Essay Review

PNINA G. ABIR-AM

DNA AT 50: INSTITUTIONAL AND BIOGRAPHICAL PERSPECTIVES

Soraya de Chadarevian, *Designs for Life: Molecular Biology after World War II*. Cambridge: Cambridge University Press, 2002. 423 pp. £35. ISBN 0-521-57078-6.

Victor K. McElheny, *Watson and DNA: Making a Scientific Revolution*. Cambridge, MA: Perseus Publishing, 2003. xiv + 363 pp. $27. ISBN 0-7382-0341-6.

Brenda Maddox, *Rosalind Franklin: The Dark Lady of DNA*. London: Harper Collins, 2002; New York, 2003. xx + 380 pp. $29. ISBN 0-00-257149-8.

Maurice Wilkins, *The Third Man of the Double Helix: The Autobiography of Maurice Wilkins*. New York: Oxford University Press, 2003. 274 pp. $27. ISBN 0-19-860665-6.

These volumes are among a large number of publications produced as part of the reflections on the past triggered by the fiftieth anniversary of DNA structure, the 'double helix', in the spring of 2003. Although each deals with a different actor, all seek to shed new light on an event that has enjoyed considerable publicity, even notoriety in films, plays, and exhibitions.[1] It is of interest to see how they manage to contribute different perspectives on the 'same' event.

[1] Previous studies include John C. Kendrew, 'How Molecular Biology Started?', *Scientific American*, 215 (1967), 240–242; James D. Watson, *The Double Helix* (New York: Atheneum, 1968); Anne Sayre, *Rosalind Franklin and DNA* (New York: Norton, 1975, 2000); Robert Olby, *The Path to the Double Helix* (London: Macmillan, 1974; New York: Dover, 1994); Horace F. Judson, *The Eighth Day of Creation* (New York: Basic Books, 1979; Cold Spring Harbor Laboratory Press, 1996); Max Perutz, 'Origins of Molecular Biology', *New Scientist*, 85 (1980), 326–329, 'The Birth of Molecular Biology'; *idem*, 114, (1987), 38–41. Francis Crick, *What Mad Pursuit* (New York: Basic Books, 1988); *Horizon TV Programme, The Race for the Double Helix* (London: BBC, 1987); Pnina G. Abir-Am,

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DESIGNS FOR LIFE: MOLECULAR BIOLOGY AFTER WORLD WAR II

Despite its metaphorical title, this volume is not so much a disciplinary history, as a multifaceted account of a major institutional actor, the Medical Research Council Laboratory of Molecular Biology (MRC-LMB) in Cambridge, England. Widely known as the centre of major breakthroughs – notably the solution of the structures of DNA and proteins in the 1950s – the LMB grew from modest beginnings in 1947, when the MRC agreed to make two research appointments, to a trend-setting laboratory with a staff of two hundred. By its fortieth anniversary in 1987, the LMB counted among its members no fewer than half a dozen Nobel Laureates. In 1962, when three LMB scientists shared in two Nobel Prizes, the suggestion was floated that the Prize should have been given to the lab itself, as a site of collective distinction.

Chadarevian's volume can be seen as an elaboration of this suggestion, as well as a refinement – using the methods of oral history, archival research, and cultural analysis – of the memories of the laboratory's leading scientists.[2] Her account covers, or rather samples, key episodes in the lives of two Laboratory directors, Max Perutz and Sydney Brenner, and three unit directors, Fred Sanger, John Kendrew, and Francis Crick. She also discusses their Nobel Laureate colleague, Cesar Milstein, whose discovery of monoclonal antibodies the LMB failed to patent, a factor of great importance in LMB's later adaptation to the commercial requirements of the biotech era.

Chadarevian's book is divided into three thematic parts. In Part I, 'Postwar Reconstruction and Biophysics', she ascribes the prestige of science in postwar Britain to the successes of British scientists in wartime operational research, radar, the atomic bomb, and penicillin. In particular, she dwells on the rhetorical appeal of 'biophysics' in facilitating a transition from the 'science of death', or physics, to the 'science of life', or biology. In a subchapter of chapter 3, entitled, 'War: Mother of all Things?', she credits wartime military experience – with its emphasis upon speed, dedication, interdisciplinary skills, and collaborative practices – with having a decisive influence upon men who later became biophys-

'New Trends in the History of Molecular Biology', *Historical Studies in the Physical and Biological Sciences*, 26 (1), (1995), 167–196. For other aspects of this anniversary, see Abir-Am, *DNA at 50: History, Memory, and Moral Genealogy* (forthcoming).

[2] Editorial, 'MRC – LMB at 40', *New Scientist*, 114 (1987), 37. This special issue includes papers by Perutz, Kendrew, Hugh Huxley, Crick, Watson, Sydney Brenner, Aaron Klug, and Cesar Milstein. At the time all but Huxley and Brenner were Nobel Laureates. Brenner received the Nobel Prize in 2002.

icists and molecular biologists, such as John Kendrew (who completed his war service as a RAF Wing Commander) and to a lesser extent other early LMB members, such as Max Perutz, Francis Crick, and Hugh Huxley.

It is frequently said that biophysics led to molecular biology by virtue of its cultural appeal to guilt-ridden physicists. But Chadarevian's argument (in Chapter 4) is particularly persuasive because of her decision to focus upon John Kendrew. Kendrew pioneered the use of digital computers and densitometers and, in the late 1950s, solved the first structure of a protein – the main goal of the LMB, indeed of all British protein X-ray crystallographers – in record time. Since he also became the first Director of EMBL (the European Molecular Biology Laboratory), Chadarevian can extrapolate from the local context of the LMB to the global rise of the discipline. The sections on Kendrew are among the best in the book.

In another innovative chapter, Chadarevian also deals with the key role of three dimensional models in creating a peaceful and positive public image for the new science. Whether exhibited on television, or at inter/national exhibitions (such as the Festival of Britain in 1951, and the Brussels fair in 1958), Kendrew's model of myoglobin and Perutz's model of haemoglobin were positively chic. Chadarevian emphasizes their appeal to 'televisual language', useful not only as research tools, but also as 'object lessons' for the public relations of science.

In Part II, 'Building Molecular Biology', Chadarevian gives a detailed account of the decisions involved in establishing the LMB in the 1950s. Opened by Queen Elizabeth II in 1962, LMB was the outcome of complex negotiations between local molecular biologists, the MRC, Cambridge University, and individual departments within the University. The departure in 1953 of the Cavendish Professor, W.L. (Sir Lawrence) Bragg, who was instrumental in founding the MRC Unit as part of the Cavendish Laboratory in 1947, exposed the process to several years of university politics.

Chadarevian is adept at highlighting the contingent and local dimensions of this process and its outcome, arguing persuasively that the key to success was an alliance between the biophysicists-turned molecular biologists and Fred Sanger, who left the Biochemistry Department to join them. Sanger's success with the sequencing of insulin in the mid-1950s was rewarded with a Nobel Prize in 1958, just at the time that a joint proposal for a new lab was being considered by the MRC (Sanger was to win another Nobel Prize in 1980 for his share in DNA sequencing). Chadarevian documents how the unexpected support of Joseph. S. Mitchell

– the Professor of Medicine, who offered a hospital site on the outskirts of the University as part of his plans for a postgraduate medical school – proved critical. Owing to vested interests, molecular biology could not otherwise find a place within the University's structure. The world-famous laboratory was to remain only loosely connected with the University of Cambridge.

Inevitably, the book has limitations. For example, since her account begins with the war, Chadarevian misses the stunning similarities between the rejection of interdisciplinarity in the mid- and late-1950s, and similar events in the 1930s, when the University of Cambridge also rejected a proposal for an interdisciplinary programme in molecular biology.[3] Similarly, although Chadarevian correctly emphasizes the commitment of the MRC, she misses the important role played by the Rockefeller Foundation, which preceded the MRC in supporting the Cambridge group by a decade; (1938–1947);[4] as well as the role of other institutions dating from the pre-war era, such as the Molteno Institute of Biology and Parasitology at Cambridge, which was of formative importance to the work of Perutz and Kendrew.

Along these lines, a solitary focus on the LMB underestimates its interaction with other, perhaps smaller but nevertheless influential laboratories, such as Bragg's at the Royal Institution, where Kendrew had a part time appointment. Bragg's permanent lobbying for a protein X-ray crystallography unit at Cambridge, after his departure in 1953, also deserves more emphasis. Similarly, there is no delving into the LMB's relationships with Dorothy Hodgkin's laboratory at Oxford, where Perutz was given critical advice on the use of isomorphic replacement, a turning point in his work on haemoglobin.

Nor is there much on J.D. Bernal, whom all the MRC Nobel Laureates in 1962 acknowledged in writing as their scientific inspiration, and whose lab at Birkbeck supplied the LMB with its TMV (tobacco mosaic virus) structure team. That team was created by Rosalind Franklin and led after her death by Aaron Klug, one of the LMB's later Nobel Laureates. Bernal, a founder of protein X-ray crystallography – the initial rationale of LMB – and Randall, who after the war established the first MRC Biophysics Lab, at King's College, are duly mentioned, but there is no sustained

[3] Pnina G. Abir-Am, 'The Biotheoretical Gathering, Transdisciplinary Authority and the Incipient Legitimation of Molecular Biology in the 1930s', *History of Science*, 25 (1), (1987), 1–70.

[4] Pnina. G. Abir-Am, 'The Rockefeller Foundation and the Rise of Molecular Biology' [Three British case studies], *Nature Reviews – Molecular Cell Biology*, 3 (1), (January 2002), 65–70.

discussion of their significance to the LMB. The LMB may well have been the largest, quasi-national laboratory, but the assumption that its story alone will enable one to grasp the history of molecular biology, is problematic. We need a network analysis of several laboratories, as well as systematic comparative studies with counterparts in the USA and France, to understand the rise of molecular biology in the twentieth century.

This point gains added emphasis from Chadarevian's exploration of the role of the 'double helix' in molecular biology at Cambridge. She argues that the double helix played no great role in this respect; indeed, that its role was preempted by protein models, notably myoglobin and haemoglobin. She also argues that the double helix played at best an indirect part in the research programme of Crick and Brenner on the genetic code. Her argument helps refute the ahistorical but common belief, which abounded at the fiftieth anniversary celebrations, that the iconic double helix had an immediate impact upon molecular biology, and led in a straight line to the Human Genome Project.[5] Chadarevian's meticulous demonstration of its marginal contribution (while resourcefully tracking down the sad fate of the metallic parts of the initial model) should remind both scientists and historians that such straight lines, or 'paths', are almost unfailingly conceived in retrospect, and as such, are not good history.

In Part III, 'Benchwork and Politics', Chadarevian explores the LMB's 'culture'. From 1962, the Laboratory was led by triumvirates of Nobel Laureates, each of whom also ran a whole floor of research labs. A comparison with the Biophysics Lab at King's College, London, run by Randall along strict authoritarian lines, would have enhanced her comments about 'culture'. Collaboration within divisions, common areas for interaction (mainly the comfortable canteen), and a constant influx of transient, mostly American, post-docs, all encouraged communication. In the mid-1970s, when British science became subject to new forms of social and political accountability, the 'lab culture' perceptibly changed. Chadarevian refrains from contrasting the situation before and after the introduction of requirements for medical payoff and industrial relevance led to reorganization along managerial lines. She does dwell briefly on the policy context that, in the wake of changes in government funding and the rise of commercial interests, may explain the 'monoclonal antibody scandal'. However, the key question, how changes in science policy may affect potential for innovation, is left for others to ponder.

In summary, Chadarevian's approach has many merits, even though it vastly underestimates the significance of the inter-war period to the

[5] 'From the Double Helix to the Human Genome: Forty Years of Molecular Genetics', *Gene*, 125 (1993), a special issue on the Cogene Symposium.

rise of molecular biology, and the LMB's dependence upon other labs, both British and foreign.[6] Although as a unit of analysis, the LMB is demanding; she navigates around the many prima donnas populating the legendary lab, deftly discussing projects and policies, yet keeping her eye constantly on disciplinary, rather than personal, issues. If her argument sometimes appears to vacillate between the sober recognition that a 'local' study can only be local, and the claim that the 'local' may somehow be 'global' as well, her analysis does make a major contribution to our understanding of the rise of molecular biology, from a scientific 'no man's land' to the very centre of the scientific imaginarium.

Watson and DNA: Making a Scientific Revolution

McElheny's is the first book to cover Watson's entire career in one narrative.[7] As such, it is a uniquely useful insight into the unfolding of what McElheny calls 'an improbable career', as well as into the experiences that produced a colourful character who basks in bizarre behaviour, while amassing influence, power, and wealth. Given Watson's public presence and celebrity status, it is strange that so little has been written about him. Even that little has been limited to his two years in Britain – between the ages of 23 and 25 – and to the autobiographical recollections (in *The Double Helix*) of a self-crafted character who excels in observing the defects of others while revealing little about himself.

McElheny received no help from Watson, neither interviews, nor access to personal papers – perhaps because Watson, cognizant of the commodity value of his name, and his de facto ownership of the DNA mystique, has been busy churning out volumes of his own. There is little overlap between this account and Watson's own books, including a sequel covering his life 'after the double helix' (1953–1968); a collection of essays on the social resonance of genes and genomes; and a companion volume to a TV series on DNA research.[8] Still, McElheny's book would have been more valuable

[6] Pnina G. Abir-Am, 'From Multidisciplinary Collaboration to Transnational Objectivity: International Space as Constitutive of Molecular Biology, 1930–1970', in Elizabeth Crawford *et al*. (eds.), *Denationalizing Science: The Contexts of International Scientific Practice* (Dordrecht: Kluwer, 1993), 153–186.

[7] John Inglis *et al*. (eds.), *Inspiring Science: Jim Watson and the Age of DNA* (Cold Spring Harbor: Cold Spring Harbor Laboratory Press, 2003) is a volume of memories compiled by colleagues and students. See the review of this book by Pnina G. Abir-Am, 'Watson's World', *American Scientist*, 92 (3), (May–June 2004), 286–288.

[8] James D. Watson, *Genes, Girls, and Gamow* (New York: Knopf, 2001); *A Passion for DNA: Genes, Genomes, and Society* (Cold Spring Harbor: Cold Spring Harbor Laboratory Press, 2000); J.D. Watson and Andrew Barry, *DNA: The Secret of Life* (London: Heineman, 2003). See the reviews by Maxine Singer in *Nature*, vol. 422, 24 April 2003, 432–434.

had it appeared earlier. Thanks to the fiftieth anniversary, Watson was so widely covered by the media that many of the spicy stories assembled by McElheny have already found their way into the public domain. McElheny acknowledges his friendship with Watson, dating from the 1970s, when he became the first director of a conference centre that Watson established at the Cold Spring Harbor Laboratory. He is evidently grateful that Watson did not attempt to stop this book, and avoids personal topics.

This ambivalent situation may explain his blend of revelation and discretion. A science journalist who has covered molecular biology since the 1960s, and who directed MIT's Program in Science Journalism until his recent retirement in 1998, McElheny knows how to interview scientists (he contacted about fifty). He justifies his biographical perspective with the argument that Watson's career is the best guide to the DNA revolution. Having participated in a great discovery early in life, Watson went on to become, according to McElheny, 'an intellectual manager on a vast scale', converting himself into a scientific impresario whose most lasting accomplishment may turn out to be in the realm of institution-building.

About half of McElheny's book discusses events covered by others. Chapters 3 and 4 retell Watson's best known period, 1951–1953; Chapters 7 and 8 tell the stories of messenger–RNA and the reception of Watson's 1968 best seller, respectively; and the last two chapters, 11 and 12, cover Watson's involvement with the recombinant–DNA controversy in the late 1970s and the Human Genome Project (HGP) in the late 1980s and early 1990s. However, McElheny also illuminates many unknown aspects of Watson's career, focusing upon what he sees as Watson's personality. McElheny's narrative includes new material on Watson's childhood in Chicago; his days in graduate school at Indiana University; his postdoctoral period at Caltech; his time as a professor of biology and textbook pioneer at Harvard; and his vocation as manager, fund raiser, event organizer, publisher, real estate and landscape developer, expanding Cold Spring Harbor Laboratory into a major international centre.

McElheny accepts Watson's cultivated stereotype as a 'mad genius' ('his problem was that he was a genius', p. 4). Nevertheless, his research into Watson's childhood, together with Watson's own comments ('Values from a Chicago upbringing') shed new light on Watson's behaviour, especially in the context of the double helix and its autobiographical rendition in 1968. McElheny (following a few scientists who have dared to express themselves in print) charitably describes Watson as brash and arrogant, lacking consideration for others (except his nuclear family), and devoid of minimal manners, cynical and utilitarian (McElheny quotes him as saying,

'being nice is something you do when you have nothing else to offer', p. 9). His formative upbringing coincided with the Great Depression, when a preoccupation with survival and a lack of opportunities apparently precipitated a rough, angry, vindictive, exploitative, and detached persona.

Watson was unable to 'edit' anything that came out of his mouth ('you were never held back by manners, and crap was best called crap', p. 16). Chapter 6, which describes his two decades at Harvard, is full of four letter 'f' words, often in triple reinforcement; gossip without restraint, invariably offensive, even character-killing. Watson appears not to mind that he comes across as rude and crude. How such a character was produced has long remained an enigma. Now McElheny resolves it, largely by supplying telling details from Watson's childhood. Being un-athletic, bookish, small, and an object of bullying and beating, he sought escape from the cruel world of his peers into the company of books and adults. By his own account (p. 1), he had never been an adolescent, and presumably passed from childhood directly into adulthood sometime around the age of forty, after both parents had died, and when he married.

The company of books, often encyclopaedias, supplied Watson with a form of learning saturated in unrelated facts, as well as with an 'information advantage' – an asset much encouraged by popular radio contests in which a minimal edge in knowledge acquisition could win instant fame. The mastery of information became a ticket to being on top. *The Quiz Kids*, airing in June 1940 when Watson was aged twelve, cultivated competitiveness, where the objective was not to play well in a team, but to be a lone winner. In that context, a winner need not be the most accomplished, but the one best placed to take advantage of mistakes made by others. The relevance to Watson's behaviour in relation to the double helix may be obvious, especially (McElheny tells us) as Watson did not last long as a contestant in *The Quiz Kids*, where the frequent winner was a girl.

In order to understand Watson's second claim to fame – his rendition of his experience in *The Double Helix*, a best-seller that popularized his view of discovery as a race and of himself as a winner – it makes sense to pay attention to another dimension of childhood experience, namely the tendency to confuse book characters with real people. By Watson's own account, he was attracted to science by its portrayal in Sinclair Lewis's Pulitzer Prize-winning 1925 novel *Arrowsmith* ('basically the only book I ever read that told me about [science] was *Arrowsmith*', p. 11). McElheny finds the novel 'full of echoes of Jim's future' (*ibid.*), but refrains from speculating whether this inspired Watson to emulate *Arrowsmith*. However, anyone familiar with Watson's infatuation with the Phage Group convenor, Max Delbruck – including his imitation of

Delbruck's German accent, haircut, tennis habits, inconsiderate interruptions of seminar speakers with blunt and non-pertinent criticism, and intimidation of others through gossip and public embarrassment,[9] will recognize a plausible 'role model' in the relationship between Martin Arrowsmith and Max Gottlieb in Lewis's novel (Gottlieb was modelled, largely though not entirely, on Jacques Loeb, who died conveniently a year before Lewis's novel was published).

In *The Double Helix*, Watson's strategy of fictionalizing people whom he encountered at King's College, London, or in Cambridge, resembles Lewis's composite fictionalizations of scientists at the Rockefeller Institute for Medical Research in New York. To be sure, Kingsley Amis's *Lucky Jim* (London, 1954), set in England's academic milieu just at the time Watson lived there, was another model. Watson's first working title, *Honest Jim*, was clearly inspired by Amis's best-seller. However, unlike Lewis and Amis, Watson felt confident that he could get away with using the real names of protagonists. This is why some who were mentioned in *The Double Helix*, or others on their behalf, either tried to prevent its publication, or wrote books to reclaim their own personae.

By the time he entered graduate school at Indiana University (Caltech, his first choice, turned him down), Watson had been well served by the University of Chicago. There, he received good advice (to go to Indiana) from the great teacher and theoretician of genetics, Sewall Wright. However, as many would-be molecular biologists, Watson prefers to credit the influence of *What is Life?*, Schroedinger's highly readable and rhetorically appealing book on how physics could revolutionize genetics, by conceiving of the genetic code as an aperiodic crystal.[10]

Watson's early tendency to benefit from the ideas and advice of great scientists, but to omit their influence in favour of a mythology of lone genius dominated by books rather than people, continued at Indiana University, where he met H.J. Muller, the discoverer of artificial mutation

[9] On the Phage Group's influence on Watson, see William Summers, 'The Phage Group, Physics, and DNA', Lectures at the American Association for the Advancement of Science Annual Meeting session on 'DNA at 50', 15 February 2003; and History of Science Society Annual Meeting, Session on 'DNA at 50', 21 November 2003. On Watson's construction of Delbruck into the main figure of his scientific genealogy, see Pnina G. Abir-Am, 'The First American and French Commemorations in Molecular Biology: From Collective Memory to Comparative History', in Abir-Am and Clark A. Elliott (eds.), *Commemorative Practices in Science: Historical Perspectives on the Politics of Collective Memory* (Chicago: University of Chicago Press, 2000), 324–372.

[10] The impact of Schroedinger's book has been discussed by many authors, most recently by Leah Ceccarelli, *Shaping Science with Rhetoric: The Cases of Dobzhansky, Schroedinger and Wilson* (Chicago: University of Chicago Press, 2001).

by X-ray radiation. In his day, Muller was the most prominent advocate of the gene theory, and a visionary promoter of partnerships between geneticists and physical scientists (as early as 1922 and later in 1946, when he reviewed Schroedinger's book).[11] A recent Nobel Laureate (1945), Muller was recruited a year earlier by Dean Fernandus Payne (who also gave a scholarship to Watson), the only living Nobel Laureate in genetics at the time (the other Nobel Laureate in genetics, Thomas H. Morgan, died in 1945). Muller was also a politically active scientist, who received considerable attention during the Lysenko scandal. It would have been impossible for young Watson to miss Muller's presence. Yet, the only reference to Muller in his autobiography, is to the effect that the best days of *Drosophila* (a major organism in classical genetics) were over, and smarter people were working on micro-organisms instead.

McElheny's early chapters help explain how, within a few years, an unknown and not particularly accomplished Watson managed to associate himself with a major discovery. A major clue comes in Watson's unusual ability to secure illustrious mentors, apparently through calculated cultivation – a skill developed during the extended childhood that he spent in the company of adults rather than peers. Extended observations of elders, especially by one who was also an avid bird-watcher, was likely to reveal their flaws and weaknesses. In *The Double Helix*, Watson displayed a remarkable talent for reducing a person to his weakest traits, which he was adept at spotting prior to discarding in favour of a new specimen.

For example, as soon as he finished his PhD, Watson dropped his adviser, Salvador E. Luria, and endeared himself to Delbruck. Although Watson's eventual focus on DNA was directly influenced by the innovative work of biochemists in the Phage Group – including Frank Putnam, Lloyd Kozloff, and Seymour Cohen – he never acknowledged this, and claimed Delbruck as his principal influence,[12] even though Delbruck's distaste for biochemistry turned out to be very poor guidance for someone who would make a kill in a master molecule.

A similar pattern obtained in Cambridge, where Watson went in order to work with John Kendrew in protein X-ray crystallography. This, Watson revealed, he was both unable and unwilling to do so. Instead, he teamed up with Kendrew's main competitor, Francis Crick. Watson derided the

[11] On Muller and Schroedinger's roles in molecular biology, see Evelyn Fox Keller, 'Physics and the Emergence of Molecular Biology: A History of Cognitive and Political Synergy', *Journal of the History of Biology*, 23 (3), (1990), 389–409.

[12] M. Biagioli and P. Galison (eds.), *Scientific Authorship, Credit and Intellectual Property in Science* (London: Routledge, 2003). For invention of scientific lineages in molecular biology, see Abir-Am, *op. cit*. note 9.

enterprise of protein X-ray crystallography that made possible his arrival in England; scared W.L. Bragg with the spectre of another Pauling coup; bullied the new arrival Peter Pauling into revealing an unpublished manuscript of his father; and pretended that he was interested in TMV so as to be accepted at the Molteno laboratory during Bragg's ban on DNA. When he referred to his time at the Molteno in *The Double Helix*, all that Watson told his readers was that its distinguished director, David Keilin, the discoverer of respiratory pigments and a key supporter of Perutz and Kendrew, had asthma, which accounted for Molteno's better heating system.

All of the above 'accomplishments' in Cambridge were dwarfed by Watson's achievements in London, where he shrewdly converted Wilkins into the crow of Lafontaine's fable on the fox and the crow, as well as noticing that, owing to the rivalry and sexism permeating the labs, he would not be taken to task if he were to help beat another lab, no matter what this might require. This capacity to manipulate people, especially people of distinction, was quite remarkable. His uncanny ability to lump, dump and jump from one mentor to another continued at Caltech, where Watson did his post-doctoral studies with Delbruck. When Watson antagonized both Delbruck and Pauling, George Beadle stepped in to protect him from being drafted, to keep him at Caltech. By then, however, Watson had already laid the ground for moving to the East Coast.

During the summer of 1954, at the Marine Biological Laboratory at Wood's Hole, Massachusetts, where many New England scientists combined business and pleasure, Watson discovered his last and most important mentor, Paul Doty, a leading biophysical chemist at Harvard, and a major figure in science policy during the Kennedy administration. McElheny's chapter on Harvard is enough reason to buy the book. It seems that Doty's friendship with McGeorge Bundy, Dean of Harvard's Faculty of Arts and Sciences, was pivotal in having Watson hired, promoted earlier than usual, and kept on for two decades. In an institution ostensibly dedicated to undergraduate teaching, Watson managed to survive – despite his habit of talking to his shoes or to the wall – by sharing with students the exciting experiments that made the 1950s and 1960s a golden age in molecular biology. As one former student put it, 'Watson is a phenomenon sui generis. In class he has no exceptional wit, or showmanship, and none of the charisma of the old-time prof., but he is clear and intuitive' (p. 100).

By contrast, graduate students had to endure the tension and competition that Watson invariably and artificially promoted. As one put it, 'You have to have an enemy. It doesn't matter whether the enemy is real or

not' (p. 102). Another recalled that Watson, 'is driven by anger at people. He is motivated by anger. He is vindictive. He demonizes competitors. He ridicules them' (*idem*). Revealingly, Harvard's renowned biologist, E.O. Wilson referred to Watson as the 'Caligula of biology'.

As McElheny relates, Watson enjoyed the self-propagated aura of genius and celebrity, and avoided accountability. No wonder that after such a spoiling decade at Harvard, he came to believe that he could get away with omitting his sources, both in science and in history, and with boasting about it. Under the threat of a lawsuit from Watson's Nobel co-Laureates (Crick and Wilkins), Harvard's President N.M. Pusey ordered Harvard University Press to renege on publishing Watson's 'literary' forays (their market value was soon recognized by a commercial publisher in New York). Nevertheless, Watson remained at Harvard for another six years, while commuting to the Cold Spring Harbor Laboratory as its part-time director.

Although McElheny received approving comments from former students, it is not obvious from his account that Watson's style helped his lab make major discoveries. Even if one accepts Doty's generous argument that Watson was not averse to risk-taking – choosing complex and important problems such as protein synthesis or RNA's regulatory role[13] – serious questions remain about the invariably negative impact of his erratic behavior on others. McElheny refrains from taking a more critical view, but he provides sufficient background to allow one more so inclined to conclude that Watson's special status enabled him to prevent other teams from getting proper recognition (e.g. by leaking unpublished results to which he gained access because of his aura and aggressive questioning, pitting scientists against each other, and creating artificial tensions). Perhaps the best-known case was the discovery of messenger–RNA.

Watson's eight-member team trailed behind a smaller team of three scientists, comprising Sydney Brenner, Francois Jacob, and Matt Meselson. However, Watson used his influence with *Nature* to stall a paper submitted by the rival group until his own group was ready, so that priority could be artificially shared by publishing the papers back to back. As a result, nobody got proper credit for the discovery of m–RNA, with unfortunate consequences for those involved.[14] It is common wisdom that Watson's most important discovery at Harvard was the discovery of Wally Gilbert, perhaps the most accomplished molecular biologist in

[13] Paul Doty, 'Watson at Harvard', in Inglis (ed.), *op. cit*. note 7, 203–210.

[14] Werner Maas, *Gene Action: A Historical Account* (Oxford: Oxford University Press, 2001), ch. 10; Judson, *op. cit.* note 1, Section II (RNA).

the USA. Gilbert, a theoretical physicist only four years Watson's junior, was initially part of Watson's team on m–RNA, and later went on to isolate the repressor; synthesize the insulin gene; develop fast sequencing methods for DNA (for which he shared the 1980 Nobel Prize); introduce the concept of 'genes in pieces'; and become a visionary leader in both the biotech industry and the Human Genome Project.[15] Watson's career at Harvard ended in the mid-1970s, with his unsuccessful bid for a cancer research institute, to take advantage of Nixon's war on cancer. Watson left permanently for Cold Spring Harbor, where he apparently found his true vocation, that of scientific entrepreneur. As McElheny intimates, Watson's transformation of a decaying laboratory on the verge of bankruptcy into 'DNA Town', a complex of laboratories, conference centre, and community outreach programmes may well become his most enduring legacy.

Next to his chapter on childhood, McElheny's chapters on the Cold Spring Harbor Laboratory are the most valuable in the book, not only because they cover less well-known events, but also because McElheny, as the first director of its conference centre, speaks from direct experience. Watson's conduct toward McElheny is emblematic – with a seductive and opportunistic beginning, followed by a frenzy of artificial tensions with imaginary enemies, and ending on a note of public embarrassment. McElheny's observations of Watson's showmanship (such as untying his shoe laces, and undoing his tie upon entering a meeting with trustees, so as to convey the image of a 'mad genius') are revealing. Ironically, Watson who has been accused by his co-Laureates of invasion of privacy, managed to maintain his own privacy until he retired from the laboratory's directorship (he is now its president).

In illuminating the 'good, the bad, and the ugly' in Watson's persona, McElheny has gone beyond mythology. However, his argument that Watson is still to be primarily credited with both the discovery of DNA's structure and its transformation into a biological revolution is not only inflated, but mistakes image for logic. In the absence of a symmetric treatment of Watson's successes and failures, it is difficult to regard McElheny's biography as definitive. Indeed, two recent books on Franklin and Wilkins provide information that affects our understanding of Watson, yet this is information that neither McElheny nor Watson has as yet assimilated.

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[15] On Wally Gilbert, see entry by Pnina G. Abir-Am in Laylin K. James (ed.), *Nobel Laureates in Chemistry, 1901–1992* (Washington, DC and Philadelphia: American Chemical Society and Chemical Heritage Foundation, 1993), 626–631.

Watson's insistence in associating his name with everything related to DNA and its mystique,[16] including his marathonic appearances at the events marking the fiftieth anniversary, raises the question as to why his possessiveness – as if DNA could be the private property of any single individual – continues to be tolerated. Such an excessive involvement may have contributed to the ahistorical features of 'DNA at 50',[17] as well as for continuing delays in giving scientific and moral restitution to the work of others.

Rosalind Franklin: The 'Dark Lady' of DNA

Maddox's biography of Rosalind Franklin provides a telling contrast with McElheny's biography of Watson, while at the same time, offering a sad commentary on the ethical world of science. Maddox is not preoccupied by the 'great injustice' or lack of recognition for Franklin's contribution to the elucidation of DNA structure, but supplies plenty of details, both old and new, that will help others better understand the confusion still surrounding the discovery of DNA's structure fifty years later. As others before her, most notably Franklin's pioneering biographer, Anne Sayre, Maddox takes Watson to task for deliberately distorting both Franklin's scientific standing and her physical appearance, as well as for using her data without permission.

To this day, Franklin remains deprived of recognition for doing all but the two last steps in the work on DNA structure. Ever since Watson's revelations in *The Double Helix*, later elaborated by others, it has been known that he and Crick were able to complete the last two steps once they came into possession of Franklin's battery of unpublished results via an MRC Report that, without Franklin's knowledge, Perutz gave to Crick and Watson late in 1952. Their breakthrough depended upon Franklin's interpretation and data (including her discovery of two interchangeable forms of DNA, A & B, and various parameters for each form, such as the number of chains, the diameter of the molecule, the pitch of the helix, the position of the phosphate groups (on the outside) and of the bases (on the inside) the space group, the anti-parallelism of form A – all obtained during the preceding two years).

Nevertheless, Crick and Watson failed to acknowledge being in possession of these results, citing instead 'general knowledge', as if precise measurements such as Ångström units (the molecular repeat in form B)

16 Dorothy Nelkin and M. Susan Lindee, *The DNA Mystique: The Gene as Cultural Icon* (San Francisco: Freeman, 1995, 2004).

17 Pnina G. Abir-Am, *DNA at 50: History, Memory, and Moral Genealogy* (forthcoming).

were not specific enough. In reality, they were in no position to publish their insights into base pairing and anti-parallel stranding without using Franklin's results. However, instead of acknowledging her input, they misled the scientific community into believing that they did their work independently of hers. Furthermore, by claiming to be independent, their model appeared to predict Franklin's results, transforming them at once into a trivial derivation, even though their experience with those results was exactly the other way around.[18]

Although Maddox discussed these matters with Crick and Klug,[19] Watson and Wilkins, among others, she is reluctant to conclude that the two steps that Franklin had not taken by 18 March 1953, when the double helix model was completed, paled in comparison with what Franklin had already achieved. Maddox also falls short of saying that, while Franklin did what she did entirely on her own, Watson and Crick could do what they did only by pretending that someone else's work was theirs. Therefore, Maddox remains trapped by the prevailing view, propagated by Watson, whom she describes as a family friend; as well as by Wilkins – namely, that although Franklin may not have been as anti-helical and/or anti-model building as they claimed all along (notions since disproved by access to her notebooks), somehow she lacked the imaginative leap required to accomplish the last two steps.

Since such a soothing conclusion is no radical break with the common and comforting sexism still prevalent in the scientific community, Maddox seeks to explain Franklin's isolation and unhappiness at King's – factors which may have contributed to her being scooped in the big discovery – in terms of race and class differences. More specifically, Maddox argues that Franklin's difficult situation at King's, which led to her leaving in the midst of the work on DNA, did not stem from the sexism of her mysoginist male colleagues, but from their prejudices against her Anglo-Jewish and upper-middle class background. Maddox is unhappy with Franklin's status as a

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[18] Lynne Osman Elkin, 'Rosalind Franklin and the Double Helix', *Physics Today*, 56 (3), (March 2003), 42–49; Lectures at AAAS Session on 'DNA at 50', 15 February 2003; American Chemical Society Annual Meeting, New York City, September 6, 2003; History of Science Society session on 'DNA at 50', 21 November 2003, mentioned in note 10; Library of Congress Series, Washington DC, 2 December 2003. See also Sayre, *op. cit*. note 1; Abir-Am, *op. cit*. note 17; and Sharon Bertsch McGrayne, *Nobel Prize Women in Science* (Seacaucus/NJ: The Citadel Press, 1998, 1993), 304–332.

[19] For Sayre and Crick, see *op. cit*. note 1. For Klug, see Sir Aaron Klug, 'Rosalind Elsie Franklin, 1920–1958', *Encyclopedia of the Human Genome* (London: Macmillan Reference, 2003), 1–2; 'Rosalind Franklin and the Double Helix', *Nature*, 248 (26 April 1974), 78; 'Rosalind Franklin and the Discovery of the Structure of DNA', *Nature*, 219 (24 August 1968), 808–810, 843–844.

feminist icon – or as she puts it, some sort of 'Sylvia Plath of molecular biology' (xviii) – and has come to believe that, since the perception of Franklin as a solitary woman turns out to be incorrect, then factors other than gender must be called upon to explain her predicament.

As Maddox shows, by the standards of the early 1950s, the King's College Biophysics Laboratory indeed employed a considerable number of women scientists (The Cavendish at Cambridge had almost no women at all). At King's, two women were relatively senior – Dame Honor B. Fell, Director of the Strangeways Laboratory of Cell Biology at Cambridge, whose presence in London as a visiting biologist was a condition imposed upon the Biophysics Laboratory by the MRC; and Jean Hanson, FRS, who achieved distinction as co-author with Hugh Huxley of the discovery of the sliding mechanism of muscle contraction. The junior women included students and technicians.

Having several women in a lab does not mean that they were considered as equals or enjoyed equal opportunities. Maddox underplays the restricted conditions under which women scientists in Britain worked in the 1950s.[20] Their exclusion from 'gents-only' dining facilities and tea rooms underscored their status as a tolerated minority in science. Nevertheless, having convinced herself that gender was not itself the key issue, Maddox sets out to argue that Franklin's predicament at King's had more to do with social prejudices on issues of ethnicity, or 'race' and 'class' differences. Maddox introduces Franklin's exceptional family – upper class Anglo-Jewry, with a considerable presence in public service and philanthropy – that came to London from Central Europe during the eighteenth century, much as did the better known Battenbergs (later Mountbatten) and Rothschilds. Franklin's relatives included Sir Moses Montefiore, adviser to Queen Victoria; Sir David Salomon, the first Jewish Lord Mayor of

[20] On women in science during the 1940s and 1950s, see Margaret W. Rossiter, *Before Affirmative Action: Women in Science, 1940–1972* (Baltimore: Johns Hopkins University Press, 1995); Pnina G. Abir-Am and Dorinda Outram (eds.), *Uneasy Careers and Intimate Lives: Women in Science, 1789–1979* (New Brunswick: Rutgers University Press, 1987, 1989). See also Barbara Low, Professor Emerita of Biophysics at Columbia University (who did pioneering X-ray work on penicillin during the Second World War and received her DPhil from Oxford a couple of years after Franklin) to the Editor of the *London Review of Books*, 17 April 2003, commenting on Robert Olby's repetition, while reviewing Maddox's book, in 2002, of Wilkins and Watson's canard, according to which Franklin could not interpret her own data. See also Georgina Ferry, *Dorothy Hodgkin: A Life* (London: Granta, 1998) for the parallel career of a British Nobel Laureate, also in the field of biomolecular X-ray crystallography. On women in the field of X-ray crystallography, see Maureen Julian, 'Kathleen and Thomas Lonsdale', in Helena M. Pycior, Nancy G. Slack, and Pnina G. Abir-Am (eds.), *Creative Couples in Science* (New Brunswick: Rutgers University Press, 1996), 235–260.

London; and Viscount Herbert Samuel, the first British High Commissioner of Palestine. The Rothschilds were second cousins. This makes exciting reading, and Maddox's discussion of anti-semitism as a factor shaping Franklin's environment greatly enriches our understanding.

In a letter to his mistress, Venetia Stanley, Prime Minister Herbert Asquith once referred to Franklin's great uncle (Lord Herbert Samuel) as a perfect illustration of Dizzy's maxim that 'race is everything' (p. 8).[21] An inter-war memo from the diplomat and writer Harold Nicholson urged the Foreign Office not to appoint Jews, on the grounds that they were more interested in international life than were Englishmen, and that, therefore, the service might be flooded by clever Jews (p. 10). The elder son of Stanley Baldwin, a future Prime Minister, once declared that he hated all Jews (p. 11). Maddox adduces many such illustrations to support her view that Franklin's ethnicity was a major liability. Maddox's perception was supported by an exhibition on 'DNA at 50', at the Wellcome Institute in London, where an installation of quotations pertinent to Franklin's life includes one from her friend Peggy Clark, referring to an anonymous Newnham student who in the late 1930s asked Clark: 'Why are you friends with Ros? Don't you know, she is a Jew'.

However, in seeking to extrapolate from social anti-semitism to the situation in the Biophysics Laboratory, Maddox's evidence is less compelling. It cannot be excluded that individual scientists may also have been anti-semitic; or that they may have refrained from making contacts with Franklin, which could have been scientifically useful to her. But in the leftist circles in which Franklin moved, political orientation (mentioned but not emphasized by both Maddox and Sayre) may have been more compelling than either ethnicity, religion, or class.

Still, there is evidence that anti-semitic attitudes in the Civil Service were not without consequences for the DNA story. There were two ways in which Franklin's unpublished work reached Watson and Crick. One was via a Progress Report written by members of the Biophysics Laboratory at King's College for a visiting committee, a committee which was established to 'keep an eye' on its Director, John Randall. A war hero with a fine reputation as co-inventor of the cavity magnetron (a key device in radar), Randall was nevertheless distrusted by the MRC's Secretary (Sir Edward Mellanby) for being too entrepreneurial ('he has too many demands and presents them too often'), and for having 'too many Jews and foreigners' in his lab.[22]

[21] The reference is to the Prime Minister, Benjamin Disraeli, born Jewish but baptized into the Anglican church as a child.

[22] This evidence is discussed in Abir-Am, *op. cit.* note 4.

Max Perutz was a member of the visiting committee, and gave his copy of the MRC Report – which in its late 1952 version included Franklin's results – to his then PhD student, Francis Crick. The report was 'Restricted', but not 'Confidential'.[23] Watson and Crick failed to acknowledge Franklin's material, in part because of the likely embarrassment involved in revealing that they had obtained access to a restricted report. It is both ironic and telling that an oversight committee, established largely as a result of anti-semitic and xenophobic prejudice, may have had a greater negative influence on her career than the more pervasive but perhaps less consequential social anti-semitism that Maddox documents.

Along these lines, Maddox seeks to clarify the poor communication surrounding Franklin, a pattern which had definite consequences for her ability to keep informed of pertinent changes in regards to DNA work at both King's and the Cavendish, while alluding to class differences, which loomed large in class-obsessed British society. Conceivably, Franklin's social status, evident in her manner of speech, and participation in grand social occasions, may have intimidated her male colleagues and those college porters who, at the end of a long working day, might have seen her being collected in a limousine and taken to a ball. Maddox's emphasis upon the role of social prejudice around ethnicity and class in fostering poor communication is well taken. But this should not obscure the fact that the problematic issues in the DNA story derived primarily from artificial scientific competition (Randall put two people to work on the same problem without clearly delineating their respective responsibilities); blurred lines of managerial authority (Wilkins asked for an assistant, but Randall hired an independent scientist better qualified than Wilkins, his assistant director), and gender consciousness.[24]

Furthermore, despite Maddox's heroic efforts to dispose of gender as an explanatory factor, it seems that sexism in both science and society led most male scientists, including Wilkins and Randall, to assume that a woman could not own scientific property, and therefore that her results belonged to the Laboratory, i.e., to themselves, as its directors. It is worth recalling that Watson and Crick's argument in 'justifying' their appropri-

[23] The MRC Report included other information, but the part useful to Watson and Crick pertained to Franklin's measurements of various DNA parameters in forms A and B. The assumption was that the report would circulate among members of the oversight committee only, certainly not among direct competitors.

[24] On gender hierarchy, especially in conjunction with race, and its role in modern science, see Donna J. Haraway, *Primate Visions: Gender, Race, and Class in Modern Science* (London: Routledge, 1989); see also Evelyn Fox Keller, *Reflections on Gender and Science* (New Haven: Yale University Press, 1985). On gender consciousness in various scientific disciplines, see Abir-Am and Outram, *op. cit*. note 20.

ation of Franklin's results rested in part on the premise that if they did not do it, Wilkins and Randall would.

Nonetheless, Maddox's biography accomplishes several goals. First, by detailing Franklin as a scientist of distinction in three different scientific research areas (coal, DNA, and virus structures), she helps lay to rest the silly notions circulated by Watson, Wilkins, and their followers that she was a bad scientist who could not interpret her own data. Her accomplishments make it clear that she was first rate, as J.D. Bernal, the head of her laboratory at Birkbeck, acknowledged in obituaries written for *The Times* and *Nature*.[25]

Maddox, much as Sayre, makes it clear that, without Franklin's work, Watson and Crick could not have taken their final steps at the time they did. What Maddox misses is the conclusion, that if, indeed, Franklin completed the lion's share of the work before Watson and Crick wrote up their model, then the two last steps cannot possibly entitle them to exclusive status as sole discoverers of DNA's structure. Although Maddox falls short of proclaiming Franklin as a 'co-discoverer', along the lines suggested by Lynne Osman Elkin,[26] she does provide decisive evidence that Franklin was the single most important worker in the story.

Maddox's biography also extends Sayre's portrait of Franklin as a remarkable human being, active in social causes, in the best tradition of her public service-oriented family. Taking Watson to task for his distortions of Franklin in *The Double Helix*, she shows the 'real' Rosalind as completely different from the bluestocking caricature invented by Watson (with help from Wilkins). Dedicated to science, devoted to her family, loyal and helpful to her friends, loving of children, a cosmopolitan intellectual at home in the US and France, a mountaineer, a heroic fighter against cancer, Franklin emerges briefly as an icon, not only for women scientists but for all scientists, especially those who have trouble, fifty years on, in properly acknowledging their sources.

The 'Third Man' of the Double Helix: The Autobiography of Maurice Wilkins

Having at last decided, after fifty years, to speak up, the revelations of Maurice Wilkins, the 'Third Man of the Double Helix', seem 'too late and too little'. In 1968, Wilkins (and Crick) sought to prevent the publication

[25] *The Times*, 19 April 1958; *Nature*, 182 (19 July 1958) 154, quoted in Maddox, *op. cit*., 308–309.

[26] Osman Elkin, *op. cit.* note 18; see also Henry Nicholls, 'So you think you know the double helix story?', *Biomednet* (Electronic journal of Elsevier), 21 April 2003, based on interviews with Darwin H. Stapleton and Pnina G. Abir-Am.

of Watson's *The Double Helix* on the grounds of distortion and invasion of privacy. If, in 1975, he, among many other scientists, found it possible to ignore Franklin's first biographer, Sayre, as a 'feminist author', this option became impossible during 2002 and 2003 when Maddox's biography received wide publicity, including radio and TV interviews and readings. Eventually, the reticent Wilkins had to enter the fray.

Wilkins has come to accept that he can no longer blame Franklin for King's missing out on the double helix, and now insists that the lack of communication in the Biophysics Laboratory derived largely, if not entirely, from Randall's strategy of 'divide and rule'. Following Randall's death in 1984, as his loyal successor, Wilkins was asked to write his official memoir for the Royal Society. In so doing, he discovered letters from Randall asking Franklin to be in charge of the DNA work, and asking Wilkins to leave it. However, Randall never told Wilkins of this invitation to Franklin, nor did he tell Franklin that since Wilkins had been collaborating with Randall on DNA research for four years, he might be expected to retain an interest in this topic, despite its reassignment to another scientist (i.e., to Franklin). Whether Randall's behaviour stemmed from his military background, as Wilkins suggests – or from a more sinister quest for absolute power, as his nickname, 'Napoleon', seems to imply – the consequences for a lab widely known as 'Randall's circus' (the title of Chapter 4) were devastating.

Wilkins owes his career to Randall (his PhD adviser), having followed him from the University of Birmingham to St Andrews University, and then to King's College. Apparently, he came to terms with Randall's darker side. But Wilkins now believes that Randall asked him to stop work on DNA in order that Randall could join Franklin's project instead. Although this is a plausible suggestion, one supported by Randall's letters to Wilkins asking him to quit working on DNA, Randall was too busy expanding his scientific empire to notice whether Wilkins continued to work on DNA, or merely wished to do so. Indeed, being cornered out of DNA by both Randall and Franklin, Wilkins shared his distress with those willing to listen. However, what may have begun as a sporadic request for compassion, soon became a systematic relationship, when the frustrated Wilkins transferred King's results to his friends in Cambridge, Watson and Crick. By his own account, Wilkins's motives for visiting Cambridge often also derived from his fondness for Francis Crick's wife, Odile, although he reassures the reader that he had no ungentlemanly intentions.

It appears that Wilkins still needs someone to blame for transferring unpublished results to a rival laboratory. His actions certainly exposed Randall to ridicule. Maddox quotes interviewees who described Randall,

learning that his lab, the biggest in the UK, had been scooped by the Cavendish – which was not even supposed to be working on DNA – as looking like a 'scalded rat'. Wilkins's action and silence also deprived Franklin of due recognition. Even now, it is not widely understood that most of Wilkins's work on DNA was done only after Franklin left King's in 1953, and consisted mainly in an extension and refinement of her results with better equipment. This was also the main basis for his share of the 1962 Nobel Prize.

Today, Wilkins avoids confronting the meaning, let alone the consequences, of his conduct in the Double Helix story. In particular, he remains unclear about the sources of his communication problems with other scientists, including Kendrew, Randall, and Franklin. His autobiography – which recalls his childhood in New Zealand, and his later upbringing in Birmingham, where his father, a public health physician, pursued progressive causes – suggests nothing in his background to account for a chronic inability to communicate. Perhaps the source of his trouble was neither race, nor class, but rather a lethal dose of conformity to a scientific hierarchy dominated by alpha males. Accustomed to the Napoleonic Randall, Wilkins did not know how to communicate with a woman scientist. Revealingly, he related how he once tried to improve matters by inviting Franklin to dinner, but on arriving at the lab in the evening, found her fixing X-ray cameras – apparently, putting him off his stride.

Ironically, Wilkins still refuses to acknowledge his immoral conduct in trading Franklin's results to Crick and Watson, in the hope of 'collaborating' with them as an equal partner. Similarly, his negotiation of a pattern of publication that obscured their dependence upon her work was conducted without consulting with Franklin, as if her results belonged to him. As a result, the scientific community in 1953 – or indeed, until Watson's revelations in 1968, which triggered clarifications from Wilkins and Perutz in 1969 – had no idea of the actual relationship between Franklin's work and the model devised by Watson and Crick.

Fifty years later, Wilkins is singing the praises of collaboration in science as a supreme value, implicitly blaming Franklin for refusing to collaborate. However, Franklin was not uncooperative. She, much as Randall, rejected him, for they both understood that Wilkins had no assets to contribute. Little did she know that the inarticulate Wilkins would prove capable of giving away her work. Fifty years later, Wilkins may still not understand that it was not Franklin or her biographers, but his co-Laureates who trapped him into an indefensible position. Unlike Crick – who eventually acknowledged that the double helix was within Franklin's reach, and who dissociated himself from the circus around 'DNA at 50' – Wilkins,

together with Watson, continues to make excuses, blame others (Randall, Franklin, biographers, feminists, and historians) and embarrass science by continuing to seek credit for the work of others. One can only wonder at the social order that produces such fossil men who insist that their moral compass, derailed during the terrible inter-war years, must remain broken despite the passing of half a century and the demise of the Cold War, which inspired the zero sum games still haunting the DNA structure saga.

CONCLUSION

Despite the new material contained in these four books, and the complementary perspectives they bring, we still do not know why a major discovery has remained mis-credited for half a century. While the DNA story remains as popular as ever, neither biography let alone autobiography, nor a sociological focus on a single laboratory, has yet succeeded in tying up all the loose ends. It is to be hoped that more comparative work – exploring interacting research groups and intersecting laboratories at other sites, including Columbia University, the Rockefeller Institute, and the Universities of Paris, Berne, Geneva, Brussels, Stockholm, Copenhagen – may shed new light on the nexus between history and memory, and between discovery and justification in the rise of molecular biology.

To some extent, our continuing ignorance may reflect a low contemporary interest in unravelling complex ethical conundra, and in giving credit where it is due. However, these books do take an important step forward, by assigning alternative credits. Chadarevian's volume suggests that DNA has not been a central concern of the LMB, whether before or after 1953; McElheny's volume suggests that Watson should be credited with the building of 'DNA town', and for other attempts to popularize public understanding of DNA, as well as for his joint efforts with Crick in clarifying the function of DNA. Wilkins's autobiography suggests that he should be credited with refining DNA structure after 1953. However, credit for solving the structure of DNA in 1953 must be given to those who contributed the most – not the least – towards that monumental feat. These volumes leave no doubt that the rightful recipients should include Rosalind Franklin, together with her doctoral student and collaborator, Raymond Gosling.

About the Author

Pnina Geraldine Abir-Am has published widely in the history of molecular biology, the history of women and gender in science, 'history and memory', and science policy. Her work has been recognized by an award for outstanding research from the History of Science Society. She has taught at the University of California at Berkeley, the University of Ottawa, the Hebrew University of Jerusalem, and Johns Hopkins University. She is currently completing a monograph, *DNA at 50: History, Memory, and Moral Genealogy*.

Program in Women's Studies
524 Holmes Hall
Northeastern University
360 Huntington Avenue
Boston, MA. 02115, USA
E-mail: p.abir-am@neu.edu