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Recent science and its exploration: the case of molecular biology

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ABSTRACT

This paper is about the interaction and the intertwinement between history of science as a historical process and history of science as the historiography of this process, taking molecular biology as an example. In the first part, two historical shifts are briefly characterized that appear to have punctuated the emergence of molecular biology between the 1930s and the 1980s, one connected to a new generation of analytical apparatus, the other to properly molecular tools. The second part concentrates on the historiography of this development. Basically, it distinguishes three phases. The first phase was largely dominated by accounts of the actors themselves. The second coincided with the general 'practical turn' in history of science at large, and today's historical appropriations of the molecularization of the life sciences appear to be marked by the changing disciplinary status of the science under review. In a closing remark, an argument is made for differentiating between long-range, middle-range and short-range perspectives in dealing with the history of the sciences.

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1. Introduction

Frederic Holmes, whose last big case studies were devoted to a couple of key experiments in the history of molecular biology—the demonstration of the semi-conservative duplication of DNA by Matthew Meselson and Frank Stahl,[1] and the genetic fine structure mapping of the rII region of bacteriophage by Seymour Benzer[2]—once remarked:

> Treating the science of former times in its own context is very different from treating contemporary science in its current context, because, as inhabitants of the present, historians must make concerted efforts to enter the mental world of scientists of the past. When dealing with contemporary science, on the other hand, we must make a concerted effort to escape from a mental world we may share with our subjects, if we are to view them with appropriate detachment. I will argue that it is easier to achieve the latter, if we have some experience with the former.[3]

With his studies on Antoine Lavoisier's oxygen chemistry of the late eighteenth century, on Claude Bernard's animal physiology of the mid nineteenth century, on Hans Krebs's biochemistry of the urea cycle of the first half of the twentieth century, and on the molecular studies of Matthew Meselson and Frank Stahl as well as Seymour Benzer, respectively, Holmes has made his way from the eighteenth, deep into the twentieth century. His conclusion was that in order to understand past science, one must try to assimilate oneself as much as possible with the context of that past; in order to understand recent science, one must try to detach oneself from that present as much as possible. With this paradox of retrospection, it appears, a possible separation line is given that lies between contemporary history and deep history, as it were, and with it, a possible demarcation criterion is formulated as well. The point of inversion, however, cannot be defined in terms of a certain number of years or generations; it has rather, on the one hand, to do with the cognitive horizon of the historian himself. For it lies at the point where the experienced necessity of distancing oneself slides over into the necessity of

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[1] Holmes (2002). For a short, preliminary version of this paper see Futura 22 (2002), 218–223.
[2] Holmes (2006).
[3] Holmes (1997), p. 166.

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having to acquaint oneself with a world that is no longer one's own. But on the other hand, the drawing of this boundary is also coupled to the changing pace of science itself: it may, in times of retardation, be further extended; in times of acceleration, it may be reached more quickly. Obviously, the endurance of scientific objects plays a role for such protractions and contractions. Where are we with respect to the history of molecular biology? A few historiographical reflections may help to clarify the matter.

2. Historical shifts

Let me start this essay with a brief outline of the historical topic. Michel Morange once described molecular biology shortly, almost to the point of a tautology, as 'all those techniques and discoveries that make it possible to carry out molecular analyses of the most fundamental biological processes—those involved in the stability, survival, and reproduction of organisms'.[4] Of course he quickly added that it is certainly not easy to decide what exactly falls under this definition and what does not, but according to his opinion, in terms of time, relatively clear boundaries can be defined for what he calls the 'molecular revolution'. To quote him again, 'The new conceptual tools for analyzing biological phenomena were forged between 1940 and 1965' and, he continues, 'The consequent operational control was acquired between 1972 and l980'.[5]

Despite this relatively precise timing, the coming into being of molecular biology was certainly not a *project* in the sense, for instance, that the human genome initiative was brought in to being as a project. It has rightly been emphasized that Warren Weaver, and with him, the Rockefeller Foundation, played a crucial role in the early years of the new biology.[6] Weaver is also credited with having coined the term 'molecular biology'.[7] I shall not question such a role, but there is one phenomenon to which I would like to direct attention here. It concerns what could be called the 'politics of sources' and points to the extent to which the histories historians write are artifacts of the available sources. The Rockefeller Foundation not only opened its archives very early on for historical work,[8] but it also invested in making the archives readily available for scientific exploration by all those historians of science who wanted to use them. Many young historians took advantage of this situation, and so, in a relatively early phase of the professional historiography of molecular biology, the impression could arise that the development of the new biology as a whole, and from its very beginnings, was a bio-politically directed enterprise that came under the rubric of the Foundation's vision of a biological control of social processes. According to our present, accumulated knowledge about the history of molecular biology, I would rather be inclined to the perspective that Paul Rabinow, in a recent interview, described using Gilles Deleuze's concept of 'assemblage'. According to Rabinow, assemblages are conglomerations in which something unfolds that 'emerges out of a lot of small decisions; decisions that, for sure, are all conditioned, but not completely predetermined'.[9] 'However, from time to time', he goes on in another essay, 'new forms emerge that have something significant about them, something that catalyzes previously present actors, things, institutions into a new mode of existence, a new assemblage, an assemblage that made things' not only appear in a different light, but 'work in a different manner'.[10]

It appears that the history of molecular biology, then, is punctuated by two such decisive shifts of assemblage—which were essentially unprecedented in the form they took. The first shift happened in the two decades between 1940 and 1960 mentioned by Morange. It had its crystallization point at the beginning of the 1950s with the characterization of the structure of the DNA double helix, and its apotheosis with the deciphering of the genetic code between 1960 and 1965. Generally speaking, the 'path to the double helix' was characterized first by a series of new analytical techniques. To use the term of the sociologist of science Terry Shinn, these were mostly 'research technologies',[11] such as ultracentrifugation, electron microscopy, X-ray structure analysis, radioactive tracing, chromatography, electrophoresis, to name the most prominent ones. These techniques had their origins in widely different research contexts, which were often, at least in the beginning, rather remote from biology. Second, the period was characterized by the transition from the classical biological model organisms, such as *Drosophila*, corn, or *Antirrhinum*, to new models such as lower fungi (*Neurospora*, yeast), bacteria (*Escherichia coli*) and viruses (tobacco mosaic virus, T-phages). Third, it involved cooperation that vitally extended over several disciplines. Molecular biology thus, according to the hypothesis, was by no means simply the next step in a linear continuation of classical genetics, which had advanced to the biological *Leitwissenschaft* over the first three decades of the twentieth century. On the contrary, it came to form an active Rabinow assemblage of its own. On the methodical level, this can be seen in the deliberate and massive import of analytical procedures from biophysics and biochemistry into biology. Although the landscape of these technologies did not take shape under the guidance of a new theoretical paradigm, in the end, it engendered one. What stood in the centre of this resulting conceptual shift was a new notion of biological specificity that found its expression in the idea of genetic information and genetic program.

The second shift of assemblage happened during the 1970s and marked at the same time the beginnings of gene technology and genetic engineering. Again, this shift brought completely new kinds of technologies into play. It is essentially characterized by the introduction of *molecular* technologies in the proper sense of the word—technologies in which the function of biological macromolecules themselves, in particular enzymes and amino acids, plays a central role. Restriction and ligation enzymes, plasmids and other vectors, and the polymerase chain reaction with DNA polymerase at its centre, are examples of such molecular biological tools. I have described the decisive step in this phase as the transition from the 'extracellular' representation of intracellular structures and processes to the 'intracellular' representation of an extracellular project.[12]

In summary, one could say: Classical molecular biology was biology operated and driven by the methods of biophysics and biochemistry, heavy analytical apparatus, big machines as a rule; gene technological molecular biology is biology operated and driven by methods that came out of the molecular biology of the first phase, methods that are rooted in molecular tools that operate in the space of the living cell itself. It is thoroughly constructive and synthetic. As Waclaw Szybalski, one of the contemporary observers,

[4] Morange (1998), p. 1.
[5] Ibid., p. 2.
[6] See, for example, Olby (1974); Abir-Am (1982); Kohler (1991); Abir-Am (2002).
[7] Abir-Am (1982), pp. 344–345; Rockefeller Foundation (1938), pp. 34–39, 203–251.
[8] The Rockefeller Archive Center, a division of the Rockefeller University, was established in 1974 to assemble, process, and make available for scholarly research the papers of the Rockefeller family and the records of various philanthropic and educational institutions founded by the family, including the Rockefeller University, the Rockefeller Foundation, and the Rockefeller Brothers Fund.
[9] Rabinow (2004), p. 63.
[10] Rabinow (2000), p. 44.
[11] Shinn & Joerges (2002).
[12] Rheinberger (2000).

himself an oncologist at the McArdle Laboratory in Madison, stated in 1978, on the occasion of the Nobel Prize award to Werner Arber, Hamilton Smith, and Daniel Nathans:

> The work on restriction nucleases not only permits us easily to construct recombinant DNA molecules and to analyze individual genes but also has led us into the new era of "synthetic biology" where not only existing genes are described and analyzed but also new gene arrangements can be constructed and evaluated.[13]

From now on, the in vitro culture of classical molecular biology started to be paralleled by a renewed in vivo culture—the manipulations were shifted back from the test tube into the cell. Not that the earlier technologies would simply have been replaced. On top of them, a new mode of doing biology came into being.

If once there existed a molecular biological discipline in the traditional sense of discipline at all, it was between circa 1960 and 1980, that is, the time between the two phases, a time in which chairs, journals, and organizations were named after it. There are good reasons to assume that since then—and in parallel to the biggest single project carried out in the history of the life sciences, the complete sequencing of the human genome—molecular biology is no longer to be considered as a discipline of its own. Today, molecular biology has found its way, in the form of molecular biological procedures, into all of the life sciences, in particular cell biology, developmental biology, and the molecular study of disease. With this second shift, the history of classical molecular biology has been relegated to deep history as characterized at the beginning of this essay, which is reflected, at least to some extent, in the historiographical moves over the past decades that I will now follow.

3. Historiographical transitions

This periodization is, at least to a certain extent, reflected in the historiography of molecular biology. It is not surprising that at the point of institutionalisation of the new discipline, in the 1960s, a time of the initiation of massive state promotion of molecular biology not only in the United States, where this had started already in the 1950s, but also in Europe,[14] that a first wave of historical writing came from the core of the new and still comparatively small and choice community of molecular biologists themselves. The molecular biologists used their historical writing deliberately as a political means of disciplinary consolidation.[15] It was during this time that a series of *Festschrift*-like volumes appeared that collected reminiscences of the actors and were devoted to one or a few of the self-proclaimed main players in a few of the centres where the new biology had come to be rooted: Max Delbrück and the phage group at Caltech in Pasadena,[16] Linus Pauling and X-ray crystallography in Pasadena as well,[17] and Jacques Monod and the group at the Pasteur Institute.[18] To Gunther Stent we owe a first periodization of molecular biology into a romantic early period, a 'dogmatic' golden age, and an academic phase—in loose translation of Auguste Comte's scheme of the three scientific ages—as well as a first differentiation into informationalist and structuralist schools in the development of molecular biology. Stent belonged to the actors of the first generation and punctuated the path of molecular biology with his reminiscences.[19]

It was characteristic of this early phase of homemade history of science by scientists that it privileged groups and schools that one could easily identify. In particular, it concentrated on the role that physics—theoretical physics—played in the foundation of the new direction in biology. Niels Bohr's paper 'Light and life' as well as Erwin Schrödinger's book *What is life?* played a central role in this foundational myth.[20] Jim Watson's story, *The double helix*, of 1968 contributed to it as well from the iconoclastic and at the same time iconogenic perspective of a young rebel.[21] Ten years later, it was Horace Judson who, with his *Eighth day of creation*, brought this period to a close and erected a first monument for those he called the 'makers of the revolution in biology'.[22] Another trait was unmistakable in these stories: they all portrayed the founders of molecular biology as disciplinary dissidents who cultivated an unconventional style of scientific communication, of cooperation without disciplinary restrictions, and an equally unconventional international scientific tourism. In short, they were painted and painted themselves as a group of modernists in whose self-representation the classical disciplinary boundaries and the traditional national boundaries no longer played a crucial role. Molecular biology was seen as the scientific incarnation of the new way of life and feeling of life that paved the path to the cultural rebellions of the 1960s. And it profited immensely from the new emphasis, after World War II, on the values of basic science with its concomitant, massive increase in state funding, first in the United States, and then in Europe as well.

It is only too consequent, then, that the first generation of professional historians who came to occupy themselves with the history of molecular biology in the 1970s and 1980s distanced themselves from the essentials of this eulogistic self-assessment. The myth of scientific schools and their leaders began to be deconstructed.[23] Historians of science like Edward Yoxen, Pnina Abir-Am, Evelyn Fox Keller, and Lily Kay undertook it also to reinsert the political context that had largely been eliminated from house history.[24] They looked critically at the bio-political mission of the Rockefeller Foundation, dealt with the exodus of physicists into molecular biology as a reaction to the Manhattan Project, and foregrounded the role of cybernetics and information technology as key technologies of the Cold War in the elaboration of molecular biology. Another characteristic of the historiography of this time consisted of, to mention only Richard Burian here,[25] emphasizing the role of biochemistry in the foundation of the experimental systems of molecular biology that had been completely eclipsed and set aside not only by the physically-minded foundation mythologists, but also neglected by their early critics.

This second reception phase coincided with the fierce debates around emerging recombinant DNA technology, and with that, the second shift in the history of molecular biology sketched above. It happened at a point in time when major players of the first period of molecular biology left the field in search of new challenges,

[13] Szybalski & Skalka (1978).
[14] See, for example, de Chadarevian & Strasser (2002).
[15] Abir-Am (1999).
[16] Stent et al. (1966).
[17] Rich & Davidson (1968).
[18] Monod & Borek (1971); Lwoff & Ullmann (1979).
[19] Stent (1968, 1969). See also Thuillier (1972).
[20] Bohr (1933); Schrödinger (1944).
[21] Watson (1968).
[22] Judson (1979).
[23] Abir-Am (1985).
[24] Yoxen (1979, 1982); Abir-Am (1982); Kay (1985); Olby (1989); Keller (1990); Kay (1993).
[25] Burian (1993).

mostly to neurobiology, among them Stent, who like many of his colleagues had foreseen an unchallenging 'academic phase' for molecular biology only a few years earlier. At the same time, this turn also coincided with a broader reorientation in the historiography of science that has been subsumed under the notion of the 'practical turn'. Both events, one of them in the space of the shifting objects of science, the other in the space of epistemological deliberations about what the proper objects were for historians of science, need to be considered here. One might even be inclined to assume a certain complicity between the two turns.

On the one hand, the emergence of gene technology led to seeing the preceding history of molecular biology in a different light. It became clear that one had to do not only with a new theoretical paradigm, but rather—in retrospect—with the beginnings of a new applied biology, a new biotechnology. With that, attention shifted to the material and practical contexts that were at the basis of this technological turn. On the other hand, the history of molecular biology was full of instruments, model organisms, and experimental systems, through which an instrument and experiment-driven research could be studied in an exemplary fashion. Although rather different in topic and methodical orientation, quite a number of historical case studies have to be seen in this context, though I mention only a few monographs here: Nicolas Rasmussen's study of the rise of electron microscopy in the United States, Soraya de Chadarevian's study on X-ray crystallography in Cambridge, Jean-Paul Gaudillière's work on the history of molecular biology and biomedicine in France, Angela Creager's study on tobacco mosaic virus research at Princeton and Berkeley, Lily Kay's study on the history of the genetic code, Bruno Strasser's work on molecular biological electron microscopy in Geneva, and my own study on the history of in-vitro protein synthesis research.[26] Although these works appeared as book-length studies only in recent years, most of the projects date back into the late 1980s and the early 1990s, and with that, testify not least to the fact that historical work concerned with the exploration of the practical side of scientific development can be a rather tedious and time-consuming enterprise. Taken together, this stage in the historiography of molecular biology is possibly best explained by a remarkable conjuncture between a deep incision in the history of molecular biology itself and a change in the exploration of new methods of practising the history of science—an assemblage thus at the level of historiography.

If the judgment holds that with gene technology, molecular biology has found its own, intrinsically molecular methods, and at the same time has initiated a development during the course of which it has ceased to exist as a narrowly confined discipline, leading to a quasi-dispersed existence as an arsenal of methods pervading all of the life sciences; and if the assumption of a relatively tight coupling between recent historiography of science and the development of the sciences themselves should stand up to scrutiny; then we ought to see a historiographical reflection of this new situation in the present state of writing the history of molecular biology. Indeed, already a decade ago, Burian pleaded for a view of molecular biology seen not as a problem-oriented discipline, but rather as a 'battery of techniques'.[27] Today, the shift of molecular biology away from disciplinary concerns and its capillary dispersal appears to document itself in a certain loss of interest in its history. And indeed, while the celebration of the fiftieth anniversary of the double helix has seen the publication of a number of remarkable biographies and autobiographies,[28] it has not given rise to major new and qualitatively outstanding professional contributions to the history of molecular biology worth mentioning. The last major encompassing history of molecular biology is the one of Morange, and it dates back more than a decade.[29] Thus it appears that, in parallel to the evaporation of molecular biology *as a discipline*, on the historiographical level we observe a certain loss of interest in the history of molecular biology as a discipline.

What will take its place in historical analysis is not yet clearly visible. On the one hand, what can be observed today, instead, is a renewed interest in the history of developmental biology.[30] Through the introduction of the technical armament of molecular biology, decisive turns have taken place in developmental biology over the past thirty years. It appears therefore only consequent that historiographical interest returns to the history of an area of biology that, during the flowering period of classical genetics and the disciplinary phase of molecular biology, did not receive much historical interest, not least because developmental biology at the beginning only marginally profited from its methods. On the other hand, shift in historical interest to the gene technology of the 1970s and 1980s with a focus on its technical feats and with that, to the second phase of the history of molecular biology, has been hesitant.[31] What has, however, recently been gaining momentum is an assessment of the wider social, cultural, and economic reverberations and contextualization of that phase.[32]

A short look into a couple of works that have appeared during the past few years might give us at least some clues for further orientation. Let me first turn to *The life of a virus* by Angela Creager and *Designs for life* by Soraya de Chadarevian.[33] Both works, each of them in its own manner, shed light on so far under-appreciated facets of the molecularization of biology from the middle into the late twentieth century. Both emphasize a particular local scientific context, the experimental practices situated within it, and the contingent networks in which these practices were embedded. In the tradition of the practical turn, they show that a microscopic gaze at the generation of knowledge is not only a condition for any analysis of 'science in the making', but—in perfect resonance with the tradition of microhistory—can bring dynamics into focus that would remain hidden when viewed through the lens of a macrohistorical history of ideas. But despite these similarities in the focus on scientific practices, both analyses show themselves also to be remarkably different. Perhaps it is not by chance that these differences, if in a rather subtle manner, find their expression in the titles of the works. In both of them, the notion of 'life' plays a crucial role. In *The life of a virus*, the concept is used as a metaphor for the multifarious, quasi-biographical fate of a scientific object, for the multidimensional research symbiosis between a scientist and his experimental system. *Designs for life* points in another direction. Taking Cambridge's molecular biology as an example, it draws attention to the changing perception of life itself as an object of research, an object that is being trimmed and reconfigured through the social, political, technical and epistemic processes that characterize the age of the atomic and the molecular sciences.

Jean-Paul Gaudillière's book *Inventer la biomédecine* goes in a similar direction, but with a different slant. It throws new light on the history of molecular biology in France in the first two

[26] Rasmussen (1997); Rheinberger (1997); Kay (2000); de Chadarevian (2002); Creager (2002); Gaudillière (2002); Strasser (2006).
[27] Burian (1993).
[28] Maddox (2002); McElheny (2003); Wilkins (2003).
[29] The French original of the book was published in 1994.
[30] See, for example, Laublicher & Maienschein (2007).
[31] See, for example, Kevles & Hood (1992); Rabinow (l996, 1999); Gaudillière & Rheinberger (2004).
[32] See, for example, Rajan (2006); Vettel (2006); Rebentrost (2006).
[33] de Chadarevian (2002); Creager (2002).

decades after World War II, a history that had largely been codified and iconized as the history of that small group of people who, with Jacques Monod and François Jacob at the Institut Pasteur, performed their Nobel-worthy work on gene regulation. Naturally, this story is not absent from Gaudillière's account, but it is placed in the context of a much broader bio-political perspective, which this time brings the life sciences into a closer connection with the development of the medical sciences in France. Gaudillière aims, as he expresses it, at an 'archaeology of a society that rests on medical biotechnologies', an archaeology that seeks to understand 'how trust in molecules, technical control, and big numbers became essential features of twentieth century scientific culture'.[34] However, Gaudillière does not try to accomplish this by telling *the* big history of molecularization of life in the twentieth century. As the other studies mentioned do, he keeps the lesson of the micro-studies in mind and shows in detail how idiosyncratic, how entrenched in local traditions, and yet how globalised the patchwork of this history appears.

In a very recent book deliberately written from the perspective of science in history, Eric Vettel traces the origins of molecular biotechnology in California's Bay Area, in particular Berkeley, Stanford, and San Francisco, with the eye of a general historian interested in the changing cultural and economic ambience of science after World War II.[35] On the example of Wendell Stanley's Biochemistry and Virus Laboratory established in Berkeley in 1948, Vettel highlights two aspects of American bioscience policy that he considers characteristic for the first two decades after World War II: first, the transition from selective philanthropic funding, such as practised by the Rockefeller Foundation, to massive federal funding that was unprecedented in its dimensions in the immediate postwar period; and second, an equally unprecedented emphasis on basic research with a concomitant explicit effort to sever traditional links of biological research to medical and agricultural departments and practices. Stanley's 'freestanding' laboratory, with its emphasis on the physics and chemistry of life, served as an incentive for Stanford as well as the University of California at San Francisco to reshape their life science research programs accordingly. At length, Vettel then describes the rise, in particular around Berkeley, of an academic as well as popular counterculture with its emphasis on environment and health, issues that the basic life sciences of the time did not appear to address, and their detachment from a generation that was perceived to have lost sight of the values of real life and had led America into the war in Vietnam. On the federal level, this movement coincided with a policy shift initiated under Lyndon Johnson and continued under Nixon. On the one hand, it emphasized the need for practical returns from the basic biosciences, and on the other, in response to the economic depression during the later 1960s, made an end to the miraculous increase of federal research money that had characterized the decade immediately after World War II and the decade following the Sputnik shock. This is the climate in which Vettel places the origins of molecular biotechnology that, on the one hand, resulted in a complete realignment of the relation between basic and applied research and with it, a complete shift in the academic self-perception of a new generation of bioscientists, and on the other hand, led to an equally complete realignment between academia and industry exemplified on Cetus, history's first biotechnology company founded in Berkeley in 1972.

All these studies, although from widely different starting points and from widely varying perspectives, do not tell the history of molecular biology of a site or of a country and *enrich* it with cultural and economic context. Rather they place molecular biology *within* social and cultural history and try to understand it in relation to its dynamics: local, national and world-historical—*Science in history*, we could say, alluding to the title of John Desmond Bernal's monumental work of almost half a century ago.[36]

4. Time Perspectives

At the end of his book on the history of molecular biology—in a section most interestingly missing from the American translation—Morange reminds us of Fernand Braudel's reflections on the differentiation of historical times and rhythms.[37] He proposes to use Braudel's differentiations for the history of science and to distinguish, more specifically in the history of molecular biology, three currents or layers, three different time regimes, each of a different duration. Each of them generates different questions. There is, first, the 'time of reduction', a current of long duration, which embeds the history of molecular biology in the secular process of reducing the phenomena of the world, including the living world, to their physico-chemical basis, and which has characterized Western science for about four centuries. In the framework of such a long-term history, one would have to ask whether, with the molecular approach to life as it took shape around the middle of the twentieth century, our understanding of the living and with it, the life sciences, as a whole has become different. As Michel Foucault, François Jacob, and others as well have argued,[38] biology as a science of its own came into existence around 1800 by assembling, in a thoroughly materialistic perspective, around the question of the specific difference between living beings and non-living systems. The question continued to haunt the life sciences throughout the nineteenth century with a tendency to equate living and non-living systems. The question for our time would be: with the molecular form of biological specificity, as codified in the concept of genetic information (for reproduction) and the concept of genetic programme (for development) do we have a new answer? And what kind of answer?

Then there is, following Morange—following Braudel—another layer that is superimposed on the first one and that can be equated with a history of disciplines. What influence had the molecularization of life on the history of the biological disciplines? I have made a few remarks regarding the institutionalization of molecular biology and its subsequent transformation. But these remarks themselves need to be embedded. Starting from the history of molecular biology, it would be rewarding to think about the dynamics that came into the disciplinary landscape of biology as a whole during the twentieth century, and to think about whether, with these new dynamics, the boundaries between biology and medicine have also entered a process of becoming profoundly reconfigured. The term 'biomedicine' is itself only an indicator of this reconfiguration. Moreover, the admittedly strong hypothesis would have to be considered if not the classical disciplines, as shaped during the nineteenth century, have entered altogether into a process of supersession, a process of which the molecularization of biology is only a particularly prominent example. This of course will then be not without repercussions for the possibility—or the growing impossibility—of conceiving and understanding the dynamics of the contemporary sciences in the framework of disciplinary histories. In this respect, Paul Forman has talked about a 'devaluation of disciplines', and he has seen this trend based not only on the growing problem orientation, but also on the growing market orientation of the contemporary sciences. 'This

[34] Gaudillière (2002).
[35] Vettel (2006).
[36] Bernal (1969).
[37] Morange (1994), pp. 333–335. Compare Braudel (1975).
[38] Foucault (1973); Jacob (1993).

reorientation toward the market', Forman postulates, 'together with the increasing orientation toward the particular problem, works powerfully to dissolve the scientist's attachment to his discipline, indeed to dissolve the disciplines themselves and their disciplinary authority'.[39] What kind of institutional structures are actually taking their place?

Finally, then, there is the third, short-range history, the history of events, the history of experiments, theorems, model organisms, instruments, and of all the cultural, institutional, social and political factors that have their place in the actual, concrete course of the development of the sciences. Most of the case studies that we have so far of the history of molecular biology are situated at this level. My own forays into the history of experimental systems operate in the same realm—but they also try to show an alternative to the history of disciplines. In this realm, Rabinow's lucky concept of 'assemblage'—the term I have been using is 'conjuncture'[40]—appears to be very appropriate for describing the specific textures in which and by which conditions are set for the generation of unprecedented knowledge.

I can only touch on these ideas here and must refrain from developing them in detail, in particular in the much-desired direction of an integration of these different levels and histories. An interesting synthetic suggestion comes from Pnina Abir-Am. She sees the molecularization of the life sciences of the twentieth century coming under the grip, in the aftermath of World War I of biochemistry, in the aftermath of World War II of biophysics, and in the aftermath of the Cold War of engineering.[41] Abir-Am's take appears, however, to be captured by an unquestioned framework of disciplines. Nevertheless, a fruitful discussion about the relation between micro-histories and macro-histories of science might ensue from such endeavours. The 'practical turn' in historiography of science has quite naturally privileged micro-histories. It is timely to ask how these micro-histories may become integrated into wider contexts, without giving up the rich knowledge provided by case studies, but also without falling back to the time of the grand narratives.

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