

A reprint from

# American Scientist

the magazine of Sigma Xi, The Scientific Research Honor Society

This reprint is provided for personal and noncommercial use. For any other use, please send a request to Permissions, American Scientist, P.O. Box 13975, Research Triangle Park, NC, 27709, U.S.A., or by electronic mail to [perms@amsci.org](mailto:perms@amsci.org).  
©Sigma Xi, The Scientific Research Honor Society and other rightsholders

# The Women Who Discovered RNA Splicing

*Most scientists who played key roles in this Nobel Prize–winning breakthrough disappeared from public memory. Why does this injustice persist 40 years later?*

Pnina Geraldine Abir-Am

In 1997, the Cold Spring Harbor Laboratory (CSHL) in New York celebrated the 20th anniversary of the discovery of RNA splicing. The first coauthor of the CSHL paper announcing the discovery, Louise Chow, was not invited to the celebration, even though first authorship usually implies a central role in the work. Richard J. Roberts from CSHL, who was one of Chow's coauthors, and Philip A. Sharp from a team at the Massachusetts Institute of Technology (MIT) that had published a parallel paper, were the centers of attention, as if they alone had made the discovery. Apparently, as the sole winners of the 1993 Nobel Prize in Physiology or Medicine awarded for the research, Roberts and Sharp made more desirable guests than those who had been excluded from this most coveted scientific award.

A similar affront occurred in 2017, when CSHL celebrated the 40th anniversary of the discovery: The organizers included Sharp and James D. Watson, who had been the director of CSHL at the time of the discovery in 1977. Chow was initially placed in one of many panels, as if she were just another new arrival in the now-crowded world of RNA splicing. She refused to accept such an inappropriate slot. Finally, Chow and Richard Gelinas, who was second author on their discovery paper, were each permitted to speak for eight minutes during the opening historical session. Other speakers were allotted the customary 20 minutes.

This event made it clear that four decades later, leading scientists remained interested in maintaining the status quo, rather than seizing the opportunity to reassess the relationship between the present and the past. As a historian of molecular biology and editor of two volumes on scientific anniversaries, I was barely able to get a one-day voucher to attend the opening “historical” session in 2017 and was surprised to realize that, then and now, what mattered most was not what really happened in the past but who can control the narrative.

The lack of recognition for women who played a key role in high-profile discoveries is especially evident in the paucity of women among Nobel Laureates or recipients of other prestigious prizes in science. As of 2019, only 21 of 723 Nobel Prizes in the sciences and economics had been awarded to women. The career obstacles women face have a long history that has been amply documented by historians of women in science.

The question persists as to what prevented the scientific community in general, and the Nobel committee in particular, from better understanding Chow's role, as well as the roles of the two other women scientists who were first coauthors on the 1977 papers associated with the discovery of RNA splicing: Sara Lavi, a staff scientist at the time at the Weizmann Institute of Science (WIS) in Israel, and Susan Berget, a postdoctoral researcher at MIT. Initially, I was excited to find that half a dozen women scien-

tists were members of teams that had contributed to this major breakthrough. In addition to Chow, Lavi, and Berget, Sayeeda Zain worked as a staff scientist at CSHL, Claire Moore was a technician at MIT, and Mia Horowitz was a graduate student at WIS. Yet, despite the many reasons for sharing glory, they have all but disappeared from the public memory enshrined in textbooks, reviews, and the justification for the 1993 Nobel Prize in Physiology or Medicine.

In addition, I was struck by the geopolitical disparity separating male and female coauthors of the same discovery papers. While male coauthors maintained lifelong careers at institutions at the center of the most desirable biomedical research hubs in Boston and New York City, the women ended up in more remote places, such as Alabama or Texas, where there are fewer opportunities for international recognition. To clarify how male and female coauthors of the same discovery papers ended up with such starkly different career outcomes, one must start from the often-overlooked perspective of these women scientists.

Out of six female coauthors of papers on the discovery of RNA splicing, Chow and Lavi best illustrate the problem of lack of recognition for women. They deserved, at the very least, a share of recognition for the discovery. Both of them were first coauthors of publications reporting the discovery, and both were independent scientists—that is, they had their own research grants and

## QUICK TAKE

**Most of the scientists who** played key roles in the discovery of RNA splicing, including six women, disappeared from public memory and were overlooked for the 1993 Nobel Prize.

**Three women were first authors** on papers announcing the discovery, two of whom were scientists with their own research grants. They illustrate how women are overlooked.

**Recognition for women, junior,** or immigrant scientists cannot be achieved without thorough vetting. The Nobel committee was remiss in according all the credit to two men.

Sara Lavi

Louise  
Chow

Mia Horowitz

Susan Berget

Claire Moore

Sayeeda Zain

were not paid by the grants of another, often more established, scientist. Therefore, their work could not be attributed to lab directors, as would have been the case for dependent scientists, such as postdocs and graduate students. Because dependent scientists are considered to be in training, the credit for any work they do is habitually attributed to their lab directors.

These general guidelines are not always followed, because the allocation of scientific credit among collaborative coauthors may become embroiled in institutional or personal rivalries. Hence historians must understand not only what various collaborators did in the context of the discovery, but also the relationship that each collaborator enjoyed with scientists in power.

### Molecular Biology in the 1970s

The discovery of RNA splicing in 1977 profoundly changed our understanding of the molecular basis for biological diversity and genetic regulation. This breakthrough introduced RNA splicing as a previously unknown genetic regulatory mechanism in nucleus-possessing organisms. *RNA splicing* is the biological process of joining mul-

Six women worked on the teams from the Weizmann Institute of Science (WIS), Cold Spring Harbor Laboratory (CSHL), and Massachusetts Institute of Technology (MIT) that published the 1977 papers announcing the discovery of RNA splicing, which later won a Nobel Prize. Yet none of these women were awarded the Nobel Prize and none are associated with the public memory of the discovery. Three of them—Lavi, Chow, and Berget—served as first coauthors of three of those papers, a position that usually indicates a central role in the work. How were these women excluded from recognition?

multiple segments of an RNA transcript copy of a genome to form a mature messenger RNA via one or more internal deletions. *Messenger RNA* (mRNA) delivers the code for making proteins to the cellular machinery that produces them. The discovery provided a molecular explanation for a biological puzzle: how a large number of human proteins (about 150,000) could be traced to a much smaller number of genes (about 20,000). The discovery also had medical ramifications, because splicing errors often result in serious diseases. The discovery culminated with the structure of the *spliceosome*, which processes the long primary RNA transcript into short messenger RNA by removing segments copied from the noncoding regions of the genome.

The discovery of RNA splicing reflected two dimensions of U.S. science policy that defined that period. The

“war on cancer” declared by Congress in 1971 provided plentiful government funding, especially for institutions that responded by creating cancer research centers and labs researching tumor viruses. The discovery of RNA splicing was made in labs specifically created in response to the war on cancer: the Cancer Research Center (CRC) at MIT and the research program on DNA tumor viruses at CSHL.

At the same time, affirmative action legislation passed in 1972 made it illegal to discriminate on the basis of gender and race. As a result, the 1970s saw an unprecedented rise in the number of women scientists. More women than ever before completed graduate school and pursued scientific careers. Yet the actual implementation of affirmative action was often slow and subject to a gradual shift from overt to covert gender bias, and even backlash. Although



Courtesy of Sara Lavi

Sara Lavi conducted the pioneering work in 1975–1976 that indicated that the 5' cap—the end of a strand of messenger RNA (mRNA), which copies and carries the code for making proteins—was coded on different locations of the genome. Her results were initially dismissed by colleagues as artifacts. The lack of support from senior colleagues and a move between institutions and countries delayed her in publishing her work until 1977, after others at CSHL who knew of her work had begun to piece together the puzzle.

women had better access to scientific careers, they remained a minority of newcomers, concentrated in the lowest ranks of graduate students, technicians, postdocs, and research assistants, with few making it as staff scientists and principal investigators or research directors on governmental grants. As such, they remained handicapped in navigating the career minefield and especially

ther considers other cultural attributes that modulate the social perception of gender, such as ethnicity, then women scientists from an ethnic or immigrant background faced an additional disadvantage, especially when pitted against male colleagues who looked and played the part of promising mentees to the preponderantly white male leadership of science in the 1970s.

### Women Who Studied RNA Splicing

Six women scientists were among the approximately two dozen participants involved as coauthors in the discovery of RNA splicing. By contrast, no women scientists can be found among the coauthors of the discovery of mRNA in 1960.

In contrast to Lavi and Chow, who were independent scientists, the other four women (Berget, Zain, Moore, and Horowitz) had different forms of dependency, which may have been used to counteract claims to credit. For example, India-born Zain, the most experienced among them, who had already published as part of teams at Yale University prior to her arrival at CSHL, was in the process of becoming a U.S. resident. Until this prolonged process was complete, she could not apply for government grants in her name. As a research assistant in someone else's lab, she depended on his discretion to join

Research Institute in New Jersey, was studying the transcription of the genome of a model virus, *simian virus 40* (SV40), and was looking at mRNA of discrete sizes. Lavi and Shatkin were at the cutting edge of this research. In a paper published in 1975 in *Proceedings of the National Academy of Sciences of the U.S.A. (PNAS)*, they established biochemical details about the structure of one end of the virus's mRNA, called the *five-prime* (5') *cap*. (The two ends of mRNA are referred to as 3' and 5', based on their molecular structure.) Experiments Lavi conducted with Shatkin indicated that the RNA sequences of the cap mapped to a region of the virus's genome other than the region expected. This result meant that the main body of mRNA and its cap were encoded by different genome regions.

At the time, these findings were dismissed as misleading results, because they contradicted the prevailing idea that protein sequences reflect uninterrupted, matching genome sequences. Lavi, however, had a hunch that those results were important, even though she was laughed at whenever she insisted on their validity. For the last six months of her postdoc, Lavi shifted her pursuit of her unusual results to a lab directed by Joseph Sambrook, an expert on DNA tumor viruses at CSHL. There, she befriended Zain and also encountered another third-year postdoc, Gelinas, who was mapping where the cap originates. Lavi and Gelinas commiserated about the meaning of the cap and the dismissal of her results by their colleagues. Both Gelinas and Zain worked in the nucleic acids lab directed by Roberts, who was an expert on restriction enzymes. Roberts provided Lavi with some of these enzymes from his growing library when she left CSHL in March 1976 for a position as a staff scientist at WIS in Israel.

In mid-1977, when two teams working with another highly studied virus, *adenovirus 2* (*Ad2*, a cause of the common cold), announced their respective findings regarding RNA splicing, Lavi finally understood that her earlier results had a new meaning. With encouragement from Maxine Singer, a senior staff scientist at the U.S. National Institutes of Health (NIH) who was then visiting WIS, and with the collaboration of another staff scientist in the same department of virology, Yoram Groner, Lavi published her earlier results with the conclusion that the

## Lavi was among the first—if not the first—to observe that the mRNA and its cap came from noncontiguous regions of the genome.

in gaining recognition from leading, invariably male, scientists as potential or actual discoverers. Mentorship is key to any career, but in the 1970s cross-gender mentorship carried the stigma of a potential sex scandal. Women also remained more burdened than men with balancing career and family life.

This systemic and persisting gender inequality shaped the perception of women scientists as supporting cast rather than main players. Unless someone was personally familiar with the ability of a given woman scientist, she was more likely to be judged by her gender than by her work. If one fur-

multiauthor publications from CSHL about RNA splicing, but she was excluded. Berget was similarly disadvantaged. Despite being first coauthor, she was gradually distanced from the discovery, and as a result she took a job with lesser visibility than the one she had had as part of the MIT team. Moore and Horowitz had not yet received their PhDs; their very presence among the coauthors was a form of recognition agreed to by their lab directors.

### Sara Lavi Solves an RNA Mystery

Lavi, together with her postdoctoral adviser, Aaron Shatkin at the Roche

5' cap and the mRNA body originated in noncontiguous segments of the genome. In other words, there must be a process by which those sequences had been put together. The paper was submitted to *PNAS* by the director of CSHL, Watson, who recalled Lavi and her earlier results from her time there.

Lavi and Groner's 1977 paper situated her 1975 findings (with Shatkin) as an earlier, independent, important result. It showed that the cap and the main body of mRNA are coded in different locations on the genome. The implication was that they must be joined together by a mechanism such as splicing.

Lavi's pioneering work can be deemed a partial discovery. The skepticism she encountered, her career moves across institutions and countries, and her effort to establish her lab delayed her pursuit of her promising results until all she could rescue was a contribution as supportive material.

Had Lavi remained at CSHL after May 1976, perhaps she would have realized the relevance of her earlier results sooner. (Her departure from CSHL in 1976 was for family reasons.) Still, Lavi was fortunate to be able to produce independent support for the discovery of RNA splicing from another model system, while also revealing her earlier, unpublished results, which demonstrated that she was among the first—if not the first—to observe that the mRNA and its cap came from noncontiguous regions of the genome. Despite her pioneering approach to the cap, Lavi is not part of the public memory of the discovery of RNA splicing, in part because such memory came to focus on the role of electron microscopy.

### Louise Chow's Electron Microscopy

From 1975 through 1984, Chow collaborated on many seminal papers from CSHL that used electron microscopy to clarify a wide variety of DNA and RNA structures. Her key role is evident in her position as first coauthor of a preponderance of such collaborative papers during those years. Chow's breakthrough was made possible by her mastery of electron microscopy together with several converging techniques.

Prior to her arrival at CSHL in 1975, Chow spent almost a decade at the California Institute of Technology (Caltech) in the lab of Norman Davidson, a leading biophysical chemist. There she became passionate about electron mi-



Courtesy of Cold Spring Harbor Laboratory Archives

Louise Chow and her husband Thomas R. Broker were jointly hired by CSHL and began their positions in February 1975, he as chief of the electron microscopy lab and she as a postdoc. Chow was promoted to staff scientist within a year of her arrival.

croscopy while researching the technology's use in unraveling DNA structure in several microbial systems. During her postdoctoral studies at the University of California, San Francisco, and at Caltech, Chow met her most important collaborator and future spouse, Thomas R. Broker, who was a postdoc at Caltech. Broker, like Chow, became totally captivated by electron microscopy.

In February 1975, Chow and Broker arrived at CSHL, where he assumed the position of chief of the electron microscopy lab and she became a postdoc in that lab. Chow and Broker were committed to an equal partnership, and once she too became a staff scientist, to equal salaries. Chow was promoted to staff scientist within a year of her arrival, eventually becoming a ten-

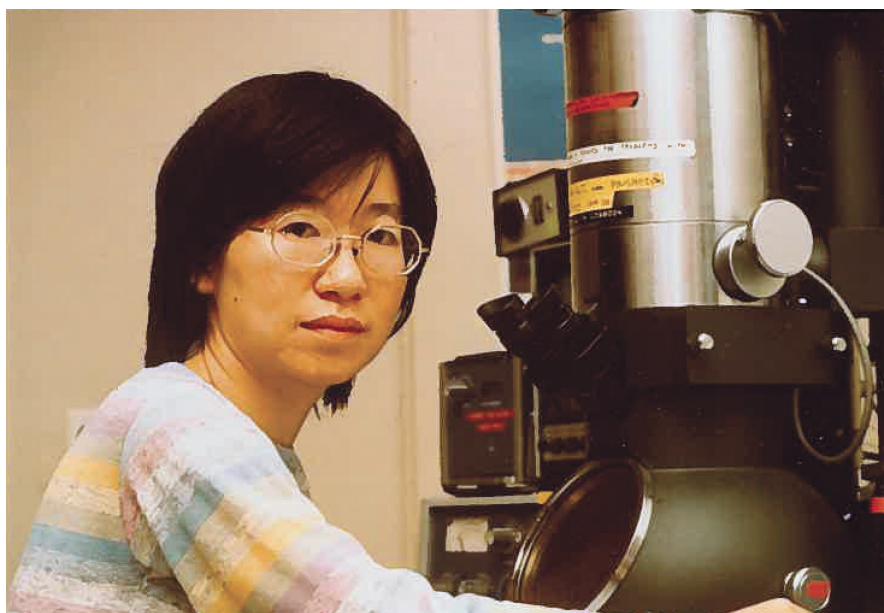


Photo by Thomas R. Broker

Chow, shown here with the electron microscope at CSHL, had trained as a doctoral student and postdoc with a pioneer of the technology, Norman Davidson of the California Institute of Technology. In the spring of 1977, Chow and Broker began collaborating with Richard Roberts and Richard Gelinas and began imaging the end of the mRNA strand. Chow and her team figured out where the mRNA 5' cap maps on the genome from which it is copied.



Courtesy of Mia Horowitz and the Weizmann Institute

Graduate student Mia Horowitz (*second from right*) pipettes a sample with her team at WIS: lab director Y. Aloni (*second from left*), technician Susan Bratosin (*far left*), and fellow graduate student Orgad Laub (*far right*). Horowitz was a coauthor with Aloni and Laub on the September 1977 paper in *PNAS* showing RNA splicing in another model system, simian virus 40.

ured senior scientist in 1979. She collaborated with other staff members as well as postdocs and students. She also came to know well and admire Barbara McClintock, a leading geneticist whose discovery of transposons—“jumping genes” that can change position within a genome—had been greeted with skepticism until the 1970s.

Around the time that Chow’s first NIH grant was to start on April 1, 1977, Broker and Chow were approached by Gelinis and Roberts about collaborating on mapping the position on the genome where the 5’ cap of mRNA originated. This collaboration began about a year after Lavi had left CSHL; her obsession with the cap was well known there. During that year, Gelinis had succeeded in determining the cap’s nucleotide composition. He also discovered that the cap was identical in all types of mRNA coding for Ad2 proteins. Gelinis and Roberts wanted to know whether a virus-associated RNA served as a primer for the cap. Chow and Broker were asked to help test this hypothesis.

By exploring where the *leader*, a segment of mRNA attached to the 5’ cap, maps on the entire Ad2 genome, Chow refuted this hypothesis about virus-associated RNA. But she also discovered that the leader maps in multiple

different places, all far from the genome segment where the main body of mRNA maps. This finding meant that mRNA is put together from these segments by a mechanism such as splicing. Chow’s prior experience with cutting-edge molecular techniques enabled her to have full confidence in her unexpected findings. Chow discovered multiple R loops—noncoding regions in the DNA that form loops when matched to RNA that does not have them (*see figure on the bottom of page 303*). She also discovered the phenomenon of alternative splicing, whereby the same pre-RNA strand can be spliced in various ways to form different functional mRNAs.

Chow’s findings clarified how the long, heterogeneous RNA transcript was processed into much shorter and more stable mRNAs, a question that had long preoccupied scientists. Much like Lavi’s earlier biochemical findings, Chow’s electron microscopy findings implied that mRNA segments coded in different sites of the genome were somehow joined. This result opened the question of how this splicing was accomplished. Moreover, the multipartite structure of the mRNA leader also led to the surprising realization that not all parts of a transcript are preserved in mature mRNA. Later,

the DNA segments that are retained were named *exons*, and the spliced-out sequences were called *introns*.

When the four collaborators discussed the publication of their joint paper, they agreed that Chow would be the first author, because the findings were entirely based on her new electron microscopy work, and that the paper would be submitted to *Cell*, the best journal for such work. Gelinis, who had prepared the DNA probes and had had the idea to collaborate with Chow and Broker, was the second author. Broker and Roberts were featured as directors of the labs in which the reported work was done.

However, Watson wanted six additional members of two other CSHL labs who had related, supportive findings to be added as authors. The initial idea was to list projected authors in alphabetical order and include all who had supportive findings.

This publishing scheme was not accepted, because junior scientists from the other two labs preferred that their contributions remain distinct. The eventual solution was to publish four separate papers back to back. The first paper was the interlaboratory collaboration between Chow and Broker of the electron microscopy lab and Gelinis and Roberts of the nucleic acids chemistry lab. The other three papers were from the teams of influential CSHL lab directors Gesteland, Sambrook, and Watson.

The infighting over the publishing format was not without consequences. The decision to wait several weeks for the completion of all the supportive papers delayed the joint publication of all four papers. The quartet of papers from CSHL was finally published in September 1977 in *Cell*, where the practice of formal reviewing and its associated requests for revisions had further slowed its publication. Chow and Broker had agreed to the idea of delaying the publication of a major discovery for reasons of collegiality.

In the early 1980s, Chow and Broker moved to the University of Rochester Medical School, where they had identical professorial appointments. Their colleague and friend from CSHL, Zain, who had moved there in 1979, had encouraged them to join her. According to several people I interviewed, Watson lamented the fact that they were not moving to a more prestigious school, but did not make any effort to help them secure such a position. (By contrast, he had helped his predecessor as CSHL director get a job at the Harvard School of Public Health). Since 1993, Chow and Broker have worked at the medical school of the University of Alabama, Birmingham. In 2012, Chow became the second member of their faculty to be elected to the National Academy of Sciences.

### The Nobel Puzzle

Although Chow and Broker remained at CSHL until the early 1980s, firmly believing that their unique electron microscopy work “speaks for itself,” as they put it, they were slow to notice that Chow’s contribution as the lead coauthor of the key CSHL paper started to erode as soon as news of the discovery began to spread. The scrambling that ensued in connection with CSHL’s publication scheme led to tensions that persist to this day. Although such conflicts are not uncommon in science, at CSHL they were exacerbated by a managerial style that pressured scientists for important results, thus forcing everyone to seek to magnify their share of credit.

Roberts, who had been at CSHL longer than the other coauthors of this series of papers, was particularly shocked to see those who had previously derided Gelin’s (and Lavi’s) work on the cap as an artifact rush to associate their results with the discovery of RNA splicing. When he shared the news of the discovery with col-



Courtesy of Robert Weinberg

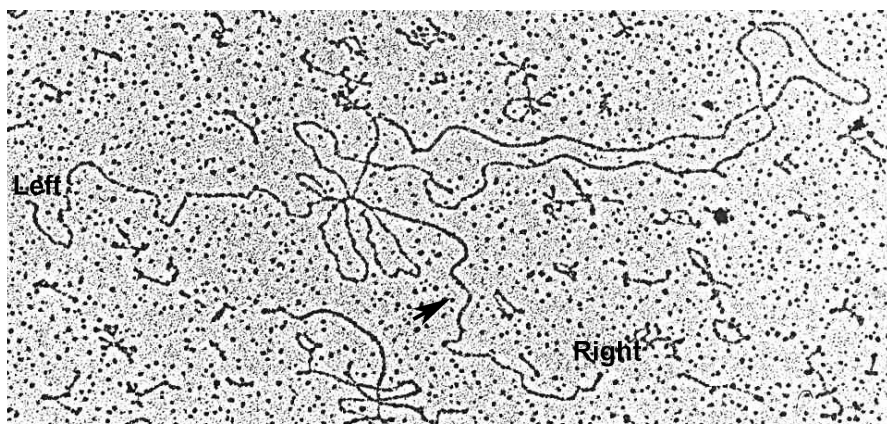
This photo of members of the MIT Center for Cancer Research includes Berget (third row from bottom, third from left), Moore (back row, fourth from left), and Philip Sharp (back row, far right), all of whom worked on the PNAS paper published in August 1977 that demonstrated RNA splicing.

leagues outside CSHL, only to find out that they already knew about it through “leaks,” Roberts decided to become more proactive in shaping the narrative and projecting his take on it.

In his view, the discovery originated in efforts to test a hypothesis that he and Gelin’s had discussed to explain the latter’s yearlong results on the cap. As a nucleic acid chemist who specialized in RNA and DNA sequencing, Roberts had no expertise in electron microscopy. Like most biochemists at the time, he thought of electron microscopy as a tool for confirming biochemical findings. Roberts therefore formed the incorrect impression that any electron microscopy operator could have tested his hypothesis, and presumably also “stumbled” upon the revolutionary results of multiple splic-

ing. In the defensive context in which he found himself, Roberts overlooked the possibility that in Chow’s hands the electron microscopy had not only refuted his hypothesis but had also become a tool for further discovery.

One can easily deconstruct Roberts’s view as a one-sided and unfounded claim to primary—let alone exclusive—discovery ownership. After all, the DNA probes prepared by Gelin’s as a result of guiding discussions with Roberts could have been prepared by any other nucleic acid sequencer. The discovery reflected a timely collaboration between two labs that contributed complementary types of expertise. If anything, Chow’s share might be said to be larger than that of Roberts, because the precise visualization of the electron microscopy loops came to be seen as the



Courtesy of Louise Chow and Thomas Broker

Electron micrographs such as this one, made in the lab of Chow and Broker at CSHL, became the discovery proof for RNA splicing. The image shows four loops of displaced adenovirus DNA that form when matched to mRNA that lacks these noncoding regions. The arrow points to the 3’ end of the strand, and the left and right sides of the DNA are labeled.



AGIP-Rue des Archives/Granger, NYC



Jack Mikrut/TT/Sipa USA

Roberts (*top, on left*) and Sharp (*bottom, on left*) received the 1993 Nobel Prize in Physiology or Medicine from Swedish King Carl Gustaf (*on right*) in Stockholm. The third slot for the prize remained unfilled. The Nobel committee did not properly investigate others' contributions.

"discovery proof," whereas the nucleic-acid sequencing and the use of DNA fragments in genome mapping were no longer novel at the time.

As to the hypothesis regarding where the mRNA cap originates on the genome, this question had already been raised by Lavi and a few others after her. No one had thought to try electron microscopy until Gelinas suggested its possibility to Roberts, more as a last resort than a strategy. This order of events is also why Roberts's

contribution cannot be separated from that of Gelinas. Gelinas seconds Roberts's view of the discovery in general, but he disagrees with the latter's assertion that Chow's experiments could have been done by any other operator of the imaging equipment.

Nonetheless, Roberts began to convey his perspective, especially outside CSHL, where Chow's unique skills were less known. In his telling, rather than being the product of a collaboration in which two labs contrib-

uted complementary types of expertise, the discovery became a relatively obvious attempt to confirm ideas he developed to explain Gelinas's experiments. In two seminar talks given in late April 1977, one at the Harvard Medical School and one at Rockefeller University, Roberts disclosed Chow's novel results and images without her knowledge or permission. Although Chow eventually stopped supplying Roberts with the crucial images, these talks played a key role in enabling other teams that had been working on related topics to figure out what they were still missing and publish at the same time as CSHL, or even sooner, by opting for a fast-track publication process.

A paper by a mixed team from the DNA tumor virus lab at the National Cancer Institute of NIH and WIS (which included Horowitz but not Lavi, who belonged to a different department) was published in September 1977, at the same time as the CSHL papers. Another paper from the CRC at MIT by Berget, Moore, and their lab director, Sharp, was published in August 1977. Both papers were sent to *PNAS*, a journal that accepted short papers of five pages or fewer and did not use a formal refereeing process (which slows publication). *PNAS* required instead submission by a member of the National Academy of Sciences and endorsement by another member. These teams' decisions to publish short papers fast thus resulted in a loss of priority for the paper by Chow, Gelinas, Broker, and Roberts, and for CSHL in general.

Questions of priority often establish the popular narrative of how a discovery was made. They also set what textbook science looks like. How such choices affected Chow's disappearance from the discovery's public memory and especially from the credit for the 1993 Nobel Prize has been a challenging question. To answer it, I have tried to clarify the claims that the discovery was made independently at two or more sites.

The priority outcome at the time of publication appears to have constrained CSHL to nominate only one person for the Nobel, so as not to upstage MIT, which had nominated only its lab director. Watson did not hesitate to invoke MIT's omission from the nomination of its own first author, Berget, as a reason for excluding Chow, presumably because a Nobel cannot be given to more than three recipients. The possibility that a Nobel Prize could

be awarded to two female nominees and only one male was not achieved until 2009, with the Nobel awarded for the discovery of telomerase.

Given Watson's prior misrepresentation of the unique contributions of assertive women scientists—such as Rosalind Franklin and McClintock, both of whom he had derided, calling the former a bad scientist and the latter “an old bag”—it is not so surprising that he misunderstood the key role played by Chow, an Asian woman scientist, whom her PhD adviser, Davidson, described as “tending to be quiet.” Even when Chow's American-born spouse, Broker, explained her contribution to Watson in great detail, the latter did not budge regarding her nomination. Watson did understand that CSHL's loss of priority made him look like a “loser,” a designation he used to apply to rival labs, most notably in his own “winner take all” telling of the story of DNA structure. This perception may be why Watson did not use his numerous public speaking opportunities to highlight the discovery of RNA splicing made at CSHL. Watson's eventual nomination of Roberts, a long-time CSHL associate and a candidate for its directorship, reflected both an institutional prerogative of highlighting CSHL as the site of the discovery and a preference for Britons, whom he favored in staffing. The choice of Roberts as CSHL's nominee for its share in the 1993 Nobel further solidified MIT's claim to the discovery, which relied on the use of electron microscopy. It is often emphasized that Sharp learned electron microscopy from a key pioneer of this technology at Caltech, Davidson, during his two years there as a postdoc. But no one seems to have registered the fact that Chow spent four times longer in Davidson's lab as a PhD student and a postdoc.

The Nobel committee failed to properly investigate the roles of Chow and the other women involved in this important discovery. But history shows that they could have acted differently. In another case, 15 years later, the Nobel committee showed the importance of thorough investigations while deliberating the 2008 Nobel Prize. For many years two lab directors, Luc Montagnier of the Pasteur Institute in Paris and Robert Gallo of NIH had each claimed to have discovered HIV. After their investigation, the Nobel committee recognized the key role played by Montagnier's colleague Françoise



Photo courtesy of Thomas R. Broker

Chow was elected to the U.S. National Academy of Sciences in 2012. Here, she poses with a poster about her work at a reception honoring her election at the University of Alabama, Birmingham.

Barré-Sinoussi and included her in the 2008 Nobel together with Montagnier. It turned out that Gallo mistook a viral sample he received from Paris as his own—some say deliberately.

Cases in which those who may deserve recognition come from the ranks of marginal social and cultural categories, such as women, junior, or immigrant scientists, should not be decided without thorough vetting. Those carrying out such vetting must conduct historical research and abandon any inclination to maintain the balance of power among the powerful. Chow's intersecting gender, race, and culture turned out to be profound liabilities. As a woman, an immigrant, and an Asian scientist, she did not fit the “profile” of discoverers in either the 1970s or the 1990s.

The Nobel committee should not continue to remain blind to Chow's role in the discovery of RNA splicing. The balance of power in science, much as that between superpowers, is an important goal that Swedes in particular cherish—as demonstrated by their 1982 Nobel Peace Prize for promoting nuclear disarmament. But such goals, however noble, cannot be attained so long as the claims of women and other victims of bias are ignored. By making Chow's predicament better known, while also highlighting the intertwining of credit allocation with institutional agendas, I hope to help science move closer to its ideal of epistemic justice.

## Bibliography

- Abir-Am, P. G. 2014. Women scientists of the 1970s. In *Writing about Lives in Science: (Auto)Biography, Gender, and Genre*, eds. P. Govoni and A. Z. Franceschi, pp. 223–262. Göttingen: Vandenhoeck and Ruprecht unipress.
- Abir-Am, P. G. 2002. Molecular biology in the context of British, French, and American cultures. *International Social Science Journal* 53:187–199.
- Abir-Am, P. G., and C. A. Elliott, eds. 2000. *Commemorative Practices in Science: Historical Perspectives on the Politics of Collective Memory*. Chicago: University of Chicago Press.
- Darnell, J. E. 2011. *RNA: Life's Indispensable Molecule*. Cold Spring Harbor, NY: Cold Spring Harbor Laboratory Press.
- Fry, M. 2016. The surprising discovery of split genes and of RNA splicing. In *Landmark Experiments in Molecular Biology*, 1st edition, pp. 481–521. Amsterdam: Elsevier Academic Press.
- Rosser, S. V. 2004. *The Science Glass Ceiling: Academic Women Scientists and the Struggle to Succeed*. New York: Routledge.
- Rosser, M. W. 2012. *Women Scientists in America: Forging a New World since 1972*. Baltimore: Johns Hopkins University Press.
- Scheffler, E. W. 2019. *A Contagious Cause: The American Hunt for Cancer Viruses and the Rise of Molecular Medicine*. Chicago: University of Chicago Press.

*Pnina Geraldine Abir-Am has published widely on the history of molecular biology, the history of women and gender in science, the history of public memory, and the history of science funding. She is a resident scholar at the Women's Studies Research Center at Brandeis University in Waltham, MA. Email: pninaga@brandeis.edu*