Designs for Life

Molecular Biology after World War II

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1

Introduction

Hardly a common term in the 1950s, molecular biology is now expected to take the dominant role in the twenty-first century that physics played in the twentieth. Our understanding of life, health and disease is as much dependent on knowledge produced by molecular biologists as the fabrication of food and drugs, trials in court, and new ways of waging wars. How, we need to ask, has molecular biology acquired such a dominant position in our society?

To approach this question the book focuses on the Laboratory of Molecular Biology in Cambridge (formerly the Medical Research Council Unit for the Study of Molecular Structure of Biological Systems) which, in the 1950s and 1960s, became an international symbol of the spectacular development of molecular biology. This was the laboratory in which, in 1953, Watson and Crick presented their double helical model of DNA. However, as I will show, this event alone, which in the 1950s attracted far less attention than it does today, cannot explain the explosive growth of the laboratory or the creation of the new science. Rather, the book takes a longer-term view, engaging with events from the immediate postwar years to the late 1970s. The history of the laboratory starts in the mid-1940s, when opportunities created by the postwar reconstruction of the sciences were used to establish new ways of producing knowledge about biological structures and processes in the laboratory. The late 1950s and 1960s saw an extraordinary expansion of activity, the formation of new networks and the use of the science policy arena to promote the new science (only now presented as molecular biology). These events set the stage for later government policies and industrial investments which in turn opened up new opportunities and expectations. Molecular biology, I will argue, was produced as much in the laboratory as in the political and the public arena. Only an in-depth study, as the one presented here, can reconstruct these processes in necessary detail.

A local study

Set up by the Medical Research Council (MRC) in 1947 as a two-man unit dedicated to the crystallographic study of proteins, the laboratory was quickly made an 'obligatory passage point' for the new science of molecular biology. The Queen had only just inaugurated the new four-storey laboratory in 1962 when James Watson, Francis Crick, John Kendrew and Max Perutz were awarded Nobel Prizes for their work in the unit on the structure of DNA and proteins. According to one witness, 'this public ratification of the eminence of the MRC Laboratory was the most important factor in the general recognition of molecular biology as a distinctive scientific discipline' (Fruton 1992, 210–11).[1] The new fame of the MRC Laboratory of Molecular Biology (LMB), together with the opportunity of numerous fellowships to travel to Western Europe, soon attracted a large number of American postdoctoral students to Cambridge. In the years of the expansion of the American universities, good career prospects awaited these researchers on their return. In this way the 'culture' of the LMB was exported to other centres. In Britain itself the LMB so dominated the field that, by the mid-1970s, the 'failure' to 'seed' the subject in universities started to be perceived as a problem.[2]

The pivotal role of Cambridge in the development of molecular biology allows the reconstruction of events and practices that have come to be seen as central to the history of the field and of the mechanisms by which they became disciplinary landmarks.[3] I analyse in particular Perutz and Kendrew's pioneering X-ray analysis of protein structure, including Kendrew's early use of the experimental electronic digital computer at Cambridge; Watson and Crick's work on the structure of DNA and the central role attributed to it as the 'origin' of the new field; early attempts at 'cracking' the genetic code; the crucial role of Fred Sanger's sequencing work for the particular research culture developed at Cambridge which combined structural and genetic approaches; Sydney Brenner's

[1] Joseph Fruton, who himself never accepted that molecular biology was anything else than biochemistry, also suggested that the appearance in 1966 of the *Festschrift* for Max Delbrück, *Phage and the Origins of Molecular Biology,* was influenced by the public esteem gained by the MRC Laboratory (Fruton 1992, 211; Cairns, Stent and Watson 1966). This volume marked the beginning of a whole series of books and articles debating the 'origins' of molecular biology.

[2] 'Cell Board Subcommittee set up to review molecular biology. Unconfirmed minutes of first meeting, 21 July 1975', file A147/14, vol. 1, MRC Archives.

[3] Molecular biology is here taken to mean more than just 'molecular genetics', as indeed was always the case in Cambridge. On the history of the term and its usage in Cambridge see below, especially chapter 7. On the effort to recover research traditions which do not fall under the narrow definition of molecular genetics to a larger 'history of molecular biology project', see Zallen (1992) and Burian (1996).

creation of *Caenorhabditis elegans* as a new model organism for the study of development; and César Milstein and Georges Köhler's invention of monoclonal antibodies. While monoclonals gave rise to a fledgling biotechnology industry, in the late 1980s the plan to sequence the whole DNA of the worm became a pilot for the Human Genome Project and the flagship project of the newly created Sanger Centre, one of the largest sequencing centres in Europe.

However, my choice of a local study is based not so much on the widely recognised excellence of the Cambridge laboratory as on the thesis that widely distributed experimental practices and scientific institutions embody local expertise and negotiations. It is only by studying in detail these local solutions, the resistances they met, and the eventual 'export' of local practices to other laboratories, that one may understand the construction of a new scientific field. Proceeding locally, therefore, need not mean being provincial. The detailed investigation of the Laboratory of Molecular Biology thus offers the possibility of studying the boundaries and connections of local, national and international developments. These structures and mediations get lost in more wide-sweeping accounts. By the same token, the study does not take for granted the excellence of the laboratory or of Cambridge science more generally, but analyses how this one laboratory came to play such a central role in the international establishment of molecular biology – at times despite or even because of local resistances.[4] Cambridge, and especially the Cavendish Laboratory where the MRC unit was first housed, boasted a long and glamorous tradition of research in the natural sciences. However, unlike Oxford, Cambridge voted to contain expansion after World War II. This choice, in addition to the fact that all decisions in the university are made by mixed bodies in which all faculties, as well as the colleges, are represented, made it difficult for new projects to find approval, especially if these were 'no one person's business'. Because of these circumstances, molecular biology at Cambridge developed mainly outside the precincts of the university.[5]

In my analysis of the mechanisms by which the laboratory came to assume such a privileged position in the establishment of the new science, I draw on current approaches in science studies. In particular, I aim to combine a fine-grained analysis of work at the laboratory bench with an analysis of the representational, institutional and political strategies

[4] I would like to distinguish this undertaking from the attempt to define the institutional conditions for 'successful science', most often measured in Nobel Prizes. In my understanding, 'success' is socially (and always retrospectively) attributed and historically contingent.

[5] Especially on the Cavendish see Crowther (1974); on the history of Cambridge University more generally see Brooke (1993) and Leedham-Green (1996).

employed to establish the field at the local as well as the national and international level, in competition with other fields and in the face of multiple resistances.[6] Molecular biology, I suggest, was constructed as much at the bench and through the circulation of tools, models and postdoctoral researchers as in institutional negotiations or political committees, in the television studio and in participants' disputes on the 'origins' of the field.

The postwar era

The postwar era in Britain has been depicted in various ways.[7] In political terms it was marked by the loss of empire and the resuming of the 'special relationship' with America, by the onset and hardening of the Cold War and the division of Europe by the Iron Curtain. Economically it was a time of recovery and growth and of low unemployment. Social reforms immediately after the war had introduced a National Health Service, a new system of social security and free secondary education. In addition, economic growth meant material affluence for all classes. However, class divisions remained strong and gender relations remained basically unaltered. The 1960s were marked by rebellion, mainly by the young generation, against these continuing divisions and established political and cultural values. A recent exhibition at the Imperial War Museum has presented a portrait of this era under the motto 'from the bomb to the Beatles', while others have described the two-and-a-half decades following World War II as 'defiant modernism' and as characterised by big technological projects.[8]

The postwar era, as it appears in this book, represents a time of rising science budgets and high public esteem for science.[9] Both were a direct outcome of what was generally perceived as the crucial contribution of scientists to winning the war. In Britain the general opinion was that radar had saved the country from occupation by Hitler's troops. In addition,

[6] For the focus on experimental practices see Galison (1987; 1997), Gooding, Pinch and Schaffer (1989) and Pickering (1992; 1995); more specifically for the life sciences, see Latour and Woolgar (1986), Clarke and Fujimura (1992) and Kohler (1994). On the 'place' of knowledge and the export of local practices see Shapin and Schaffer (1985), Latour (1987; 1988) and Ophir and Shapin (1991). On representations see Lynch and Woolgar (1990), Rheinberger (1997) and de Chadarevian and Hopwood (forthcoming). On instituting science see Lenoir (1997). On constructivist approaches in the history of science see Golinski (1998) and the review by Kohler (1999).

[7] On Britain after World War II see Marwick (1982), Morgan (1990), Holland (1991), Hennessy (1992) and Clarke (1996). See also Milward (1984) and Ellwood (1992) on the political and economic reconstruction in Europe.

[8] On the exhibition at the Imperial War Museum see Gardiner (1999). 'Defiant modernism' was the title of a conference held at the Science Museum in London, 25–26 June 1999. See also Bud *et al.* 2000, 158–83.

[9] Science policy became a central political issue only in the late 1950s; see Vig (1968). On science and scientists in Britain after World War II see Edgerton (1996a), Gummett (1980) and Wilkie (1991).

penicillin (a British discovery) and the atomic bomb (developed with the decisive help of British scientists and engineers) had saved thousands of lives, the first by controlling infections in wounded soldiers, the second by ending the war.[10] British scientists had fought for an active role in the war effort. After the war they publicised their contributions and argued for an equally important role of science in postwar reconstruction. 'The problems of reconstruction', Archibald V. Hill, Nobel Prize winning physiologist and high-level scientific administrator and military adviser, noted in his diary, 'will be to an important extent scientific ones.'[11] Politicians were responsive to these views and approved the disbursement of large government funds for research and development. While always small when compared to the military R&D budget, in the early and mid-1960s the annual budget for civil science was growing at an average rate of 13.5 per cent in real terms. Studying the spending for military R&D, Edgerton has suggested that postwar Britain has been as much a 'warfare state' with a solid industrial base as a 'welfare state' (Edgerton 1992, 141).

In Britain, as in France, technological prowess, symbolised above all in an independent atomic bomb project, made up for the loss of empire. The American reconstruction plans for Europe included important measures for the support of science and technology as pillars for security and economic welfare.[12] In the rising tensions of the Cold War, the United States built their own supremacy and that of the West more generally on scientific and technological dominance.

The postwar lustre of science began to fade with the questioning of the role of science and technology in the Vietnam War, loudly voiced on American campuses and throughout Europe in the wake of the student revolts. In Britain, however, civil science budgets continued to rise (if at a lower rate) until the mid-1970s when the oil crisis, general recession and the following devaluation of the pound imposed cuts on government expenditure for science. My study covers this 'long' postwar period.

The making of a new science

The rapid growth of molecular biology after World War II is often assumed to have occurred almost exclusively in the three countries that dominated

[10] On the role of penicillin and the myths surrounding it in the reconstruction of Britain and of her self-image see Bud (1998).

[11] A. V. Hill, 'Memoirs and reflections' [unpublished manuscript, p. 568], Hill Papers, AVHL I, 5/4, Churchill Archives Centre, Cambridge.

[12] On Britain see Gowing (1974a; 1974b) and Agar (1998c); on France see Hecht (1998). On the decisive importance of the so-called Berkner Report on Science and Foreign Relations of April 1950 for the formulation of the American policy towards European science see Needell (1996).

the winning coalition: Britain, France and the United States. It has been suggested that an important reason lay in their relative economic strength (Allen 1978, 188). However, economic strength alone does not seem to explain why molecular studies of life processes were privileged over others. To account for the meteoric rise of molecular biology after World War II, the historian Edward Yoxen has suggested that molecular biologists were part of the new scientific establishment which after the war directed the new flow of money towards specific research projects. He has also argued that a biology which conceived of life in terms of a programme fitted the managerial research system which took hold after World War II (Yoxen 1981; 1982). This last thesis, however, fits only a very narrow research agenda, one which to some extent became dominant in the 1970s with the new recombinant DNA technologies and their commercial applications. Focusing on earlier developments, I will argue that molecular biology in Britain (as a distinct scientific enterprise under this name) took form only in the late 1950s – the Cambridge laboratory being the first institution which officially carried that name. In the 1940s and 1950s, much of the research later claimed by molecular biologists (including Watson and Crick's work on the structure of DNA) fell under the heading of biophysics, a larger and more diverse field which attracted considerable support after the war.[13]

By drawing attention to the fortunes and legacies of postwar biophysics I do not intend to create a new 'origin' account or to add a new candidate to the number of disciplines which allegedly contributed to the emergence of molecular biology. My intention is rather to avoid starting with a cognitive (or any other) definition of the field (as most histories of molecular biology do) and to study disciplines as political and cultural institutions.[14]

The opportunity for biophysics after World War II stemmed from a host of new physical approaches developed for the war effort. The hope of

[13] For a similar thesis regarding the development of molecular biology in America see Rasmussen (1997a). On the making of molecular biology in France see Gaudillière (1991; in press). For efforts to build up molecular biology in other European countries, including Belgium, Germany, Switzerland, Spain and Italy, see Deichmann (1996, chapter 7), Burian and Thieffry (1997), Santesmases and Muñoz (1997a; 1997b), Strasser (in press) and de Chadarevian and Strasser (forthcoming). On Japan see Uchida (1993).

[14] Robert Kohler in his history of biochemistry also presented disciplines as political institutions (Kohler 1982). While building on this notion, the present study aims to discuss disciplines not just in terms of intellectual programmes and academic politics, but by considering the institution of experimental practices. In his later work Kohler himself moved to consider the material culture and moral economy of experimental practices, but set this approach apart from the study of disciplines and their institutions (Kohler 1994, especially p. 14). On the need to combine the study of disciplines with the study of experimental practices see Lenoir (1997, especially introduction and chapter 3).

turning these technologies, especially those of nuclear physics which had led to the celebrated yet deadly weapon, to peaceful ends gave biophysics its cultural and political appeal. A 'physics of life', with the promise of biomedical applications, fitted neatly into the political discourse of postwar reconstruction. Biologists, physicists and medical researchers alike seized on this opportunity and took advantage of new government funds made available for 'fundamental research'.

Biophysics in postwar Britain comprised at least three different groups: the 'radiation group' which investigated the effects of radiation on the body and ways to protect it as well as biological and medical uses of radioactive isotopes; the 'nerve–muscle group' which exploited new recording devices developed in the context of radar research; and the 'structural group' which used a series of physical techniques and especially X-ray diffraction, decisively aided by the advent of electronic computers, to study complex biological structures. All three groups built on prewar research traditions, but greatly expanded after the war.[15]

The main patron for biophysics in Britain was the Medical Research Council, which had seen its own funds and authority greatly increased as an effect of its role in the wartime mobilisation. The MRC Unit for the Study of the Molecular Structure of Biological Systems fell under this heading, as did, for instance, the Unit for Biophysics at King's College London and the Radiobiological Research Unit at the Atomic Energy Research Establishment at Harwell, all set up in 1947.

In 1957, the MRC unit at Cambridge changed its name to MRC Unit for Molecular Biology. This was a local move which followed a serious institutional crisis for the unit, then still housed in the Physics Department. The only solution was seen in the application for a new and independent laboratory. The plan included new allies and required a new name, then also adopted by the unit. Significantly, around the same time, Kendrew changed the name of the new journal, the editorship of which he had taken on, from *Journal of Molecular Biophysics*, as originally proposed, to *Journal of Molecular Biology*. The journal, edited for many years from Kendrew's college office, is generally credited with having done most to propagate the term. By that time the term 'biophysics', which had served to attract funds after the war, was losing its appeal. Such names, I suggest, are more than mere labels.

In the 1960s, Cambridge molecular biologists skilfully used political channels and connections, many of which dated back to wartime acquaintances, to put the promotion of their new science at the national and the

[15] On the centrality and explosive political implications of research on radiation damage and protection in the atomic age see Beatty (1991) and Lindee (1994).

European level on the governmental agenda. In these negotiations, science policy was as much a tool in the hands of scientists as a governmental tool to manage and regulate research. Changes in government policies in the early 1970s, and later regarding the development of biotechnology in Britain, also affected work practices and the position of the Cambridge laboratory.

Britain's special relationship with the United States, a crucial element of Britain's foreign politics which long dominated over European commitments, not only played a key role in Britain's atomic politics, but in many ways also affected the building of molecular biology. In the immediate postwar years, when there were restrictions on foreign currencies for imports, American grants were crucial to buy scientific apparatuses manufactured abroad.[16] On other occasions, however, the MRC, as a government body, was keen to underline that British science could stand on its own feet and expected its leading scientific staff to attract American researchers to Britain rather than to travel to learn from them. On this ground the MRC, for instance, denied Perutz permission to take up a Rockefeller Foundation Travelling Fellowship to visit American laboratories in 1948.[17] Later, scientists used the growing predominance of American biomedical sciences and the fear of a 'brain drain' from Britain to America to argue for more funds. Similar arguments were brought forward in the negotiations for the European Molecular Biology Laboratory (EMBL), which saw Cambridge molecular biologists centrally involved, despite strong opposition from their own peers. As already mentioned, postdoctoral fellowships for American researchers to spend up to five years in Europe as part of their education also played a crucial role in the economy of the LMB and the export of its research culture. The number of available fellowships increased sharply in the wake of Sputnik, the first space satellite launched by the Soviet Union, and America's politics of stepping up the Cold War mobilisation of science and technology throughout the Western alliance. However, despite, or perhaps in response to, America's hegemony, British molecular

[16] While the Rockefeller Foundation stopped supporting Perutz (as other European grantees on the natural sciences programme) directly after the war, the Cambridge MRC unit continued receiving grants for additional expenses, including fellowships for its members to travel to the United States, until the mid-1960s. Money also came from other American grant-giving bodies. On the economic situation of Britain after World War II and the convertibility problem of the pound see Milward (1984) and Dore (1996).

[17] Perutz later claimed that, had he gone to America, he might have found out from Pauling that the bond which links amino acids in proteins is planar. This information might have saved the Cambridge laboratory the embarrassment of publishing a structure of the polypeptide chain which was not consistent with stereochemical data. The problem was later brilliantly solved by Pauling through model building (Olby 1994, 267–95).

biologists developed and stressed their own research traditions, or what later became known as the 'British (or Structural) School of Molecular Biology' (Kendrew 1967).[18] In some research fields, like protein X-ray crystallography, British scientists reckoned they held the world lead. When the crystal structure diagrams (including some of proteins) were used to launch a novel design for interior decoration at the Festival of Britain in 1951, the strong national tradition in that branch of science was underlined. The emergence of local research traditions and national 'schools', despite the importance of international exchanges and networks in the making of molecular biology (Abir-Am 1992b), further justifies local and national historical studies. Only they can provide the basis for comparative studies and new 'big picture' accounts, as I will argue below.

Histories of molecular biology

The account presented here addresses some key historiographical issues. I will discuss three main points: the place of 'origin' and discovery accounts; the role of World War II; and the relations of local and 'big picture' accounts.

Participants, historians and science writers have given ample attention to the story of Watson and Crick's elucidation of the structure of DNA (e.g. Kendrew 1967; Sayre 1975; Portugal and Cohen 1977; Watson 1980; Judson 1994; Olby 1994; Edelson 1998).[19] Through popular writings and media presentations it has become one of the most widely known events in the history of science. This book is no exception to the trend. Having become such an integral part of the existing history and iconography of the field and with the story being located at Cambridge, in the institution which lies at the centre of this study, the subject imposed itself. In taking it up, my account continues to depend on the dominant historiography. However, I have tried to approach it in a new way.

[18] The notion has been taken up by historians; see Olby (1994) and for a critique Abir-Am (1985). On 'schools' as historiographical topic see Geison and Holmes (1993).

[19] Molecular biologists have been particularly active in writing the history of their field. In the view of one participant, this can be explained by the 'fantastically rapid' development of the new science, which allowed molecular biologists to look back on their own research and that of their colleagues with an unprecedented 'depth of historical perspective' (Stent in Watson 1980, ix). Most of the accounts are autobiographical, but for exactly that reason claim authenticity. See especially Watson (1968), who inaugurated the trend. For a vocal defence of the figure of the scientist–historian see Fruton (1992; 1999). For a critique of Fruton's position see de Chadarevian (1996b). The standard references in the history of molecular biology are still Olby's thorough study, though only up to 1953, and Judson's highly readable but rather journalistic account. Both books have recently been reprinted (Judson 1994 and Olby 1994).

The retrospective character of discovery and 'origin' accounts and their legitimatory functions have been amply demonstrated. Such analysis has also been applied to the DNA story (Abir-Am 1982b; 1985; see also Forman 1969–70; Olby 1979; Brannigan 1981). While I draw on these studies, my strategy has been to place the double helix back into its local context and to examine the role of Watson and Crick's work in shaping research traditions and institutional developments in the laboratory in which it was performed. I argue that the double helix played only a subordinate role in the negotiations over the future of the Cambridge unit. It was rather in the course of these events and in the following debates concerning the origins and boundaries of the new science that the helix gained its central role (Part II). The uncertainties surrounding the fate of the 'original' model of which only a few pieces survive, I shall suggest, reflect the retrospective construction of the helix's importance. The few surviving plates and model bits were later used to build a model 'the nearest there is' to the original one, for display in the Science Museum in London.[20] Stressing the retrospective construction of the year 1953 as the origin of molecular biology, I do not intend to belittle Watson and Crick's scientific achievement. The making of a science, however, requires more than scientific 'breakthroughs', as the scientists involved seem very well to know. By placing the double helix back into its local historical context, we can retrace these processes and negotiations and reconstruct the work which was necessary to turn the double helix into the icon of a new science.

Scholarship in the history of molecular biology has focused on the funding policies and social agenda of the Rockefeller Foundation in the 1930s and 1940s (Abir-Am 1982a; 1987; Kohler 1991; Kay 1993a), on the development, politics and industrial exploitation of recombinant DNA technologies in the late 1970s and 1980s (e.g. Yoxen 1981; Krimsky 1982; 1991; Bud 1993; Wright 1994; Rabinow 1996; Gottweis 1998a; Thackray 1998) and on the most recent developments regarding the Human Genome Project, which came with a (modest) budget for historical research (Kevles and Hood 1992; Cook-Deegan 1994; Sloan 1999; Fox Keller 2000). In addition, much of this work has been dedicated to developments in the United States. In these studies, World War II, if mentioned at all, is mainly portrayed as an 'interruption' of prewar pursuits or in very general political terms, while our view of postwar science is dominated by studies of the physical sciences (especially nuclear physics) and engineering and the making of 'big science' in the form of the military–industrial–academic complex (e.g. Forman 1988; de Maria, Grilli and

[20] The quotation is from the label in the Science Museum.

Sebastiani 1989; Galison and Hevly 1992; Leslie 1993; Galison 1997).[21] Molecular biology in the 1950s and 1960s was not 'big science' in the sense physics was (although to the extent to which molecular biology was part of 'biomedicine' a similar case could be made).[22] Yet, like nuclear physicists, molecular biologists (and before them biophysicists) responded to postwar concerns and effectively used opportunities created after the war and as an effect of the war to further their science. Their efforts, I will argue, set the stage for later developments which gave molecular biology the privileged position it occupies today. Focusing on the changes following World War II does not mean denying continuities with prewar pursuits. However, stressing the opportunities offered by World War II and postwar reconstruction does involve a reassessment of the role of the Rockefeller Foundation in the creation of the new science. The Foundation's 1930s programme, designed by Warren Weaver, aimed at funding chemical and physical approaches in the life sciences, is generally viewed as a decisive factor in the foundation of molecular biology. The institutionalisation of molecular biology in the late 1950s and 1960s, however, cannot be understood as merely subsidiary to intellectual programmes and practices set in place in the interwar years.[23] The focus on Britain not only fills an important gap in the literature, but also moves attention to developments in Europe more generally, despite decisive differences in the way the war affected scientific developments in other European countries.

Finally, how does my local study relate to 'big picture' accounts of the history of molecular biology? Overarching histories of the field continue to be produced.[24] Yet despite an increased focus on details of

[21] There is, however, a growing number of booklength studies on the biological and biomedical sciences in the 1940s to 1960s on which my research draws; see for instance Gaudillière (1991), Kay (1993a; 2000), Lindee (1994), Fox Keller (1995a), Rasmussen (1997b) and Rheinberger (1997).

[22] Lily Kay has argued that with the development of large instruments like the Tiselius electrophoresis apparatus, the life sciences, in the 1930s and 1940s, entered the era of 'big science' (Kay 1988). However, as Kay herself has pointed out, postwar commercialisation made 'big apparatuses' more affordable and easy to use. It remains none the less true that investigations at the subcellular level required an increasing quantity of costly apparatus. Molecular biologists partly shared some of the big instruments developed for nuclear physics; this is particularly true of their use of electronic computers in the 1950s and early 1960s and of synchroton radiation, from the 1970s. Both instruments were used for structure determinations of biological molecules.

[23] On molecularisation as a more long-term strategy, involving the state and private funding agencies, the laboratory, industry and the clinic, see de Chadarevian and Kamminga (1998b). Molecularisation, however, gained new momentum with the biomedical mobilisation of World War II. Molecular biology (as a disciplinary formation) was one face of these later endeavours; see especially the introduction and the chapters by Creager, Gaudillière and de Chadarevian in de Chadarevian and Kamminga (1998a).

[24] Recent examples include Morange (1998a) and Corbellini (1999). On the continuing need for 'big pictures' see Secord (1993).

experimentation and instrumentation as well as on 'social factors', they all start with a definition of what molecular biology is and remain 'result' orientated. In these generalised pictures, local contexts, the prime sites of knowledge production, tend to get lost. Is this a necessary price to be paid to gain a wider understanding of historical processes? Questioning the usefulness of traditional big pictures (including the common kind of disciplinary histories), Ludmilla Jordanova has called for a new kind of big picture emerging from in-depth local studies rather than from wide-sweeping generalisations. Only fully contextualised local accounts, she claims, can provide an understanding of the processes involved in knowledge production at a given time. If such an understanding is achieved, 'the result would be a big-picture history' (Jordanova 1993, 480).[25]

My account of the making of molecular biology at Cambridge is not a history of molecular biology in the grand sense. It is not even a history of one institution. In both respects it is too selective in its choice of topics. However, the topics were selected in such a way as to highlight the processes involved in the local production of the field. Because of the very fact that it insists on local contingencies, the account given here cannot be generalised. But it points to the multiple arenas and the complex web of negotiations involved in building molecular biology after the war. More local studies will enrich the picture. But any new picture which emerges will form in the intersections of these local accounts, not by moving beyond them.

I would like to mention one more point which has bothered me throughout. A brief flick through the pictures included in this volume will suffice to confirm that the story here told is dominated by male actors, with most women being relegated to crucial but clearly subordinated tasks backstage, from washing up, contouring, drawing, and operating machines, to running the cafeteria (molecular biology was hardly different in that from other sciences or public domains). I do at times point to this unequal division of labour and to unguarded value judgements regarding women and their work, but do not probe any further. Strikingly, protein crystallography was a field which in its beginnings saw several key women practitioners. But after the war, few new women joined. If, as I argue, skills and networks acquired during the scientific wartime mobilisation projects played an important role in the postwar restructuring of the sciences, this certainly put women at a disadvantage. Other authors have focused on the few woman

[25] Jordanova's position resonates with more general discussions regarding the explicative value of qualitative in contrast to quantitative approaches in the social sciences; see e.g. Bude (1988).

actors in the field and the ways their gender has shaped their careers (e.g. Sayre 1975; Fox Keller 1983; 1996; Ferry 1998). More importantly perhaps, we start to understand how key theoretical notions of molecular biology were deeply gendered (Fox Keller 1995a). Though outside the present frame of analysis, these studies seem crucial for an understanding of the cultural inscriptions of molecular biology.

Too early, too late?

Writing on recent science raises a particular set of questions (Söderqvist 1997). By definition, 'recent science' implies that at least some of the actors are still alive. For some people this circumstance, in addition to the missing historic perspective on events which are too close to our own times, imposes serious limitations on historical scholarship. From another point of view, the possibility to elicit information from some of the actors offers the unique chance greatly to enrich our historical records. Oral history projects aim to safeguard this information for later generations of historians. Even among historians of recent sciences, however, the value of 'oral' information is hotly debated (de Chadarevian 1997a). Regarding written sources, the situation is no less complex. While on the one side there is an overflow of records, on the other side many records on which historians are used to relying, such as letters or handwritten memos, have been supplanted by modern technologies of communications, especially the telephone and, more recently, electronic mail (a change with which later historians of the period will also have to come to terms). In addition, many records are still in private hands or unavailable because of the various restrictions regulating access to recent institutional and personal records or simply because they remain uncatalogued. Finally, the overflow of material forces institutions and individuals to reduce their records drastically and also to destroy recent files. The selection will invariably disappoint some historians. Among the MRC records of the LMB, for instance, files relating to the purchase of equipment are difficult to find.

All these conditions have shaped this book. I have used interviews, though mainly as a guide to archival work and to test interpretations. Having become accustomed to being able to rely on these personal exchanges, the death of some participants while research was still in process became a serious loss. Often interviews led to access to personal papers without which this book could not have been written. Papers from private hands as well as from institutions were released in stages, sometimes

unexpectedly. Other files remained closed. This often required resetting the agenda and rewriting. Several collections (among these the MRC, Kendrew and Monod Papers) have recently been recatalogued, which required extensive correction of references.

Once a special permission lifted the thirty-year-rule concerning MRC material, I gained access to an abundance of institutional records. Personal records were harder to come by (an exception being Kendrew's and Brenner's rich collections). This imbalance has certainly shaped my perception of the history and may well have induced me to focus more strongly on institutional and policy aspects than I originally planned.

Was it too early to embark on this history, as some colleagues working on earlier periods suggested? More records will become available. The LMB for one is actively engaged in collecting material and setting up its own archive. But by that time oral, written and material sources to which I had access may not be available any more. On several occasions my interest saved some papers or other records from being dumped, while especially the reorganisation of files at the MRC made unavailable some records I had consulted earlier. This book, no more or less than others, is as much a record of its own time as of the period it deals with. The archive will change and other stories, building on new insights, will follow.

Preview

Each of this book's three parts addresses one main question. Part I investigates the impact of the scientific mobilisation during World War II on the place of science and the fortunes of biophysics in postwar Britain. Chapter 2 offers a brief discussion of the involvement of British scientists in World War II. It points to the active mobilisation of scientists and their planning for postwar needs which started well before the end of the war. Chapter 3 investigates the moves which led to the creation of a number of biophysics groups, including the Cambridge unit, after the war. It focuses on the (wartime) careers of those involved in building up biophysics and on the postwar attractions of a 'physics of life' with the promise of medical applications. Chapter 4 focuses on protein X-ray crystallography, the key technology of the Cambridge unit. It discusses especially Kendrew's early use of the experimental digital computer at Cambridge, linking his interest in the new machine to experiences acquired in operational research during the war and reflected in his approach to research and in the organisation of his research work. Chapter 5 investigates the place of models in the experimental practice of protein crystallography and follows their display in public arenas, including early television programmes on science. Models,

I will argue, played a crucial role in promoting a new understanding of life, based on molecular structures and their functions.

Part II considers the role of the double helix in the making of molecular biology. It starts with a review of the debate on the 'origins' of molecular biology, fought out among practitioners in the late 1960s, which retrospectively attributed to Watson and Crick's work a key role in the history of the field (chapter 6). The rest of chapter 6 and chapter 7 place the work on the double helix back into its local context, studying investigative practices and institutional moves following the collaborative effort of the two researchers. I argue that the work on the genetic function of DNA undertaken in the unit, which got going seriously only once Sydney Brenner moved to Cambridge in 1957, did not follow directly from work on the double helix. However, 1953, the year of Watson and Crick's celebrated achievement, marked the beginning of a serious institutional crisis, precipitated by the move of the unit's patron in the Physics Department, the Cavendish Professor of Physics Lawrence Bragg, to London. Fruitless attempts to find another niche in the university finally led to the plans for a new and independent Laboratory of Molecular Biology. Crucial allies in this plan were Sanger and his group working on protein sequencing in the adjacent Biochemistry Department. In the negotiations for the new laboratory the double helix played only a subordinate role. Chapter 8 confronts this local account with the standard story on the place and role of the double helix in the history of the field.

Part III serves a double purpose. On the one hand, it offers a detailed reconstruction of molecular biologists' use of the political arena to promote their science and to turn it into an item on the governmental agenda. The 'Kendrew Report' on molecular biology, issued by the Council of Scientific Policy, the government's central advisory body for science, and the negotiations for EMBL as well as the controversies surrounding these initiatives form the centre of this discussion (chapter 10). On the other hand, Part III points to changes in government policies for science and to increasing competition among molecular biologists, both of which affected work practices and the place and role of the LMB. Two examples serve to illustrate this point: Brenner's worm project, which aimed at a 'complete' description of the worm, eventually including the full sequence determination of its genome, performed in a newly created sequencing centre, and the 'scandal' concerning the 'failure' to patent the technique of producing monoclonal antibodies, developed by researchers at the LMB in 1975, with the subsequent 'push' towards commercialisation under the Thatcher government (chapter 9 and 11). Chapter 11 also discusses the rising political expectations of medical 'applications' from 'fundamental'

biological research. Scientists themselves had helped create these expectations when applying for biophysical and biomedical research projects after the war. In the 1970s these expectations led to new policy interventions, new justifications for research and changes in research directions. The interest of government and industries in the field was not the direct and sole effect of new technologies, especially those for cutting, recombining and multiplying DNA, born from fundamental research. Instead, the 'commercial turn' in molecular biology was well prepared by discussions and developments in the preceding decades. The conclusions reflect on the connections to current trends in the history and historiography of the biomedical sciences.