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MOLECULAR BIOLOGY AND ITS RECENT HISTORIOGRAPHY: A TRANSNATIONAL QUEST FOR THE 'BIG PICTURE'

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A Machine to Make a Future: Biotech Chronicles. Paul Rabinow and Talia Dan-Cohen (Princeton University Press, Princeton and Oxford, 2005). Pp. x + 199. $24.95. ISBN 0-691-12050-1.

Molecular Biology in Postwar Europe. Ed. by Bruno J. Strasser and Soraya de Chadarevian (Special issue, *Studies in History and Philosophy of Biological and Biomedical Sciences*, vol. 33C, no. 3, September 2002; Pergamon, Meppel, 2002). Pp. iv + 203. (Paperback). ISSN 1369-8486.

Creating a Tradition of Biomedical Research: Contributions to the History of the Rockefeller University. Ed. by Darwin H. Stapleton (Rockefeller University Press, New York, 2004). Pp. viii + 314. $30. ISBN 0-87470-061-2.

Inventer la Biomédicine, La France, L'Amérique et la Production des Savoirs du Vivant (1945–1965). Jean-Paul Gaudillière (Éditions de la Découverte, Paris, 2002). Pp. iv + 392. €33.50 (paperback). ISBN 2-7071-3607-7.

The Life of a Virus: Tobacco Mosaic Virus as an Experimental Model, 1930–1965. Angela N. H. Creager (University of Chicago Press, Chicago, 2002). Pp. xiv + 398. $75 (hardcover), $27.50 (paperback). ISBN 0-226-12025-2 (hardcover), 0-226-12026-0 (paperback).

Félix d'Herelle and the Origins of Molecular Biology. William C. Summers (Yale University Press, New Haven, 1999). Pp. xii + 230. $30 (paperback). ISBN 0-300-07127-2.

Meselson, Stahl and the Replication of DNA: A History of "The Most Beautiful Experiment in Biology". Frederic Lawrence Holmes (Yale University Press, New Haven, 2001). Pp. xiv + 503. $47.00. ISBN 0-300-08540-0.

The Man who Invented the Chromosome: A Life of Cyril Darlington. Oren Solomon Harman (Harvard University Press, Cambridge, MA, 2004). Pp. xiv + 329. $52.50. ISBN 0-674-01339-6.

Making Genes, Making Waves: A Social Activist in Science. Jon Beckwith (Harvard University Press, Cambridge, MA, 2002). Pp. xii + 242. $29.95. ISBN 0-674-00526-0.

The Molecular Gaze: Art in the Genetic Age. Suzanne Anker and Dorothy Nelkin (Cold Spring Harbor Laboratory Press, Cold Spring Harbor, 2004). Pp. xxiv + 216. $45. ISBN 0-87969-697-4.

These ten books, among others published in the last five years,[1] testify to increasing scholarly activity in the history of molecular biology,[2] as well as evincing a multitude of methods aimed at reconciling the elusive desideratum of the 'big picture' for a discipline at the very centre of socially pertinent science, with the persistent difficulty of extrapolating from limited, local, but more manageable case-studies often revolving around a single unit of analysis, such as an experiment, a laboratory, a scientist, or a scientific institution. This essay review identifies several analytical themes that loomed large in the recent historiography of molecular biology, and compares how different books address those themes. It concludes with a brief discussion of missing themes, while also suggesting new lines of research.

1. A DUO ETHNOGRAPHY OF A RECENT BIOTECH COMPANY: MOLECULAR BIOLOGY IN ITS INDUSTRIAL CONTEXT

Ever since the rise of the first biotech companies in the 1970s, amidst public controversies over safety and patenting, the biotech industry has increasingly affected the social, economic, and legal context of research in molecular biology; most molecular biologists have come to participate directly (as managers and employees) or indirectly (as consultants, or stock holders) in biotech's promise for ever newer products and processes. The rapid speed of growth in biotech poses a major challenge for historians, who require archival documentation as well as a certain temporal distance, to the effect that most of the academic authors on biotech have come from political, legal, and social sciences.[3] Paul Rabinow has been among those fascinated by the biotech phenomenon, while choosing to direct his ethnographic gaze at new biotech companies, as well as new laboratories participating in the Human Genome Project (hereafter HGP).[4]

Biotech chronicles is the latest in a series of works reflecting the combined influence of the early Michel Foucault's "archeology of knowledge" and the late Foucault's concept of "biopower".[5] It further translates Rabinow's quest for an "anthropology of reason", which he had formerly addressed in more abstract terms such as the "archeology of late modernity", or "modernity and post-modernity in anthropology", into an ethnography of a new biotech firm in the (San Francisco) Bay area. The firm (founded and observed in 2003) embodies biopower in the present, as a nexus where the "ethos, logos, and pathos" of bioscience and bioscientists, firms and managers, money and investors, intersect in multifarious ways.[6] Those complex ways, the book implies, are so challenging that they may be better observed by a team of two post-modern ethnographers, rather than a traditional lone one.

Retaining a semblance of 'fieldwork', the classical method of anthropology, as a theoretical framework, Rabinow and Talia Dan-Cohen seem to have done their share in salvaging the shrinking domain of classical anthropology, invariably mired in various post-colonial crises, as well as a few persisting scandals,[7] with an infusion of new opportunities in the biotech world. Such professional ingenuity is most remarkable, especially when the allure of biotech is fully recognized; it ranges over cultural valence (shaping evolution); broad social impact on medicine and agribusiness

(via bioengineered hormones, drugs, and resistant crops); economic power (the IPOs — or initial preferred offering [of stock] — of some biotech companies reached the figure of $500 millions); and endless bioethical dilemmas (from patenting of life forms to use of stem cells from embryos).[8]

Biotech chronicles consists of edited interviews with eight workers at Celera Diagnostics. This is a new firm aiming for a leading position in the fast emerging market of customized, molecular diagnostics, by combining newly available technologies such as high throughput, robotics, and bioinformatics, so as to search databases of sequenced genes, at high speed. Unravelled by the Human Genome Project (hereafter HGP), these sequences contain information on genetic predisposition to various medical conditions. Celera Diagnostics is a spinoff of Celera Genomics, a private venture led by Craig Venter, which challenged the speed and vision of the (US) government-backed HGP. As is well known to all those who watched the ceremonies at the White House featuring US President Bill Clinton, the UK Prime Minister Tony Blair, Craig Venter, and Francis Collins (the Head of the US-NIH's HGP project) in June 2000, the public and the private efforts eventually shared the glory of completing HGP's first phase of sequencing, though not before the competition between them reached a high level of acrimonious exchanges.[19]

By 2002, Venter, who had been much in the media eye in the late 1990s when his challenge to the governmental project attracted both wide attention and sharp criticism, allegedly lost interest in Celera Diagnostics (which remains a solid but relatively modest enterprise when compared to Celera Genomics in its heyday). In a surprising move for an industry with very few women executives, Venter was replaced as CEO by the publicly unknown Kathy Ordonez. This original move was made by Tom White, an experienced biotech manager who as Cetus Vice President for Research was responsible for the conversion of PCR (polymerase chain reaction) into one of biotech's biggest success stories. (The rights for PCR were sold to a big pharmaceutical company in the early 1990s for $300 million.)

The authors' interviews with Ordonez, her managerial partners, administrative assistants and technical personnel provide a rare window into the post-genomics phase in the biotech industry, and the great opportunities it seemingly gave to women, as executives, administrators and of course bench scientists. The authors notice these opportunities but do not situate them in a wider context of gender equity, whether in the biotech industry or its affiliated worlds of academic molecular biology, venture capital firms, or governmental regulatory agencies. Ironically, this book appeared just as the public debate on the under-representation of women in science became the "debate that won't go away" in the spring of 2005.[10] *Biotech chronicles*, much as *Making PCR* beforehand, owes its existence to Tom White, Rabinow's long term "biotech native", who authorized the interviews with company personnel including CEO Ordonez, two male partner VPs (himself included), a male managing technician, two female administrators, and a male project director. In a move reflecting a new trend in anthropology toward sharing credit with "informants", all eight interviewees were named and identified by photos and a brief résumé (chaps. 2–6).

Topics covered include the crucial aspect of maintaining an innovative edge in a rapidly shifting and highly competitive industry in which all worker categories change jobs frequently; the delicate rhetoric of persuading investors; the risky efforts of recruiting and retaining suitable scientific, technical, and administrative personnel; the legal efforts in patenting or licensing intellectual property; and the business efforts at constantly repositioning the company's market position via licensing agreements, cooperative alliances with universities, major pharmaceutical companies, other biotech companies, or even changes in the business plan, all this in a highly regulated, competitive, and global market.

Since the authors' [edited] interviews appear to be lightly guided monologues by the interviewees *qua* "biotech natives", *Biotech chronicles* provides numerous insights into the technological platform and human resource issues shaping second-generation biotech companies, i.e. those built upon the genome revolution, yet had to scale back from their initial "genetic fundamentalism" in the post-genomics era. The book discusses "epigenetic" biotech companies that go beyond gene variations; functional versus structural genomics as it affects health; business models; company structures; strategies for outsmarting big PHARMA and other, smaller competitors, while reinventing oneself all the time in response to a rapidly changing business ecology.

The approach of letting informants deliver long monologues is instrumental in creating "an anthropological archive of a biotechnological event" which other scholars may use for more analytic or critical purposes in the future. However, this approach also has its limits. For example, in their zeal to avoid what they call the "dissonance" between science journalism which presumably depicts a world of "passion and achievement", and science studies, which presumably depicts a world of "power, profit, and illusion", the authors miss the strengths of both approaches. Their effort to record "the native's point of view" in a timely fashion, though most desirable whether in classical or postmodern anthropology, still may not be sufficient for understanding the culture of biotech. After all, leaders of biotech companies, of the sort interviewed by Rabinow and Dan-Cohen, command considerable "power, profit, and illusion", yet they choose to play down those delicate aspects so as to project a positive public image. Relying almost entirely on the natives' viewpoint thus produces an outcome resembling a PR operation, or an outcome not conducive to acquiring a critical understanding of the biotech phenomenon, indeed, of any macro understanding, whether critical or not. At the same time, while the book's lengthy interviews with eight company personnel and a few other, more distant, colleagues are less upbeat than the rhetoric of passion and achievement common in science journalism, they also lack the penetrating edge of investigative journalism.

The authors seek to avoid the traditional anthropological monograph, which aims at what has been increasingly seen in recent times as the unrealistic, illusionary, and positivistic goals of providing a full description of "a culture, a place, an epoch, or even an event" (p. 6). By their own account, their approach does not lead to better but rather to different results, mostly to timely descriptions from a "point of view".

If so, it might be helpful to include a wider diversity of such "points of view", so as to gain a more critical perspective on a biotech industry that increasingly dominates the health, food, agribusiness, and biodefence, among other sectors.

The authors further claim that one of their book's novel aspects resides in its deployment of a different "author-function", namely its inclusion of a second author functioning as "another observer to observe the observers observing". Having two ethnographers as co-fieldworkers and co-authors is a promising idea, though it is neither new nor always compelling. Classical anthropologists such as Margaret Mead and Gregory Bateson used it in the late 1930s, while Clifford and Hildred Geertz, or Jean and John Comaroff, among others, used it in the 1960s and 1990s.[11] The justification provided by Rabinow and Dan-Cohen, namely to guard from overfamiliarity between a veteran ethnographer (Rabinow) and the biotech interviewees, is not particularly persuasive. A more concrete methodological advantage that I encountered while operating in a parallel situation as part of an oral historian team interviewing molecular biologists in Europe and the US in the late 1980s,[12] is that a team of two observers of different genders benefits from the sheer removal of both gender and personality as potential sources of incompatibility in the all too crucial encounters between anthropologists and "natives", oral historians and practising scientists, indeed interviewers and intervieews in diverse contexts involved in a co-production of new knowledge. This methodological aspect requires more attention than it is feasible to give it here, as it raises complex issues on the interactive aspects of co-production of new knowledge, the reflexivity or lack of it on the part of both interviewer and interviewee, and the shifting ideals of observing, and recasting the perspective of the "other" in the interviewer's theoretical terms.[13]

Rabinow and Dan-Cohen could have strengthened their claims to a new author-function, had they explained how their respective contributions meshed in the fieldwork, as well as in the process of writing their ethnography, especially since co-authorial collaborations between a veteran scholar and an undergraduate student remain rare. Since completing a relatively short book based on eight interviews is well within the range of a single ethnographer, let alone one of Rabinow's seniority, one can only wonder whether the collaboration was simply a device for gaining time, so as to establish priority in a rapidly growing world of biotech observers.[14] Another plausible justification includes the need for power balancing, something hinted at by Rabinow in a previous book where he commented on the power disparity between the lone ethnographer and an interviewee who was both a big lab manager and co-owner of biotech companies.[15] Thus, bringing one's collaborator, however junior, empowers the ethnographer in an otherwise potentially asymmetric encounter.

Alternatively, the quest for a new author function may stem from former missed opportunities. Rabinow had planned to write *Making PCR* together with his key informant, the biotech executive Tom White; they even co-presented at a conference at MIT in 1992. But White was too busy to participate in the writing, so Rabinow ended up authoring it alone. Perhaps the mixed results persuaded him to try to collaborate again. One way or another, such a new author-function is likely to become

more common in the ethnography of science in the postmodern era; if so, situated fragments of viewpoints, extracted by duos through joint fieldwork in a high tech industry situated in close geographical proximity to the ethnographer's home institution, may increasingly replace the former romantic diary of a lone ethnographer, willing to inhabit challenging and distant habitats so as to capture the "native's point of view", or the key to the elusive totality of an utterly foreign culture and society. This shift is likely to hold, especially since the classical process of producing an ethnography has been criticized as involving not only romanticized but fictionalized accounts, accounts that are increasingly contested in the post-colonial era by former natives, unhappy with their (ethnographer filtered) public image and the overall lack of help to improve their conditions.

Biotech chronicles suggests how the world of high tech industry can be effectively studied by interdisciplinary, international, cross-gender, among other diversified teams and author-functions, much as *Laboratory life* has suggested a generation earlier how social studies of scientific laboratories could proceed via participant observation by social scientists. Then and now, the questions persist as to how academic and industrial high tech labs — key sites for observing the culture of the future — should be best approached by interdisciplinary methods, or new combinations of "blurred genres" at the interface of ethnography and history of science in the post-modern era.[16]

2. THE GEOPOLITICS OF MOLECULAR BIOLOGY: PAN-EUROPEAN POLICIES AFTER THE SECOND WORLD WAR

Molecular biology in postwar Europe engages in an elusive quest for the 'big picture' in molecular biology, an outcome its editors awkwardly call "glocal", meaning the challenge of finding a proper hybridity between "local" case studies of modest scope and an integrated "global" picture of the entire discipline. This volume includes nine essays, five on case studies from the UK, France, Italy, Spain, and Germany; and the rest on various intra-European or transatlantic interactions. Since the UK and France (together with the US) have received the bulk of previous scholarly attention,[17] the co-editors are to be commended for including lesser-known case studies from Italy, Spain, Germany, and Switzerland, though they missed the opportunity to compare formerly Fascist countries (Italy, Germany, Spain) where the role of statist systems of higher education in resisting interdisciplinary innovation of the sort associated with molecular biology has been emphasized by individual essays (see below). Of particular interest are the case studies that link a national with an all-European development.

For example, the case study of the International Laboratory of Genetics and Biophysics (ILGB) in Naples, told in archival detail and perceptive interpretation by Mauro Capocci and Gilberto Corbellini, illustrates the institutional obstacles to the rise of molecular biology posed by the Italian system of higher education, and its autocratic power brokers (*baroni*). They focus on the short-lived success of Adriano Buzzati-Traverso, a cosmopolitan science statesman and the founder of ILGB, who managed to convert the Naples Laboratory into an international centre of molecular

biology during the 1960s, while including bi-lateral agreements with US institutions, such as the National Science Foundation and UC-Berkeley. However, the political instability in Italy, especially its inability to handle the "events of May 1968" which led to massive protests of students and the "occupation" of the Laboratory for months, put an end to these efforts, to the effect that ILGB lost its international position. (For an eye-witness account of ILGB during the 1968 'occupation' see Jon Beckwith's *Making genes, making waves*, reviewed below.)

Another fascinating case-study is Ute Deichmann's "Emigration, isolation, and the slow start of molecular biology in Germany" in which she examines the impact of the forced emigration of Jewish scientists from Germany after 1933. She argues that the expulsion of would-be distinguished molecular biologists deprived Germany of leaders for the new field, as well as contributing to a strong decline of "dynamic biochemistry", an area in which Germany had been traditionally strong and which also proved pivotal for the rise of molecular biology. Deichmann also dwells on the rigid structure of the German university system which blocked interdisciplinary research upon which molecular biology so strongly depended; and the impact of the international and self-isolation of German scientists after the Second World War. She concludes that these factors delayed the institutionalization of molecular biology for two decades after the end of the war. This situation was remedied by mid-1970s due to events in European science policy (examined elsewhere in the volume by Krige and Straser, see below) which led to the establishment of a European Molecular Biology Laboratory (EMBL) in Heidelberg, thus putting Germany at the centre of European collective efforts in molecular biology.

The policy steps leading to EMBL are told in archival detail and interpretative zeal by John Krige who analyses the delays, over a decade (1962–74), in the opening of a European supra-national laboratory of molecular biology from the vantage point of his vast experience with writing the history of the European Centre for Nuclear Research (CERN), which served as an inspiration and an initial model institution for EMBL. Krige, who elaborates on British science policy including differences in outlook between John Kendrew (the first EMBL Director) and C. H. Waddington, argues that EMBL was the product of prioritizing national interests in science policy. Maria Santesmases further clarifies the pivotal links between national and international policies, highlighted by Krige, with special reference to Spain. She argues that membership in EMBL helped catalyze the otherwise tardy institutionalization of molecular biology in that country.

Strasser examines four national science policies toward molecular biology: the Swiss, the French, the German, and the British, as well as their impact on the creation of new institutes. Having examined the relationships between basic and applied research, the drive toward interdisciplinary research, and "Americanization", he argues that in each national case the respective new institute was situated within a national science policy that focused on post-war economic reconstruction and social modernity. These are major themes that need to be further developed for they can help link not only the local to the global but also the history of science with the general

history of the twentieth century. Though Strasser's conclusion that "beyond the local and national accounts there is a European history of molecular biology" is a bit of a stretch, since a European identity remains grounded in policies and institutions rather than in research strategies, his comparative effort is suggestive of the steps needed to reach a 'big picture'.[18]

However, the volume misses an opportunity for considering the role of women molecular biologists in various European countries, a strange omission since the volume is rather generous in including repetitive material, or items published elsewhere in closely related forms. Ironically, women were active in both science and global politics; for example, the biochemist Marianne Manago, co-discoverer of RNA polymerase, became the first woman President of the French Academy of Science (1994). Nobel Laureate (1987) Rita Levi-Montalcini, co-discoverer of the nerve growth factor (NGF), is very active in science policy in Italy and remains the most honoured European scientist for her activism in numerous civic causes. Nobel Laureate (1995) Christiane Nusslein-Volhard of Germany is a world-renowned speaker on developmental genetics and society. Ruth Arnon of Israel (a country founder of EMBO), an inventor of drugs for autoimmune and genetic diseases, as well as smart vaccines, is President of the Asian Academy of Science. Rosalind Franklin (1920–58), whose career provides an excellent opportunity for comparing science and science policy in the UK and France after the Second World War, is similarly missing. So is Dorothy Hodgkin (1910–94), a British Nobel Laureate who solved the structure of vitamin B-12 among other bio-organic compounds and became involved in national and international science policy as President of Pugwash and adviser to former Prime Minister Margaret Thatcher. Indeed, Hodgkin's "glorification" by European scientists as "happily married" reveals that the European quest for modernity has yet to grapple with the issue of gender equity in and out of science.[19]

3. BIOMEDICAL RESEARCH TRADITIONS AS A CONTEXT FOR MOLECULAR BIOLOGY: THE ROCKEFELLER AND PASTEUR INSTITUTES

Creating a tradition of biomedical research: Contributions to the history of the Rockefeller University, edited by Darwin Stapleton, and Jean-Paul Gaudillière's *Inventer la biomédicine*, situate several case studies that are pertinent to the history of molecular biology (and related disciplines) in the context of biomedical traditions grounded largely though not entirely in the Rockefeller and the Pasteur Institutes, respectively. Stapleton's collection covers a whole century, as befits a publication planned as part of the centennial of the Rockefeller University (formerly the Rockefeller Institute for Medical Research, hereafter RIMR), while Gaudillière's volume covers the two decades after the Second World War. Both include discussions of transatlantic exchanges, as well as case-studies from a variety of biological and medical disciplines such as cell biology, immunology, virology, microbiology, clinical research, among others. Stapleton's Introduction highlights RIMR's connections with other closely related institutions, most notably the Rockefeller Foundation, whose role in the rise of molecular biology has received considerable attention from historians in the last

two decades.[20] Other Rockefeller philanthropies and neighbouring institutions, such as the renowned Sloan-Kettering Institute for Cancer Research, are also discussed as pertinent institutional context for RIMR.

The collection includes essays on RIMR's directors Simon Flexner, Detlev Bronk, and Herbert Gasser, the flamboyant scientists Alexis Carrell and Hideyo Noguchi, and those pillars of cancer research Peyton Rous, James Ewing, and James B. Murphy. An essay on "Women scientists at the Rockefeller Institute, 1901–1940", by Elizabeth Hanson, fills an important lacuna in our knowledge of women's place at RIMR, a leading centre for biomedical research for over a century. She examines the careers of a variety of women hired by Flexner, RIMR's legendary founding Director, and shows that they were invariably relegated to the lower ranks. Only one, Florence Sabin, became a Member of the Rockefeller Institute; as one member observed, "Simon Flexner just wasn't giving membership to ladies". It is remarkable that by the time Hanson wrote her essay around 2001, the vice president of Rockefeller University (who incidentally was in charge of RIMR's centennial preparation) was a woman (the molecular biologist Alice Lustig).

Notable for its blend of innovative research in cell biology and science policy is the essay "Paul A. Weiss, 1898–1989: The cell engineer", by Sabine Brauckmann. She highlights the career of an innovative scientist who as science administrator with the then (1951) nascent National Science Foundation (NSF) had refused to adjust James Watson's postdoctoral fellowship so as to allow him to leave Copenhagen for Cambridge (UK) in order to study DNA structure there. Watson went anyway and the rest is history. Instead of having NSF associated with one of the most important discoveries in molecular biology, Watson acknowledged instead the National Foundation for Infantile Paralysis which gave him a research grant replacing the one that Paul Weiss had refused to adjust.

RIMR's contribution to molecular biology is addressed by Robert Olby's essay "The Rockefeller University and the molecular revolution in biology". In addition to discussing Oswald Avery and Alfred Mirsky, whose antagonistic views on DNA adversely affected the reception of DNA as the sole genetic substance in the mid- and late 1940s, Olby focuses on Central European émigrés Karl Landsteiner, who inspired Linus Pauling's immuno- and stereo-chemical research (which in turn inspired research on protein and nucleic acid structures), and Max Bergmann, a leading organic chemist whose alleged mistakes in the quest for protein structure Olby labours hard to justify. Perhaps as a result of his apologetic preoccupation with Bergmann, Olby misses altogether Rollin Hotchkiss's seminal work on the genetics of transformation; Fritz Lipmann's work on protein synthesis and the genetic code; and Norton Zinder's career of over half-a-century at Rockefeller University, which included the discovery of transduction in the 1950s, and a major role in the Human Genome Project in the 1990s, yet remains vastly underestimated.[21]

It is not much to expect that a volume originating in a (2001) conference marking the centennial of the Rockefeller Institute should address the topic of biomedical commemorations, whether of the Institute itself, or in comparison with institutes it

emulated such as the Pasteur Institute in Paris, the Koch Institute in Berlin, and the Lister Institute in London — even more so since commemorative studies of biomedical discoveries, discoverers, and institutions have abounded in the 1990s, in connection with the bicentennial of the French Academy of Medicine, and the centennial of Pasteur's death (*l'année Pasteur* or a year-long celebration of Pasteur's legacy in biomedical research). Analytical essays by historians of science and medicine have also become available in the late 1990s, in both English and French, while demonstrating for the first time the crucial role of "history and (cultural) memory" in understanding anniversaries as analytical bridges between the past and the present. For example, excellent essays on the centennials of Pasteur and the Pasteur Institute, by Christiane Sinding and Anne-Marie Moulin; or similar essays on the commemoration of notable biomedical scientists and their institutions, including Claude Bernard, Jean-Marie Charcot, Robert Koch, and André Lwoff.[22]

Along these lines, a volume whose publication coincided with the sixtieth anniversary of Avery, McLeod and McCarthy's discovery at RIMR of the DNA nature of genetic changes in bacteria, and whose preparation took place in parallel with the fiftieth anniversary of DNA structure in 2003, could have featured an essay on the history and memory of DNA whether at RIMR or in Greater New York.[23] A unique opportunity was sadly missed to resolve open historiographical questions pertaining to the role of RIMR's infra-structure in biomedical research (laboratory space, top of the line instruments, experienced investigators free of the need to seek funding, collaborative proximity to leading scientific communities at Columbia and NYU) in the seminal discovery of DNA's genetic properties, the ensuing resistance to it, and the efforts to preserve its memory, half-a-century later.

In the same vein, chapters on officers of the Rockefeller Foundation, especially those in charge of medical sciences (Allan Gregg) and natural sciences (Warren Weaver, whose vast oral history at Columbia University contains underutilized material), could have better clarified RF's relationship to RIMR than was possible in Stapleton's Introduction. Since that relationship anticipates and parallels that between intramural and extramural research in the main governmental agency for funding bioscience, NIH, such (missing) topics retain both historiographical and pragmatic interest for understanding the shaping of bioscience in the twentieth century. Lastly, since RIMR has previously inspired not only scientific books and papers, but also popular best sellers such as Sinclair Lewis's *Arrowsmith* and Paul de Kruif's *Microbe hunters*, a comparison of American fiction surrounding RIMR with French fiction surrounding the Pasteur Institute could have generated great cultural, historical, or literary, value.

Unlike Stapleton's volume, which is relatively free from historiographic agendas other than showcasing RIMR's diverse legacy, Gaudillière's *Inventer la biomédicine*, inspired in its title by Victoria Harden's *Inventing NIH*,[24] appears intent on overturning previous work on French biomedicine, especially its emphasis on American influence, usually via funding, scientific exchanges, and instrument and book purchases sponsored by the Rockefeller Foundation, as well as NIH, among other agencies

and foundations.[25] Gaudillière's discussion of French biomedical research includes chapters on antibiotics, poliovirus, cancer research, medical statistics, clinical genetics, and molecular biology, a rich and diverse combination of research areas whose mutual relevance remains unclear beyond its constitutive role as building blocks of what he eventually labels as the biomedical "complex".

Gaudillière labours to show that in the case of molecular biology, the influence was not unilateral (i.e., the much detested "Americanization") but involved mutual exchanges of personnel, techniques, and ideas between French and American scientists. This argument, which is further qualified as time-dependent (namely, the French were influenced by the Americans in the 1950s but reversed this situation in the 1960s) is plausible, though premature, especially since Gaudillière does not discuss comparative research on French–American relations in molecular biology by other historians. Only a systematic study of those exchanges, while covering the French school of molecular biology in its entirety, can settle the issue of mutual influence.[26] For the time being, it is fair to say that the benefit of US scientists from their stay in France can hardly match the French scientists' success in winning a Nobel Prize in molecular biology prior to American scientists or the first Nobel Prize in 30 years in 1965 (shared by André Lwoff, 1902–94, and his younger associates Jacques Monod and François Jacob, for the discovery of cellular mechanisms of genetic regulation). As the laureates themselves widely acknowledged, their prize would have been inconceivable without their ongoing rapport with American science.

Aside from his concern with refuting the "Americanization" thesis associated primarily with J.-F. Picard, Gaudillière further suggests that the biomedical research complex in both France and the US came to resemble the so-called "military-industrial complex" which epitomized 'big science' in the US. (It became so powerful that President Eisenhower admitted being scared of it at the time!) This suggestion may have a growing appeal nowadays when the Human Genome Project, the rise of biotech, and other, welfare-related factors have greatly expanded the biomedical research system, to the effect that its mega-budget started to rival that of the military-industrial one in its own heydays. Still, Gaudillière could have substantiated this suggestion well beyond its metaphoric value, had he explicitly engaged with ideas presented by historians of biology and medicine at a workshop in Paris on "Big Science in the Biomedical World",[27] instead of limiting himself to literature on 'big science' in the physical sciences.[28]

The centre-piece of Gaudillière's comparison of French and American biomedical complexes is cancer research. He resourcefully builds upon previous studies of cancer campaigns in France by his colleagues at INSERM, Patrice Pinel and Ilana Lowy, among others, while comparing it with his own work on the US National Cancer Institute's campaign on viruses as a cause of cancer in the 1970s, to the effect that the contrast between the French and the American biomedical policies becomes clearer. As others have noted,[29] Gaudillière does not include drug development in his volume, even though this is an excellent topic for better understanding a key constituent of any biomedical system. The recent resignation of the commissioner of the FDA (Food

and Drugs Administration) in the US amidst several deaths from bioengineered drugs approved on a fast track, made this point crystal clear.

The last three chapters (7–9) cover work previously published by Gaudillière, as well as others, on the molecular biologist Jacques Monod (1910–76) of the Pasteur Institute; the science policy initiative for molecular biology DGRST during the 1960s, which Monod headed in the late 1960s; and the work on m-RNA, pursued by Monod and his associates especially François Gros, François Jacob, and Agnes Ullmann. Gaudillière links this material to the biomedical context via a well-taken emphasis on the 'demedicalization' policy that molecular biologists pursued under Monod's leadership, so as to dissociate their new discipline from the conservative medical institutions, while also retaining its aura of a promising, new, basic science.

By highlighting the biomedical context of molecular biology in France, as well as the role of the State, Gaudillière's volume sheds new light on previous work by Michel Morange which limited itself to describing molecular biology as a basic science.[30] Similarly, by demonstrating the role of state-controlled biomedical policy, Gaudillière's volume complements Picard's work (often in association with William Schneider) among others, on the funding of medical research in France by American philanthropic foundations, to the effect that both private and public funding are now understood to have shaped biomedical research in France.

Gaudillière's claim of a rupture in French biomedical research induced by the Second World War is less persuasive. Much as most of the authors in *Molecular biology in post-WW2 Europe*, he did little research on the inter-war era, or the war years, whether in France or elsewhere in Europe. Indeed, anyone familiar with primary sources from the inter-war period, will recognize important continuities, as well as some obvious discontinuities.[31] But this issue can be addressed only in a work that covers both the pre- and post-war eras, as well as not only France, but also the UK and the US, the key countries active in molecular biology prior to EMBO.[32]

4. THE ROLE OF MODEL ORGANISMS AND "BEAUTIFUL" EXPERIMENTS IN THE RISE OF MOLECULAR BIOLOGY

Creager's *The life of a virus: TMV as a model organism*, which happens to have the very same length as Gaudillière's volume, perhaps as a symbolic marker of their previous collaboration, focuses on the entrepreneurial success that Wendell Stanley created around TMV research, first at the Rockefeller Institute's branch in New Jersey, and after its closure in 1947 at UC-Berkeley. Stanley's crystallization of TMV in 1935 became a big sensation, due to its perceived pertinence as a challenge to the living/non-living divide. It further earned him a third-share of the 1946 Nobel Prize, even though his claim that TMV crystals consisted of proteins turned out to be incorrect.

By her own account, Creager set to examine TMV's trajectory as an ever-resourceful research topic for various laboratories and scientists, while following Rheinberger's previous example of writing the story of transfer-RNA as an emerging "epistemic" object, as well as Kohler's emphasis upon the moral economy of exchange

in laboratory materials, such as the fruit fly Drosophila, the favourite organism of classical genetics.[33] Though she spends considerable effort on re/covering familiar territory previously studied by others,[34] on the role of the ultracentrifuge, the electron microscope, isotopic tracers, viruses, even TMV itself (chaps 1–4), eventually Creager ventures into more promising territory when she seeks to compare TMV with phage, as two model organisms in molecular biology (chap. 6).

Building upon studies of model organisms by philosophers of biology,[35] Creager seeks to understand why phage, but not TMV, became the favourite model organism in nascent molecular biology. However, in the absence of comparable experience with research in the history of phage, Creager's foray into comparative research remains modest. Along these lines, her decision to leave out the exciting story of X-ray crystallographic work on TMV is surprising, especially in view of her interest in different types of instrumentation. Moreover, the X-ray crystallography of TMV not only led to the solution of TMV structure but the relocation of the research group (led initially by Rosalind Franklin at Birkbeck College in London and after her untimely death by Sir Aaron Klug who won the 1982 Nobel Prize for this, among other, work) to MRC-LMB in Cambridge in the late 1950s, put the TMV structure group at the very centre of the nascent molecular biology. TMV was also the rationale for collaboration among Franklin, Klug, Watson, Crick, and other leading figures in molecular biology. Hence, if one wants to know whether TMV compares favourably with phage as model organisms in molecular biology, then it is a good idea, perhaps even a necessary one, to examine the research on X-ray crystallography of TMV, even if it took place mainly outside the US.

Similarly, two of the most important episodes in TMV research were not limited to the US but can be described as transatlantic. The first, dated to the 1930s, pertained to a controversy that raged between Stanley's group and the British team of Bawden and Pirie of ARC-Rothampstead Experimental Station. The second controversy, which unfolded in the 1950s, involved TMV reconstitution and proof that infectivity resided in RNA; it raged between Stanley's group and the German team of Schram and Gierer at the KWI (MPI after 1948) in Tübingen, a centre of TMV research before and after the Second World War. Hence, a systematic comparison of Stanley's American team with its British and German counterparts remains crucial for understanding the life of this virus as an object of historical research. Once again, it is surprising that these controversies have not received a more central place, especially in view of the great scholarly interest in the dynamics of controversies from both historians of science and social scientists, and Creager's own interest in comparative research, as well as the 'big picture'. What better avenue to reap methodological advantage from comparative research while also getting closer to the 'big picture' than comparing two controversies that involved teams from key countries, such as the US, the UK, and Germany?

Another work that also chose to clarify the role of a scientific object in the rise of molecular biology is Holmes's massive study of the Meselson-Stahl experiment. This series of interdependent experiments deserves the caring attention that Holmes is uniquely capable of bestowing for several reasons: first, it consists of an ingenious

and integrative application of biological, physical, and chemical techniques (e.g. the ultracentrifuge, isotopic tracers, phage and bacterial multiplication), thus exemplifying the inherent transdisciplinarity of molecular biology. Second, the experiment stabilized the status of the double helix hypothesis, thus becoming a critical step in the process of the institutionalization of molecular biology. Third, the experiment was made possible by a collaboration between Matt Meselson and Frank Stahl, thus enabling the historian to explore the role of collaborative innovation in science. Fourth, the experiment was conducted against the backdrop and advice of key members of two schools of molecular biology: the structural chemistry school to which Meselson belonged as the last Ph.D. student of the legendary Linus Pauling; and the so-called informational school or the Phage Group, to which Stahl belonged. Hence, the experiment can be seen as a product of interaction between two research schools and their respective conceptual and experimental legacies or as a instance of 'collective creativity'.

Holmes starts with a thorough summary of the conceptual status of the replication problem, which he retrospectively recasts as "the most central issue in genetics" even though most geneticists at the time entertained other priorities (chap. 1). He further provides a detailed discussion of the crucial issue of instrumentation including the role of Jerome Vinograd who, as the 'owner' of the most advanced cesium gradient ultracentrifuge at the time, long persisted in considering himself part of the experimental team (chaps. 4–8). The institutional context at Caltech is also mentioned, although, not being fond of institutional history, Holmes does not make much of Caltech's acclaimed subculture, beyond struggling to understand the evident preoccupation of his protagonists with finding women (Caltech did not accept women students or faculty in the 1950s).

Perhaps the best part is Holmes's careful reconstruction of the experiment *qua* discovery from the viewpoint of the two scientist collaborators (Meselson expressed the view that the "most beautiful experiment" was the product of a collaboration among equals, while Stahl continued to insist that it was mainly Meselson's design and vision), while further attempting to compare his own version to theirs (chap. 9: "One discovery, three stories"). This chapter is methodologically instructive as Holmes tries not only to account for the diverging memories of his two experimentalists, but also to cope with methodological issues stemming from discrepancies between the archival record and the memories of living actors that may superimpose diverse events in time and space.

Though Holmes seemed to appreciate the work of historians of science on "history and memory", work that explains the rationale for such superpositions of layers of memories as well as the discrepancies between them and the "actual past", he chose not to elaborate on this perspective. Instead, he conceived this case study as the last component in a series of "investigative pathways"[36] or non-linear trajectories of scientific creativity that Holmes presented as engines of the historical development of science since the eighteenth century. This experiment thus fits into a line of exemplary "investigative pathways", ranging from Lavoisier's and Bernard's research

in the eighteenth and nineteenth centuries respectively, Krebs's in the 1930s, and Meselson and Stahl's in the 1950s ("Afterword", p. 437). Holmes's encounter with philosophy of science, which includes a brief disquisition on simplicity versus complexity in science as markers of aesthetic beauty, suggests that Holmes was seeking his own model of scientific change, to be sure non-positivist, as part of the historian of biology's apparently growing impulse toward generalization from historically unique case studies.

By his own account, Holmes's book not only includes a meticulous reconstruction of the Meselson-Stahl experiment from archival, oral, visual, and secondary sources, but also topics that remain tangential to his main theme as well as topics that require greater sociological sensitivity than he was inclined to acquire — most notably the reception of the experiment by different disciplinary and interdisciplinary constituencies (biochemists, geneticists, microbiologists, physical chemists, and so on) or its impact on the formation of the new discipline of molecular biology. Yet Holmes's effort to highlight the rational reconstruction of the experiment that ensued so that it could fit the self-image of the new discipline and its proponents (chaps. 10 and 13) inadvertently highlights the importance of new studies on history and memory, or studies that examine how changing cultural constructions of memory seek to fit newly available historical evidence. Ultimately, his comparison of three versions of the same experiment (the two discoverers' and the historian's) is more telling than Rabinow and Dan-Cohen's generation of smooth 'native' monologues that are expected to speak for themselves and never reflect conflicting interpretations.

5. THE BIOLOGIST AS A CONTROVERSIAL FIGURE IN SCIENCE AND POLITICS

Summers, Harman, and Beckwith's books return to biography, a classical topic of history, while focusing on controversial biologists, with controversy taking place in science, politics, or both. Though controversial biologists can be found in other volumes, Summers, Harman and Beckwith pursue explicit biographical projects in which the personality of a controversial scientist is not a mere feature but the central axis of an entire book.[37] Among them they cover the twentieth century, for Summers's subject had been active in the first half of the century, Harman's in the period 1920–70, and Beckwith's in the last half of the century.

Summers brings to life d'Herelle's versatile and exotic career as a co-discoverer of the phage, inventor of phage therapy as a tool of health policy, and pioneer of pest control in many countries including his native Canada, the US, Mexico, Guatemala, Argentina, France, Egypt, Indo-China, India and Soviet Georgia. He further argues convincingly that d'Herelle should be accepted as a founder of molecular biology since phage, an object he discovered, became a key experimental system in the early days of molecular biology. Summers also examines the controversy over priority in the discovery of phage between the French-Canadian d'Herelle, the British Twort, and the Belgian Gratia, though he does not utilize the analytical apparatus of science studies for evaluating the dynamics of this controversy.

Other important themes include the Pasteur Institute's role in both European and

colonial health policy before the First World War and in the inter-war period; and d'Herelle's strict methodology and experimental practices that anticipated the founders of the Phage Group, the self-declared founders of molecular biology in the US,[38] most notably Delbruck and Luria. As Summers convincingly argues, they have often rediscovered d'Herelle's extensive work on phage, and especially its interaction with resistant and non-resistant bacteria. Summers makes excellent use of d'Herelle's extensive diaries and succeeds in establishing him as a major figure in the history of microbiology, and yes, molecular biology, while shedding new light on the early days of the Phage Group, as well as on the vagaries of the process of allocation of credit in science.[39]

Oren Harman's *The man who invented the chromosome*: *A life of Cyril Darlington* also examines a biologist as a controversial figure in science and politics, while focusing on the British cytogeneticist Darlington, a leading figure in the inter-war period who had been forgotten in the last decades, not unlike other figures from that period. Similarly, d'Herelle's key role in anticipating most of the early work of the Phage Group has remained unknown and unacknowledged. Both authors shower impressive attention upon their biographees, while greatly benefitting from the existence of extensive diaries, which they resourcefully combed for excellent quotations that enliven the text with a colourful authenticity. Both works make a much better reading than the adjacent volumes revolving around less humane categories such as biomedical traditions, institutes, and policies.

Harman provides a detailed account of Darlington's career and writings, based on Darlington's extensive papers and especially his diaries in the Bodleian Library. He builds resourcefully upon the work of other historians of biology such as V. Betty Smocovitis, Nicolai Krementsov, and Diane Paul, while situating Darlington in a wider scientific and sociopolitical context. However, in view of Harman's emphasis upon Darlington's public persona, it is unfortunate that he refrains from exploring Darlington's relationship with Muller, the leading American geneticist after 1945, or Haldane, a leading British geneticist, both central figures in the Lysenko affair which led to a permanent rupture between Darlington and his former mentor Haldane, while also affecting all the geneticists in the US, the UK, Europe, and the Soviet Union. Surprisingly, Harman says too little on Darlington's role in the Klampenborg meeting in 1938, a meeting considered by Waddington as a precursor of molecular biology; or on Darlington's influence upon the Biophysics Unit at King's College, London, the only laboratory that pursued active research on DNA structure after the Second World War.

Darlington's negative reception of DNA structure also requires further explanation, especially since he held a chair (in Botany) at Oxford after 1953. Darlington's Oxford period, which saw various efforts to establish molecular biology there (including invitations to John T. Randall, but eventually culminating with the appointment of the molecular biologist David (later Lord) Phillips to head the Laboratory of Molecular Biophysics in 1966), is not covered, thus leaving the issue of his influence on the new biology unaddressed. By contrast, Harman dwells on Darlington's emergence

as a conservative scientist, who spouted racist, sexist, and eugenicist views, right up to his death in the late 1970s.

Harman refrains from commenting on Darlington's controversial public pronouncements or pathological indifference to his five children. It is not obvious why Harman selected a topic and method that are not particularly conducive to illuminating the history of biology in the twentieth century. After all, what is important for the history of biology is not Darlington himself — the UK was full of eccentric scientists in the twentieth century — but rather how his interaction with other scientists in the UK, but also in the US and the Soviet Union, made a difference to the development of genetics and its increasing social and political valence. Harman's clinging to his subject's diaries provides for some delightful quotations, but in the end it misses an opportunity to fill a genuine lacuna in the history of twentieth-century biology.

Jon Beckwith's *Making genes, making waves: A social activist in science* is a moving autobiographical statement by a molecular biologist who managed to combine a distinguished scientific career with permanent involvement in public issues. His first-rate research on genetic regulation in bacteria early in his career while still a post-doc without prospects (when he showed that mutations preventing gene expression were not located in the gene but in its promoter) led to his being sought after by major figures in molecular biology, including François Jacob, Sidney Brenner and Bernard Davis, as well as to a 'plum' job the latter offered to him at Harvard Medical School. This early job security is essential for explaining Beckwith's capacity for involvement in every conceivable social cause, including the Vietnam war, Cuba, genetic engineering, sociobiology, genetic determinism, star wars, cultural wars of science, race and gender equality, along with many other more local causes that coloured the decades from 1960s to 2000, especially in the Harvard context.

Beckwith attributes his passion for social activism in part to the inspiration of the late Luigi Gorini (1902–76), a molecular biologist at Harvard Medical School who was one of the hundred signatories of the manifesto against Mussolini in 1931, who remained an active anti-fascist before and during the Second World War, and who rehabilitated children from concentration camps who had reached the outskirts of Milan after their rescue by the Israeli brigade within the British Army.[40] For those activities Gorini had been awarded the title of "Righteous Gentile" by the State of Israel. Beckwith, who regarded Gorini and his scientist and activist wife Annamaria Torriani (a professor of biology at MIT who still teaches at age 87) as his mentors, further raises the question as to whether his ongoing social activism had adversely affected his level of scientific achievement. (The notable molecular biologist Arthur Pardee, also of Harvard Medical School, had warned Beckwith that he would miss the Nobel Prize if he continued to involve himself in social activism.)

Beckwith admits that in the late 1960s and early 1970s he had indeed neglected science in favour of political activism, but suggests that he has since managed to combine these two inseparable calls of his life. Beckwith notes with pride that the productivity of his lab has remained high even though the lab has always attracted a large number of radical scientists.

6. BIOMOLECULAR ART OR THE "NEW GROTESQUE": THE AESTHETICS OF FEAR FROM MOLECULAR BIOLOGY

A new theme shaping the historiography of molecular biology includes the sheer diversity and visual intensity of numerous artistic responses to the growing place of molecular biology in our cultural imaginarium. The artist Suzanne Anker and the (late) sociologist of science Dorothy Nelkin teamed up in *The molecular gaze: Art in the genetic age* to chronicle and interpret the meaning of over a hundred images including paintings, drawings, sculptures, all selected for their visual power to illustrate contemporary responses of artists to the growing role of genetic issues in social and cultural life.

Though the book covers artistic renderings of a wide variety of culturally loaded topics, including transgenic animals, monsters, the commodification of genes and the quest for perfect babies, of special interest is the first section on "Deciphering DNA: The art and science of a supermolecule", which discusses the spectacle surrounding a plethora of provocative images of DNA, a macromolecule that became such a pervasive cultural icon that the art historian Martin Kemp compared its superimage status to that of the Mona Lisa.[41] Salvador Dali described it as proof of the existence of God, while the population geneticist Richard Lewontin complained about "molecular erotica", or endless anthropomorphic renditions of DNA's two "hugging" strands, both already in the early 1960s.

According to Anker and Nelkin, this dense artistic expression is a phenomenon of the 1990s when DNA art became a prominent feature of galleries and museums, in the US, Europe, and Australia. Such art, they emphasize, does not resemble traditional 'medical art' but rather reflects a response to the perception that molecular biology reduces life and the body to molecular scripts, while its applications in genetic engineering threaten the boundaries between species as well as prevailing notions of biological reproduction.

Along these lines, the last section on "Science as culture: Through the artist's lens" suggests that the artist's broad appropriation of the iconography of DNA, the genetic code, the chromosome and the autoradiograph, in a variety of aesthetic strategies ranging from portraiture to calligraphy, reflects not only a fascination with the powerful objects of molecular biology but also a "new grotesque" of deep fears grounded in ramifications of molecular biology such as the blurring of the boundaries between species, the production of chimeras, and the reduction of the body to commodified genes.

Anker and Nelkin further inquire into the meaning of this changing narrative between art and science, especially the meaning of veracity, authenticity, and aesthetics in each domain. They conclude with a reflection on the notion of beauty in art and science, to the effect that images generated by scientists and artists are grounded in different epistemologies and temporalities. Yet, the broad movement of artists toward new aesthetic strategies, tools, and materials originating in the scientific laboratory that reached a peak in the 1990s with the institutionalization of "DNA art", is a reminder that recent molecular biology has not only stirred a myriad of moral and

social issues that will remain with us in the twenty-first century but also acquired a pervasive cultural presence and visual power.

CONCLUSIONS

These books raise the issue of the most suitable unit of historical analysis in molecular biology, while providing a very wide range of possibilities: a biotech company, an inter/national policy, a biomedical tradition, a model organism, a beautiful experiment, a controversial scientist, an artistic exhibition. To some extent, all the volumes seek to combine several such units, even though they usually revolve around one of the above enumerated guiding principles; for example, Summers, Harman, and Beckwith combine the history of key experiments with the life stories of controversial scientists. Not surprisingly, the biographical genre survives, despite ongoing analytical attacks, simply because it makes a better read, as the volumes by Summers, Harman, and Beckwith amply demonstrate.

These volumes make major contributions to our understanding of the history of molecular biology by expanding our knowledge of the interlocking networks of scientists, experiments, organisms, administrators and policy makers involved in this discipline. At the same time, considerable efforts are made to inform the reader on the interaction of science and society, especially by Beckwith, Harman, Gaudillière, Summers, and several authors in the collective volumes. These volumes thus provide a solid foundation on which to build the next wave of scholarship. Inevitably, the persistence of a few lacunas will stimulate more research in the near future.

First, despite a broadly professed interest in the so-called 'big picture', most of the works revolve around local case studies, which, however informative and illuminating, still do not provide adequate methodological links for a 'doable' transition to a 'big picture'.[42] Comparative studies, especially cross-national ones, which are much more demanding than a single case study, remain rare, with only Strasser and Gaudillière explicitly engaging in cross-national efforts. Along these lines, key aspects of historical analysis such as gender, and the role of women in science, are almost entirely missing. Also missing is a preoccupation with the ethics of research for both scientists and historians. Many case studies involve ethical issues in both past and recent science that are not always recognized, or given specific attention. Furthermore, many works reflect a limited engagement with pertinent literature even when such literature is cited, which is not always the case, especially with less recent items. As a result, repetitive material that does not represent progress in scholarship, but reflects both clever and clumsy efforts at recasting conventional work as novel, invariably conveys the appearance of larger volume and greater originality than is evident to those who know the literature. Whether one focuses on the era before the Second World War, as do Summers, Stapleton, Creager and Harman; on the immediate post-war period, as do Strasser, Chadarevian, Gaudillière and Holmes; or on yesteryear as do Rabinow, Dan-Cohen, Anker, and Nelkin — the challenge of exploring the history of molecular biology is never ending. These diverse works pave the way toward greater experimentation with interdisciplinary methods, new

topics, bold theoretical orientations, perhaps even a turn toward dramatization of the history of scientific events on both stage and screen. This author hopes that her next essay review will include a critique of plays and films, not "merely" monographs. As the reviewed authors have amply shown, the lives of many of their subjects are perfectly ripe for such an adaptation.

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21. On the international context of protein structure see Pnina G. Abir-Am, "Small versus large-scale investments in protein research: The Rockefeller Foundation's international network, 1930–1960", in Gemelli (ed.), *American foundations and large scale research* (ref. 20), 220-41; on Lipmann, see his *Wanderings of a biochemist*. (Cambridge, MA, 1975); on Zinder see his "Life with Jim" in Inglis *et al*. (eds), *Inspiring science* (ref. 1), 243–56.
22. All essays, except Moulin's, in Abir-Am and Elliott (eds), *Commemorative practices in science* (ref. 16). For Moulin's see Abir-Am (ed.), *La mise en mémoire de la science: Pour une ethnographie historique des rites commémoratifs* (Paris, 1998), 207–24.
23. On DNA research at RIMR see René Dubos, *The professor, the institute, and DNA* (New York, 1976); Maclyn McCarthy, *The transforming principle: Discovering that genes are made of DNA* (New York, 1995). On its popular status, see Dorothy Nelkin and M. Susan Lindee, *The DNA mystique: The gene as a cultural icon* (San Francisco, 1995; New York, 2005). On recent books related to DNA history, including many references to DNA literature, see Abir-Am, "DNA at 50: Institutional and biographical perspectives" (ref. 1). The omission of a paper or a monograph on DNA is particularly glaring in view of its status as the most important discovery made at RIMR; the absence of an historical study that uses recently available archival material; and the fact that the Rockefeller University undertook numerous activities related to its centennial in 2001, thus suggesting that budgetary constraints played no role (e.g. conferences, exhibitions, banquets, calendars, a coffee table elegant commemorative volume, and the collection edited by Stapleton, whose authors received grants from a centennial-related special funding initiative for enhancing access to RIMR's archives).
24. Victoria A. Harden, *Inventing the NIH*: *Federal biomedical research policy, 1887–1937* (Baltimore, MD, 1986).
25. For example, Jean-François Picard, *La fondation Rockefeller et la recherche médicale* (Paris, 1999); Giuliana Gemelli, Jean-François Picard and William Schneider (eds), *Managing medical research in Europe: The role of the Rockefeller Foundation, 1920–1950s* (Brussels, 1999).
26. On the French school of molecular biology see a chapter with this title in Michel Morange's *Histoire de la biologie moléculaire* (Paris, 1994); R.M. Burian and J. Gayon, "The French school of genetics: From physiological and population genetics to regulatory molecular genetics", *Annual review of genetics*, xxxiii (1999), 313–49; Pnina G. Abir-Am*,* "Molecular biology in the British, French, and American cultural context" (ref. 12); *idem*, "The first American and French anniversaries in molecular biology" (ref. 18).
27. *Journée d'étude: La "Big Science" dans les sciences biologiques et médicales au xx siècle,* 27 June

2000; sponsored by CRHST-CSI-CNRS and Maison Suger / Maison des Sciences de l'Homme, Paris; organized by Pnina G. Abir-Am and John Krige; Gaudillière and Picard were among eight invited speakers from Europe and the US.

28. See Peter Galison and Bruce Hevly (eds), *Big science* (Cambridge, MA, 1992).
29. Viviane Quirke, "French biomedicine in the mirror of America", *Studies in history and philosophy of biological and biomedical sciences*, xx (2003), 201–12.
30. Morange, *Histoire de la biologie moléculaire* (ref. 24); also in English*, idem*, *A history of molecular biology* (Cambridge, MA, 1998).
31. See Claude Debru, Jean Gayon and Jean François Picard (eds), *Les sciences biologiques et médicales en France, 1920–1950* (Paris, 1994); Richard Burian, Jean Gayon, and Doris Zallen, "The singular fate of genetics in the history of French biology, 1900–1940", *Journal of the history of biology,* xxi (1984), 357–402; Doris Zallen, "Louis Rapkine and the restoration of French science after the Second World War", *French historical studies*, xvii (1991), 5–37; and Ilana Lowy, "On hybridizations, networks, and new disciplines: The Pasteur Institute and the development of microbiology in France", *Studies in history and philosophy of science,* xxv (1994), 655–88.
32. See Abir-Am, *Research schools of molecular biology in UK, US, and France, 1930–1970* (ref. 12); *idem*, "From multidisciplinary collaboration to transnational objectivity: International space as constitutive of molecular biology, 1930–1970", in Elizabeth Crawford, Terry Shinn, and Sverker Sorlin (eds), *Denationalizing science: The contexts of international scientific practice* (Dordrecht, 1993), 153–87; and Strasser, *La fabrique d'une nouvelle science* (ref. 18).
33. See Hans-Jorg Rheinberger, *Toward a history of epistemic things: Protein synthesis in the test tube* (Stanford, 1997); and Robert Kohler, *Lords of the fly* (Chicago, 1994).
34. For example Ton van Helvoort, "History of virus research in the 20th Century: The problem of conceptual continuity", *History of science*, xxxii (1994), 185–235; and *idem*, "What is a virus? The case of TMV", *Studies in history and philosophy of science,* xx (1991), 557–88. Nicolas Rasmussen, *Picture control: The electron microscope and the transformation of American biology, 1940–1960* (Stanford, 1997) has a chapter on TMV.
35. For example, Richard M. Burian, "How the choice of experimental organisms matters: Epistemological reflections on an aspect of biological practice", *Journal of the history of biology,* xxvi (1993), 351–68; Evelyn Fox Keller, "Models of and models for: Theory and practice in contemporary biology", *Philosophy of science*, xlvii (2000), S72–S86; *idem*, *Making sense of life: Explaining biological development with models, metaphors, and machines* (Cambridge, MA, 2002); and Rachel A. Ankeny, "Model organisms as models: Understanding the 'lingua franca' of the Human Genome Project", *Philosophy of science*, xlviii (2001), S251–S261.
36. Frederic Lawrence Holmes's *Investigative pathways: Patterns and stages in the careers of experimental scientists* (New Haven, 2004). Holmes's study of Seymour Benzer's experiments on gene structure, to be published post-humously (ref. 1) is also included as a case study illustrating Holmes's primary interest in a general interpretation of scientific creativity.
37. For the concept of an "existential biography" in science see Thomas Soderqvist, *Science as autobiography: The troubled life of Niels Jerne (*New Haven, 2003). See also the review by Pauline Mazumdar, "Immunocompetent", *American scientist,* xlii (2004), 90–91.
38. On the construction of Delbruck as the founder of the Phage Group see Pnina G. Abir-Am, "The first American and French commemorations in molecular biology …", in Abir-Am and Elliott (eds), *Commemorative practices in science* (ref. 16), 324–72. See also Salvador E. Luria, *A slot machine: A broken test tub*e (New York, 1984); and Rena Selya, "Salvador Luria: Scientist and social activist", doctoral thesis, Harvard University, 2002.
39. See Dame Marilyn Strathern, *Property, substance, and effect: Anthropological essays on persons and things* (London, 1999); *idem*, "What is intellectual property after?", in John Law and John Hussard (eds), *Actor network theory and after* (Oxford, 1999), 156–80; and Mario Biagioli and

Peter Galison (eds), *Scientific authorship: Credit and intellectual property in science* (London, 2003).

40. See Aharon Megged, *The story of the Selvino children: Journey to the Promised Land* (London, 2002). This unique story is also available as a book in Italian and Hebrew, and as a documentary film in Hebrew.
41. Martin Kemp, *Visualizations: The nature book of art and science* (Cambridge, 2000). On science and art see also Peter L. Galison and Caroline A. Jones (eds), *Picturing science: Producing art* (London, 1998); and Bettyann Kevles, "DNA art", Paper read at a conference on "DNA at 50", held in the Caspary Auditorium of Rockefeller University, New York City, 14 May 2003.
42. The issue of a 'big picture' in molecular biology has been discussed in some detail at a conference, "History and Epistemology of Molecular Biology and Beyond: Problems and Perspectives", 13–15 October 2005, held at the Max Planck Institute in Berlin, co-sponsored by the Pasteur Institute and the French Society for the History and Epistemology of Life Sciences, and organized by Soraya de Chadarevian and Hans-Jorg Rheinberger. Hopefully, the conference's proceedings will seek systematically to address this issue, while including comparative, transnational research that is necessary for exploring the transition from local case-studies to global events.