



**Beyond Deterministic Sociology and Apologetic History: Reassessing the Impact of Research Policy upon New Scientific Disciplines (Reply to Fuerst, Bartels, Olby and Yoxen)**

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7. W. Weaver, Oral History Memoir, Columbia Oral History Record Office, No. 434, 266-67.

8. Ibid, 495.

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10. C.H. Waddington, *The Strategy of the Genes* (London: Allen and Unwin, 1957).

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12. E.J. Yoxen, 'The Interaction of Information Theory and Developmental Biology in the Career of C.H. Waddington', in T.J. Horder and J. Witkowski (eds), *History of Embryology* (Cambridge: Cambridge University Press, in press).

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14. J.D. Bernal, 'W.T. Astbury', *Biographical Memoirs of Fellows of the Royal Society*, Vol. 9 (1963), 1-36.

15. Olby, op. cit. note 6, 326-27.

16. J.A. Fuerst, 'The Definition of Molecular Biology and the Definition of Policy: The Role of the Rockefeller Foundation's Policy for Molecular Biology', *Social Studies of Science*, Vol. 14 (1984), 225-37.

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*Responses and Replies* (continued)

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## Beyond Deterministic Sociology and Apologetic History: Reassessing the Impact of Research Policy upon New Scientific Disciplines (Reply to Fuerst, Bartels, Olby and Yoxen)

**Pnina Abir-Am**

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**My questioning of the received view**, which held that the research policy of the Rockefeller Foundation (RF) in the 1930s had a direct impact upon the rise of molecular biology, a generation later,<sup>1</sup> seems to have shocked the social studies of

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science community — or, at least, its historically minded section. The four Responses published above<sup>2</sup> present us with a welcome opportunity to re-examine and elaborate on the challenge posed by my reinterpretation to received views on the effectiveness of the RF's policy, on the historiography of molecular biology, and on the relationships between them. The Responses display a spectrum of opinion, ranging from genuine attempts to reconsider the validity of the received views to what I take to be systematic misreadings of my argument — a pattern possibly related to the fact that the respondents have made their own investments in the credibility of the received views, as well as to the space restrictions tightly imposed on my 'provoking' paper, which led to the deletion of many qualifying paragraphs.

In this Reply, I shall argue that all the respondents have missed the basic point that my paper challenged, not only the specific contents of the received view, but also the validity of the accepted procedures for evaluating, on one hand, the impact of policy upon scientific change, and, on the other, the historiography of molecular biology. This failure to grasp the social-theoretical and historiographic difference of my approach accounts for the Responses' strategy of reiterating old 'facts', while remaining confined to a limited theoretical framework bound by deterministic sociology and apologetic history — a framework which my paper did not share. While attempting to make explicit the underlying theoretical stances responsible for this spectrum of Responses, I will first address themes common to several Responses (A, B) and second, themes unique to a given Response (C, D, E, F).

*(A) Were the physical scientist grantees of the RF opportunistic respondents to its policy, or were they genuinely interested in biology?*

All the Responses affirm that Astbury and Pauling had a 'healthy and genuine interest in biology', and, in their own ways, try to defend their images from my suggestions that these two long-term grantees typified the opportunistic response to RF's policy on the part of many physical scientists, and that this opportunism was encouraged by the RF's conception of biological progress, with its emphasis on technology transfer from the physical sciences to biology. Among the respondents, Fuerst and Olby make a direct attempt to 'counteract' my interpretation, with Bartels and Yoxen indirectly concurring.

Since Fuerst assumes the major burden of 'proving' that my argument is totally 'incorrect', let us first examine his own form of 'counter-evidence'. Fuerst marshals an impressive battery of six footnotes demonstrating Astbury's biological thinking, and nine demonstrating Pauling's.<sup>3</sup> However, Fuerst's sources consist mainly of pieces explicitly commissioned to celebrate, not to question, the inevitability of scientific progress and its simplistic derivation from experimental results. In support of Pauling's deep biological commitments, six of his nine footnotes rely exclusively upon evidence from Pauling himself. Moreover, the evidence comes from belated historical excursions by Pauling in the 1970s, while purporting to testify for his degree of interest or actual investments in biology a generation earlier.<sup>4</sup> These sources provide important information on the conformity of Pauling (and of the scientist-editors who commissioned these pieces) to the scientific ethos in the 1970s, but they can by no means be taken at face value as conclusive evidence on events in the remote past. This historically retrospective and sociopolitically loaded context of asserting intellectual property rights usually produces both simplistic and tendential historical allusions, which abound in Pauling's celebration-related pieces, as they do in those by other individuals or groups of scientists.<sup>5</sup> These historical allusions are subordinated

to systematic sociopolitical efforts to secure legitimation for contested definitions of scientific reality in the present (in the case under discussion, to defining molecular biology in the 1970s as primarily structural or genetical), and they have to be recontextualized before they can be properly understood. Thus six of Fuerst's nine sources can be plausibly called into question as self-serving rhetoric.

The remaining three footnotes include reference to two scientific papers published by Pauling in 1940.<sup>6</sup> These papers do indeed suggest that Pauling displayed some interest in biology in the 1930s, or at least in 1940. However, the question posed by my paper was not whether Pauling had any interest in biology: I enumerated his occasional contributions in the 1930s. Specifically, the question probed the nature of that interest. Was it more than a token response, necessary to satisfy the minimal requirements of the RF grants, but insufficient to re-orient biology along new theoretical lines? Numbers speak louder than words on this occasion. By 1940 Pauling had over a hundred papers in structural chemistry, and in many other chemical fields.<sup>7</sup> Thus, the two 1940 papers invoked by Fuerst as evidence in favour of Pauling's genuine interest in biology can only be regarded as such a token response.<sup>8</sup>

A simpler deconstruction procedure applies to Fuerst's footnotes on Astbury. Once again, Fuerst fails to recognize the significance of the contexts in which those public statements were made — contexts ranging from respectful obituaries to indulgent memorial lectures, apologetic secondary literature or occasional biological excursions in scientific publications devoted primarily to reports of physical results.<sup>9</sup> The latter, in particular, have to be contrasted against the broader context of a scientist's research priorities before they can be judged as representative (as Fuerst assumes), rather than as exceptional.

My broader recontextualization of ancestral figures (such as Pauling and Astbury) in molecular biology in their historical and micro-political context of interaction with the RF's policy in the 1930s, has received a more interesting response from Yoxen and Olby. Though they, too, have rallied to defend Astbury (presumably they accept my reinterpretation of Pauling), their justifications for including Astbury within the shifting frontier of molecular biology are made in more sophisticated terms. While Yoxen emphasizes additional institutional setbacks, Olby has produced archival (as opposed to retrospective) evidence that Astbury had a more profound biological outlook than my interpretation would suggest. Nevertheless, both efforts fall short of challenging my interpretation.

Yoxen's emphasis on two institutional setbacks suffered by Astbury in the 1940s can only complement and strengthen my suggestion that the RF was the single major source of support for Astbury. Given the refusal of Astbury's home university to recognize him as the incumbent of a Chair in 'molecular biology' in 1945, and the Medical Research Council's refusal to grant him support in the late 1940s, the RF's policy remained the major influence on Astbury's biological adventures, and on their subsequent limitation to the production of physical results. At the same time, Olby has attempted to prove Astbury's profound biological thinking by quoting a letter from Astbury in which he displayed enthusiasm about Avery's work as early as 1945.<sup>10</sup> It is useful to know that Astbury was one of the few scientists to have shown an early interest in Avery's 'neglected discovery', especially since many would-be central figures in molecular biology have admitted missing its significance.<sup>11</sup> Nevertheless, Olby's archival effort assumes significance only for those still attempting properly to allocate positions to various 'predecessors' along the 'path' to the double helix, while they confuse the history of the double helix and molecular genetics with that of molecular biology.<sup>12</sup>

For one keen on understanding how and why scientific change occurs (or does not occur), as opposed to reconstructing such change retrospectively, it is equally important to find out what Astbury actually did with his enthusiasm, besides displaying it in letters. (Incidentally, Astbury's archive shows that he was enthusiastic about a wide range of things.) As Olby admits, Astbury did not translate his enthusiasm for Avery into abandoning the prevailing views on the respective biological roles of proteins and DNA; these were views constrained by physical, not biological, results. Olby attributes Astbury's subsequent limitations in building upon his 'healthy and genuine' interest in biology (and early enthusiasm about Avery — that is, about DNA's exclusive role in genetic transmission) to experimental difficulties. He further suggests that these experimental difficulties constitute an 'alternative explanatory strategy' in (better) explaining Astbury's subsequent location outside the frontier in molecular biology. Unfortunately, Olby appears to assume that experimental difficulties are experienced in a theoretical vacuum. His 'alternative explanatory strategy' sounds more like a traditional excuse, commonly deployed by practising scientists as an ultimate explanatory resource, especially in public discourse.<sup>13</sup> We cannot further explore here why practising scientists usually appeal to experimental difficulties (rather than acknowledging their deeper dependence upon a theory-bound world-view) as the ultimate constraint upon the courses of action they take. Nor can we examine why Olby adopts this empiricist excuse, despite some twenty years of philosophical and sociological deconstruction of empiricism as the ideological basis of the cultural authority exercised by scientists in general and by their leaders in particular. But it seems to me to be more plausible that it was the *superficiality* of Astbury's and Pauling's preoccupation with biological theory (by which they endowed their many physical results with biological meanings), rather than their occasional experimental difficulties, that situated both outside the shifting frontier of molecular biology.

(B) *Why did the RF suspend Needham's and Waddington's project?*

In line with their defence of Astbury's and Pauling's ancestral status within molecular biology, the respondents cling to the notion that the RF suspended its support for Needham's and Waddington's (NW's) research because it realized that these (once favourite) grantees were heading off the 'path' to future success in molecular biology. Fuerst has devoted the greater part of his Response to demonstrating obvious points about NW's research priorities in experimental embryology.

Since this area did not come to share in the molecular-biological synthesis of the 1960s (which revolved around different experimental objects, most notably bacterial viruses), Fuerst concludes that NW's research was rightly suspended by the RF. He has further supported his conclusion by discussing their supposed anti-reductionism, so different from the perfect reductionism which Fuerst believes characterized the molecular biology of the 1960s. According to his arguments, Needham and Waddington were disqualified by the RF in the 1930s because they failed to anticipate the subject-matter, and alleged philosophy, of molecular biology in the 1960s.

Fuerst's view, to which Bartels and Olby also subscribe, leaves Yoxen alone among the respondents willing to entertain some doubts about the soundness of the received view on Needham and Waddington. Unfortunately, that view is not supported by the archival record, or by any non-retrospective explanatory logic.

First, the view implies that the RF knew in advance how the 'right' or successful molecular biology would look, and distributed its grants accordingly, by supporting

'real' (Bartels' term) predecessors like Astbury and Pauling, and suspending future or potential 'failures'. But there is no evidence that the RF or its grantees commanded such a teleological view of scientific progress. Rather, this view is an artifact of current stances among philosophers of science (most notably the Lakatosian rational reconstruction of research programmes, to which Bartels and Fuerst, in particular, subscribe). To be sure, those retrospective endeavours are grounded in some ultimate cultural values, such as a belief in the inherent rationality of science and assorted elements of progress (for example, Fuerst's concept of 'reductionism'). However appealing those values might be, the retrospective reconstructions to which they lead do not stand up to close scrutiny by the historical record, viewed in its entire temporal and spatial context. One has to assess parts of the historical record against the long-term trends before one can tell whether they were representative (as Fuerst assumes), or merely tokenist.

Second, the trend revealed by the detailed archival record is quite opposite to that inferred by Fuerst and the other respondents. The RF initiated its support of Needham and Waddington in 1934, and supported them until 1938. Moreover, the RF was willing to invest enormous sums in their projects, sums much larger than those it gave to either Astbury or Pauling (or, indeed, to most other RF grantees). The reasons for the RF's numerous overtures were simple: Needham, Waddington and their colleagues were the only ones to have had a coherent conception of molecular biology in the 1930s.<sup>14</sup>

Once again, one must remember that the meaning of 'molecular biology' in the 1930s was different from its diverging meanings in the 1960s.<sup>15</sup> Moreover, those earlier meanings (especially those prevailing in the RF) cannot be extrapolated back in time from the meanings of the 1960s, as the respondents invariably do. Rather, RF's conception of 'molecular biology' in the 1930s has to be retrieved from documents of that time — most notably its Annual Report of 1938, which first expounded that conception, and was quoted in my paper. There, 'molecular biology' was an interchangeable term with 'sub-cellular biology'. The term referred to a (yet to be explored) more basic level of biological organization, since it transcended classical biology's smallest unit of analysis, the cell.<sup>16</sup> Given that this conception of 'molecular biology' prevailed within the RF, its support of NW's projects in exceptional ways, including salary supplements, becomes meaningful. Their project in 'physico-chemical morphology' proceeded from supracellular to subcellular levels of biological organization. The RF's officers referred to NW's project as unique in its superb match with the RF's programme.

The question thus remains: why did the RF eventually suspend those it perceived to be its most relevant grantees? Was it because it suddenly discovered their supposed antireductionism or embryological subject-matter, as Fuerst suggests? In answering this question, the archival records are again a unique resource.

First, the reductionism (or antireductionism) of a grantee was never a criterion of his/her eligibility, nor was it even an issue. Therefore, I will not enter into a discussion over whether RF's policy was reductionist (along the retrospective lines discussed by Fuerst and questioned by Yoxen), since this factor was irrelevant to the grant decision-making process. Second, the detailed archival record of the events leading to the suspension of NW's project shows that RF's intent to expand support to Needham and Waddington came into open conflict with the still more basic goals of the RF in securing legitimation for its intervention in British science. More specifically, the RF (and Weaver in particular) sacrificed those it considered its best grantees to appease the overt prejudice of several top administrators in British science, who objected to

the RF making major investments in junior scientists of radical sociopolitical and scientific leanings. Weaver came to believe that continuing any type of support to Needham and Waddington would jeopardize the RF's intervention in British science in general, by antagonizing those top leaders who quite explicitly expressed their objections to Needham and Waddington, even though those objections were not scientifically based.<sup>17</sup>

*(C) The nature of the RF's policy: colonizing biology through technology transfer dominated by physical scientists, or by reductionist ideas operating in a sociopolitical vacuum?*

Among the three Responses bearing on this question, Fuerst's alone has attempted to provide an alternative interpretation of the nature of the RF's policy. Bartels and Yoxen have commented only briefly on my interpretation. Both regard the metaphor of technology transfer as inadequate, but have not offered any argument to support this assertion, nor have they provided an alternative vector to explain the inherent sociopolitical asymmetry among grantees sustained by the RF's policy — a part of my interpretation which they accept. Fuerst's unique objections are examined in this section.

After taking issue with the metaphor of colonization because of its perceived inadequacy, Fuerst proceeds to adopt it, suggesting that the colonization of biology was a result of the (mysterious) working of reductionist ideas, apparently operating without intervention by human agents. In Fuerst's view, the impact of the RF's policy resulted from its anticipation of the philosophical reductionism supposedly typical of molecular biology in its successful phase. He contrasts his view to my emphasis upon technology transfer as the key feature of the RF's policy, while charging that I did not recognize the (more important and lasting) philosophical underpinning of that policy.

Fuerst's objection stems from his misreading of my characterization of the RF's policy, a characterization which *did* include a philosophical underpinning: I described it as being a slightly outdated form of positivism, to which Weaver in particular subscribed, and I quoted from his working papers to support my interpretation. Furthermore, I described his policy as a strategy to salvage the unity of science, under the aegis of his own discipline of classical physics, from the threats posed by the 'adventuresome' and 'spiritual' quantum theory which he loathed.<sup>18</sup>

Nevertheless, I further argued that, despite the philosophical underpinning of the RF's policy, it was the translation of that philosophy into an investment strategy revolving around technology transfer (a translation influenced by Weaver's background in engineering) which weighed heavily in the concrete context of policy implementation. Prospective grantees were not asked whether they subscribed to reductionism, but rather whether they would engage in a project whose central feature was not a biological (or physical) problem, but rather the deployment of physical technology upon biological material. The philosophical intent never manifested itself in the process of policy implementation, and therefore could not have had the impact Fuerst attributes to it, on the basis of retrospective reconstructions from 'outcomes' — such as expressions of intent by scientists or policy-makers made public a generation later.<sup>19</sup>

Fuerst's second objection is that the colonization of biology was accomplished primarily by philosophically reductionist ideas and concepts, as opposed to my emphasis upon sociopolitical asymmetries among groups of grantees. Those asymmetries were triggered, I suggested, by the RF's definition of its policy as

revolving around technology transfer, and eventually led to domination by one such group (the physical scientists) over another (the biologists). While Fuerst dislikes the suggestion that such asymmetries prevail among scientists, he deems colonization by (reductionist) ideas and concepts desirable and progressive. Unfortunately, Fuerst fails to explain how human agents were involved in that colonization. If human agents (including scientists) are needed to make ideas work (progressively or otherwise), can Fuerst show that they are not sociopolitically differentiated? As my analysis amply demonstrated, different groups and individuals came to command markedly different resources (material as well as human) in the process of policy implementation.<sup>20</sup>

Eventually Fuerst reveals his hand. He asserts that ‘philosophical roots may remain firm where “power relations” change and disappear’.<sup>21</sup> Possibly this belief persists in philosophy departments where the handling of the eternal (philosophy) is deemed superior to the handling of the ‘mere’ temporal (historical analysis of power relations, for example). Yet I doubt that the ontology of power relations is so degraded as to put the entire phenomenon in quotation marks (thus questioning its ultimate reality), even in those hunting grounds for eternal verities. Perhaps we should ask the following question: Why does the suggestion that scientists are sociopolitically differentiated and parties to domination within science, prove so disturbing to latter-day apologists? Could they be anxious to gloss over cracks in the apolitical image of science?

*(D) Why did the RF support biology at all? The inadequacy of both personal and social-deterministic ‘explanations’ of the origins of the RF’s policy.*

Bartels alone raises the important question of why the RF supported biology, molecular or otherwise, in the first place. She also points out the inadequacy of former explanations — either the personal-motivational one advanced by Olby (‘explaining’ the policy’s origins in terms of Weaver’s personal motivation to make biology look scientific), or the class-bound one advanced by Yoxen (‘explaining’ the policy’s origins as resulting from the RF’s interest in obscuring the class relations under capitalism). Bartels hopes for a synthesis between these two ‘explanations’, though she does not propose how such a synthesis could be accomplished. Perhaps she glimpses that such mono-causal explanations should be dismissed altogether, as an outdated positivist and reductionist legacy.

Unfortunately, Bartels’ urge for a synthesis between two inadequate ‘explanations’ apparently leads her to miss the much more subtle explanation for the RF’s involvement in biology offered in my paper. There, the RF’s policy was presented as the contingent result of several mutually reinforcing factors. These included: the RF’s internal reorganization, and search for both direct accountability over its large distribution of funds (especially in the aftermath of the Great Depression, which made its former loose philanthropic style outdated) and a progressive response to the then prevailing ‘social unrest’; the officers’ mandate to frame and implement a new policy, exercising *de facto* but not *de jure* control over the choice of projects; Weaver’s personal lack of biological background (which made him dependent upon the rhetoric and advice of scientific leaders, which he often took at face value — most notably on biological progress as a simplistic guarantor of social progress, and on the difficult question of technology transfer as the means rather than the ends of biological progress); and the lessons learned by the Rockefeller Institute in fostering research along non-disciplinary lines (which Weaver sought to incorporate for its congruence with the transfer of technology from one discipline [the physical sciences] to another [biology]: it also suited his need to demonstrate continuity with the RF’s

overall accomplishments to internal critics who objected to the new 'interventionist' policy, harking back to research performed directly under the auspices of the Rockefeller philanthropies). All of these multiple factors became objects for negotiation in subsequent drafts of the RF's policy documents.<sup>22</sup>

Detailed attention to the archival record over time, and to the dual level of both the framing and the implementation of the RF's policy, shows that no single cause (personal, institutional, class-bound) can explain the complex contingencies of the decision-making process which initiated and sustained the RF's policy of biological progress throughout the 1930s and the 1940s. Bartels' question thus brings us to another related theme, pertaining to the reassessment of the effectiveness of the RF's policy, and especially of its omnipotent mono-causal agent, Weaver.

*(E) Reassessing Weaver's performance, or deflating the inflated 'role' of the science manager as creator of a new scientific discipline.*

Yoxen alone among the respondents attempts to preserve the received historiographic picture of Weaver (as the creator of a new scientific discipline characterized by omnipotent managerial ingenuity, hard work and foresight in anticipating scientific progress) from my deflation of Weaver's total and mono-causal capacities in controlling the course of biological progress.

Despite the intricate analysis I presented (which amply demonstrated the numerous constraints and mutual dependencies which impinged on Weaver as an implementer of RF policy, and which affected his conduct in ways different from what one might infer from the mere declaration of his policy's initial goals), Yoxen persists in his attempts to restore Weaver's mono-causality. He has labelled my case studies 'selective', and my reinterpretation of Weaver as a 'mere' party to complex negotiations of authority among various, internally differentiated, groups of actors (such as the scientist grantees and advisers; the trustees; outside science administrators), as 'implausible'.

With regard to the first charge, my conclusions are not merely generalized from three (possibly selective) case-studies: they were carefully constructed after an analysis of the findings of the Committee of Appraisal, a committee which evaluated all the RF's investments (in natural sciences). That committee also concluded that the overall pattern of investments was bent toward safe projects at the expense of innovative but riskier ones. My conclusions can be deemed representative since they were endorsed by a panel of observers who examined *all* the projects. At the same time, Yoxen's own evidence comes entirely from Weaver's oral recollections, or from secondary sources which eulogize the 'role' of the entrepreneurial science manager as the fulfilment of a liberal commitment to professional management.<sup>23</sup>

With regard to the second charge, I would suggest that Yoxen proves impervious to my reinterpretation of Weaver's impact, not because of its alleged implausibility but rather because Yoxen's own favoured explanation requires an inflated and omnipotent science manager to be cast as the powerful capitalist agent, solely controlling the RF's 'purpose' in obscuring and naturalizing exploitative class relations. If, as I suggested, Weaver was not the all-powerful determinant of scientific progress, but was rather a 'mere' party to complex negotiations in which he himself was 'exploited'; if he had no more power or knowledge than other agents; and if he was a victim of dissensus among scientist advisers rather than the supreme manager of consensus — then Yoxen's 'explanations' of Weaver's 'determining role', based on his supposed all powerful and knowledgeable (capitalist) agency, collapses.

Furthermore, Yoxen's interpretation — like the 'role'-bound functionalist interpretation in which he found the initial picture of the omnipotent entrepreneurial science manager (as well as his primary sources) — lacks micro-foundations.<sup>24</sup> Both interpretations deduce the properties of individual conduct (as 'determined' or 'determining') from those of social systems, by a procedure which confers upon individual conduct an inflated degree of (social) determinism. Both have ignored the complex contingencies of the process of policy implementation, which revolves around negotiations and shifts in power relations among the relevant historical actors (the officers, the trustees, the scientist grantees and advisers, the president), all locked in unstable patterns of mutual dependencies since each controlled assets important to others. Evidently, no agent alone could possibly control the process of policy implementation (and its expected result of biological progress), though some may have aspired more than others, and others may have deluded themselves.

Therefore, it is safe to conclude that Weaver's inflated 'role' as the mono-causal agent behind 'outcomes' of biological progress, including the rise of molecular biology, is an artifact created by two brands of deterministic sociology in close partnership with an apologetic history. The latter has inferred the policy's impact on the basis of self-serving rhetoric and superficial quantitative indicators, all reconstructed a generation later by a limited number of successful actors.<sup>25</sup>

Having started with historiographic objections (A, B) and continued with policy-related ones (C, D, E), it is appropriate to close this 'anthropological circle'<sup>26</sup> with a major historiographic objection.

(F) *'Black-and-white' historiography: a problem of writing or reading?*

Unlike the other respondents who have struggled in their own ways with various aspects of my reinterpretation of the RF policy's impact upon molecular biology, Olby has chosen a short-cut. He sees no need to consider the intricate analysis of the sociopolitical context of policy implementation. Rather he dismisses my entire argument, since it supposedly rests on a discredited black-and-white historiography.

It is possible that formal properties of my argument, especially the extreme conciseness imposed on a complex drama by space restrictions (with the deletion of many qualifying paragraphs) and the ironic style, could have encouraged a black-and-white reading. At the same time, such a reading would also have required a predisposed mind. Indeed, Olby has taken ironic liberties of his own in using theological metaphors, and in casting the actors as 'goodies' and 'baddies'. I cannot see the purpose of these categorizations, apart from creating, in a retrospective manner, the very black-and-white associations which he then claims to have found in my text. This strategy of substituting imputations for a serious attempt to cope with my 'truly provocative interpretation' eventually misfires, while exposing Olby's unique reading as a product of his own predilection for philosophical reconstructions. For example, Olby's tendency to extrapolate beyond the deliberately restrained or implicit argument in my text is evident in his objections numbered as (3) and (4). The first objection (3) stems from Olby's misreading of my reference to the historical actors' derogatory attitude to 'dead' material as proper sources for conclusions in molecular biology (an attitude which prevailed among classical biologists in the 1930s) as my own attitude. The second (4) results from Olby's extrapolation of my description of Astbury's long dependence upon the RF as also implying that the RF's intervention preceded Astbury's biological adventures; Olby then challenges me to

produce evidence for this illegitimate extrapolation. Since I examined both Astbury's and the RF's records in detail, I am well aware that Astbury had some interest in examining material of biological origin before the RF transformed him into a permanent grantee. Yet this 'predecessorship', so much emphasized by Olby, did not preclude Astbury's evolving dependence upon the RF once they became partners. Once again, Olby has ignored my micro-political analysis, while reverting to the exclusive realm of experimental priority.

A more important misreading occurs in Olby's objection (6), where he defends himself from my suggestion that he had not examined how or why the successful molecular biology of the 1960s came to focus upon DNA. Olby replies that, contrary to my 'allegation', his work uncovered numerous predecessors to Watson's and Crick's (belated) focus upon DNA. Nevertheless, Olby's goal of rescuing 'forgotten pioneers' from oblivion (with the hope of retrospectively readjusting the inflated credit which may have accrued to the proponents of the double helix) reveals his major preoccupation with distributing positions along the 'path' to the double helix. This preoccupation with distributing scientific credit more justly than authority disputes in science would allow (that is, by conferring consolation prizes upon losers in the form of historical spots on a retrospective 'path') has led Olby to misread my suggestion. By focusing on 'predecessors', Olby, no doubt, has accomplished a broad study of the past; yet he has left the reception of the double helix as unproblematic, as a natural derivation of its scientific correctness. In the last chapter of his *The Path to the Double Helix*, Olby briefly discusses the question of its reception, and attributes the wide acceptance of the double helix to the experimental 'proofs' it enjoyed in the late 1950s and onward. Now, the question of why and how an initial hypothesis becomes a hard fact of nature,<sup>27</sup> even a 'discovery',<sup>28</sup> is extremely important, since the entire history of biology in the twentieth century has been reconstructed by Olby as having 'led' to that ultimate objective discovery through a proper 'path' to which Weaver allegedly gave the final push. In contrast to this (and to similar 'improving' interpretations embedded in the Responses), I suggest that something special happened in the 1950s and onward that had not happened before: this suggestion may help to explain the appeal of DNA at that time, despite so many precursors. But those happenings cannot be found solely in the realm of 'experimental proofs', and have therefore not been detected by the current historiography of molecular biology. What this historiography has omitted may well turn out to be of greater importance for the understanding of the origins and reception of molecular biology than what it has included.<sup>29</sup>

All the Responses have failed properly to identify, much less to confront, the key questions raised by my 'truly provocative alternative interpretation': How is new scientific knowledge constructed and validated as a social reality? How is it invested with authority? How does it acquire the objective status of hard facts or discoveries? And what impact, if any, did an early research policy have on these historical processes?

● NOTES

1. P. Abir-Am, 'The Discourse of Physical Power and Biological Knowledge in the 1930s: A Reappraisal of the Rockefeller Foundations's "Policy" in Molecular Biology', *Social Studies of Science*, Vol. 12 (1982), 341-82.

2. John A. Fuerst, 'The Definition of Molecular Biology and the Definition of Policy: The Role of the Rockefeller Foundation's Policy for Molecular Biology', *Social Studies of Science*, Vol. 14 (1984), 225-37; Ditta Bartels, 'The Rockefeller Foundation's Funding Policy for Molecular Biology: Success or Failure?', *ibid.*, 238-43; Robert Olby, 'The Sheriff and the Cowboys: or Weaver's Support of Astbury and Pauling', *ibid.*, 244-47; Edward Yoxen, 'Scepticism about the Centrality of Technology Transfer in the Rockefeller Foundation Programme in Molecular Biology', *ibid.*, 248-52.

3. Fuerst, *op. cit.* note 2, notes 6-11 and 12-20, respectively.

4. *Ibid.*, notes 12-16, 20. For the (1970) paper mentioned in notes 12, 15 and 20, the context of celebrating progress is evident in its title. Also of great interest is the editors' historiographic contribution in the form of a long note alerting the reader that, were it not for travelling obstacles which prevented Pauling from seeing the right X-ray data on DNA, he would have certainly discovered the double helix. The (1974) paper mentioned in notes 13-15 was commissioned for the 21st anniversary of the double helix: see the cover, editorial and other contributions preceding and succeeding Pauling's paper in *Nature*, Vol. 248 (26 April 1974). Finally, the (1968) book edited by Rich and Davidson, note 20, was a festschrift produced by Pauling's students and collaborators on the occasion of his sixtieth anniversary, in a clear reply to Delbrück's festschrift produced in 1966: J. Cairns, G.S. Stent and J.D. Watson (eds), *Phage and the Origins of Molecular Biology* (Cold Spring Harbor, NY: Cold Spring Harbor Laboratory, 1966).

5. For an analysis of the historiographic implications of scientists' progress-celebrating rituals, with a special emphasis upon molecular biologists, see P. Abir-Am, 'How Scientists View Their Heroes: Some Remarks on the Mechanism of Myth Construction', *Journal of the History of Biology*, Vol. 15 (1982), 281-315.

6. Fuerst, *op. cit.* note 2, notes 17-19.

7. For details, see Pauling's bibliography at the end of his festschrift, in A. Rich and N. Davidson (eds), *Structural Chemistry and Molecular Biology* (San Francisco, Calif.: Freeman, 1968).

8. Fuerst, *op. cit.* note 2, notes 17 and 19. Furthermore, a qualitative examination of these two papers will immediately reveal the primacy of the chemical argumentation, with biological inferences, if any, being of secondary importance. Incidentally, Pauling's immunological adventures in the late 1930s and early 1940s revolved around the chemically ambitious and biologically naive and simplistic project of producing artificial antibodies. Lavishly supported by the RF, at the expense of other, more biologically sensitive grantees, the project was evaluated by a panel of outside advisers as a total 'fiasco': see Rockefeller Archive Center, Series 205D, California Institute of Technology, Chemistry and Biology, 1944-45.

9. Fuerst, *op. cit.* note 2, notes 11, 10 and 7-9, respectively. I should stress that Pauling's degree of opportunism was different from Astbury's — a point made quite clear in my paper, yet one which does not affect the main argument in this section. This difference may also explain why more respondents rallied to defend Astbury than Pauling.

10. See Olby, *op. cit.* note 2, note 9.

11. On the reception of Avery's work, see R. Olby, *The Path to the Double Helix* (Seattle, Wash.: University of Washington Press, 1975), Section 3; P. Abir-Am, 'From Biochemistry to Molecular Biology: DNA and the Acculturated Journey of the Critic of Science Erwin Chargaff', *History and Philosophy of the Life Sciences*, Vol. 2 (1980), 3-60.

12. For a detailed argument on how this confusion is perpetuated, see P. Abir-Am, 'The Current Historiography of Molecular Biology: A Deconstructing Critique of Apologetic Histories', forthcoming.

13. For details of the technique of discourse analysis, see Michael Mulkey and G. Nigel Gilbert, 'What is the Ultimate Question? Some Remarks in Defence of the Analysis of Scientific Discourse', *Social Studies of Science*, Vol. 12 (1982), 309-19; Mulkey and Gilbert, 'Joking Apart: Some Recommendations Concerning the Analysis of Scientific Culture', *ibid.*, 585-613.

14. For a detailed argument, see note 60 in Abir-Am, *op. cit.* note 1, 377.

15. For some of the diverging meanings of molecular biology in the 1960s, see note 17 in Abir-Am, *op. cit.* note 1, 372; also Abir-Am, 'On the Verge of Legitimation: The Meanings and Structure of the Resistance to Molecular Biology in the 1960s', forthcoming.

16. For details, see the quotations in Abir-Am, *op. cit.* note 1, 344-45.

17. For a detailed argument, see my forthcoming article 'The Impact and Limitations of Research Policy on the Emergence of New Scientific Disciplines: the Case of Early Molecular Biology'.

18. See note 37 in Abir-Am, *op. cit.* note 1, 374.

19. See note 19 in Abir-Am, *op. cit.* note 1, 372.

20. On the great diversity of the scale of appropriations among the RF's grantees, with grants pertaining to both research assistants and materials, see Abir-Am, *op. cit.* note 1, 358.

21. Fuerst, *op. cit.* note 2, 233.

22. For details, see Abir-Am, *op. cit.* note 1, 348-53.

23. See note 3 in Yoxen, *op. cit.* note 2, especially the paper by Kohler.

24. For details on the problem of micro-foundations, see K.D. Knorr-Cetina and A.V. Cicourel (eds), *Advances in Social Theory and Methodology: Toward an Integration of Micro- and Macro-Sociologies* (London and Boston, Mass.: Routledge & Kegan Paul, 1981); J. Elster, 'Marxismo', *The London Review of Books* (18-31 March 1982), 6-8; R. Boudon, *The Crisis in Sociology: Problems of Sociological Epistemology* (New York: Columbia University Press, 1980), especially Chapter 3.

25. See Abir-Am, *op. cit.* note 1, 345.

26. For a most inspiring source, see M. Augé, *The Anthropological Circle: Symbol, Function, History* (London and New York: Cambridge University Press; Paris: Editions de la Maison des Sciences de L'Homme, 1982).

27. For studies attempting to answer just this question, see B. Latour and S. Woolgar, *Laboratory Life: The Social Construction of Scientific Facts* (London and Beverly Hills, Calif.: Sage, 1979); K.D. Knorr, R. Krohn and R. Whitley (eds), *The Social Process of Scientific Investigation* (Boston, Mass.: Reidel, 1980), especially the essays by Callon and Latour.

28. See Augustine Brannigan, *The Social Basis of Scientific Discoveries* (Cambridge and New York: Cambridge University Press, 1981).

29. For some clues, see Abir-Am, *op. cit.* notes 5 and 12.