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The Politics of Macromolecules

Molecular Biologists, Biochemists, and Rhetoric

By Pnina G. Abir-Am*

MOLECULAR BIOLOGY, considered as a broad transdisciplinary movement of people and ideas at the interface of biology, physics, and chemistry, became a sociohistorical reality in the 1930s.¹ But not until a generation later, in the 1960s, did the scientific establishment begin to register and respond to this ongoing restructuring of the scientific order away from traditional disciplinary regimes, with their monopolies over the academic reproduction of “knowledge-power” systems and resulting social control.²

Spokespersons for previously established “neighboring” disciplines initially responded to the rise of molecular biology with enthusiasm or resistance, depending primarily on their disciplinary provenance and rhetorical situations. While the rhetorical occasions ranged from lengthy presidential addresses before disciplinary scientific societies, to brief letters to the editors of scientific journals, to science policy statements and counterstatements, the respondents’ disciplinary provenance determined the form of the response. Spokespersons for the various disciplines were chiefly concerned to defend their own disciplines’ boundaries and image, rather than to reflect upon molecular biology’s impact on the scientific order in its entirety. These disciplinary lines of defense in part reflected the role of

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¹ See, e.g., Robert C. Olby, “The Molecular Revolution in Biology,” in *A Companion to History of Science*, ed. R. C. Olby, G. N. Cantor, J. R. R. Christie and M. J. S. Hodge (London/New York: Routledge, 1990), pp. 503–520; Edward J. Yoxen, “Giving Life a New Meaning: The Rise of the Molecular Biology Establishment,” in *Scientific Establishments and Hierarchies, Sociology of Science Yearbook VI*, ed. Norbert Elias *et al.* (Dordrecht: Reidel, 1982), pp. 123–143; and P.G. Abir-Am, “The Biotheoretical Gathering, Transdisciplinary Authority and the Incipient Legitimation of Molecular Biology in the 1930s: New Perspective in the Historical Sociology of Science,” *History of Science*, 1987, 25:1–70, and references cited there.

² For the conceptualization of the knowledge-power relation as complementary, and for its relevance to disciplinarity as a form of social control, see Michel Foucault, *Discipline and Punish* (New York: Vintage Books, 1979); and Foucault, *The History of Sexuality*, Vol. I: *An Introduction* (New York: Vintage Books, 1980), esp. Pt. 4, Ch. 2. For an application of Foucault’s approach see P. G. Abir-Am, “The Discourse on Physical Power and Biological Knowledge in the 1930s: A Reappraisal of the Rockefeller Foundation’s ‘Policy’ in Molecular Biology,” *Social Studies of Science*, 1982, 12:341–382; and Abir-Am, “Beyond Deterministic Sociology and Apologetic History: Reassessing the Impact of Research Policy upon New Scientific Disciplines,” *ibid.*, 1984, 14:252–263. On the rhetoric of disciplinarity and cross-disciplinarity see also a special volume of *Social Epistemology*, 1990, 4, esp. Ellen Messer-Davidow and David Shumway, “Introduction to Symposium on Crossdisciplinarity,” pp. 261–266.

disciplinary authority in compartmentalizing the scientific establishment. Perhaps also the multiple disciplinary origins of molecular biology's fast, often startling results obscured for a while its broader, cumulative meaning as a restructuring or revolution within both the biomolecular and the biological orders.

The tenor of the criticism suggests that molecular biology posed a threat to a wide range of disciplines, because it was redefining, and hence appropriating, many concepts, both central and peripheral, around which the "classical" disciplinary monopolies were constituted. The concepts of "virus" and "gene," for example, are central to the disciplines of virology and genetics. These concepts were redefined when the concept of the structure of nucleic acids (especially DNA and the variety of RNAs) was elevated from a peripheral position within biochemistry—in which only their building blocks, the nucleotides, rather than their sequences were of interest—to the very center of molecular biology, as carriers of biological information. These changes forced biochemistry, virology, and genetics to redefine the meaning of their classical concepts and hence their scientific authority. Similarly, the new concept of "molecular evolution" forced evolutionary theorists to reconsider organismic evolution as the only major unifying theme in biology.

Though the earliest commentators on the rise of molecular biology perceived a diverse spectrum of possible threats or benefits, few of them appeared interested in assessing its conceptual or methodological novelty. They all did, however, refer to the social and science-policy reality of molecular biology, or its success in recruiting human and material resources, that is, "students" and "grants." They themselves were located primarily in three disciplinary areas within the scientific establishment: cell biology and microbiology, organismic biology, and biochemistry.³

Support, with various degrees of enthusiasm, was expressed by spokespersons from cell biology, microbiology, and microbial genetics. Their statements suggest that many cellular and microbial biologists, especially the young, turned easily to molecular biology, for both scientific and professional reasons. On the scientific front, their move was facilitated by their familiarity with research objects such as subcellular organelles, microbes, and viruses that were key tools in effecting molecular biology's shift toward uncovering the unity of mechanisms underlying biological diversity. On the professional front, the rise of molecular biology enabled cell biologists and especially microbiologists, who were often part of auxiliary, less-prestigious units in medical schools, to join the professional vanguard of basic biological research.⁴

Resistance to molecular biology came chiefly from organismic biology and biochemistry, two large, philosophically divergent, and institutionally separated dis-

³ For a more detailed rhetorical analysis of responses to the rise of molecular biology by spokesmen of various other disciplines in the early 1960s see the epilogue in P. G. Abir-Am, *The Biotheoretical Gathering in England, 1932–1938, and the Origins of Molecular Biology: An Essay on the Construction, Legitimation, and Authority of Transdisciplinary Knowledge in a Historical Context* (Ph.D. diss., Univ. Montreal, 1984).

⁴ See Honor B. Fell, "Fashion in Cell Biology," *Science*, 1960, 132:1925–1927; Edward A. Adelberg, "Bacterial Viruses" (a review of *Molecular Biology of Bacterial Viruses*), *Science*, 1964, 143:345; and Joshua Lederberg, "Molecular Biology, Eugenics, and Euphenics," *Nature*, 1963, 198:428–429.

ciplinary establishments. Organismic biologists contested the claims of molecular biology to radical novelty, especially its claim to have displaced evolution as the central problem of biology, thus upsetting the prevailing unity of the biological order. Theoretical, developmental, and population geneticists, such as Conrad H. Waddington, Theodosius Dobzhansky, and Richard Lewontin, aimed at restricting the epistemological and ontological claims of molecular biology and retaining the supremacy of evolutionary theory. Systematists and evolutionary theorists, such as George Gaylord Simpson and Ernst Mayr, tended to emphasize the limited biological relevance of molecular biology, which they referred to as "mere" chemistry.⁵

The response of biochemists similarly focused on resisting loss of authority in the biomolecular order, a loss derived from the rediscovery of nucleic acids by molecular biologists, and their transformation into new objects such as messages of biological information, messages that transcended the biochemists' traditional preoccupation with small molecules. In this paper I examine the struggle between molecular biologists and biochemists over the shifting hegemony in the biomolecular order from the latter to the former, in a variety of contexts, including statements by science policy committees, anniversaries of scientific societies, and historiographically motivated scientific autobiographies.

I explore primarily the semantic manifestation of this authority contest over "biology at the molecular level," while also inquiring into cross-national aspects of this confrontation. For example, in Great Britain and France the predominant context for the struggle for hegemony in the biomolecular order was science policy, especially governmental advisory committees. This was because the relevant science policy establishments, the Medical Research Council (MRC) and the Centre National de la Recherche Scientifique (CNRS) were centralized in both countries.⁶

In the United States many skirmishes took place in peer review committees and the science policy panels of various governmental agencies, most notably the National Science Foundation (NSF) and the National Institutes of Health (NIH), as well as in the American Society of Biological Chemistry (which changed its name in 1987 to the American Society of Biochemistry and Molecular Biology) and the National Academy of Sciences. Yet possibly because the structure of American science policy agencies and academic institutions was more decentralized, biochemists advanced the most eloquent responses to molecular biology outside policy contexts in such arenas as endowed prestigious lectures, historically oriented meetings, and autobiographical recollections.

Many U.S. biochemists, especially in such times of social and cultural change as the 1960s, also appeared compelled less by allegiance to a disciplinary tradition that was largely European than by the pragmatic prospects of rapid career ad-

⁵ For more on this subject see P. G. Abir-Am, "Organismic Biologists Respond to the Rise of Molecular Biology: The Rhetoric of a Biological Order in Transition," *Journal of the History of Biology*, forthcoming.

⁶ On the MRC see Joan Austoker and Linda Bryder, eds., *Historical Perspectives on the Role of the MRC* (Oxford: Oxford Univ. Press, 1989). On the role of CNRS and other governmental bodies in France in the rise of molecular biology see Xavier Polanco (with P. G. Abir-Am and Michel Callon), "The role du CNRS et DGRST dans la biologie moléculaire," *Cahiers pour l'Histoire du CNRS*, 1939-1989, 1991, 7:123-156.

vancement in an expanding and liberalizing academic system. Thus when the biochemical approach to cracking the genetic code proved superior to the genetic approach, and the problem of protein synthesis, restated as a problem of transcription and translation, came to integrate the biochemical and the genetic approaches in what became a major and distinctive research agenda of molecular biology, even veteran biochemists such as Fritz Lipmann (Nobel Prize, 1953) and Severo Ochoa and Arthur Kornberg (Nobel Prize, shared 1959) were coopted into the agenda of molecular biology in the 1960s.⁷

In the following two sections I analyze the rhetoric of collective statements and counterstatements by committees of molecular biologists and biochemists. Those statements revolve around the politics of small versus large molecules, in varied social contexts or “rhetorical situations.” The contests over scientific authority, so vividly articulated in these committee documents in which collective disciplinary authority was formally vested, turn out to be more than a debate between two modes of scientific authority, that is, between a traditional, disciplinary, empiricist, method-oriented, slow, small-scale, vocational mode and a progressive, transdisciplinary, conventionalist, model-oriented, fast, large-scale, entrepreneurial mode. The very essence of scientific authority—whether it was derived from nature or socially constructed—was at stake. The debates between biochemists and molecular biologists in the 1960s proved to be part of the transition to a postmodernist global society, in which transdisciplinary scientific authority is no longer contested but has become the new way of gaining identity in a pluralistic, participatory, and socially responsive scientific order.

I. “BRAIN DRAIN” AND DISCIPLINARY HEGEMONY: MOLECULAR BIOLOGISTS AND SCIENCE POLICY IN GREAT BRITAIN IN THE 1960s

In April 1966 the Council for Science Policy set up a committee or “Working Group”—chaired by John C. Kendrew and composed of eight leading scientists, mostly from MRC’s research units in Cambridge, Edinburgh, and London—to “inquire into the present conditions of, and future plans for, teaching, recruitment and research in molecular biology in the United Kingdom.” Kendrew was one of the two most veteran members of the MRC Unit at Cambridge, which in 1955, then still housed in a “hut,” became the first institution worldwide to be named “Laboratory of Molecular Biology.” By the time it had moved to a new, modern building in 1962, shortly before three of its members received the Nobel Prize (Max Perutz and John Kendrew in chemistry and Francis Crick in physiol-

⁷ See, e.g., M. Daniel Lane, “May the Next 80 Years be Even Better”, *The Biochemist*, 1991, 13:4 (Lane is president of the American Society for Biochemistry and Molecular Biology); and Arthur Kornberg *et al.*, eds. *Reflections on Biochemistry* (in honour of Severo Ochoa’s seventieth birthday) (New York: Pergamon Press, 1974). This last includes scientific and personal recollections by forty-nine biochemists, ten of them Nobelists (Carl Cori, Fritz Lipmann, Hugo Theorell, Konrad Bloch, Fyodor Lynen, Arthur Kornberg, Paul Berg, H.G. Khorana, Ernst Chain, and Hans Krebs). Many of these biochemists, like Ochoa himself, turned to molecular biology in the 1960s. See also Olby, “Molecular Revolution in Biology” (cit. n. 1); Robert E. Kohler, *From Medical Chemistry to Biochemistry: The Making of a Biochemical Discipline* (New York: Cambridge Univ. Press, 1982), Ch. 12; R. C. Olby, “Biochemistry as a Political Institution” (review of Kohler’s book), *Science*, 1983, 222:145–146; and P. G. Abir-Am, “From Biochemistry to Molecular Biology: DNA and the Acculturated Journey of the Critic of Science Erwin Chargaff,” *History and Philosophy of Life Sciences*, 1980, 2:3–60.

ogy), the MRC Laboratory of Molecular Biology had become “the world’s informal communications center for the young science of molecular biology.”⁸

The “Working Group on Molecular Biology” presented its report two years after it was constituted. It inquired into both the research and the teaching aspects of molecular biology and presented specific proposals for halting the supposedly decreasing British prominence in that field. One problem faced by the Working Group in Molecular Biology was the need to settle on an operative definition of molecular biology. The report of the Working Group took a historical approach, commenting first on the enormous and rapid progress made in biology in the last fifteen years and comparing it with that made in physics during the first quarter of the twentieth century.⁹

Biology’s progress took place, we are told, chiefly in the area now “known as molecular biology,” an area that had for its object the “description of the structure, organization, and function of living cells”; molecular biology is further described as based on concepts of physics, chemistry, and mathematics, as well as on new powerful physical technologies. This combination, which had produced a powerful stimulus to “fundamental biological research,” focused first on the molecular mechanisms of heredity and later on those of more complex areas such as embryology, immunology, neurology, and pharmacology. The impact of research in molecular biology not only had a “deep intellectual significance” but was likely to yield “social and economic dividends of inestimable value” through biomedical and agricultural applications.¹⁰ The report also stressed the prestige brought to British science by British scientists’ early leadership in molecular biology, singling out for special mention the elucidation of the structure of proteins and DNA: among the five molecular biologists receiving Nobel Prizes in 1962 four were British, including the Working Group’s chairman, Kendrew, and a Working Group member, Maurice Wilkins.

The Working Group therefore presented a threefold argument for maintaining a high level of support for molecular biology. First, molecular biology was a source of international prestige for British science. Second, it was a good investment, relatively inexpensive and likely to produce high socioeconomic dividends. Third, it was a fundamental approach to the whole of biology, and therefore its rate of progress determined the progress of biology as a whole. Only at this stage did the report come to confront the touchy problem of defining molecular biology.

Yet the Working Group felt that molecular biology did not “correspond to any real subdivision of biology”; it merely adopted the term for its political mandate since it had become “so fashionable.” Rather than defining molecular biology, the report stated that biochemistry, cell biology, molecular genetics, and molecular biology not only overlapped but formed a coherent field—the molecular approach being dominant in all of them, so distinguishing them easily from either organismic or population biology. The Working Group thus redefined its task as

⁸ Mahlon Hoagland, *Toward the Habit of Truth* (New York: Norton, 1990), p. 99. See also Max Perutz, “Origins of Molecular Biology,” *New Scientist*, 31 Jan. 1980, pp. 326–329; and Joan A. Steitz, “Shaping Research in Gene Expression: Role of the Cambridge Medical Research Council Laboratory of Molecular Biology,” *Perspectives in Biology and Medicine*, 1986, 29:S90–S95.

⁹ John C. Kendrew (Chairman), *Report of the Working Group on Molecular Biology* (London: H.M.S.O., 1968), no. 3752.

¹⁰ *Ibid.*, p. 3. All discussion in this section refers to consecutive pages of this report.

an investigation into the status quo in basic research and teaching in the field of "biology at the molecular level."

Lacking "objective criteria by which to determine what proportion of national resources allocated to biology should be devoted to investigations at the molecular level," the Working Group limited itself to the domain of "facts" as opposed to that of "values." The "facts" came from a compilation of publications in fifteen journals and research grants awarded by a number of governmental Research Councils and private foundations during the period 1964–1966. The Working Group found that about one tenth of all British published papers in biology were "molecular" by their broad definition, a number too small vis-à-vis the potential of molecular biology. Greater national resources should therefore be devoted to molecular biology, and equally important was their way of distribution.

Noting the interdisciplinarity of the field (zoology, biochemistry, genetics, physiology, chemistry, physics, and mathematics were mentioned), the report stated that as to organizational structures, molecular biology needed both "relatively large groups," so that the variety of resources needed for its integration could be found in one place, and close association between scientists from different disciplines. While the traditional university structure did not necessarily prevent close association, institutional arrangements that positively encouraged it were pronounced a necessity, among them the "relaxation of traditional departmental boundaries." Thus the report identified a major problem faced by researchers in molecular biology since the first conception of its possibility a generation earlier: that the traditional university structure was not in fact conducive to its pursuit. Indeed, in most universities the departmental subdivisions "often inhibit real contacts" among the relevant scientists.

The Working Group recommended changing the curriculum at the teaching level and the organizational structure at the research level to encourage molecular biology. The problem was less the actual resources available than their distribution. Many tenured appointments in biology departments were kept by tradition for activities that were "by any reckoning out of date." The report contrasted the situation in the United States, where molecular biologists could obtain tenured appointments with greater ease because the university system there was more flexible. This situation produced a constant "brain drain" from Britain to the United States, one that the report warned would continue if conditions in Britain were not "greatly improved." Equally important were the training of suitable personnel and the incorporation of molecular biology into the curriculum, goals likewise impaired by the "difficulty of transcending the established pattern of departmental boundaries." For teaching the report recommended "flexibility of syllabus" at the elementary level, outbreeding among universities, establishing postdoctoral fellowships, and updating established members of the university staff through reorientation courses.

Since devising a "more modern approach to research and teaching in fundamental biology" was likely to be rather slow "within the framework of existing organizations," and the need to support a modern, molecularly oriented biology was urgent, the report proposed creating "a number of special centers of research and advanced teaching, specifically designed to focus on relevant scientific disciplines in a common attack on fundamental biological problems." Those "focal centers" were envisaged as complementary to the existing science departments of

the universities in which they were to be established, even to the extent of sharing staff. Rather than confronting the touchy problem of how to change the entire disciplinary order in academia to make space for a transdisciplinary endeavor like molecular biology, the report asserted that appointing "the most talented individuals" as leaders of the proposed "focal centers" would place molecular biology at the frontier of modern biological thought. The report also stated that two or three centers already existed and others were emerging. Its concrete proposition was that one or two additional centers be established immediately, with half a dozen as the final goal, although rules for their establishment were left to "local circumstances."

The report glossed over the contradiction between its proposals and the prevailing university structure in several respects. It claimed that though the scheme was "a radical departure from the conventional university arrangement," the envisaged organization "would naturally form an integral part of an existing university," with its full cooperation. Any future implementation of the policy proposals was to link teaching and research within the university framework. Creating the "focal centers" should be the joint responsibility of the governmental Research Councils, especially the MRC, which had just inaugurated a new large building for molecular biology in Cambridge, and of the University Grants Committee, which allocated grants from the government on a university basis. Yet a third partner was required in the scheme, since the expansion of the curriculum and the encouragement of new attitudes toward molecular biology were the primary responsibility of the universities themselves. Although the proposals could be interpreted as in part an expansion of existing activities of both the Research Councils and the University Grants Committee, some required specific coordination between these two bodies. "The possible machinery for such coordination," however, was not in the report's "terms of reference."

In conclusion the report listed four major objectives: (1) to encourage a multidisciplinary approach to research and teaching of biology at the molecular level; (2) to improve the teaching of biology at the molecular level; (3) to make greater use of the untapped teaching potential available in nonuniversity research laboratories; and (4) to create "focal centers" of teaching and research at a few selected universities. Four appendices followed, of which Appendix D, on how a "focal center" in molecular biology should be conducted, is of special interest. The appendix states that the status of the "focal center" as an independent organization or as part of a school of biology should be determined by "local circumstances." The center should be housed so as to allow effective interaction (for example, by locating it within walking distance of the relevant disciplinary resources in the area). The report urged that the structure of the senior staff be nonhierarchical, all group leaders having equal responsibilities and appointments as tenured professors. To achieve flexibility the report suggested appointing molecular biologists as tenured professors to personal chairs without specific titles, or with titles tailored to a given incumbent, and giving staff double appointments in a center and existing departments, with the proportion of time spent on each allowed to vary.

Another feature of the "focal center" was its need for long-range support, since its success depended on integration of resources from different disciplines, rather than the two- to three-year projects normally approved by the Research Councils. As to the actual establishment of the "focal centers," the appendix suggested

either inviting applications from the universities once the Research Councils and the University Grants Committee announced the decision to build the centers, or offering a center to universities known to possess a suitable environment. No concrete examples were mentioned.

Stressing that to control progress and avoid stagnation depended on the control of three key elements—staff, space, and money—the report suggested distributing responsibilities among the universities, the researchers, and the governmental Research Councils. The control of staff, through voluntary curriculum reform and staff reorientation, was primarily assigned to the universities. The control of space was assigned to a scientist's peers, through periodical reviews by a laboratory committee of senior staff with rotating chairmanship. Finally, the control of money remained the responsibility of the (governmental) Research Councils, to which each researcher would apply independently for funds. The councils were described as “well placed to maintain quality” by virtue of their knowledge of science at the national level and of the need to balance different scientific fields at that level. Besides being experienced overseers of science, the councils also had a tradition of encouraging “new lines of research at the frontier of biology.”

The Report of the Working Group in Molecular Biology encompassed a series of issues, such as the persistence of “local circumstances,” the role of the Labour government in launching a systematic science policy, and the limited, ineffective participation of leading scientists in science policy as a result of the ambiguity of their political mandate. Its most immediate interest for this article, however, is that it documents the intertwining of science policy in the 1960s and the ultimate legitimization of molecular biology as a form of transdisciplinary knowledge that transcended the prevailing institutionalized forms of social control in academic science.

The report does not merely display the political relations of molecular biology as an external and “given” phenomenon. Rather, it exemplifies the molecular biologists' deployment of a contingent and limited role in science policy (addressing the issue of brain drain in British science) as a major instrument for legitimizing the field both as a scientific reality and as *the* authority in fundamental biology. Molecular biology did not have academic legitimacy in the mid 1960s, but it acquired political legitimacy by maneuvering itself into the position of being an object of a new initiative, by a new government, in science policy.

It was under such a political mandate that molecular biologists endeavored not only to provide an exemplifying case of a prestigious scientific field in need of greater governmental intervention, but also managed to redefine molecular biology and impose an expanded meaning of the field as the sole authority over all “biology at the molecular level” while simultaneously seeking to undermine the authority of academically established but politically less astute disciplines, especially biochemistry.

Quite aside from its prospects for implementation (the Scientific Council recommended publishing the report as a stimulant for discussion rather than as a guideline for implementation), as an official document the report managed to circulate, under an aura of political legitimacy, not only a new conception of molecular biology but also a new conception of scientific order: pluralistic, participatory, and transdisciplinary.

II. THE BIOCHEMICAL SOCIETY RESPONDS: ACADEMIC FREEDOM AND AUTHORITY OVER "BIOLOGY AT THE MOLECULAR LEVEL"

Shortly after the Report of the Working Group in Molecular Biology was published, the British Biochemical Society charged a subcommittee with replying to it. The molecular biologists' report adversely affected the public status of biochemistry by ignoring its traditional preoccupation with "biology at the molecular level" and subsuming it under molecular biology. The mandate of the subcommittee was "to consider the Report on Molecular Biology, the present state of biological sciences in this country with particular reference to biochemistry and especially in connection with research and teaching."¹¹

To counteract properly the weight of the opposition, the subcommittee also numbered eight members and included a Nobel Prize winner as its chairman. Only two of its members were affiliated with governmental research units, as against six of the eight members of the Working Group; the rest held academic positions, indicating biochemistry's longer institutional presence as an academic discipline. Yet only two members, as against seven of the molecular biologists, were Fellows of the Royal Society. This social composition suggests that biochemistry had more local rather than central power, and that its representatives were less prominent socially and politically. The exception was the subcommittee's chairman, Sir Hans Krebs, the most celebrated figure in British biochemistry in the late 1960s, who had held the chair in biochemistry at the University of Oxford since 1954 and was both F.R.S. and a Nobel Prize winner.

The counterreport eventually put out by the subcommittee became, according to leading biochemists, a best-seller—apparently because it combined a micro-political argument for regaining the traditional authority of biochemistry over "biology at the molecular level" with a macropolitical one for limiting the governmental intervention in science that molecular biologists were demanding. The latter argument had a long-standing resonance in the British scientific community, having originated in the late 1930s in the confrontation between two factions with different political philosophies for both society and science: a socialist faction led by the physicist-turned-X-ray-biocrystallographer J. D. Bernal, who had just moved from Cambridge University to a professorship at Birkbeck College in London; and a liberal one led by the émigré physical chemist Michael Polanyi, based at the University of Manchester, a locale accustomed to vying for national leadership, both culturally and scientifically.¹²

Bernal was the founding father of British molecular biology, having originated its main theme of protein X-ray crystallography in 1934. During World War II he had personally recruited Kendrew in Burma, where they met when Bernal arrived as a science policy adviser to Mountbatten. It was under Bernal that Max Perutz began working on the structure of hemoglobin as a research student at Cambridge

¹¹ See Sir Hans Krebs, F.R.S., *Biochemistry, "Molecular Biology" and the Biological Sciences* (London: Biochemical Society, 1969); further described as a "Report of a Subcommittee of the Biochemical Society on the Report of the Working Group on Molecular Biology ("Kendrew Report") and the present state of the biological sciences in this country, with particular reference to biochemistry."

¹² See William McGucken, *Scientists, Society and State: The Social Relations of Science Movement in Britain, 1931-1947* (Columbus: Ohio State Univ. Press, 1984), esp. Ch. 9 and the epilogue. See also Gary P. Werskey, *The Visible College* (London: Lane, 1978); and J. D. Bernal, *The Social Function of Science* (London: Heinemann, 1939).

in 1937, the work for which he shared the Nobel Prize with Kendrew in 1962. Bernal inspired Francis Crick to move to the interface of physics, biology, and chemistry after the war and prompted Rosalind Franklin and Aaron Klug to continue the work on the structure of the tobacco mosaic virus that he initiated in the mid 1930s, which also received a Nobel Prize (in 1982, for Klug; Franklin had been dead since 1958). Bernal, who had also pioneered science policy analysis in the late 1930s, was one of the authors of the science policy platform that the Labour government of the 1960s had just begun to implement, and Kendrew's Working Group was only one aspect of a new political framework more conducive to steering science in the national interest. An explicit resonance of the counterreport of the Biochemical Society with the cause of academic freedom was therefore inevitable.

The success of the counterreport was not due to its logical structure, which (as we shall see below) was marred by circularity, rhetoric applying a double standard, and clumsy appeals to history, but rather derived from a historical alignment between micropolitical and macropolitical conceptions of disciplinary and social power. By contesting the molecular biologists' political-turned-scientific mandate, the biochemists found themselves almost inevitably in the camp of "academic freedom," an alternative political framework available within the British scientific community since the late 1930s. It was a most convenient one, since the Soviet invasion of Czechoslovakia and its ensuing cultural repression had discredited strong governmental intervention in Britain—especially any led by those known to have ties to the Soviet Union.

Although the biochemists were not Polanyi's disciples in the dual—scientific and political—sense that the molecular biologists were Bernal's, their social background, especially that of their formal leader, Krebs, resonated well with Polanyi's. Both came to Britain in 1933 after being dismissed from promising careers at the most advanced scientific institutions in their native Germany, the Kaiser Wilhelm Institutes in Berlin, precisely as a result of governmental intervention in science. That background informed Polanyi's leadership of the British society for academic freedom in the 1940s, as well as his social philosophy of science, in itself a key element in the then-emerging notion of the "scientific community" in the 1960s; that philosophy would have been known to Krebs and to any other prewar scientist.

The biochemists' argument against the governmental intervention demanded by molecular biologists was also fueled by more utilitarian concerns, most notably preserving their hard-won institutional gains in academia. Ironically, the spokesmen for biochemistry appeared to have forgotten that "academic freedom" had resisted the entrance of their own discipline into academia a generation or so earlier, denying it autonomous status on account of its hybrid character at the interface of physiology and chemistry.¹³ Now they were equally unable to share dis-

¹³ On the institutionalization of biochemistry and its hybrid disciplinary image see David Bearman and John T. Edsall, eds., *Survey of Sources for the History of Biochemistry and Molecular Biology* (Philadelphia: American Philosophical Society Library, 1979); Joseph S. Fruton, *Contrasts in Scientific Style: Research Groups in the Chemical and Biochemical Sciences* (Philadelphia: American Philosophical Society Library, 1990); Kohler, *From Medical Chemistry to Biochemistry* (cit. n. 7); and P.G. Abir-Am, "The Philosophical Background of Joseph Needham's Work in Chemical Embryology," in *A Conceptual History of Modern Embryology*, ed. Scott F. Gilbert (New York: Plenum, 1991), pp. 159–180.

ciplinary power, but mounted a futile rearguard effort to deny it to a newcomer field, one that had weaker academic representation but had nonetheless established a power base in both the national and the international arenas.

The Biochemical Society subcommittee sought initially to surpass the very basis of the mandate of the molecular biologists' Working Group, while arguing ever more confidently that all biology, not just the candidate areas of differentiation and immunology mentioned by the report, would be molecularized in the future. Stating that all biology with a material basis was in part "molecular," the subcommittee reiterated the strongly reductionist and empiricist ethos of biochemistry, as if unaware that this once-revered ethos was no longer sufficient, whether as a philosophical recipe for scientific progress or as a political strategy for competing with molecular biology when it came to colonizing classical biological and related establishments in the late 1960s.

This outdated appeal to the good old days of the 1920s, when biochemistry could portray itself as the champion discipline of scientific progress by leading an ongoing reductionist assault on vitalistic and quasi-vitalistic targets, reveals how the hold of tradition and old glory on a community becomes an impediment to change. The biochemists' outdated belief in the omnipotence of the reductionist ethos as a spur to scientific progress was largely responsible for their subsequent slow adaptation to new concepts such as biological information and molecular regulation, which were taking the center stage in the biomolecular revolution in the mid 1960s. The social composition of the subcommittee further reveals the human substratum of such beliefs, for the biochemical leaders in the late 1960s were individuals who, like Krebs, had undergone their formative period in the interwar period.

The subcommittee, much like the Working Group, began with semantics, discovering that "biology at the molecular level" was "uncomfortably longwinded." The subcommittee proposed to use instead the term "biochemistry," which was said to include not only "biology at the molecular level" but also biophysics. Both reports presented their alternative semantic representations for the range of phenomena captured by the phrase "biology at the molecular level"—"molecular biology" and "biochemistry"—as a mere semantic preference. Both groups of scientists had difficulty acknowledging that their differences were not just a matter of semantic taste, but rather a reflection of a power struggle over the exclusive authority to define "biology at the molecular level," since both groups believed that exclusive, as opposed to shared, authority was necessary for controlling the symbolic and socioeconomic dividends associated with advances in "biology at the molecular level" at that time. The issue was simultaneously scientific and political.

The committees may have disregarded this aspect not only because of the prevailing ideology of science as an apolitical endeavor, but also because they held a zero-sum game conception of both scientific authority and scientific resources: if the British government were to implement the recommendations of the Working Group on Molecular Biology, biochemistry stood to lose its traditional position of authority over the chemical aspects of biology—recently extended to encompass macromolecular aspects—both symbolically and materially. In another ironic twist, this struggle to maintain an outdated exclusive form of control rather than share it took place just at the time that molecular biology and biochemistry

joined forces in a biotechnology based on recombinant DNA, thus creating new power and wealth on a scale that surpassed the universe of presumably fixed resources—symbolic and material—they were contesting with so much semantic zeal and self-contradiction.¹⁴

The subcommittee, for example, though eager to expand the traditional meaning of “chemical” to include “macromolecular,” lamented that “molecular biology” had expanded its own “real” meaning once it became a “fashionable catchphrase.” Suggesting that molecular biology should have adhered to its early narrow meaning—W. T. Astbury of the University of Leeds initially described it as a field dealing with the shape of biological macromolecules—the subcommittee presumed that biochemistry alone could rightly expand its meaning.¹⁵ In the 1950s, however, the term “molecular biology,” modestly used in the scientific literature since the late 1930s, came to be used in connection with the “study of the chemistry, synthesis and biological functions of macromolecules”; it then gained wide circulation with the successful launching of the *Journal of Molecular Biology* in 1959 by scientists who, not being trained in biochemistry, needed to distinguish themselves and their endeavor from it.

Biological macromolecules thus became a territory contested by two groups of scientists with different rationales for pursuing that domain. While biochemists viewed macromolecules as a more challenging extension of their traditional work on the structure and function of smaller biological molecules, molecular biologists came to view the spatial structure of macromolecules as an ultimate site of key biological processes such as respiration, heredity, and regulation, formerly pursued at the cellular level only. The biochemists resisted, although the two fields shared a successful semantic strategy. At the turn of the century “biochemistry,” much like “molecular biology” in mid century, had prevailed owing to its semantic advantage, registered by scientists as being a better catchphrase, in conveying a new scientific identity: it replaced a longer and more accurate description of an increasingly irrelevant past, here “physiological chemistry,” which connoted marginality within both chemistry and physiology, with a short, vaguer designation that promised an autonomous future, here extending chemical explanations over all of biology, over all the phenomena of life.

The question remains, Why and how were these essentially parallel and complementary sociohistorical perspectives construed by their practitioners as rationales for clash rather than collaboration? For example, the subcommittee argued that even within the narrow meaning (such as Astbury’s definition, which referred almost entirely to the X-ray crystallography of biological macromolecules and did not raise any objections from biochemists in the 1940s and 1950s) molecular biology did not merit autonomy. According to the subcommittee, stereochemistry had originated in 1874 in the tradition of organic chemistry, a parent discipline of

¹⁴ The biochemists who worked with large macromolecules became known as “macromolecular biochemists.” See, e.g., Franklin Portugal and Jack Cohen, *A Century of DNA*, (Cambridge, Mass.: MIT Press, 1977); Arthur Kornberg, *DNA Synthesis* (San Francisco: Freeman, 1974); Kornberg, *For the Love of Enzymes: Odyssey of a Biochemist* (Cambridge, Mass.: Harvard Univ. Press, 1989); and Kornberg *et al.*, *Reflections on Biochemistry*, (cit. n. 7), Sect. IV.

¹⁵ See W. T. Astbury, “Adventures in Molecular Biology,” *The Harvey Lectures* (Springfield, Ill.: Thomas, 1951), pp. 3–44; and Astbury, “Molecular Biology or Ultrastructural Biology,” *Nature*, 1961, 190:1124.

biochemistry; hence the three-dimensional study of macromolecules (which included stereochemistry) was also a legitimate part of biochemistry, a discipline whose *raison d'être* was the application of organic chemistry to living matter.¹⁶

But the subcommittee also recognized that this historical argument was insufficient to undermine the legitimacy of molecular biology. Its members agreed that macromolecules “exhibit many special features” and were not a large addition of “small molecules familiar from organic chemistry.” It was precisely their large size which made possible new properties. Faithful to its overall goal of minimizing the novelty and *raison d'être* of molecular biology as a distinct endeavor, the subcommittee nonetheless argued that those new properties, which depended upon the complexity of the spatial structure of large molecules and could not be deduced from the properties of small molecules, were mere “subtle refinements.”

As an example, it mentioned the high specificity of enzymes and their regulatory capacity, derivative of their conformational changes. This choice of examples was in itself a device designed to emphasize biochemistry's traditional rights over biology at the molecular level, since enzymes had been uniquely associated with biochemistry's topical repertory. Similarly, the subcommittee refrained from speaking of the regulatory capacity of RNA or DNA, since not only were those concepts clearly associated with molecular biology, but biochemists had either missed or actively resisted their significance. Thus the subcommittee also acknowledged that phenomena such as neighbor influence on chemical reactivity, known from traditional biochemistry, were of a “different order” when exerted on biological macromolecules.¹⁷

Rather than concluding that molecular biology and biochemistry were dealing with different phenomena, since the biological roles of large and small molecules were profoundly different, and accepting that these two perspectives overlapped in the crucial area of interaction between small and large molecules (e.g., in operon-regulated metabolic pathways), the subcommittee attempted to justify overlooking large molecules in biochemistry. Digressing once again into history, it acknowledged that organic chemists, whom biochemists accepted as methodological ancestors, had resisted the concept of the macromolecule. As a result of this heritage (a heritage labeled by a biochemist-turned historian as the “dark age of biocolloidology”) not only did biochemists fail to discover the significance of macromolecules first, thus losing priority, but the macromolecules became a reminder of biochemistry's possibly limited and outdated disciplinary strategy in the progress of science.¹⁸

¹⁶ See J. S. Fruton, *Molecules and Life: Historical Essays on the Interplay of Chemistry and Biology* (New Haven: Yale Univ. Press, 1972); Fruton, “The Emergence of Biochemistry,” *Science*, 1976, 172:327–334; Marcel Florkin, *A History of Biochemistry* (Amsterdam: Elsevier, 1972); and Kohler, *From Medical Chemistry to Biochemistry* (cit. n. 7).

¹⁷ On the complex history of macromolecules in chemistry and biochemistry see, besides Florkin, *A History of Biochemistry*, and Fruton, *Molecules and Life* (both cit. n. 16), J. T. Edsall, “Proteins and Macromolecules: An Essay on the Development of the Macromolecule Concept and Some of its Vicissitudes,” *Archives of Biochemistry and Biophysics*, 1962, *Suppl. 1*:12–21; Norman W. Pirie, “Patterns of Assumptions about Large Molecules,” *ibid.*, pp. 21–29; and R. C. Olby, “The Significance of Macromolecules in the History of Molecular Biology,” *History and Philosophy of the Life Sciences*, 1979, 1:1–12.

¹⁸ See Marcel Florkin, “The Dark Age of Biocolloidology” in his *A History of Biochemistry* (cit. n. 16), pp. 279–284; and Neil Morgan, “The Strategy of Biological Research Programmes: Reassessing the ‘Dark Age’ of Biochemistry, 1910–1930,” *Annals of Science*, 1990, 47:139–150.

Biochemists could not explain their failure by deploying such customary excuses as experimental difficulties, since scientists trained in other disciplines did focus their attention on macromolecules, despite empirical difficulties. Those who approached the problem of the structure of biological macromolecules after the 1930s were not biochemists by either training or ethos, but rather physical scientists like Astbury and Bernal, the very ancestors Kendrew had invoked in several publications and radio talks, in an effort to demarcate the social and historical distinctiveness of molecular biologists like himself.¹⁹

Besides belittling and trivializing the conceptual difference between small and large molecules as an instance of "mere" historical accident that should be overlooked in view of the later logic of their interrelationship, the subcommittee report tried to undermine the explanatory ideal of molecular biology—the correlation of the structure and function of biological macromolecules—while asserting that biochemistry had always advocated this explanatory ideal. Once again semantic confusion was deployed to deprive molecular biology of any substantive or methodological distinction, in this case by blurring the distinction between the correlation of structure with function in small and in large biomolecules, especially in the matters of conservation, expression, and transfer of biological information.²⁰

By asserting that biochemistry and molecular biology had the same aim, yet appropriating the history of molecular biology by claiming that the influx of physical scientists—associated with the rise of molecular biology—had a great impact on biochemistry, the subcommittee conflated logic and history. Its report incorporated an ideological assertion, the belief that molecular biology was a natural development of biochemistry, within the very premise designed to corroborate that belief. In attempting to obliterate the social and historical distinctiveness of molecular biology in order to protect its own classical disciplinary ethos, the (British) biochemical establishment extended the role of scientific societies beyond the customary one of facilitating communication. It acted as a custodian of biochemistry's scientific authority at the national level, since it believed that not only legitimacy in defining scientific reality but the allocation of resources was also at stake.

As to resources, the subcommittee argued that there was no need to establish new departments of molecular biology, but an urgent need to expand biochemistry. "Molecular biology" could exist only under the jurisdiction of biochemistry, as one of its subdivisions, asserted its report, listing immunochemistry, enzymology, molecular genetics, and intermediary metabolism as other fields properly subsumed under biochemistry. Only biochemistry was not arbitrary but remained immutable in the eyes of its authoritative representatives, who had ap-

¹⁹ See J. C. Kendrew, *The Thread of Life* (Cambridge, Mass.: Harvard Univ. Press, 1966); Kendrew, "How Molecular Biology Started?" *Scientific American*, March 1967, 216:244–246; and Kendrew, "Information and Conformation in Molecular Biology," in *Structural Chemistry and Molecular Biology*, ed. Alexander Rich and Norman Davidson (San Francisco: Freeman, 1968), pp. 187–197.

²⁰ See, e.g., the separate chemical and biological chapters in Portugal and Cohen, *A Century of DNA*, (cit. n. 14); see also Scott F. Gilbert, "Intellectual Traditions in the Life Sciences: Molecular Biology and Biochemistry," *Perspect. Biol. Med.*, 1982, 26:151–162; and Abir-Am, "From Biochemistry to Molecular Biology" (cit. n. 7).

parently forgotten how much opposition their own field had encountered when it first strove to establish its own autonomy a few decades earlier.

The Report of the Biochemical Society suggested reorganizing the teaching and research of biology along more fundamental, analytic, and molecular lines, suggestions made as if biochemistry was the only authority in these matters. A new analytic approach in biology was ascribed to a union of biochemistry and genetics, though the report refrained from explaining how such a union had been achieved or whether molecular biology was instrumental in biochemistry and genetics' realization of their mutual relevance.

Both the Report on Molecular Biology in 1968 and the Report on Biochemistry in 1969 are major documents of disciplinary politics, conceived and circulated in the broader context of alternative visions of national science policy. They reveal the strategies and tactics deployed in disciplinary contests over scientific authority.²¹ As a strategy, both attempted to incorporate the competition as a mere division of their own greater and more legitimate scientific reality, denying the opponent the phenomenological and methodological distinctiveness necessary to claim exclusive authority over scientific reality. As tactics, both reports deployed semantic confusion and historical selectivity. They knew that suppressing changes in the meaning of scientific terms across disciplines and across time is easy in a culture that believes both in the supremacy of logic over historical process and in the universality of scientific terms as mirrors of natural reality rather than as sociohistorical constructs.

If history is important for scientists only to the extent that it ratifies the present, then such "subtle" changes as the shift in explaining the interrelation between structure and function in terms of large rather than small molecules are not that important, and the stance of the Report on Biochemistry is vindicated. A question, however, persists as to the social mechanisms operating in a system like science, whose continuous functioning depends on a continuous suppression of its history. Both reports conducted their battle for exclusive authority over "biology at the molecular level" as if semantic and historical arguments among their leaders, rather than the social reality backing those arguments, were the decisive elements in settling the dispute.

While authorities from molecular biology described their discipline as comprising "biochemistry, cell biology, molecular genetics and molecular biology," authorities from biochemistry described theirs as comprising "molecular biology, immunochemistry, neurochemistry, enzymology, intermediary metabolism." This mutual rhetorical appropriation was possible in the late 1960s not only because conceptual changes in both biochemistry and molecular biology had led to important areas of overlap—especially when operon-controlled cellular regulation turned out to involve an interaction between large and small molecules—but because many biochemists had shifted their research agendas to large molecules, eroding one early distinguishing mark of molecular biology. The Report on Biochemistry then used the result, that the phenomenology of macromolecules was

²¹ On authority as a theoretical concept and its application to the history of molecular biology see Abir-Am, "The Biotheoretical Gathering" (cit. n. 1) and references cited there to literature in social theory. See also Theodore M. Porter, "Objectivity and Authority," *Poetics Today*, 1991, 12:245–265.

no longer sufficient to distinguish molecular biology from biochemistry, as evidence that no such distinction ever existed.

The profound political intent of the Biochemical Society Report's definitions is further evident if contrasted with a definition of molecular biology, given by its chairman, Krebs, two years earlier, in the context of an essay on scientific creativity.²² There Krebs defined molecular biology as a new area combining aspects of biochemistry, biophysics, and genetics; he pointed out that its creativity and non-conformism were supported by governmental research units rather than academia. This time, well before he chaired a public dispute with molecular biologists over scientific authority, he implied that molecular biology was distinct from biochemistry. In the later, political context, this earlier view was superseded by one denying the distinctiveness of molecular biology and subsuming it under biochemistry.

Both reports capitalized on the metaphorical flexibility and hence ambiguity of "molecular biology" to reach opposite conclusions, despite similar rhetorical strategies. Molecular biologists interested in extending the institutionalization of molecular biology from governmental research units to academia expanded its initial, narrow meaning—as an area concerned with the structural and functional interrelationship of biological macromolecules—to include all biomolecules, both large and small, disregarding both biochemistry's priority over small molecules and the conceptual kinship between "chemical" and "molecular," a kinship that led biochemists to believe that, despite their historical neglect of biomacromolecules, they could easily welcome these objects back into the biochemical fold, like lost kin.

Biochemists, interested in preserving their academic monopoly over "biology at the molecular level," overlooked the historical and social consequences of their neglect of biomacromolecules: the irreversible rise of a new community with a sociohistorical identity of its own. If molecular biologists overstated the case for discontinuity in the biomolecular revolution, then biochemists overstated the case for continuity. Each side phrased its argument for its legitimate scientific authority in terms of a logic of survival; the lethal arsenal included semantic confusion, abuse of history, and arbitrary definition of subject matter, all revolving around consistent patterns of double-standard rhetoric.

In Britain the debate between biochemists and molecular biologists was clear-cut, although in the United States and Europe the bulk of the resistance to molecular biology came from classical biology. There many biochemists joined the molecular biology bandwagon, and joint units for biochemistry and molecular biology became a common form of institutionalization. What led to these differences?

First, the British context was unique in that there was alignment between the disciplinary and the ideological aspects of science policy, with molecular biologists aligned in favor of governmental intervention for both micropolitical and macropolitical reasons, while biochemists favored nonintervention, also for both disciplinary and ideological reasons. Second, the historical role of the MRC as a central patron of both biochemistry (after World War I) and molecular biology

²² Hans Krebs, "The Making of a Scientist," *Nature*, 1967, 215:1441–1445. See also Frederic L. Holmes, "The Fine Structure of Scientific Creativity," *History of Science*, 1981, 19:60–70.

(after World War II) created a historical and institutional context of competition over a key agent in science policy. Third, in Britain molecular biology began earlier than elsewhere, in part due to the vision of Bernal and his associates, so that of the first five Nobel Prizes awarded to molecular biologists (two in chemistry, three in physiology), four went to British scientists, supporting the meteoric rise there in the prestige of the field. Fourth, *The Double Helix*, published in 1968 and a best-seller, publicized the exploits of its would-be heroic biologists in the early 1950s and highlighted the prejudice they encountered in the British scientific establishment. Fifth, the leveling off of postwar economic growth in the late 1960s was particularly felt in Britain, whose global stature had been continuously declining since the end of World War II. The sudden realization that national resources devoted to science were now scarce, reflected in the brain-drain problem, the subject the Kendrew committee was supposed to address, was conducive to the zero-sum game politics characteristic of the debate between biochemists and molecular biologists.

Yet another factor that determined the unique course of the debate between the two groups in Britain was the existence of the British Biochemical Society itself, a well-organized nationwide power base for biochemists. Established in 1911 as a dining club, the society evolved into a powerful professional organization whose mandate stretched well beyond the customary role of scientific societies in facilitating communication to guarding the professional status, even welfare, of its members. At the anniversary of its five-hundredth meeting, also in 1969, the Biochemical Society once again mobilized its wide membership for a vigorous defense of their glorious disciplinary tradition.

III. THE BIOCHEMICAL SOCIETY'S ANNIVERSARY MEETING: A RITE OF DISCIPLINARY REAFFIRMATION AND COOPTATION

In conformity with the conclusion of the Biochemical Society's subcommittee that molecular biology was "a natural development of biochemistry," an anniversary volume celebrating the society's five-hundredth meeting featured molecular biology as one of several subdivisions of biochemistry (the others were immunology, intermediary metabolism, and separation methods). The strategy of incorporation was evident in the volume's structure and content, but especially in the introduction to the first section by its chairman, J. N. Davidson, C.B.E., F.R.S., an international authority on nucleic acids, a former Honorary Secretary of the Biochemical Society (1947–1951) and, since 1948, the Gardiner Professor of Biochemistry at the University of Glasgow.²³

Molecular biology appeared in the first section of the volume, even though other topics, most notably intermediary metabolism, were more deserving of that honorary position—thus stressing the strategy of cooptation. The content of the section on molecular biology also seems calculated to dilute whatever distinctiveness molecular biology still possessed. Such topics as virus and protein structure, central to the rise of molecular biology, are treated from a biochemical perspec-

²³ See *Biochemical Society Symposia*, 1969, 30, esp. Davidson's introduction, pp. 3–4. On the Biochemical Society see T. W. Goodwin, *History of the Biochemical Society, 1911–1986* (London: Biochemical Society, 1987).

tive only. For example, only studies of plant viruses were included, since those were done by biochemists, while studies of animal and bacterial viruses, associated with cellular and molecular biologists, respectively, were omitted. Though it might seem reasonable for biochemists to celebrate what they did accomplish rather than what they did not, in the context of controversy over scientific authority this strategy was no longer neutral; it was evasive because it masked the existence of nonbiochemical perspectives on viruses. This omission enabled biochemists to convey the impression that viruses were a biochemical territory; since viruses were key research objects in molecular biology, this strategy supported the enclosure of molecular biology within biochemistry. Thus a selective structure of topics became a subtle form of undermining other disciplines, molecular biology in particular, as legitimate contenders for the scientific accomplishments and authority associated with virus research.

This strategy of appropriation by projecting a partial contribution as a total claim also appears in the choice of "crystallographic enzymology" as the topic of another article in the section. "Enzymology" resonated with biochemistry, since enzymes were the first distinct phenomenology of biochemistry, while crystallography was a key technique deployed in molecular biology; it was particularly prominent in the contribution of British molecular biologists, including the four Nobelists in 1962 and one in 1964. The aspect associated with biochemistry was thus given semantic primacy by becoming the substantive name or essence of scientific reality ("enzymology"), while molecular biology's perspective was relegated to the secondary status of a descriptive adjective ("crystallographic"). Once again the substantive metaphor "drags" the descriptive one, conveying the meaning that crystallographic enzymology belonged more to biochemistry than to molecular biology.

The section also includes an article on "the primary structure of proteins," a traditional biochemical topic. Including it here implies that molecular biology's unique contribution to the elucidation of the tertiary structure of proteins logically complemented and depended on knowledge of the primary structure—an accomplishment of biochemistry. But this implication obscures the historical independence and precedence of tertiary studies of protein structure, which were conducted at a time when the primary structure of proteins was not known.

These implications of the section's contents are reinforced by Davidson's introduction to the whole section. Davidson was particularly suited for this job, since he was an authority on nucleic acids before they were reconceptualized by molecular biologists. He thus represented a key area where the authority of molecular biology and biochemistry had clashed, and he exemplified the historical and experimental priority of biochemistry over the disputed area. Nucleic acids were a central topic in molecular biology, while in biochemistry they had been a marginal one for a long time. Although it is difficult to imagine molecular biology without nucleic acids in the late 1960s, it is quite possible to imagine biochemistry without them.

Precisely because of this greater dependence of molecular biology on the phenomenology of nucleic acids (to be sure, recast as primary and secondary semantides or carriers of biological information), in his introduction Davidson tried to undermine its claim to this subject area, while drawing on the biochemists' (experimental) priority in it, even though the latter's conception of nucleic

acids was very different from that introduced by molecular biologists. This strategy of ignoring later theoretical changes in the meaning of nucleic acids while emphasizing experimental priority, however partial or marginal, informs his entire introduction.

Describing molecular biology as “that field of Biochemistry which has become commonly known as ‘molecular biology,’ ” Davidson assumed the very premise he wanted to prove (and stressed it with the capital *B* for biochemistry, as opposed to the quotation marks he used around molecular biology). Acknowledging that the precise meaning of molecular biology “has been the subject of endless, and sometime heated argument,” Davidson did not mention that similar heated debates accompanied the emergence of biochemistry at the turn of the century (and for that matter any other new field). By casting molecular biology as a controversial topic in a subculture where consensus is believed to reflect nature (or at least a “respectable tradition”), he unmistakably conveyed an impression that something was wrong with the field. He himself regarded it as “fundamentally Biochemistry,” call it “classical Biochemistry, if you like, with a smattering of Genetics, Virology, Cell Biology and Biophysics.” Here he substituted a personal preference for a real attempt to explain why biochemistry could claim to subsume all these other disciplines. The frequent use of the clause “if you like” conveyed that his definition was chosen in accordance with his audience’s preference and not arbitrarily.

Still, it was precisely his audience’s exclusive right to define other disciplines, and molecular biology in particular, that was at stake. The only evidence Davidson gave that biochemistry did subsume them was the inclusion of considerable material on virology and genetics in the *Annual Review of Biochemistry* (*ARB*) in 1969. But *ARB* was not a neutral source of arbitration but rather another piece of the biochemical establishment, which since its inception in 1932 had always expanded the boundaries of biochemistry under the banner of efficient communication.

Davidson’s double standard for the two disciplines shows further in his calling it futile and unprofitable to define the limits of any branch of science—and honoring that credo for molecular biology while endorsing the boundless limits of biochemistry and its natural capacity to engulf half a dozen disciplines. He asserted that a discussion of molecular biology’s meaning was a suitable topic for an after-dinner speech, but not for a chairman’s introduction. Yet his introduction did define biochemistry. Although he treated the meaning of molecular biology as undeserving of serious attention, Davidson nevertheless pointed to the Biochemical Society Subcommittee’s Report on Biochemistry and Molecular Biology as a reliable source that had already dealt with this evidently troubling and persistent question and had even turned out a “best-seller.” Its “findings” were that molecular biology was a natural development of biochemistry.

Yet this was precisely the issue between biochemistry and molecular biology: who had the right to talk authoritatively about their shared subject. Both reports studied above claimed this authority exclusively for their own discipline and subsumed their competitor under an expanded version of their discipline’s traditional authority. Davidson used the same type of logical fallacy, claiming that the Biochemical Society had been talking about “biology at the molecular level” during its past 499 meetings. This statement rested on the assumption that the mean-

ing of "biology at the molecular level" had remained unchanged during the period 1911–1969, an assumption that was evidently false, since in the first three decades of the twentieth century the concept of macromolecules was not even accepted by either chemists or biochemists, who accepted the methodological hegemony of organic chemistry over their own discipline.

The context of an anniversary rite facilitated Davidson's task of denying the reality of molecular biology. By drawing on the continuous institutional reality of biochemistry, Davidson conveyed an image of the field as possessing a continuous conceptual reality. Important celebrations in any social system, and the Biochemical Society's five-hundredth anniversary was no exception, exist for the very purpose of consolidating social allegiance to key values and hidden presuppositions underlying that social order. In such a context, biochemical reality was a key value to be protected rather than an object of investigation and questioning. Therefore its defense by its members transcended any logic, historical evidence, or social reality. In essence, it was bound only to the logic of survival. Biochemistry had to be defended, no matter what. This explains why Davidson could engage in such contradictory rhetoric without even noticing its formal flaws. For members of a given social system, the key values of their system are not equivalent to those of other systems. Molecular biology could be investigated by, questioned by, and denied as a threat to biochemists who at the same time accepted biochemical reality as if it were universal and immutable, not yet another, older social construct of scientific reality.

When Davidson's rhetoric reached the level of social reality, however, he admitted that the difference between molecular biology and biochemistry was real, but nevertheless irrelevant. He acknowledged that not all biochemists were molecular biologists and not all molecular biologists were biochemists. Yet he was willing to deny this obvious social distinctiveness, simply because some biochemists, such as himself, were increasingly perceived as molecular biologists, because of the then-changing meaning of their subject matter. Because the subject matter of molecular biology and biochemistry increasingly overlapped (especially after the mid 1960s, when the biochemical approach to cracking the genetic code proved superior to other methods), Davidson, as a spokesman for biochemistry, thought that *all* molecular biology should be subsumed under biochemistry.

Even while introducing Kendrew as a Nobel Prize winner and chief spokesman for molecular biology, one who had insisted that he was not a biochemist, Davidson stressed that Kendrew had been elected to membership in the Biochemical Society, subsuming him as well. Once again Davidson both capitalized on the social meanings of ritualistic contexts and deployed social reality as respectable evidence when it suited the cause of biochemistry. Yet he denied the relevance of social reality when the argument suited the cause of molecular biology, as in his previous sentence, when he acknowledged that a social distinction between molecular biologists and biochemists could be construed as an argument in favor of their conceptual distinctiveness.

Kendrew's own talk, "Some Remarks on the History of Molecular Biology," was, like Davidson's, a selective deployment of history and social reality. The rhetoric was less fierce, but then Kendrew's interests were less under fire than Davidson's. Kendrew spoke from the vantage point of multiple glory: as the first scientist to solve the structure of a protein, a main goal of molecular biology since

the 1930s, especially in the British context; as a Cambridge graduate who stayed there for his entire career; and as a Nobelist. In the mid 1960s he had turned to championing the cause of molecular biology outside science, as a BBC commentator, as an author, and especially as a statesman of science.²⁴

If Davidson drew attention to the overlap between molecular biology and biochemistry in the late 1960s, but ignored their different pasts, Kendrew did just the opposite. He ignored the conclusion of his own Working Group's report, that distinguishing molecular biology from biochemistry was, by the late 1960s, quite difficult. He instead exalted their past distinctiveness, stressing rather than minimizing the historical inability of biochemistry to handle complex biological molecules, an inability due to biochemists' accepting the hegemony of the chemical method and denying the reality of macromolecules because that method could not handle them technically. He further stressed the conceptual and methodological distinctiveness of molecular biology by reminding the audience that though at the present time knowledge of macromolecules and of small molecules was complementary, knowledge about the three-dimensional structure of macromolecules, which alone explained biological function, was not and could not be deduced from knowledge of small molecules—the empirical province of biochemistry. Kendrew claimed that for historical, methodological, and conceptual reasons molecular biology could not be deduced from biochemistry and was therefore autonomous.

Even better evidence for distinguishing the two fields was the existence of a school of molecular biology in the United States which not only focused almost entirely on phage genetics but thought of itself as the very antithesis of biochemistry. Kendrew had negotiated a definition of molecular biology with these other contender-insiders only two years earlier, but he now used the product of those negotiations to demarcate molecular biology (despite its internal disputes) from biochemistry, further establishing himself as a spokesman for molecular biology as a whole rather than for one of its schools.²⁵ To each school of molecular biology—the structural school centered in Britain and the informational school developed in the United States—Kendrew assigned proper ancestors as a mark of historical authenticity. These ancestors had nothing to do with biochemistry because in Kendrew's view they were all physicists, one of whom was explicitly hostile to biochemistry, the others of whom simply ignored it.

The historical picture of molecular biology Kendrew was using was the product of the rhetorical needs and power structure of contestant schools, in the recent past (the mid and late 1960s) rather than of interest in the past in its own right. Earlier Kendrew had contested the self-propagated history of the informational school as history of molecular biology, yet he had come to accept the American school as a legitimate contender with his own because it had made an impressive show of power in the present, even if, as he observed, its link to things "molecu-

²⁴ See J. C. Kendrew, "Some Remarks on the History of Molecular Biology," *Biochem. Soc. Sym.*, 1970, 30:5–10; and, e.g., Kendrew, "European Molecular Biology Organization," *Nature*, 1968, 218:840–842. Kendrew became the first director of the European Molecular Biology Laboratory (EMBL) in 1975, serving until 1982.

²⁵ See Kendrew, "Information and Conformation in Biology" (cit. n. 19). For more on the negotiation between the two schools of molecular biology see P.G. Abir-Am, "Themes, Genres, and Orders of Legitimation in the Historiography of Molecular Biology," *Hist. Sci.*, 1985, 23:73–117.

lar” was somewhat belated. Now, in the context of his debate with the biochemists, the history of the informational school with its self-professed distrust of biochemistry became an asset. It countered the biochemists’ claim that molecular biology was a “natural” development of biochemistry, and hence contested their right to incorporate molecular biology under their authority in the name of their historical priority.

Ritual celebrations of biochemistry’s history, especially of its historical priority in dealing with “biology at the molecular level,” like those carried out in London by the Biochemical Society in 1969, are not uncommon. In 1978 the New York Academy of Sciences conducted a three-day meeting and published a volume of the proceedings that used similar rhetorical strategies—marginalizing certain key topics in the rise of molecular biology, such as nucleic acids and viruses, in the context of a dominating “classical” biochemical agenda. Anniversaries of leading biochemists can also provide opportunities for reaffirming biochemistry’s historical priority and molecular biology’s dependence on key developments in the earlier field. One example is *Reflections on Biochemistry*, a volume of autobiographical reminiscences read at meetings in honor of Severo Ochoa’s seventieth birthday in 1974. Its authors constitute almost a “who’s who” in biochemistry, especially in the United States. Their description of Ochoa as “the biochemist of the biochemists and the ideal of what the true scientist should be” highlights the biochemical heritage and identity of a key decoder of the genetic code, a project of utmost importance for the conceptual repertory of molecular biology, yet one which turned out to depend critically on biochemical tools, such as the enzyme polynucleotide phosphorylase.²⁶

Anniversaries of journals have also been deployed as occasions for reaffirming—both explicitly, as in reflexive retrospective accounts, and implicitly, as in the “mere” collective display of numerous contributions—the priority of biochemistry over a range of topics appropriated by molecular biology. On the occasion of its thousandth issue in 1989, *Biochimica and Biophysica Acta* published a series of reflections on key papers the journal published between 1947 and 1964 on nucleic acids and proteins. The authors, including Christian B. Anfinsen, Jean Brachet, Mildred Cohn, M. Grunberg-Manago, Mahlon Hoagland, Robert W. Holley, Vernon M. Ingram, Alexander Rich, and Charles Yanofsky, all document the priority of the biochemical perspective and provide explicit and implicit insights into biochemistry’s loss of hegemony to molecular biology in the 1960s.²⁷

A similar but historiographically more explicit effort is the series of essays in *Trends in Biochemical Sciences* (1984), by Seymour Cohen, Lloyd Kozloff, Max Lauffer, and Paul Zamecnick. These four pioneered biochemical approaches to virus multiplication and protein synthesis, two experimental systems that acquired utmost importance in the rise of molecular biology.²⁸ The series editor,

²⁶ See P. M. Srinivasan, J. S. Fruton and J. T. Edsall, eds., *The Origins of Biochemistry: A Retrospect on Proteins* (New York: New York Academy of Sciences, 1979). See also Abir-Am, “Modern Biochemistry,” *British Journal for the History of Science*, 1982, 12:301–305; Kornberg et al., eds., *Reflections on Biochemistry* (cit. n. 7); and Kornberg, *For the Love of Enzymes* (cit. n. 14).

²⁷ See esp. J. Brachet, “Recollections on the Origins of Molecular Biology,” *Biochimica and Biophysica Acta*, 1989, 1000:1–5.

²⁸ See esp. Seymour S. Cohen, “The Biochemical Origins of Molecular Biology: Introduction,” *Trends in Biochemical Sciences*, 1984, 9:334–336; see also the papers by Max Lauffer, *ibid.*, pp. 369–371; Lloyd M. Kozloff, *ibid.*, pp. 422–423, and Paul Zamecnick, pp. 464–466, all with introductions

Seymour Cohen, a discoverer of virus-induced enzymes and a pioneer of phage biochemistry, has been a major contributor since the late 1960s to a historiographic literature that seeks to recover biochemistry's key role in the rise of molecular biology.

Recent autobiographical accounts by leading American biochemists whose work was essential for the agenda of molecular biology, most notably Arthur Kornberg's *For the Love of Enzymes, The Odyssey of a Biochemist* (1989) and Mahlon Hoagland's *Toward the Habit of Truth* (1990), are yet another major source of insights into biochemistry's key role. Both authors document from personal experience the undisputable priority and centrality of the biochemists' contributions, especially to topics such as DNA, RNA, and protein synthesis. As Kornberg put it: "DNA and RNA write the script but the enzymes do the acting."²⁹

Yet even these two perceptive authors fail to explain how despite indisputable priority and indispensable contributions, biochemists hold no position in the public perception of the biomolecular revolution. That revolution is identified with the rise of molecular biology as a new discipline in the 1960s, rather than ascribed to the continuous flourishing of the more veteran discipline of biochemistry. Perhaps the best clues for understanding the enigmatic transfer of hegemony from biochemistry to molecular biology come from the early writings of Erwin Chargaff (1905–).

IV. "MOLECULAR BIOLOGY IS THE PRACTICE OF BIOCHEMISTRY WITHOUT A LICENSE": CRITIQUE OF SCIENCE IN THE UNITED STATES

An émigré from Central Europe and a member of the Department of Biochemistry at Columbia University since 1935, Chargaff emerged after the late 1950s as the most eloquent critic of the rise of molecular biology as "big" science—intellectually presumptive and appropriating, morally slick, philosophically speculative and artificial, socially entrepreneurial. He summarized the relationship between biochemistry and molecular biology in the famous aphorism that heads this section, capturing biochemists' impression of the transition of the scientific order in "biology at the molecular level" in the 1960s from the hegemony of their own discipline to that of the molecular biologists—opportunistic and more responsive to social trends.

Chargaff's "Amphisbaena," the stylized dialogue between an "Old Chemist" and a "Young Molecular Biologist," was written in 1961, at the onset of the biomolecular revolution. Rather than his many other later writings, including a scientific autobiography (1978), this work captured most vividly and perceptively the scientific, philosophical, social, and cultural contrast between the "mentalités" of the biochemists and the molecular biologists.³⁰ In part this is be-

by Cohen. See also Robert Olby, "Biochemical Origins of Molecular Biology: A Discussion," *ibid.*, 1986, 11:303–305.

²⁹ Kornberg, *For the Love of Enzymes* (cit. n. 14), p. 298.

³⁰ Erwin Chargaff, "Amphisbaena," *Essays on Nucleic Acids* (Amsterdam: Elsevier, 1963), pp. 174–199. "Updated" versions of the famous dialogue appeared as Chargaff, "Voices in the Labyrinth: Dialogues around the Study of Nature," *Perspect. Biol. Med.*, 1975, 18:251–285; and Chargaff, "A

cause the dialogue builds on actual conversations between his various colleagues at the time, so that Chargaff's literary agency, vested in an aphoristic style, social sarcasm, and cultural erudition, becomes a vehicle for historical authenticity as well. The relative ages of the two parties to the dialogue capture the social reality of the biomolecular revolution as a struggle between two different scientific generations.³¹

While the protagonists of molecular biology entered science immediately after World War II and soon became the beneficiaries of new initiatives within new and old governmental agencies (e.g., NSF and NIH in the United States, MRC in Great Britain, CNRS in France) and sharp increases in governmental funding after the launching of the first Sputnik in 1957, its biochemist competitors were born, like Chargaff, around the turn of the century. Like him they endured numerous social and political upheavals in the interwar period, when their careers were hindered by the Great Depression of the early 1930s and the rise of fascism in Central Europe. These biochemist refugees, survivors of major political events that put an end to the interwar golden era of European biochemistry, of which they saw themselves as an integral part, were understandably less inclined to seek new revolutions—scientific or social—in the period after World War II, although that period was dominated by the exuberant extraconfidence of atomic scientists turned policy advisers.³²

Although migration to the United States saved the lives of many of Chargaff's biochemist contemporaries, it also put them at a professional disadvantage in a brave new world whose different culture, language, and scientific institutions were not only less impressed by scientific mandarins but soon turned against them. The scientific opportunities and professional welfare of politically active, especially senior, scientists in the United States—both homemade, such as Robert Oppenheimer and Linus Pauling, and imported, such as Salvador Luria—were adversely affected during the McCarthy era. Scientists whose formative period coincided with these difficult experiences of intertwining politics and science, whether in Europe in the 1930s or in the United States in the late 1940s and early 1950s—aside from any “natural” mellowing as a result of sheer age—would have had little inclination to join the quasi-revolutionary bandwagon of the 1960s. Even when they felt strongly about such phenomena as the appropriation of some aspects of their own discipline, often in the name of questionable standards of novelty, they could always console themselves with being part of the wider science-policy bonanza of the post-Sputnik era. In that context even the socio-cultural revolution in biomolecular science ultimately proved to be rather harm-

Dialogue and a Monologue on the Manufacture of Souls,” *ibid.*, 1987, 31:81–93. Chargaff's autobiography is *Heraclitean Fire: Sketches from a Life before Nature* (New York: Rockefeller Univ. Press, 1978). For more on Chargaff's views see Abir-Am, “From Biochemistry to Molecular Biology” (cit. n. 7).

³¹ On scientific change as a generational phenomenon see Lewis Feuer, *Einstein and the Scientific Generations* (Toronto: Univ. Toronto Press, 1974).

³² On Chargaff's acculturation see Abir-Am, “From Biochemistry to Molecular Biology” (cit. n. 7). On Central European émigré scientists, with special emphasis on biochemists, see David Nachmansohn, *German-Jewish Scientists in Physics, Chemistry and Biochemistry, 1900–1933* (New York/Berlin: Springer Verlag, 1979); on the role of atomic physicists turned science policy advisers see William Lanoette, *In the Shadow of Genius: A Biography of Leo Szilard* (New York: Scribners, 1992).

less, in the sense that the discomfort endured by that part of the older generation which refused cooptation as a scientific option was primarily semantic rather than material.

Chargaff's strategy, later followed by other biochemists, was to regain DNA as a research tradition by recalling its "prehistory," and highlighting the philosophical limitations of molecular biology.³³ The dialogue "Amphisbaena" starts with the Old Chemist's assertion that the cell is not a machine and that, contrary to the strictly "mechano-morphic" view of nature portrayed by molecular biology, no understanding of biology was possible before the interplay of determinism and chance could be sorted out. When the Young Molecular Biologist retorts that the machine is a cell, the Old Chemist replies that his answer is typical of "modern biophysics," ever since model-building launched the double helix and triggered an epistemological *qua* ontological inversion by giving precedence to models over experiments. This shift is contrasted with the naive realist view of the Old Chemist (Chargaff's own self-mocking portrait), according to which the understanding of life depends upon direct communion with nature via experimentation rather than on artificial models. Those, according to Chargaff, may enable one to out-guess nature but also constrain one to deal in "artifacts."

Rather than inquiring how a market for such artifacts has come into existence at this time, Chargaff further grounds the supposed arbitrariness of molecular biology in its strategy of combining and redefining resources from several preexisting disciplinary traditions, a strategy he takes to imply that molecular biology has no obvious counterpart in the natural order. Or as he puts it: "If physics is the science of states, chemistry is the science of the conversions of matter, and biology comprises the application of their laws to animate nature, what could be meant by molecular biology?" By defining biology as comprising biochemistry and biophysics only, Chargaff is able to portray molecular biology as a chimera that disturbed a scientific order presumably grounded in an immutable natural order.

The discrepancy in the mentalities of these two scientific generations is further evidenced by the Young Molecular Biologist's admission that though he cannot define the meaning of molecular biology, he does not care about such a philosophical shortcoming, since he can easily substantiate its social reality by pointing to the existence of many individuals who call themselves molecular biologists. He further suggests that work published by those individuals, especially in the *Journal of Molecular Biology* (established in 1959) can be considered as a definition of molecular biology.

Such social definitions, as opposed to the philosophical definitions Chargaff seeks, were advanced about that time by Francis Crick, a leading molecular biologist who emphasized theoretical inferences and contrasted his approach to biochemistry's traditional disdain of theory and emphasis on experimental detail. It was Crick who candidly revealed that the term "molecular biology" originated first and foremost to designate a social, rather than a conceptual reality: to describe people like himself who had no background or loyalties in traditional disciplines but who were venturing into new transdisciplinary domains at the in-

³³ For details see Abir-Am, "From Biochemistry to Molecular Biology" (cit. n. 7).

terface of crystallography, stereochemistry, genetics, and microbiology and needed a catchy, short description for their new adventures.³⁴

It is in response to the Young Molecular Biologist's self-professed difficulty in providing a definition of molecular biology that Chargaff's Old Chemist offers his soon-to-become-famous aphorism: "Molecular biology is the practice of biochemistry without a license." This definition conflates science as a profession with science as a vocation, while further suggesting that professional aspects should take precedence, so as to ensure law and order. Chargaff's obsession with law and order, or rather with the devastating consequences of their loss in Central Europe before the war, led him to overlook the possibility that during rapid social change professional standards are inevitably redefined to accommodate the fast arrival of many newcomers. Further, in those circumstances more relaxed forms of social control are not only more conducive to innovation but necessary, since strict licensing by standards of reference that may have become obsolete can no longer be enforced.

Indeed, how could molecular biology emerge without the simultaneous collapse of the strict licensing system of traditional disciplines, a system that kept many of them in splendid isolation, further insulated by self-erected philosophical fortresses of pride and prejudice? If the old order prevailed, molecular biologists would have needed multiple licenses, in crystallography, stereochemistry, microbiology, genetics, and biochemistry, among other fields. So impractical a procedure of professional certification would have hampered a rapidly developing multidisciplinary field, especially at a time when science-policy initiatives were encouraging a fast increase in the number of researchers, and hence tougher competition for visibility, greater anonymity, and fast results grounded in automated instrumentation. This ensemble of sociotechnological opportunities encouraged a new scientific style focused on entrepreneurship and short cuts.

Indeed, the Young Molecular Biologist agrees with the Old Chemist that putting a new label on old bottles helps with "symposia, congresses, new journals, more money to be had more easily," and admits that there is an opportunistic rationale for using a fuzzy term such as "molecular biology." The Old Chemist then pillories this attitude by noting that molecular biology allows one to "be a pioneer at no extra cost," an ability in great demand given the many "DNA tycoons" and those who "made a killing in RNA," all of whom are engaged in ambitious promotion campaigns.

Chargaff's dialogue, a critique of the changing subculture in the biomolecular order, depicts the entrepreneurial opportunities open to those temperamentally ready to seize them when easy funding combines with topics and methods that require little, if any, prior licensing or apprenticeship. Chargaff, still believing in the eternal prevalence of prewar standards of excellence of the sort he observed in the last years of the Weimar republic at the Kaiser Wilhelm Gesellschaft Institutes of Biochemistry in Berlin, regards this entrepreneurial, largely American, age of science as a degradation—a transformation of science from "an adventure of the highest" into "the survival of the slickest and the quickest." But he also points to the particular context of the counterculture in the 1960s, with its revulsion from

³⁴ See Francis Crick, "Recent Research in Molecular Biology: Introduction," *British Medical Bulletin*, 1965, 21:183–186; and Crick, "The Double Helix: A Personal View," *Nature*, 1974, 248:766–769.

traditional authority, as an era in which novelty is valued for its own sake, and “rootless hybrids” such as molecular biology can not only survive but replace a previous order.

Chargaff's philosophical critique of molecular biology as “merely” capable of explaining, not of understanding, life and his elevation of understanding, a traditional goal of human sciences, to a higher epistemological status than explanation, the natural sciences' traditional goal, reflects his own gradual move from science to a literary and cultural critique of science. But it also reflects the slowness of his generation in seeing that the loss of hegemony of the logical empiricist philosophy of science in the 1960s also resonated with the “model before experiment” approach propagated by molecular biologists. This decline of logical positivism in favor of conventionalist approaches best summarized by Thomas S. Kuhn and Joseph Agassi—approaches championed by such leading and theoretically inclined molecular biologists as Jacques Monod—created a scientific climate in which a new form of licensing or an emphasis on modeling could be more easily legitimized.³⁵ Thus molecular biology resonated not only with science policy and trends in the counterculture but also with the latest philosophical gloss on science itself.

The importance of the cultural, philosophical, social, and political resonance of the biomolecular revolution with the world at large was finally evident in Chargaff's heaviest charge against molecular biology, namely its supposed destruction of the concept of the molecule. Like many other biochemists of a previous generation, Chargaff had difficulty grasping the significance of the discontinuity between small and large molecules, as not just a matter of size but also a matter of storing, transferring, and expressing biological information. In a social world soon to be dominated by the computer, a discipline like molecular biology, which allied itself with an overall societal concern with information transfer and management and further extended it to the most basic level of life properties, had a distinct advantage in recruiting both followers and funds over disciplines that may have had a glorious past but chose to deride pervasive social trends as “sloganification.”

Indeed, Chargaff's difficulty with a new world of molecular cybernetics is evident when he asks the Young Molecular Biologist if molecular regulation meant that the cell was a dictatorship of DNA, an echo of cell biologists' objections to the totalitarian resonance of models of molecular-genetic regulation which portrayed DNA as a “master molecule.”³⁶ This brief encounter with cellular politics was sufficient to send Chargaff back into his once-safe biochemical world, but even there he found new artifacts such as “unscrewase” (a mocking term for en-

³⁵ See Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: Univ. Chicago Press, 1962); Joseph Agassi, *Towards a Historiography of Science* (Middletown, Conn.: Wesleyan Univ. Press, 1963); and Jacques L. Monod, “De la biologie moléculaire à l'éthique de la connaissance” (1967), in Monod, *Pour une éthique de la connaissance* (Paris: Découverte, 1988), pp. 151–169 (texts selected by Bernardino Fantini).

³⁶ For discussions of the political resonance of cytogenetical theories see David L. Nanney, “The Role of Cytoplasm in Heredity” in *The Chemical Basis of Heredity*, ed. W. D. McElroy and H. B. Glass (Baltimore: Johns Hopkins Univ., 1957), pp. 146–164. See also Evelyn Fox Keller, *A Feeling for the Organism: A Biography of Barbara McClintock* (San Francisco: Freeman, 1983); Jan Sapp, *Beyond the Gene* (New York: Oxford Univ. Press, 1987); and Donna J. Haraway, *Primate Visions: Race, Gender and the Culture of Modern Science* (London: Routledge, 1989).

zymes that separate the strands of DNA before duplication, coined when their existence was only postulated) or messages that no one had “seen” (short-lived messenger-RNA molecules whose existence was inferred from experiments with radioactive pulses).

Confronted by alienation from nature (“I thought it was the task of the natural sciences to discover the facts of nature, not to create them”), Chargaff already saw in 1963 that molecular biology, following the earlier example of physics, had shifted its frame of reference from the humanities to technology. But even a scientific Jeremiah like Chargaff could not foresee the more “applied” commercial feats of recombinant DNA in the 1970s, let alone those of the Human Genome Initiative in the late 1980s. In both areas the boundaries of the biomolecular revolution are being rapidly pushed into both nature (“new forms of life” has become a goal if not quite an actual accomplishment) and culture (the play “Better People: A Surrealistic Comedy on Genetic Engineering” was inspired by the career of Wally Gilbert, a Nobelist molecular biologist from Harvard who had been in the forefront of both the movement of the commercialization of biotechnology and the Human Genome Initiative).³⁷

In view of massive changes still to come as the century comes to an end, the struggles between biochemists and molecular biologists in the 1960s seems as outmoded as the cross-national perspective on science seems in the newly emerging global economic order that, it is believed, will prevail in the twenty-first century. The implications of the biomolecular revolution appear everywhere, while its ethical consequences are raised every time a new biomolecular sequence or message is analyzed into its smaller building blocks.

The politics of small versus large molecules turned out to be more than a debate over molecular size between conservative and progressive types of scientific mentalities. In the dispute between the empiricist, reductionist, and disciplinary ethos of biochemistry and the conventionalist, integrative, and transdisciplinary ethos of molecular biology, how authority in a scientific revolution was legitimated, whether as derived from nature or socially constructed, was also at stake. But as Chargaff learned the hard way at the onset of the postmodernist era, when the text has replaced the author as the origins of intentionality, a molecule can no longer be a mere molecule but becomes a “message,” indeed a message with distinct biological, social and political consequences.

³⁷ Karen Malpede, *Better People* (a surreal comedy on genetic engineering), performed on 15 June 1990, Cambridge, Mass., at the American Repertory Theatre.