that he eventually lost his temper: "For God's sake, Maria, write it up!" But she remained reticent; Fermi should be a coauthor, she argued, since he had asked the question that unblocked her thoughts. Fermi refused to see the logic. Were his name anywhere on the paper, he would get all the credit, he promised her, and he had no intention of undermining her efforts. Her model explained irregularities of the stability in nuclei, as well as all the phenomena of nuclear spectroscopy, including beta and gamma decay. Before his death in 1954, Fermi took it upon himself to nominate Mayer for the Nobel Prize.<sup>38</sup>

Mayer's years in Chicago were happy ones, but they ended in 1960 when she was lured away. The deciding factor was not her lack of formal status, but rather that the men she knew had left—Teller in 1952, followed by Libby in 1954, and Urey in 1958, when he accepted a position at the University of California at San Diego (UCSD). Administrators throughout the California system wanted big names to build up its nascent science programs and were amenable to hiring married couples if necessary. Urey convinced them that his longtime friend would bring stature to UCSD, and in 1959 Maria received an offer of full professorships for her and Joe both. She didn't object to her half pay for nine months to Joe's full twelve-month salary; the money was far better than what she had received anywhere else, and the official appointment would allow her to earn more through grants and contracts. Because the position was tenured, she would not have to think about contract renewals. Urey occupied an office near hers in the chemistry-physics building. The California climate was ideal for resuming her cultivation of orchids and joining Joe for lunchtime swims at the beach.<sup>39</sup>

At four in the morning on an early November day in 1963 Mayer got a call from a Swedish journalist, who was the first to tell her that she had won the Nobel Prize for her shell-orbit theory. Joe poured champagne and the two sat on their deck eating bacon and eggs until the sun came up, enjoying the quiet before the storm. The phone rang incessantly for the rest of the day as the media converged on them. Bumming cigarettes from the journalists, Maria was able to get through many hours of lights, cameras, and interviews with composure intact. Urey managed to sneak past reporters to give his friend a tight embrace. "Life will never be the same," he said in her ear. Joe left to sit on a chemistry exam committee and returned wearing an understated short-sleeve shirt and cotton trousers. He had no intention of upstaging his wife at her finest hour. As the press moved to the lawn outside, the two Mayers stood arm in arm answering questions. A journalist asked Joe whether he thought of Maria as a wife or a scientist, as if they were mutually exclusive categories. Incredibly, Joe bothered to answer: "Why a wife, of course, and a very wonderful one." More fiercely than at any other time, he felt compelled to defend her normality as a woman.<sup>40</sup>

Mayer received letters and telegrams of congratulation from scientists all over the world, as well as from local schoolchildren, who were excited to be near the "San Diego mom" who had won the prize. In

one letter, a local woman invited Mayer to stop by some day after three o'clock, since her fifteen-year-old daughter would be impressed to meet a female Nobel Prize winner: "I'll make us some coffee + we can smoke together." The letter seems inappropriately familiar, but then again Mayer's letters from male graduate students also suggest that they had always felt more comfortable approaching her than they did men of her professional stature. As a woman, she was a scientist stripped of all that made scientists imposing. Sometimes she felt like an impostor, as if her Nobel Prize had been won fraudulently—or at least fortuitously. When a fifth-grade girl wrote to ask how a woman could have achieved so much in science, Mayer replied: "I have been very lucky in my career. Lucky in coming to America when I did, lucky in meeting my husband, and lucky in my choice of research." This was not a whitewashed answer for a child's ears. Lucky was truly how she felt.<sup>41</sup>

### **Maria Mayer and Gerty Cori: Rethinking the Nobel Success Story**

It took a Nobel Prize for Americans to understand what a handful of elite scientists already knew about Maria Mayer: that she was one of the most brilliant theoretical physicists in the world. In 1896 Alfred Nobel had left his fortune to fund prizes in the burgeoning fields of physics, chemistry, and medicine, as well as literature and peace. That he united those who made technology with those who made peace testified to his belief that science could and would improve the human condition and that its greatest practitioners deserved any messiah complex that might result from their accomplishments. When psychiatrist Helen Tartakoff coined the term "Nobel Prize complex" to describe certain narcissistic and obsessively ambitious patients, she undoubtedly had scientists rather than poets or peace activists in mind. Each December dignitaries convene in the Stockholm Concert Hall, dressed in formal evening attire. After the Royal Hymn the new class of laureates files to the left of the stage to accept gold medals, on the backs of which are the female forms of *Natura* and *Scientia*. The physicists walk across the stage first, followed by the chemists, the medical researchers, and the nonscience laureates. In 1963 Maria Mayer was a rare woman heading this ritualized procession.<sup>42</sup>

For each Nobel laureate there have been thirteen members elected

to the American Academy of Science and twenty-six listed in *American Men and Women of Science*. Since 1901 the Nobel Prize has been the mark of supreme honor in Western science, and no more so than after World War II. Although economists and writers can also win the prize they understand that their selection may be more subjective than the selection of scientists. Perhaps the difference in perception stems from the positivist tradition of science itself. Scientists are trained to think of phenomena as measurable, of preeminence as empirically knowable. And thus it follows that, as a culturally accepted measure of greatness, the Nobel in science generates authority for its own selection process and for those bestowed with the prize. Upon winning, scientists discover that their opinions hold greater weight, whether in reference to lab work or world peace. After Maria Mayer won her Nobel, she was asked to show support for causes ranging from population control to Lyndon B. Johnson's presidential campaign.<sup>43</sup>

It is not news that Nobels are rare, or that they have been rarer for women. Out of the hundreds of prizes awarded to scientists, we can almost count women Nobelists on two hands, thanks only to the selection of more women of late: Marie Curie (1903, 1911), her daughter Irene Curie-Joliot (1935), Gerty Cori (1947), Maria Mayer (1962), Dorothy Crowfoot Hodgkin (1964), Rosalyn Yalow (1977), Barbara McClintock (1983), Rita Levi-Montalcini (1986), Gertrude Elion (1988), Christiane Nüsslein-Volhard (1995), Linda Buck (2004), Françoise Barré-Sinoussi (2008), Elizabeth Blackburn (2009), Carol Greider (2009), and Ada Yonath (2009). Of these women, even fewer have won prizes without sharing them with husbands, male collaborators, or men whose unrelated accomplishments have merited distinction at the same time. In the rare instance when a woman has won a Nobel, many assume that her dedication allowed her to perform outside her female skin, or, if she is not truly exceptional, that she positioned herself strategically on the coattails of worthy male recipients. Marie Curie suffered the stigma of being described at both extremes.

In either case the underlying assumption is that normal women don't possess the goods to win Nobel Prizes. And thus, in the few instances in which women have won them, their scientific accomplishments have been less newsworthy than their eccentricities, contradictions, and transgressions as typical women. The headlines tell the story: "La Jolla Mother Wins Nobel Prize"; "She Cooks, She Cleans, She Wins

the Nobel Prize”; “The Chemistry-Minded Mother”; “A Nobel Woman’s Hectic Pace”; “British Winner is a Grandmother”; “At Long Last—a Nobel for a Loner”; “Winner Woman”; “An American Mother and the Nobel Prize—a Cinderella Story in Science.”<sup>44</sup> Although most people on the street know Marie Curie won the Nobel Prize, it’s unlikely they can tell you what she won it for. Nobel Prize-winning women are the news, not the science they command.

Nobel laureates are popularly recognized as part of a “knighthood” or a “science aristocracy” for whom, as for any elite class, access to resources has been integral to maintaining status and passing it on to professional progeny. Sociologists confirm that women’s historical inability to “attend superior training centers,” work as “apprentice[s] for master scientists,” and occupy “facilities to carry out their research ideas” has led to their virtual omission from this rarified caste. In 1979 Jonathan Cole underplayed the importance of sexism in women’s poor standing in elite science, but he couldn’t deny that inertia was working against them: “If women are more apt to be in such disadvantaged positions than their male counterparts, then the career histories of men and women of science may be explained in part by processes of accumulation of advantage and disadvantage. . . . the ‘rich getting richer,’ but also ‘the poor getting poorer.’” Men inherit more than their mentors’ connections: they gain also their mannerisms, values, and swagger as members of the professional elite. Once they have achieved modest acclaim, their pedigrees prevent derailment off successful paths. Robert Merton termed this self-perpetuated success the “Matthew effect.” Along with pedigree, the greatest predictors of Nobel Prizes have been the multiple honors—the Laskers, MacArthurs, inductions, and honorary degrees—a man and his mentors have already received. Recognizing the all-too-frequent correspondence that this dynamic has to gender, Margaret Rossiter has exchanged Merton’s biblical Matthew for “Matilda,” as in the suffragist Matilda Joslyn Gage, who believed that Christianity, like science, perpetuated ideas that allowed men to reap what women had sown. As an obscured figure in her own right, Gage herself was a victim of the “Matilda effect.”<sup>45</sup>

Indeed wealth begets wealth in the rarified world of science, and inheritance historically has been patrilineal. Lists of Nobel winners reveal generational patterns: fatherly lab directors followed eventually by their male graduate and postdoctoral offspring. These have been genealogies

in both the figurative and literal sense, since several younger laureates have been related to former winners. Aage Bohr won the physics Nobel fifty-three years after his father; microbiologist Frederick Robbins won eight years after his father-in-law won in chemistry. Alan Hodgkin, a Nobelist in medicine, married the daughter of American laureate Peyton Rous; and Paul Dirac, a laureate in physics, married the sister of another physics laureate, Eugene Wigner, who married the sister of his colleague John Wheeler. The list of inbreeding goes on, suggesting that women have been integral to forging the social bonds that create laureates even when they have not been laureates themselves.<sup>46</sup>

Women in the immediate postwar years could only envy the social advantages of and proximity to mentors that male peers enjoyed in and outside the lab. Thanks to the circumstances of birth and marriage, Maria Mayer was a rare woman who had access to such people. Biochemist Gerty Cori, a winner of the Nobel Prize in 1947, suffered institutional sexism but was also buoyed by a supportive husband who considered her his intellectual equal. Carl and Gerty Cori pioneered the study of the hormonal regulation of blood sugar and the role of insulin in the body's regulatory system, and by all accounts they also enjoyed a most compatible marriage. Carl simply refused to work without his wife, and thus, when sexist and anti-Semitic sentiments prevented her from getting research jobs in Europe, he charted a professional path for them both in the United States, first at New York's State Institute for the Study of Malignant Diseases, and then at Washington University in St. Louis. His decision to work with his wife frustrated his superiors and delayed his own promotion. He refused a tenured position at the University of Rochester, for example, when he discovered that Gerty couldn't work with him in the lab. The department chair chastised her for getting in her husband's way. It was simply "un-American" for a woman to encroach on her husband's turf, he told her, but Carl hardly thought his wife a liability. Her hand in elucidating the process of glycogen metabolism opened doors that he took advantage of on her behalf.<sup>47</sup>

Antinepotism policies and sexist bias conspired to keep Gerty Cori in low-paying positions with little prospect of advancement, but she never complained. She wasn't envious of Carl, especially not when his promotions led to more administrative tasks outside the lab. This was a woman who rushed back from the birth of her son so that she could continue isolating glucose-1-phosphate, soon to be known as "Cori

ester,” in order to trace it to the breakdown of polysaccharides. With infant and housekeeper at home, Cori continued to work full-time and finally was promoted to research associate in 1937, to associate professor in 1943, and to full professor of biochemistry in 1947, sixteen years after Carl had become a full professor and months before receiving her own Nobel Prize. Her promotional ladder was decidedly longer than for men with identical credentials and talent. Awards poured in just as she was diagnosed with a chronic bone marrow disease that was, like Marie Curie’s malady, probably triggered by exposure to radiation. When her hemoglobin fell to lethal levels, Carl stopped smoking to make it easier for her to quit, too. When she became too weak to walk the corridors, she called out for “Carly,” who picked up her frail frame and carried her down to lunch. The two were inseparable until she died in 1957.<sup>48</sup>

In tales of Western science, the Nobel is the fairytale ending. Few historians have written about Cori’s professional struggles before the prize or the pain of chronic illness that quickly followed. Feminists, too, have chosen to highlight the Nobel Prize as the pinnacle of the lives of Marie Curie and Maria Mayer without qualified discussion of how events that preceded and followed gave meaning to their lives. The memoirs of male laureates are formulaic: one’s professional coming of age reads as a male rite of passage, entailing ascent on a promotional ladder through prestigious labs in one’s twenties and thirties. Wives accept their husbands’ lengthy work hours and their meager and sporadic participation in family life. Maria Mayer and Gerty Cori’s twenties and thirties were stimulating years in which they, too, felt enraptured by science, but Mayer also felt inadequate as a mother, and Cori held motherhood off until she was forty to avoid similar feelings of guilt. Unlike men, with their linear progress towards scientific prestige, women scientists may feel torn by competing interests and plagued by institutional constraints.

When one considers women scientists, then, one must think afresh about where greatness begins and how it ends. Maria Mayer and Gerty Cori were like many women who stagnated on promotional ladders or who knowingly derailed careers to tend to children. Sometimes accolades for these women would come later than for their male colleagues. Perhaps they would never receive salaries comparable to their husbands’, or promotions, grants, and honorary appointments until prizes proved their eminence, not the other way around. Mayer was the lone

woman in a physics dynasty that produced forty-three male Nobelists; Cori, the only female among thirty-nine other men.<sup>49</sup> No one disputes their intellectual abilities, but given women's grim prospects in post-war science, it's likely that blood and marriage worked together in their favor. Neither woman would have come to the United States, let alone secured work as a scientist of any kind, had she not had male support. Most women could not counter the sexism of scientific institutions. By comparison, the careers of Mayer and Cori were charmed.

### **Part 2: Single and Snubbed: Rosalind Franklin and the Politics of the Nobel Prize**

In June 2003 James Watson, the president of Cold Spring Harbor Laboratory on Long Island, was a featured guest on the Public Broadcasting Service-syndicated *Charlie Rose Show*. Rose and Watson had lots to talk about. A discovery that would serve as the pinnacle for most careers was just the prelude to Watson's. At twenty-five he had figured out the double helix structure of DNA, and in his thirties won a Nobel Prize; by his forties he was a best-selling popular author and since then had earned honors and appointments and had become an integral part of the Genome Project. In 1990 *Life* magazine listed him and Jonas Salk as the only biologists on its roster of the one hundred most important Americans of the twentieth century. Watson had just filmed a retrospective of his fifty years of research in a documentary film, with a companion book on DNA. Rose could have begun anywhere, and yet he chose to revisit some nitpicky details of Watson's Nobel-winning discovery in 1953:

CR: If you hadn't done it, who would have done it?

JW: Probably Rosalind Franklin or Maurice Wilkins.

CR: . . . Did Rosalind deserve to share the Nobel Prize?

JW: Well, you could argue it either way.

CR: Which way do *you* argue?

JW: (Pause) . . . Well . . . you . . . it was impossible—you could only give a Nobel Prize to three people and there were four of us, so . . .

CR: Who made the decision, the Swedes [Nobel committee]?

JW: Yes. Well, Rosalind was dead, so arguing over a hypothetical thing was . . . was she a good scientist? I'd like to say yes.

CR: Absolutely.



JW: Did she find the structure? No.

CR: Did her photograph—whatever you call it—contribute to it?

JW: Oh, sure, because when I saw it . . . It had to be a helix. . . . Rosalind at that time didn't want to think of helices because Wilkins said it was a helix and you know there was conflict between the two of them.

CR: But he's the one that showed you the photograph, right?

JW: Yes . . . but it wasn't taken the day before, it was taken at least six months before. . . . It was a beautiful picture; it need not [have] been that good for me to have . . .

CR: If you had not found it . . .

JW: She should have found the structure by the time I arrived in England.

CR: She *should* have?

JW: Yeah, because she should have built models. You know, it might not work, but try it. She wanted her approach to work. We'd try any approach. . . . I just wanted the answer. . . . We didn't think of it in those days as Rosalind's problem. I thought it was Wilkins's problem . . .

CR: Everyone wants you to speak about Rosalind, it seems to me.

JW: Yeah.

CR: They almost want you to say, "Well we overlooked, we didn't give her enough credit," seems to be what people want you to say.

JW: We didn't even know Rosalind. Rosalind didn't want to see me. She didn't like me.

CR: Why didn't she like you?

JW: Probably in part because she saw me believing in helices, that's why . . .

CR: Was she discriminated against because she was a woman?

JW: No, I don't think so.<sup>50</sup>

Watson seemed happy to change the subject, but not surprised by the questions; journalists have asked him to defend his discovery of the double helix and to explain Rosalind Franklin's role in it many times. The prevailing narrative in the minds of Americans who know of Franklin is a victimology, a story of a woman cheated of her just rewards. She was an English scientist, not an American, but her story has been converted into a transatlantic tale of sexism that has captured the imagination of

American feminists more completely than that of any scientist in the United States. In the twenty-first century Brenda Maddox has called her “the Sylvia Plath of molecular biology, a martyr whose gifts were sacrificed to the greater glory of the male.”<sup>51</sup> If Maria Mayer’s tale of proximity to men is a success story, Franklin’s is a cautionary one about what happens when proximity is lacking. The events before and after the discovery of the double helix are worth revisiting for the light they shed on the gendered politics of elite science in the postwar years. How significantly can we attribute Franklin’s fate to the masculine traditions of institutional science? Had she been married to a male scientist or protected by male mentors, would events have transpired differently?

Feminist biographers remind us that Rosalind Franklin had the natural talent and masculine ambition to win the Nobel Prize. She was born to an Anglo-Jewish banking family in London in 1920, and like her brothers, she quickly learned to tinker with gadgets. By the age of six she excelled in mathematics. Rather than discourage her from career paths, her parents enrolled her in one of the few English schools that took the occupational training of girls seriously. By sixteen, she had chosen to pursue science. She enrolled at Newnham, the women’s college of Cambridge, and graduated with a degree in physical chemistry in 1941. The Cavendish Laboratory of Cambridge was home to some of the most esteemed scientists in the world, but Franklin did not find that her associations with elite men translated into ready opportunities in the work world, largely because no one took the initiative to take up her cause. She may not have gone further in science except that the British war effort needed scientists who understood the properties of coal in order to maximize its use. She secured a post at the British Coal Utilisation Research Association as an assistant research officer. Her findings there became the foundation of five papers and a PhD thesis that she submitted at Cambridge in 1945. At the age of twenty-six, she had become one of the premier coal chemists in the world.<sup>52</sup>

One of Franklin’s early influences had been Adrienne Weill, a physicist who had worked with Marie Curie and who had left France to avoid the Nazis. A forebearer of modern feminists, she had a daughter but no husband, a sophisticated style, leftist tendencies, a penchant for existential philosophy, and an unshakable sense of who she was and wanted to be. She helped to cultivate Franklin’s love of French people and culture and secured a job for her in the Paris lab of Jacques Mering, who

helped Franklin hone the X-ray diffraction techniques that defined her career thereafter. Franklin thrived in Paris. There Simone de Beauvoir could ruminate on philosophical questions without suffering judgment of her unconventional sex life, and there, too, Franklin could be a respected professional with no love life at all.<sup>53</sup> She developed expertise in crystallography, a lab-centered field that was exceptional for its historic embracing of women. Sir Lawrence Bragg, the originator of the field, welcomed women into his Cambridge labs during decades when they were to be found nowhere else in the university. While the percentages of women in crystallography historically have been higher than in other physical science fields, they haven't been high compared with those in biological or nonscience fields, and thus the characterization of crystallography as "woman's work" likely has had much to do with men's perception of it as a "service" to other science disciplines—as phlebotomy is to medical research or as "computing" was to turn-of-the-century astronomy.<sup>54</sup>

Crystallographers seemed to be the helpmeets of other scientists, and in 1950 good ones were indispensable. J. T. Randall offered Franklin a three-year fellowship to analyze protein structures in his biophysics unit at King's College, London, though privately, lab chief Maurice Wilkins was urging Randall to make the structure of DNA Franklin's primary area of concern. Chemists had already identified the protein bases that made up DNA and had figured out the amino acid sequences for insulin polypeptides. Caltech chemist Linus Pauling was months away from announcing his discovery of the  $\alpha$ -helix. Wilkins was abreast of these developments and felt that he was on the precipice of a major discovery in a field that was innovating daily. He needed someone qualified to capture the X-ray diffraction images of DNA so as to identify the mechanism transferring genetic information. Randall was confident that Franklin could provide what was needed.

But immediately there were misunderstandings. By virtue of Randall's assigning Franklin to DNA, Wilkins assumed that he was in charge, but Franklin had been told otherwise. Raymond Gosling, a graduate student assigned to X-ray work before she had arrived, recalled their introductory meeting. Randall first handed him over to Franklin, then showed her the X-ray diffraction pictures taken to date and told her to resolve the structure. Thus Franklin thought she was heading up a small and independent team of two. Tensions rose immediately.

The more Wilkins tried to tell her what to do, the more Franklin dug in her heels. Wilkins found her “fierce” and overpowering in arguments. Lines of communication deteriorated and then completely broke down. Wilkins complained of her unwillingness to collaborate; she was a lone wolf who hoarded her data, a woman without the grace to concede points when she started to look combative.<sup>55</sup>

If Wilkins found Franklin blustery, it was partly because she had few outlets for airing frustrations. King’s, like Cambridge and like English science generally, was a gentleman’s club to be navigated, not the community she had experienced in Paris. When she joined the biophysics unit, less than 1 percent of the British Royal Society was female; the marginalization of women reverberated down into their day-to-day activities in lab settings. Of the thirty-one scientists on staff in the biophysics unit in 1952, eight were women, and only one or two worked with Franklin directly. Forbidden entrance to the common rooms where male staff smoked and took their coffee, Franklin often ate in the student hall or left campus altogether. Randall was kind, but others were misogynists. “She was used to a civilised intellectual life, discussing painting, poetry, theatre and existentialism,” a graduate student explained to Brenda Maddox. “Now she found herself among people who had never heard of Sartre, whose chief reading was [the] *Evening Standard*, and who enjoyed ‘the type of girls that would get drunk at departmental parties and be passed from lap to lap having their bra undone.’” Franklin put her head down and tended to her experiments. Men mistook her discomfort for haughtiness, severity, an appalling lack of femininity. She refused to flirt or suffer the pranks pulled at her expense—so she was declared an outsider from the start. Men patronized her to make it clear that they doubted her scientific abilities.<sup>56</sup>

Enter the young and self-propelling James Watson. A prodigy of sorts, he had been featured on the national radio program *Quiz Kids*, had entered the University of Chicago at fifteen, and had earned a PhD from Indiana at twenty-two. The literary heroes of science enthralled him; his dreams of emulating Martin Arrowsmith and Paul de Kruif’s *Microbe Hunters* kept him motivated, and Erwin Schrodinger’s *What Is Life* turned his focus to the workings of the gene. In Bloomington he studied the effects of X-rays on bacterial viruses with Salvador Luria and Max Delbrück, men who, ironically, won Nobel Prizes after he had won his. He worshipped Delbrück and tried, like a good disciple, to po-

sition himself next to him at meals in the hopes of taking away nuggets of wisdom. A friendship developed over tennis and beers after summer seminars at Cold Spring Harbor. Luria, an Italian, brushed up on his English by rewriting Watson's doctoral thesis. Looking back, Watson was sure that one of the keys to his success had been removing neckties and using first names early—being seen as an equal to important men from the start. Men formed bonds over cognacs or while grilling in the backyard, but women were shunned when they seemed to mix domesticity and lab work. For young Franklin, informal relationships had not been seen as improper in Paris, but they were viewed so in London. Her relationships with possible advocates at King's remained stilted and strained.<sup>57</sup>

Franklin had no mentors, while Watson's exuded influence enough to cross the Atlantic. Luria cleared paths to his success, securing for him a National Research Council fellowship and the opportunity to get crystallography experience with John Kendrew and Max Perutz at the Cavendish. Perutz's graduate student was the garrulous, thirty-five-year-old Francis Crick, the resident theorist of the unit. He and Watson bonded immediately, perhaps because each fueled the other's designs on DNA structure. Crick explained X-ray diffraction to Watson and introduced him to Maurice Wilkins in October 1951. Wilkins explained to Watson that the X-ray patterns captured at King's suggested that DNA would consist not of single polynucleotide chains but, rather, of two or three helical chains intertwined and bonded to each other. Franklin was the one with the latest data, Wilkins told Watson, and she was presenting her findings in a month's time. Watson made sure to attend her presentation, convinced that unlocking the structure of DNA was his ticket to the Nobel Prize.<sup>58</sup>

Lawrence Bragg, director of the Cavendish, made it clear that DNA was King's problem, but Watson and Crick thought the discovery of the structure a masculine race to be won. To lessen the look of impropriety, Bragg later described the discovery as a combination of Wilkins's patient plodding and Crick and Watson's "rapid fire solution," passing over and ultimately ignoring that Franklin was the only scientist in the world who had consistently been obtaining data on the structure during the two years that led to its discovery. Crick and Watson built models of DNA in a trial-and-error fashion, bouncing ideas off each other until some of them stuck. Their process was social; they talked about

DNA over tennis, at the pub, and during lavish dinners Odile Crick prepared for them. Crick recalled that neither felt compelled to spare the other's feelings in this man-to-man exchange: when one posited a theory, the other sought to "demolish it in a candid but non-hostile manner." Neither feared miscues, short cuts, or dead ends—cocksureness wouldn't let him. Emboldened by what each man had been led to believe about himself as an heir to brilliant men, they had both acquired the "youthful arrogance" to think recklessly.<sup>59</sup>

Looking back, Crick could hardly blame Franklin for proceeding with less abandon. As a woman, she had to be a careful, consummate professional, rejecting rash hypotheses that her data could not yet confirm. Crick believed that what all good scientists needed was a partner who communicated with "perfect candor, rudeness if need be." In the *Eighth Day of Creation*, Horace Freeland Judson lamented that Franklin had never had that partner at Kings: "It is evident from her notebooks that she needed one, and clear from what we know of her character that she would have worked well—candidly, rudely if need be—with the right one." Her graduate student Raymond Gosling was the only associate with whom she occasionally reviewed her findings; he, too, agreed that she needed "someone of her own standing" with whom to have frank discussions. She loved a good scientific argument and never let it get personal, yet her collecting of data at King's remained a long and lonely process.<sup>60</sup>

For Crick and Watson it was no process at all, since they carried out no experiments of their own. Watson understood Franklin's data only superficially, and Crick was frustrated that at the November talk in 1951 Watson didn't take notes, for he recalled inaccurately her highly technical jargon and measurements. Watson's misapprehending Franklin's terms "unit cell" and "asymmetric unit" was responsible for the delay in configuring their final model. Still, Watson was clever enough to keep abreast of Franklin's progress and to pry details of her work out of Wilkins whenever he would come round to complain about Franklin into Watson's sympathetic ear. Watson knew that Franklin's impeccable diffraction technique would ultimately yield the visual evidence to solve the structure, for Wilkins was showing him images of hers that were getting "prettier and prettier" by the day. She had photographs of two DNA forms—A, or "dry," and B, or "wet"—and meticulous notes on the amount of water in each. She concluded that the phosphates were lo-

cated on the outside of the molecule, encased in a shell of water, but the helical structure remained ambiguous. The B-form measurements supported helicalism, but calculations from her A-form images were inconclusive.<sup>61</sup>

Franklin spent a year reconciling the disparity, ultimately deciding that both forms consisted of two helices. Watson, meanwhile, proceeded as if helices were a fact. Early in 1953 Wilkins went into Franklin's lab and pulled out the image she had labeled "Photo 51," the "B form" of DNA. When he showed it to Watson, it made Watson's heart race; the pattern was simpler than anything he had seen before. Although he couldn't ascertain exact measurements, the crosses were formed so clearly in Franklin's image that Watson knew he was holding a portrait of a helix. He pressed Wilkins at dinner for more density values, water-content data—anything that could help him discern how many strands she was dealing with. Linus Pauling had figured three, and Wilkins's limited knowledge of the data did nothing to suggest otherwise. Crick and Watson scrapped their old models and began working on a new one based on the insights provided in Photo 51. They had no idea that Franklin was in the midst of figuring out mathematically what they would soon show with their model; and she was oblivious to the fact that her image had opened the floodgates.

Soon they were helped along by another windfall of her making: Max Perutz, an unsuspecting accomplice, had collected reports for the Medical Research Council (MRC) and saw no reason to deny Watson and Crick's request to see the one Franklin had submitted a year earlier, complete with measurements of the face-centered monoclinic unit cell. The density data supported the theory of two chains, as distinct from Pauling's hypothesis of three. Franklin had concluded that the B-form diffraction pattern appeared helical and likely contained two to four coaxial nucleic acid chains per helical turn.<sup>62</sup>

Crick and Watson's reading of the report has been compared to an enemy's happening to find the other side's codebook. The metaphor would be more appropriate had Franklin been aware of a war being waged. Within days of receiving the MRC report Watson and Crick disclosed their model of the double helix: two intertwined polynucleotide chains on which each adenine nucleotide corresponded to a thymine nucleotide, guanine to cytosine. Hydrogen bonds formed between the two opposite nucleotides at each rung of the molecule, making all of

it consistent with Franklin's data and images. They drafted a paper for submission to *Nature*, despite having no data of their own. Watson's sister typed the text; Crick's wife drew the twisting chains of the helices. When Franklin heard they had figured out the base pairs she took a train to Cambridge to see the tinker toy model herself. She was gracious, even as Watson related it. He supposed that even she, in her stubbornness, could see that "the structure was too pretty not to be true." In the spirit of cooperation she offered a paper containing measurements and images, including the infamous photograph 51, thinking that no one had yet seen it.<sup>63</sup>

Franklin was likely so conciliatory because the model was no surprise; her supporting paper was a simple redraft of one she had already written. Wilkins and team submitted a third paper, "Molecular Structure of Deoxypentose Nucleic Acids," to round out the submissions to *Nature*. Crick and Watson's was the lead paper, the one that announced the discovery, while the others supported their claims. Nowhere in their submission did they include explanations for how they obtained their experimental proof, just a vague aside: "We have also been stimulated by a knowledge of the general nature of the unpublished experimental results and ideas of Dr. M. H. F. Wilkins, Dr. R. E. Franklin, and their co-workers at King's College, London." Crick apparently offered to put all three papers under one title so that the King's people would be coauthors, but Wilkins refused. Franklin was not presented with an offer to consider; then and always, Crick and Watson let Wilkins act on her behalf.<sup>64</sup>

After the fact, microbiologist André Lwoff pondered other roads that might have been taken more honorably and honestly: Crick and Watson could have offered Franklin joint authorship directly or they could have submitted a paper strictly on the scheme of base pairing, since it required the use of less of her data. A year later Crick and Watson explained in the footnote of another paper that their formulations of DNA structure "would have been most unlikely" without King's data. Many colleagues thought the gesture was too little, too late.<sup>65</sup> Franklin had long applied for a transfer to Birkbeck College; letters to friends confirm that she felt too beaten down at King's to stay. Randall told her that she could not take her data with her, a mandate that he would never have passed down to men in the unit. His terse advice was to take up something else, which she did. Under J. D. Bernal she worked primarily on the structure of the tobacco mosaic virus. But before she left King's she



had the grace to tie up ends, providing a full Patterson synthesis of the A form and confirming the DNA structure in crystallographic terms.<sup>66</sup>

The world recognized Crick and Watson as the discoverers of the double helix, but Crick admitted that it would be more accurate to say that the double helix got *them* discovered. Crick finished his dissertation, left for Brooklyn Polytechnic and a visiting professorship at Harvard, and won a Lasker Prize and fellowships from the American Academy of Science and the Salk Institute before winning his Nobel Prize. Watson, too, became a sought-after commodity in biological science as well as a featured bachelor in *Vogue* magazine. He returned to the United States to a senior research fellowship at Caltech and in 1956 joined the faculty of the biology department at Harvard. In 1968 he left to become the director of Cold Spring Harbor, six years after winning the Nobel Prize. He presented an acceptance speech in Stockholm alongside Crick and Wilkins. Only Wilkins mentioned Franklin that night, and only in a passing point of information.<sup>67</sup>

Debates persist over whether or not Rosalind Franklin should have won a posthumous prize. Crick conceded that she would have figured out the puzzle of DNA within months, if not weeks, of his discovery; all she had left to sort out was the significance of Chargaff's rule and the pairing of protein bases. Lynne Osman Elkin has looked over her notebooks and agreed that Franklin "was very close"; unlike anyone else, Franklin knew the parameters of the helical backbone of the structure. She was the one who determined that DNA had two forms and was aware of hydrogen bonding and the differences between the enol and keto forms. Had she simply published the original draft of the paper she submitted with Crick and Watson's at the time that she wrote it, they would have been forced to credit her for substantiating their claims.<sup>68</sup>

Watson doesn't refute this, but insists it took his blend of outside perspective and opportunism to put all the pieces of the puzzle in place. "We used her data to think about, not to steal," he said in 1984, and in the twenty-first century he has remained steadfast in his characterization of Franklin as anti-model building and anti helical. He saw her as a doer, not a thinker, an observer, not an analyst. Like the women of the Harvard Observatory, she had the patience for mundane data collection, and he the propensity for epiphany. Aaron Klug, the South African chemist who worked side by side with Franklin at Birkbeck, viewed Franklin's deliberateness differently. "It's not just good needle-

work,” he countered. “She worked *beautifully*. . . . The kind of single-mindedness that she had made her an absolutely first-class experimental worker.” Moreover, her notebooks proved that she had the analytical mind Watson revered, for privately she pointed to helicalism well before Watson thought of it himself.<sup>69</sup> Notes she wrote for the 1951 colloquium Watson attended, for example, acknowledged the presence of “a big helix or several chains.” Her belief that the sugar-phosphate groups were on the *outside* of the molecule was hers alone for a long time, and only when Crick proposed taking her advice in this respect did he and Watson create a successful model of DNA. Her images of the A and B forms suggested a helical structure, but didn’t prove it. “We are not going to speculate,” she told Gosling; “we are going to wait.” Such was the mark of a conscientious scientist, Klug believed; she wouldn’t read into what wasn’t there.<sup>70</sup>

If Franklin had ever been tempted to right the record herself, she lost her chance when ovarian cancer took her life in April 1958, her illness likely caused by radioactive exposure; Gosling had worried about her standing for long hours in front of X-ray beams, adjusting and re-adjusting her lenses. During the eighteen months in which she suffered with the disease she continued to work in the lab, stopping only for surgeries or during periods of unbearable pain. Had she lived past the age of thirty-seven, there is no telling where her work on the tobacco mosaic virus would have led. She had already determined the location of its RNA and moved on to other viruses that likely would have resulted in the winning of a Nobel Prize with Klug, who won one in 1982. Upon her death, colleagues at Birkbeck chose to remember her accomplishments, not her slights. Bernal wrote a heartfelt tribute for *Nature* and *The London Times*: “As a scientist Miss Franklin was distinguished by extreme clarity and perfection in everything she undertook. Her photographs are among the most beautiful X-ray photographs of any substance ever taken. . . . She did nearly all this work with her own hands. At the same time she proved to be an admirable director of a research team and inspired those who worked with her to reach the same high standard.”<sup>71</sup>

Fixated on the DNA race at King’s, most people will likely never know that Franklin enjoyed social camaraderie with men. Although Crick admitted to being one of her patronizers in 1951, he befriended her once she left for Birkbeck and took care of her at his home after

her fatal diagnosis. It is interesting to note that Crick, Klug, Bernal, and others who ended up singing her praises were men whose married status nullified or sublimated the sexual tensions of the lab. Studies have since confirmed that men prefer mentoring and collaborating with married women, to avoid sexual tensions and misunderstandings. Crick noted Franklin's preference for married colleagues too; she befriended their wives, lavished affection on their children, and breathed relief to be among men who didn't conjecture wildly about her sex life—at least not within earshot of her or their wives.<sup>72</sup>

Many of the details of the DNA race we know because Watson chronicled them thirteen years later in a manuscript titled “Honest Jim,” later renamed “Base Pairs” and eventually “The Double Helix.” His dream was to write a book as good as *The Great Gatsby*. Editors agreed that it was absorbing, but Houghton Mifflin turned it down because of its potentially libelous material. Harvard University Press agreed to publish it but backed out when scientists began contesting facts in the book. Abbreviated versions were printed in *Atlantic Monthly*, and the complete form, *The Double Helix*, eventually became a best seller in 1968. Like a good tabloid, it was gossipy, loaded with sordid details about scientists' foibles. Watson's “behind the scenes” exposé crushed the Arrowsmith myth: scientists were ambitious and selfish and played games of approximation; their frailties tainted their science. Many of the faults Watson found in scientists were his own, but Pauling, Crick, Kendrew, and Wilkins charged that they had been gratuitously misrepresented in the volume and that Watson had done more than betray a professional trust—he had crossed lines of decency to glorify himself.<sup>73</sup>

Although *The Double Helix* is controversial, it is the version of events best known by elite scientists as well as suburban book club members, high school students, and popular editorialists. It has left an imprint on the American mind—not only of the crude sociology of science but also of Rosalind Franklin, the amorphous female presence in the lab whom Watson less than endearingly called “Rosy.” In his hands, she was not a person, but a caricature—an old maid, an evil stepmother, a wicked witch—all combined in the form of a scientist. Watson sized up “Rosy” in sexual terms, but she emitted no sexuality back to endear him. “By choice she did not emphasize her feminine qualities,” he explained. She wore no lipstick and her dress was drab. As she stood in front of an au-

dience presenting her research, Watson could only wonder how she'd look if she vamped it up by taking off her glasses and styling her hair. Underneath her bluestocking exterior was an unstable and aggressive woman who grew enraged when colleagues challenged her. "She had a good brain," he conceded. "If she could only keep her emotions under control." A pivotal scene in *The Double Helix* was one in which "Rosy" took offense at his claim that DNA was helical. When he accused her of incompetence, she moved to strike him; Watson dodged her and walked away unscathed.<sup>74</sup>

Wilkins, who witnessed the scene, maintained that Watson exaggerated; Franklin defended herself with words, not fists. Max Perutz thought Watson had maligned a "gifted girl" for Watson's own aggrandizement, and Franklin's family thought the portrait so despicable that they preferred that she be forgotten altogether. Feminists, meanwhile, charged him with playing the sex card, using Franklin's emotionality and unwomanly appearance against her at the same time, if in fact his descriptions were accurate. A psychoanalyst might suggest that Franklin's unwillingness to be Watson's sexual object resulted in his own passive aggression, manifested in his defaming of her to the world. Mary Ellmann, a reviewer of *The Double Helix*, likely had it right when she diagnosed his hostility as a reaction to the sense of contradiction Franklin embodied. She was "the one bug in the helix," Ellmann explained, "the woman who *studies* DNA like a man. . . . Why couldn't she content herself with playing assistant to Wilkins (and over his shoulder, to Crick and Watson)? Why was she ambitious for herself as well as competent in X-ray diffraction photography? Why wouldn't she cooperate?"<sup>75</sup>

Apparently her contradictions no longer mattered once Watson had successfully used her data for his ends. In accounts of the aftermath of the DNA race, he described Franklin more appealingly as athletic (a "gutsy mountaineer"), sexy and sophisticated, not a prickly bluestocking but an "obsessively professional scientist," "direct and data-focused," who "would occasionally change out of her lab coat into an elegant evening gown and disappear into the night." In the days and months after his discovery, "Rosy" was converted into a warm, likable human being who saw the error in her ways. As she supplied the supporting data for his structure, Watson concluded that finally she produced "first-rate science, not the outpourings of a misguided feminist." John Lear, who reviewed *The Double Helix* for the *Saturday Review*, saw the irony in de-

picting her as the enlightened convert: "If Watson had been willing to consider Rosalind Franklin as an intellectual equal instead of deriding her as a mindless shrew, he could easily have seen how to accept her thesis that the sugar-phosphate backbone of the DNA structure must be on the *outside* and the plates *within*. That's the conclusion he reached in the end."<sup>76</sup>

Since 1968, feminists and filmmakers have offered correctives to Watson's depictions. Anne Sayre, Franklin's close friend, wrote a biography in which she charged Watson with inventing a new literary form: the "as-if-it-were-true" memoir justified on the basis of "I-didn't-know-better-then." "Rosy" was a figment of the imagination, someone Sayre couldn't recognize. People can rub people differently, Sayre conceded, but you cannot put glasses on a woman with "the eyesight of an eagle." "Those spectacles were not a matter of opinion," she chided, yet they served as the finishing touch to "Rosy's" dowdy costume, just as the label "Rosy" itself was a fictitious flourish for effect. Anyone who knew the woman, Sayre insisted, would not have dreamed of calling her anything but Rosalind.<sup>77</sup>

If his depictions were insensitive, Watson followed the advice of a female editor and attached qualifying statements about Franklin in the epilogue to *The Double Helix*:

Since my initial impressions of her, both scientific and personal (as recorded in the early pages of this book), were often wrong, I want to say something here about her achievements. The X-ray work she did at King's is increasingly regarded as superb. The sorting out of the A and B forms, by itself, would have made her reputation; even better was her 1952 demonstration, using Patterson superposition methods, that the phosphate groups must be on the outside of the DNA molecule. . . . All traces of our early bickering were forgotten, and we [Watson and Crick] both came to appreciate greatly her personal honesty and generosity, realizing years too late the struggles that the intelligent woman faces to be accepted by a scientific world which often regards women as mere diversions from serious thinking.<sup>78</sup>

For all his enlightenment about sexism in science, Watson went on to write accounts of his early career that featured women in limited and stereotypical ways. In *Genes, Girls, and Gamow* (2001), he confessed to being a twenty-five-year-old more interested in sex than in science. In the transatlantic world he created, the women of note were mothers,

wives, and sisters of the men he took seriously or the faceless objects of his sex-starved gaze. The occasional competent female in his world was a technician or typist. If an intellectually rigorous scientist in her own right—as in the case of Franklin and the British crystallographer Dorothy Wrinch—she was prickly, only “fun” once she conceded defeat in the game of discovery and returned to the social play of women. In his world of science and philandering, women functioned as fantasy material or as pawns that men placed into close proximity to male scientists and their data. Linus Pauling’s daughter, John Kendrew’s wife, and even Watson’s own sister were women Watson exploited for his competitive designs. He described his double helix as a woman who finally revealed herself to him. She was “beautiful,” “too elegant” to refute. She was, like all women, an object of adoration.<sup>79</sup>

So how does one reconcile the Watson who treated women like diversions (usually sexual ones) and the one who supplied the sensitive insights in the epilogue of 1968? No doubt pressures of the nascent feminist movement made his apology a necessity, but he defended his book as a rendition of not what *is*, but what *was*—as his scientific fraternity saw it in 1953. Men at King’s really did feel that Rosalind had to be “put in her place”; it really was the prevailing sentiment that “the best home for a feminist was in another person’s lab.” Other Nobel-winning scientists saw their discoveries through sexual lenses. Richard Feynman told a room full of men at Caltech, for example, that he had fallen deeply in love with QED, and though, like any aging woman, she no longer turned heads, his theory deserved respect for mothering children who did. Young Watson’s world was a milieu in which George Gamow convened the exclusive “RNA Club,” in which members went by amino acid code names and wore neckties with double helixes stitched into them.<sup>80</sup> How could Franklin have fit into this nexus?

Watson warned young scientists in his 2007 memoir, *Avoid Boring People*, that “mopping up the details . . . will not likely mark you out as an important scientist. Better to leapfrog ahead of your peers.” Although Rosalind Franklin had all the data to come up with the structure of DNA before he did, what she lacked, as he saw it, was his spark of impetuous energy. E. B. Wilson used the same logic in assessing the work of unsuspecting Nettie Stevens in the early 1900s; he and Stevens came up with identical conclusions about the mechanisms of sex identity, but

he packaged their work collectively as her female labor clarified by his brilliant synthesis. His assumptions about women's limitations persist. A woman lamented to Vivian Gornick in the 1980s that her male associates still classified their peers into idea people, technique people, and people who work hard. "They think women fall into the last category most frequently, if not always."<sup>81</sup> This labeling is an integral part of the story of women prizewinners and nonprizewinners, as is the social organization of the science itself.

### Notes

1. Willard Libby, Bernd Matthias, Lothar Nordheim, and Harold Urey, "Maria Goeppert Mayer, Professor of Physics, 1906–1972," Box 9, folder 8, Maria Goeppert Mayer Papers, Special Collections, University of California at San Diego (hereafter MGM).
2. Edward Teller with Judith L. Shoolery, *Memoirs: A Twentieth-Century Journey in Science and Politics* (Cambridge, MA: Perseus, 2001), 241–42.
3. Maria Goeppert Mayer, "The Shell Model," Nobel Lecture, *Nobel Lectures, Physics, 1963–1970* (Amsterdam: Elsevier, 1972), 29–30.
4. Joan Dash, *A Life of One's Own: Three Gifted Women and the Men They Married* (New York: Paragon House, 1988), 311.
5. Horace Freeland Judson, "No Nobel Prize for Whining," *New York Times*, October 20, 2003; Noemie Bencer-Koller, "Chien-Shiung Wu," in *Out of the Shadows: Contributions of Twentieth-Century Women to Physics*, ed. Nina Byers and Gary Williams (New York: Cambridge University Press, 2006), 260–66.
6. C. S. Wu, E. Ambler, R. W. Hayward, D. D. Hoppes, and R. P. Hudson, "Experimental Test of Parity Conservation in Beta Decay," *Physical Review* 105 (1957): 1413; L. M. Jones, "Intellectual Contributions of Women to Physics," in *Women of Science: Righting the Record*, ed. G. Kass Simon and Patricia Farnes (Bloomington: Indiana University Press, 1990), 205–8.
7. Sharon Bertsch McGrayne, *Nobel Prize Women in Science* (Washington, DC: Joseph Henry Press, 1998), 268, 276.
8. Daniel Kevles, *The Physicists: The History of a Scientific Community in Modern America* (Cambridge, MA: Harvard University Press, 1987), 227; Sandra Harding, *The Science Question in Feminism* (Ithaca, NY: Cornell University Press, 1986), 76.
9. Ferenc Morton Szasz, *The Day the Sun Rose Twice: The Story of the Trinity Site Nuclear Explosion, July 16, 1945* (Albuquerque: University of New Mexico Press, 1984), 9–10; Leona Marshall Libby, *The Uranium People* (New York: Charles Scribners' Sons, 1979), 45–51; Ruth Lewin Sime, *Lise Meitner: A Life in Physics* (Berkeley: University of California Press, 1996), 326–27, 369–70; "Lise Meitner," in *Out of the Shadows*, 74–82; Jones, "Intellectual Contributions of Women to Physics," 193–95.
10. Helena M. Pycior, Nancy G. Slack, and Pnina G. Abir-Am, eds., *Creative Couples in the Sciences* (New Brunswick, NJ: Rutgers University Press, 1996), ix, 3; Ferdinand V. Coroniti and Gary A. Williams, "Susan Jocelyn Bell Burnell," in *Out of the Shadows*, 419–26.
11. Stephen G. Brush, "Nettie M. Stevens and the Discovery of Sex Determination by Chromosomes," in Sally Gregory Kohlstedt, ed., *History of Women in the Sciences*:

*Readings from Isis* (Chicago: University of Chicago Press, 1999), 343–44; G. Kass Simon, “Biology Is Destiny,” in *Women of Science*, 225–26.

12. Harriet Zuckerman, *Scientific Elite: Nobel Laureates in the United States* (New York: The Free Press, 1977), 53–54; Marianne Wenzel quoted in McGrayne, *Nobel Prize Women*, 189.

13. Maria Mayer to Hans Jensen, [November 1949]; Jensen to Mayer, November 2, 1949; Jensen to Mayer, December 22, 1949, Box 1, folder 16; “The Moon, the Atom . . .” 10–11, Box 9, folder 24, MGM; Maria Goeppert Mayer and J. Hans Jensen, *Elementary Theory of Nuclear Shell Structure* (New York: Wiley and Sons, 1955).

14. C. W. Wong, “Harriet Brooks,” in *Out of the Shadows*, 66–73; Ruth Howes and Caroline Herzenberg, *Their Day in the Sun: Women of the Manhattan Project* (Philadelphia: Temple University Press, 1999), 36; Londa Schiebinger, *Has Feminism Changed Science?* (Cambridge, MA: Harvard University Press, 1999), 95–96; Natalie Angier, “Geek Chic and Obama, New Hope for Lifting Women in Science,” *New York Times*, January 20, 2009, science section, 1, 4.

15. It is interesting to note that single women did not start winning Nobel Prizes in science until after the women’s movement began in the late 1960s and early 1970s. After Maria Mayer (1963), Dorothy Crowfoot Hodgkin (1964), and Rosalyn Yalow (1977), the next Nobelists were the single women Barbara McClintock (1982), Rita Levi-Montalcini (1986), and Gertrude Elion (1988). Margaret Rossiter, *Women Scientists in America: Struggles and Strategies to 1940* (Baltimore: Johns Hopkins University Press, 1982), 393; *Women Scientists in America: Before Affirmative Action, 1940–1972* (Baltimore: Johns Hopkins University Press, 1995), 329–32; Pnina Abir-Am, forward to *Creative Couples*, xi; Schiebinger, *Has Feminism Changed Science?* 97.

16. Maria Mayer to Lisa Keller, March 17, 1969, Box 2, folder 9; “The Moon, the Atom . . .” 7; “Maria Goeppert Mayer,” typed autobiography, Box 9, folder 2, MGM.

17. See, for example, Elizabeth Rona, *How It Came About: Radioactivity, Nuclear Physics, Atomic Energy* (Oak Ridge, TN: Oak Ridge Associated Universities, 1978), 1; Evelyn Fox Keller, *A Feeling for the Organism: The Life and Work of Barbara McClintock* (New York: Henry Holt, 1983), 20–22, 34–35; Rita Levi-Montalcini, *In Praise of Imperfection: My Life and Work* (New York: Basic Books, 1988), 4–16; Fay Ajzenberg-Selove, *A Matter of Choices: Memoirs of a Female Physicist* (New Brunswick, NJ: Rutgers University Press, 1994), 3, 14, 20; Moira Reynolds, *American Women Scientists: 23 Inspiring Biographies, 1900–2000* (Jefferson, NC: McFarland, 1999), 112.

18. 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. Jacquelyn A. Mattfeld and Carol G. Van Aken (Cambridge, MA: MIT Press, 1965), 91; Anne Roe, *The Making of a Scientist* (New York: Dodd, Mead, 1953), 92; Philip Wylie, *Generation of Vipers* (New York: Rinehart, 1942).

19. Zuckerman, *Scientific Elite*, 66; E. N. Plank and R. Plank, “Emotional Components in Arithmetic Learning as Seen Through Autobiographies,” in *The Psychoanalytic Study of the Child* (New York: International University Press, 1954); Evelyn Fox Keller, *Reflections on Gender in Science* (New Haven, CT: Yale University Press, 1995), 91n; Londa Schiebinger, *Has Feminism Changed Science?* 59; Joan Dash, *Triumph of Discovery: Women Scientists Who Won the Nobel Prize* (Englewood Cliffs, NJ: Julian Messner, 1991), 3, 233–40; Olga Opfell, *The Lady Laureates: Women Who Have Won the Nobel Prize* (Metuchen, NJ: Scarecrow Press, 1986), 226–27; Reynolds, *American Women Scientists*, 81; McGrayne, *Nobel Prize Women*, 175–80.



20. Mary Harrington Hall, "The Nobel Genius," 108, Box 9, folder 30, MGM.
21. Transcript of tape-recorded interview of Maria Goeppert Mayer by Thomas Kuhn, February 20, 1962, tape 2, side 1, Center for History of Physics, American Institute of Physics, College Park, MD; "The Moon, the Atom . . .," 7.
22. Interview with Maria Goeppert Mayer, February 20, 1962; McGrayne, *Nobel Prize Women*, 180–82; Dash, *A Life of One's Own*, 244, 253.
23. Transcript of interview between Joseph Mayer and Lillian Hoddeson, January 24, 1975, 13–14, Center for History of Physics, American Institute of Physics; McGrayne, *Nobel Prize Women*, 182.
24. "The Moon, the Atom . . .," 8; Maria Mayer to Joan Hellweg, March 29, 1968, Box 2, folder 8; Hall, "The Nobel Genius," 69; "Biography," Box 9, folder 2, MGM; McGrayne, *Nobel Prize Women*, 183–84; Jones, "Intellectual Contributions of Women to Physics," 200.
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32. Max Born to Maria Mayer, June 8, 1939, Box 1, folder 6; John Kirkwood to Mrs. Mayer, January 9, 1941, Box 1, folder 8, MGM; Hall, "Nobel Genius," 108; Shiels, *Winners*, 101; Darlene Stille, *Extraordinary Women Scientists* (Chicago: Children's Press, 1995), 125; Reynolds, *American Women Scientists*, 84.
33. Laura Fermi, *Atoms in the Family: My Life with Enrico Fermi* (Chicago: University of Chicago Press, 1954), 80–81; Hall, "Nobel Genius," 68, 108; Dash, *Life of One's Own*, 301.
34. Constance Warren to "Mrs. Mayer," April 18, 1942; April 23, 1942, Box 1, folder 9; Max Born to Maria Mayer, January 20, 1943, Box 1, folder 10; Maria Mayer to Constance Warren, January 16, 1945, Box 1, folder 12; Maria Mayer's speech for "Women in Science Program," 1964, Box 2, folder 3, MGM.
35. McGrayne, *Nobel Prize Women*, 192; Shiels, *Winners*, 104–105; Stille, *Extraordinary Women*, 125; Reynolds, *American Women Scientists*, 84; Dash, *Triumph of Discovery*, 16–17.
36. Dash, *A Life of One's Own*, 300–301; *Triumph of Discovery*, 17–20; Opfell, *The Lady Laureates*, 231–32; Hall, "Nobel Genius," 68; The University of Chicago Comptroller to

Maria Mayer, September 17, 1958, Box 1, folder 25, MGM; Schiebinger, *Has Feminism Changed Science?* 59.

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38. Dash, *Triumph of Discovery*, 27–28; *Life of One's Own*, 321; Opfell, *Lady Laureates*, 235; Teller, *Memoirs*, 241–42; Jones, "Intellectual Contributions of Women to Physics," 237.

39. Within months of Mayer's first contract, Roger Revelle changed the terms so that she was paid full time for nine months (\$14,208 instead of the original \$6,780). Roger Revelle to Joe and Maria Mayer, July 14, 1959; October 9, 1959, Box 1, folder 26; "The Moon, the Atom . . .," 2, MGM; Rossiter, *Women Scientists in America: Before Affirmative Action*, 162.

40. "Pre-Dawn Party (For 2)—That's Nobel Prize Winner's Celebration," *San Diego Union*, Box 9, folder 28, MGM.

41. Linda McCausland to Dr. Mayer, November 10, 1963; Stella Hull to Maria Mayer, February 13, 1964; Marcia George to Maria Mayer, December 7, 1963; Maria Mayer to Marcia George, January 2, 1964, Box 10, folder 19, MGM.

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