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Bio-R/Evolution in Historiographic Perspective: Some Reflections on the History and Epistemology of Biomolecular Science

ABSTRACT

Does the molecular vision of life signify a unique revolution in biology or a more general evolution of the life sciences in the twentieth century? This paper visits this 'big question' by reflecting on a series of major debates in the historiography of molecular biology, especially those regarding its origins and the periodization of its development. For instance, while some have suggested that the discipline emerged in the 1930s, others have argued for its birth in the post-WWII era. Above all, the impact of the Rockefeller Foundation and the physical sciences on the formation of molecular biology remains a central topic of discussion among historians of biology. Unlike earlier historians of biomolecular science, recent scholars have also started to pay closer attention to the laboratory and material cultures that had conditioned its historical shaping. This paper argues that, ultimately, these debates all rest upon one fundamental historiographical problem: the absence of a unifying understanding of 'molecular biology' among historians (and practitioners) of biological science. This heterogeneous conceptualization of 'molecular biology', however, should be viewed as valuable because it allows for multiple approaches to resolving the 'revolution versus evolution' debate that together enrich our interpretation of the twentieth-century biomolecular vision of life.

BIOGRAPHY

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Bio-R/Evolution in Historiographic Perspective: Some Reflections on the History and Epistemology of Biomolecular Science

Although molecular biology is only one branch of the modern life sciences, the discipline has acquired a remarkable social status over the course of the twentieth century. Historians of science who have written about this specific trajectory within the history of twentieth-century biology have offered different chronological organizations and interpretations of its development. For instance, citing Michel Morange, Hans-Jörg Rheinberger has suggested that the history of molecular biology is characterized by two decisive shifts: the forging of molecular biology between 1940 and 1960, and the introduction of molecular technology during the 1970s.[i] Robert Olby has provided a slightly different set of turning points in the discipline's history: the 1930s when molecular biology was 'broad' in scope, the 1950s when the field got 'narrowed' down to the determination of residue sequence, and the 1980s when the discipline became much more sophisticated and complex with the general aim of finding ways to control these fundamental sequences.[ii] Alternatively, addressing the political context within which scientists operate, Pnina Abir-Am contends that the three definitive stages of biomolecular science compliment the three major wars in the twentieth century: the post-WWI stabilization of biochemistry, the post-WWII stabilization of molecular biology, and the post-Cold War stabilization of biotechnology.[iii]

Based on their diverse interpretations of twentieth-century biomolecular science, historians of biology disagree on one 'big question': Does the molecular vision of life signify a unique revolution in biology or a more general evolution of the life sciences in the twentieth century? This paper will come back to this question after visiting some major nexuses of contention in the historiography of molecular biology, especially those debates concerning the science's origins and the periodization of its development. For instance, while some like Olby have suggested that molecular biology emerged in the 1930s, others like Abir-Am have argued for its birth in the post-WWII era. Above all, the influence of the Rockefeller Foundation and the physical sciences on the formation of molecular biology remains a central focus of discussion among historians of the life sciences. Unlike earlier historians of molecular biology, recent scholars have also started to pay more serious attention to the laboratory and material cultures that had conditioned its historical shaping. My synthesis will show that, ultimately, the central problem that drives these historiographical debates is the lack of a unifying definition of 'molecular biology' in the current historiography of biological science.

One of the principal debates featured in the historiography of molecular biology revolves around the role of the Rockefeller Foundation in the disciplinary emergence of molecular biology. Based on their varying positions in this debate, historians of biomolecular science differ in opinion with respect to its chronological origin. On the one hand, the early historiography has richly documented the prominent role played by the Rockefeller Foundation in the forging of the field of molecular biology, and, accordingly, has argued for its beginning in the 1930s.[iv] In addition to the generous research grants provided by the Foundation to various scientists in the United States and Europe working on biomedical research, the most frequently cited evidence for the birth of molecular biology in the 1930s is the coinage of the term 'molecular biology' in 1938 by Warren Weaver, then the Director of the Natural Sciences Division of the Rockefeller Foundation.[v] Robert Olby's early contribution to the historiography, *The Path to the Double Helix* (1974), for instance, relies on the personal accounts of W. Lawrence Bragg and Max Delbrück, two important scientists who studied the physical properties of genes and proteins respectively before the formal institutionalization of molecular biology.[vi] In Olby's book, both Bragg and Delbrück explicitly acknowledge the importance of Weaver's financial and institutional support in the establishment of biomolecular studies prior to the Second World War (and thus long before the discovery of the DNA structure).

This early historiographical argument for the influential role of the Rockefeller Foundation in 'molecularizing' the life sciences is further substantiated, and to some degree stapled, by two monographs published in the early1990s. Robert Kohler's *Partners in Science* (1991), an expansion of his earlier essay 'The Management of Science', holds the general thesis that 'science [is] a complex social system with many actors, in which securing resources, negotiating with patrons, creating departments and disciplines, competing for talents, designing products and services, and projecting public images [are] no less essential than bench research'.[vii] In particular, Kohler seeks to demonstrate the intimate and intricate connections between researchers and their patrons, especially those of the Rockefeller Foundation, in the making of the natural sciences, their institutions, and their disciplinary relations.

The second book, Lily Kay's *The Molecular Vision of Life* (1993), contributes to the historiography of molecular biology more directly and provides a refreshing perspective on the relationship between the Rockefeller Foundation and the rise of molecular biology in the 1930s.[viii] According to Kay, 'The molecular vision of life was an optimal match between technocratic visions of human engineering and representations of life grounded in technological intervention, a resonance between scientific imagination and social vision'.[ix] Acknowledging Weaver's initial definition of 'molecular biology' as an approach to biological science that borrowed laboratory technologies from the physical and chemical sciences, but unlike earlier scholars, Kay situates the Foundation's molecular biology program in a 'broader intellectual and social agenda within which the program was nested'.[x] Namely, Kay argues that the molecular vision of life was born out of the Foundation's effort to reorient their agenda in overseeing academic pursuits in the human and social sciences. The Foundation's investment in conceiving and designing the molecular style of the modern life sciences, according to Kay, needs to be grounded in the Rockefeller philanthropies' historical affiliations with promoting the sciences of social control in the early twentieth century. Based on Kay's interpretation, the field of molecular biology, informally institutionalized at the California Institute of Technology before the postwar era, grew out of its distinct financial support from the Rockefeller Foundation in the 1930s.

On the other side of the debate, some historians of biology argue that rather than acting as the midwife to the birth of molecular biology, the Rockefeller Foundation actually played a relatively peripheral role. These historians not only refuse to trace the origins of molecular biology back to its historical affiliation with the Rockefeller Foundation, but they also refuse to see the 1930s as the precise time period for the rise of molecular biology. Pnina Abir-Am, for example, published an influential article 'The Discourse of Physical Power and Biological Knowledge in the 1930s' in the journal *Social Studies of Science* in 1982 that initiated a series of replies from several major figures in the field of the history of biology.[xi] In her article, Abir-Am begins by citing Michel Foucault's conception of discourse as a locus where power and knowledge are transformed into one another. Suggesting that under the Rockefeller Foundation, the 'molecularization' of biology was conceived of as a function of 'technology transfer' from physical power to biological knowledge,[xii] Abir-Am argues against the earlier historiographical emphasis on the research policy of the Foundation as having a direct impact upon the making of molecular biology. Abir-Am shows that two of the projects that the Foundation had funded—one directed by William Astbury at the University of Leeds and one directed by Linus Pauling at the California Institute of Technology—failed to generate important contributions to biomolecular science; whereas, one of the projects that the Foundation withheld financial support—a project proposed by Joseph Needham and Conrad Waddington—actually embodied a molecular conception of biology more accurately than any other studies that the Foundation had funded. Quite simply put, for Abir-Am, just because the Rockefeller Foundation aimed for transfer from physical to biological sciences, it could not revolutionize biology.

In 1984, *Social Studies of Science* published four responses to Abir-Am's essay from John Fuerst, Ditta Bartels, Robert Olby, and Edward J. Yoxen, as well as Abir-Am's final reply to them. From the outset, Fuerst criticizes many aspects of Abir-Am's work and 'corrects' much of the historical evidence Abir-Am used, suggesting that, for example, Astbury and Pauling should really be regarded as molecular biologists of their time while Needham and Waddington should not.[xiii] Reflecting on Abir-Am's original article and Fuerst's reply, Bartels welcomes Fuerst's correction of Abir-Am's historical accuracy but also acknowledges the preciousness of Abir-Am's argument. At the same time, Bartels raises some of her own objections to Abir-Am's study, such as the inadequacy of Abir-Am's use of the concept 'technology transfer'.[xiv] In defense of his own book *The Path to the Double Helix*, which Abir-Am criticized, Olby also take issues with Abir-Am's interpretation of Weaver's support for Astbury and Pauling; in his reply, Olby in fact dismisses Abir-Am's entire argument.[xv] Admitting that himself shares a similar view with Fuerst, Yoxen describes Abir-Am's article as the product of a 'careful selection of case studies',[xvi] and emphasizes the multiple connotations that the term 'molecular biology' had and continues to have, which, according to Yoxen, Abir-Am's essay failed to demonstrate.[xvii]

Responding to the many criticisms articulated on different levels—including debates over the place of the reductionist versus anti-reductionist agenda in the historiography of molecular biology—Abir-Am ultimately maintains her initial argument, namely that the Rockefeller Foundation financially supported projects not crucial to the birth of molecular biology, and missed several opportunities for funding research studies significant to the 'molecular revolution' in biology, such as the ones conducted by James Watson and Francis Crick in Britain.[xviii] Even though earlier scientists like Astbury, Pauling, Needham, and Waddington were already studying vital processes or biological materials on the molecular level, for Abir-Am, the determination of the DNA structure specifically and the discovery of the nucleic acid property of the genetic material more generally were more instrumental to the 'molecularization' of biology in the mid-twentieth century. [xix] As such, Abir-Am's analysis implies that the early 1950s, and not the 1930s, marked the real beginning of the field of molecular biology.

Soraya de Chadarevian's *Designs for Life* (2002), a recent addition to the historiography, takes up this historiographical thread propounded by Abir-Am and continues it with an analysis of the development of biomolecular science at Cambridge.[xx] Like Abir-Am, de Chadarevian challenges the earlier historiographical tendency to trace molecular biology back to the 1930s; de Chadarevian in fact pushes the chronology of the establishment of molecular biology to the late 1950s and argues that most of the molecular studies of biological problems using physical, chemical, and mathematical techniques in Britain before the late 1950s are more appropriately categorized under the rubric of 'biophysics'. This 'physics of life', according to de Chadarevian, 'with the promise of biomedical applications, fitted neatly into the political discourse of postwar reconstruction'.[xxi] Emphasizing the post-WWII socio-political context within which experiments in the life sciences, such as those using radioisotopes as biological markers, were conducted, de Chadarevian writes:

> The [Rockefeller] Foundation's 1930s programme, designed by Warren Weaver, aimed at funding chemical and physical approaches in the life sciences, is generally viewed as a decisive factor in the foundation of molecular biology. The institutionalisation of molecular biology in the late 1950s and 1960s, however, cannot be understood as merely subsidiary to intellectual programmes and practices set in place in the interwar years. The focus on Britain not only fills an important gap in the literature, but also moves attention to developments in Europe more generally, despite decisive differences in the way the war affected scientific developments in other European countries.[xxii]

Therefore, according to their different interpretations of 'molecular biology', historians who dismiss the pivotal role played by the Rockefeller Foundation for supporting the study of vital processes (such as gene structure and function) using physical and chemical methods in the 1930s tend to (re)locate the actual consolidation of molecular biology in the postwar era: in the immediate postwar years for Abir-Am, for example, and in the late 1950s for de Chadarevian.

In addition to the significance of the Rockefeller Foundation, another key focus of historiographical debates regarding the foundation of molecular biology centers on the role of physics and physicists. The early historiography claims that molecular biology emerged in the late 1930s and 1940s largely due to the migration of scientists from physics to biology: in part, this historical interpretation relies on the formation of the 'phage group' under the influence of Max Delbrück, a theoretical physicist by training.[xxiii] The 'phage group' is a collective term that refers to all those scientists, roughly between 1940 and 1970, who were interested in using bacteriophages (bacterial viruses) as a model system for studying the microstructures and functions of biological organisms.[xxiv] According to Michel Morange, the main explanation for physicists like Delbrück to become attracted to biology might be that 'it seemed to harbor a large number of unsolved fundamental problems and to be the "new frontier" of scientific knowledge. Quantum physics, and the new chemistry it had produced, appeared to be able to provide the tools and concepts necessarily for understanding the mysteries of biology'.[xxv] Besides Delbrück, other famous examples of physicists who contributed to biomolecular studies in significant ways include George Gamow, one of the fathers of the 'big bang' theory, Leo Szilard, one of the key scientists who participated in the making of the atomic bomb, Linus Pauling, the physical chemist who studied the x-ray crystallography of large biological molecules at Caltech mentioned earlier, and most notably Erwin Schrödinger, the author of the book *What Is Life?* that convinced many young scientists of the time that the mystery of life could eventually be explained in physical and chemical terms.[xxvi]

Recognizing that the existing body of literature has already documented the diverse aspects of the importance of physics and physicists in the development of molecular biology, Evelyn Fox Keller provides an alternative viewpoint in her 1990 article 'Physics and the Emergence of Molecular Biology: A History of Cognitive and Political Synergy'.[xxvii] Earlier scholarship, for instance, focused primarily on the contribution of physics and physicists to the life sciences in terms of technical skills (e.g. methods such as electron microscopy or the use of radioisotopes) and cognitive abilities (e.g. the conceptualization of life that reduces the investigation of the structure and functions of organisms down to the microscopic level). Keller, however, focuses on 'a different kind of contribution, arguing that physics and physicists provided a resource of considerably greater import for the success of molecular biology than any particular skills: namely, social authority and social authorization'.[xxviii] For Keller, biology's borrowing of language, agenda, attitude, expertise, names, and technique from physics ultimately amounts to a complete reframing of the life sciences that generated 'the technological prowess of molecular biology' in the twentieth century.[xxix]

Other historians of biology, however, disagree with the popular interpretation that the importation of ideas and techniques from the physical sciences shifted biology towards the molecular dimension and agency. Like de Chadarevian's book, by redirecting the attention of historians of science to Europe more generally and France in particular, Jean-Paul Gaudillière's *Inventer la biomédicine* (2002), for example, revises the earlier historiography of molecular biology in terms of geographical focus.[xxx] In so doing, whereas de Chadarevian challenges the traditional overemphasis on the role of the Rockefeller Foundation in the development of molecular biology and the usual tracing of the discipline's historical roots to the 1930s, Gaudillière's study demonstrates that the physical sciences were not the sole 'colonizer' of the life sciences but that *medical* research policy was equally, if not more, significant in reorienting biology towards a molecular vision of life.[xxxi] Whereas for Keller, the mid-twentieth century institutionalization of molecular biology both reflected and constructed the transference of the eminent social status and power from modern physics to modern biology, Lily Kay, as discussed earlier, has contextualized the emergence of molecular biology in the 1930s within Rockefeller Foundation's larger intellectual agenda for funding *human* and *social* scientific research.[xxxii]

In fact, Horace F. Judson has already argued against the traditional historiographical overemphasis on the role of physics and physicists in the shaping of molecular biology in an early essay 'Reflections on the Historiography of Molecular Biology'.[xxxiii] In this article, Judson treats the 'molecular revolution' in the life sciences as a distinct, internal biological revolution—especially with consequences for transforming the discipline of biochemistry—and not merely the product of the physical sciences' 'colonization' of biology.[xxxiv] Here, Judson has already suggested that 'the model of DNA—the double helix—that Crick and Watson built…*was not instrumental to the change*' in 'the ruling preconceptions of biochemistry itself'. According to Judson, Watson and Crick's DNA model simply 'gave legitimacy to the new understanding of biological specificity in a physical form of compelling explanatory power'.[xxxv] Reviewing de Chadarevian's *Designs for Life*, Angela N. H. Creager agrees with Judson on a similar point:

> De Chadarevian stresses that the renaming and expansion of the [Medical Research Council] unit was *not* a consequence of Watson and Crick's achievement, but rather the result of a protracted institutional crisis. The institutionalization of molecular biology, however, gave retrospective significance to the double helix as a discovery and an icon, and Cambridge scientists have astutely exploited this local achievement by giving it a new pride of place in their own history.[xxxvi]

Unlike the earlier literature in the history of science, recent scholarship has also started to pay more serious attention to the laboratory and material cultures that conditioned the formation of molecular biology, treating the discipline more correctly as an operational science instead of just a theoretical science. This historiographic revision in the material epistemology of biomolecular science, like the historiographical debates over the direct impact of the Rockefeller Foundation and the physical sciences, brings with it diverging ideas about the chronological origin and definitions of molecular biology. For instance, by focusing on research instrumentation, experimental tools, and laboratory techniques and practices, a number of scholars have offered impressive individual accounts of the historical development of analytical ultracentrifugation, electrophoresis, spectroscopy, electron microscopy, and liquid scintillation counters.[xxxvii] Confirming the earlier historiography of molecular biology, most of these studies, except for Hans-Jörg Rheinberger's on scintillation counters, trace the origins of these instrumentations alongside the rise of the molecular life sciences to Rockefeller Foundation's support in the 1930s and 1940s.

Moving away from this early kind of historiographic interpretation, Rheinberger's *Toward a History of Epistemic Things* (1997), a culmination of his work since 1992 and now a classic reference for epistemological and historical insights on molecular biology, brings into sharper focus the significance of post-WWII biochemical studies of protein biosynthesis in the consolidation of the field of molecular biology.[xxxviii] In his book, Rheinberger describes the research structure of these biochemical studies as representing an 'experimental system', by which he means 'a basic unit of experimental activity combining local, technical, instrumental, institutional, social, and epistemic aspects'.[xxxix] Between the mid-1950s and early 1960s, according to Rheinberger, molecular biologists' clarification of the mechanism of protein synthesis (which unified the relationship between the DNA and the protein) relied on two different experimental systems that had transformed two central objects of molecular biology: soluble RNA into transfer RNA and microsomal template RNA into messenger RNA. 'It was the conjuncture of these two epistemic things', writes Rheinberger, 'their transposition, grafting, dissemination, hybridization, and bifurcation that brought the genetic code into experimental existence'.[xl]

Reflecting the more recent historiographical tendency to situate the rise of molecular biology in a post-1950 historical context, Rheinberger's book specifically argues that 'much of the laborious work that established the molecular details of what later became codified as the process of replication, transcription, and translation of genetic information between 1953 and 1963 was the result of biochemical endeavors *that had not at all been set up from the perspective of molecular genetics*'.[xli] In making such a claim with a greater degree of sensitivity to the experimental dimension of biomolecular knowledge, Rheinberger's account of the 'discovery' of the transfer RNA and messenger RNA envisions molecular biology and its temporal origin in a way that is very different from Kay, Abir-Am, or Keller's understanding. Whereas Kay, Abir-Am, and Keller, especially in their earlier studies, have all treated the field solely on the level of ideas and reconstructed its historical origins through the approach of scientific discourse analysis, Rheinberger's emphasis on experimental systems depicts molecular biology as a dynamic field that is about not only *thinking* but also *doing* science.[xlii]

Therefore, adding onto their recent focus on laboratory cultures, historians of biology have begun to broaden their perspective by looking at the material culture of science more generally. With respect to molecular biology in particular, Edward Yoxen has argued that its formal institutionalization depended largely on the postwar emergence of a new medium of mass communication: the television.[xliii] Building on Yoxen's argument, Soraya de Chadarevian even asserts that physical models of biomolecules allowed molecular biologists to develop what she calls a 'televisual language', a form of communication central to the disciplinary establishment of biomolecular studies and the public conception of it in the 1950s.[xliv] Hence, Yoxen and de Chadarevian's analyses together challenge the traditional positioning of molecular biology's birth in the 1930s by explicitly pointing out the importance of the postwar technological context that fostered the discipline's method of internal and external communication. In fact, as late as 1961, the prestigious scientific journal *Nature* published a debate between biologists Astbury and Waddington over the correct label—'molecular biology' or 'ultrastructural biology'—for the newly emerging scientific discipline, supporting de Chadarevian's refusal to apply the term 'molecular biology' to biophysical research conducted prior to the late 1950s.[xlv]

So, was there a 'molecular revolution' in biology, or is the rise of molecular biology simply a reflection of a broader evolution of the modern biological sciences in the twentieth century? Rather than seeking an ultimate solution to this revolution versus evolution debate, perhaps it would be more valuable to treat the debate as an intellectual point of departure that enables different approaches to the study of the history and epistemology of molecular biology specifically, and of the life sciences more generally. Reflecting on the historiographical debates over the roles of the Rockefeller Foundation, the physical sciences, and the material culture of science in the history of molecular biology, I have shown that these nodes of contention all rest upon one fundamental historiographical problem: the absence of a unifying understanding of 'molecular biology' among historians (and practitioners) of biology.

If one is willing to accept those studies of the physical and chemical properties of genetic material driven by the nucleoprotein gene theory and by the funding from the Rockefeller Foundation in the late 1930s and 1940s as the beginning of a kind of 'molecular biology', then the path from these studies to post-1980s genetic engineering may very well signify a more general evolution of the twentieth-century life sciences.[xlvi] Alternatively, if one is only willing to grant the discovery of the DNA property of the genetic material as *the* pivotal historical moment that gave birth to 'molecular biology', then there may have been a mid-twentieth century 'molecular revolution'— a Foucauldian epistemic rupture or a Kuhnian paradigm shift—that has completely reorganized the meaning of life in terms of the genetic code, and molecular biology proper consolidated in the 1950s and not before.[xlvii] Or, if approaching 'molecular biology' from the perspective of material condition or technological possibility, one would arrive at yet another set of diverging conceptions about its meaning, origins, and epistemology.[xlviii] Above all, viewed from a larger historiographic perspective, as considered in this paper, these heterogeneous and even competing ways of defining 'molecular biology' historically have enriched, rather than hindered, our interpretation of the twentieth-century biomolecular vision of life.

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[xxxvii] See, for example, Boelie Elzen, "Two Ultracentrifuges: A Comparative Study of the Social Construction of Artefacts," *Social Studies of Science* 16 (1986): 621-662; Lily E. Kay, "Laboratory Technology and Biological Knowledge: The Tiselius Electrophoresis Apparatus, 1930-1945," *History and Philosophy of the Life Sciences* 10 (1988): 51-72; Howard H. Chiang, "Separating Molecules, Building Biology: The Evolution of Electrophoretic Instrumentation and the Material Epistemology of Molecular Biology, 1945-1965" (paper presented at the 2007 Biennial Meeting of the International Society for the History, Philosophy, and Social Studies of Biology, Exeter, Devon, United Kingdom, July 25-29, 2007); Doris T. Zallen, "The Rockefeller Foundation and Spectroscopy Research: The Programs at Chicago and Utrecht," *Journal of the History of Biology* 25 (1992): 67-89; Nicolas Rasmussen, *Picture Control: The Electron Microscope and the Transformation of Biology in America, 1940-1960* (Stanford, CA: Stanford University Press, 1997); Hans-Jörg Rheinberger, "Putting Isotopes to Work: Liquid Scintillation Counters, 1950-1970," in *Instrumentation: Between Science, State, and Industry*, ed. Bernward Joerges and Terry Shinn (Dordrecht: Kluwer Academic Publishing, 2001), 143-174.
[xxxviii] See Hans-Jörg Rheinberger, "Experiment, Difference, and Writing: Tracing Protein Synthesis," *History and Philosophy of Science* 23 (1992): 305-331; idem, "Experiment, Difference, and Writing: The Laboratory Production of Transfer RNA," *History and Philosophy of Science* 23 (1992): 389-422; idem, "Experiment and Orientation: Early Systems of In Vitro Protein Synthesis," *Journal of the History of Biology* 26 (1993): 443-471; idem, "Experimental Systems: Historiality, Narration, and Deconstruction," *Science in Context* 7 (1994): 65-81; idem, "From Microsomes to Ribosomes: 'Strategies' of 'Representation' 1935-1955," *Journal of the History of Biology* 28 (1995): 49-89; idem, "Comparing Experimental Systems: Protein Synthesis in Microbes and in Animal Tissue at Cambridge (Ernst F. Gale) and at the Massachusetts General Hospital (Paul C. Zamecnik), 1946-1960," *Journal of the History of Biology* 29 (1996): 387-416; idem, *Toward a History of Epistemic Things: Synthesizing Proteins in Test Tube* (Stanford, CA: Stanford University Press, 1997).
[xxxix] Rheinberger, *Toward a History*, 238.
[xl] Rheinberger, *Toward a History*, 220.
[xli] Rheinberger, *Toward a History*, 5 (emphasis added).
[xlii] The most notable exception is Kay, "Laboratory Technology." On the practical dimension of science, see also Andrew Pickering, *The Mangle of Practice: Time, Agency, and Science* (Chicago: University of Chicago Press, 1995); Karin K. Cetina, *Epistemic Cultures: How the Sciences Make Knowledge* (Cambridge, MA: Harvard University Press, 1999); and Davis Baird, *Thing Knowledge: A Philosophy of Scientific Instruments* (Berkeley, CA: University of California Press, 2004).
[xliii] Edward Yoxen, "Giving Life a New Meaning: The Rise of Molecular Biology Establishment," in *Scientific Establishments and Hierarchies*, ed. Norbert Elias et al., (Dordrecht: E. Reidel, 1982), 123-143.
[xliv] Soraya de Chadarevian, "Models and the Making of Molecular Biology," in *Models: The Third Dimension of Science*, ed. Soraya de Chadarevian and Nick Hopwood (Stanford, CA: Stanford University Press, 2004), 339-368.
[xlv] W. T. Astbury, "Molecular Biology or Ultrasctructural Biology?" *Nature* 190 (17 June 1961): 1124; C. H. Waddington, "Molecular Biology or Ultrasctructural Biology?" *Nature* 190 (17 June 1961): 1124-1125.
[xlvi] See, for example, Olby, "The Origins of Molecular Genetics"; idem, *The Path to the Double Helix*.
[xlvii] See, for example, Keller, "Physics and the Emergence of Molecular Biology"; Kay, *Who Wrote the Book of Life? A History of the Genetic Code* (Stanford, CA: Stanford University Press, 2000); and Evelyn Fox Keller, *The Century of the Gene* (Cambridge, MA: Harvard University Press, 2000).

[xlviii] Besides those mentioned earlier, for recent additions to the historiography, see, for example, Paul Rabinow, *Making PCR: A Story of Biotechnology* (Chicago: University of Chicago Press, 1996); and the essays in Jean-Paul Gaudillière and Hans-Jörg Rheinberger, eds., *From Molecular Genetics to Genomics: The Mapping Cultures of Twentieth-Century Genetics* (London/New York: Routledge, 2004).