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Abstract. In general history and popular culture, the long 1960s, a period roughly beginning in the mid-1950s and ending in the mid-1970s, has been held to be a period of change. This paper offers a model which captures something of the long 1960s as a period of 'sea change' resulting from the interference of three waves. Wave One was an institutional dynamic that drew out experts from closed and hidden disagreement into situations where expert disagreement was open to public scrutiny. Wave One also accounts for the multiplication of experts. Wave Two consisted of social movements, institutions and audiences that could carry public scrutiny and provide a home for sea-change cultures. In particular, Wave Two provided the stage, audience and agents to orchestrate a play of disagreeing experts. Wave Three was marked by an orientation towards the self, in diverse ways. Modern science studies is a phenomenon of Wave Three. All three waves must be understood in the context of the unfolding Cold War.

If we collect together and review the secondary literature on science and technology in the 1960s, alongside general histories of the period, what patterns can be found? Can a synthesis be made? What is clarified? What is obscured? What is left out? In the science-studies literature we lack sufficiently synthetic accounts,[1] while the general histories have barely begun to address science and technology, beyond a handful of familiar topics: the pill, elite critical thought, television, the Apollo programme.[2]

This essay asks whether the 'long 1960s' is a helpful category for historians of science and technology. It sets out a 'three-wave' model that, I propose, captures much of the best analysis from scholarship that has otherwise not been connected. Wave One is an institutional dynamic drawn from institutional history. Wave Two centres on

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1 The best surveys are E. Mendelsohn, 'The politics of pessimism: science and technology circa 1968', in *Technology, Pessimism and Postmodernism* (ed. Y. Ezrahi, E. Mendelsohn and H. Segal), Amherst, 1994, 151–73; J. R. Ravetz, 'Orthodoxies, critiques and alternatives', in *The Companion to the History of Modern Science* (ed. R. Olby *et al.*), London, 1990, 898–908.

2 For example, otherwise admirable texts such as D. Farber, *The Age of Great Dreams*, New York, 1994; and his edited collection *The Sixties: From Memory to History*, Chapel Hill, 1994; M. Isserman and M. Kazin, *America Divided: The Civil War of the 1960s*, Oxford, 2000; and A. Marwick, *The Sixties: Cultural Revolution in Britain, France, Italy, and the United States, c.1958–c.1974*, Oxford, 1998, feature science and technology in a perfunctory way. An exception is H. Brick, *Age of Contradiction: American Thought and Culture in the 1960s*, New York, 1998.

social movements, on which there is an immense literature. Wave Three features the distinctive long-1960s strategy of analysis turned inward, of a self-critical self-consciousness. The three waves are viewed not as consecutive events but as phenomena that interfere, sometimes building on each other, sometimes cancelling each other out. They are not confined to one place, but nevertheless their meanings are locally understood. Indeed, one of the main implications of this paper is that we need locally sensitive but internationally comparative studies of the sciences and technology in the long 1960s. At present national stories of transnational phenomena are often accounted for through nationally specific causes, which is inadequate.

The three-wave model organizes content from secondary literature in a way that builds to make a positive and novel contribution to historians' understanding of science and technology in the twentieth century in general and the long 1960s in particular. The three waves interfere to produce what I label a sea change. One measure of the helpfulness of a periodization is the extent to which it prompts further questions, research programmes and lines of inquiry. I will indicate where these appear and might lead. The intention throughout has been to make bold, positive statements where possible. If they are quibbled with, transformed or knocked down by future study I will not be unhappy.

The 'long 1960s'

No interesting periodization would have the 1960s beginning on New Year's Day 1960 and ending on 31 December 1969. However, there is no consensus on when a long 1960s might begin or end.[3] Subjects shape historiography. Historians of popular musical culture might choose a long 1960s that stretched from the birth of rock 'n' roll (1956, say) to Altamont (1969) or punk (1976).[4] A media commentator has proposed a long 1960s starting with the first artificial satellite (1957), with its implications for communications and surveillance, and ending with the media-saturated televisual spectacle that was Watergate (1974).[5] Social-movement historians have a surfeit of dates, but one suggested long 1960s runs from Greensboro (1960) to the Congressional approval of the Equal Rights Amendment (1972).[6] Another might end with the United States' departure from Vietnam (1975). Historians narrowly focused on the history of the New Left might choose a long 1960s from the formation of the Students for a Democratic Society (SDS – 1960) to the internal splits over violent action that ended SDS as a political force (1969). In *The Sixties* Arthur Marwick favours an economically

3 Note, too, that one can regard the long 1960s as containing critical moments of change without subscribing to a 'long 1960s'. A plausible case can be made, for example, for the shattering importance of the years 1971–4: the end of the Bretton Woods monetary exchange system (with its implications for how investments were permitted to flow) and the oil crisis (with its consequent flood of new petro-money to be invested).

4 P. Friedlander, *Rock and Roll: A Social History*, Boulder, CO, 2006.

5 J. Hoberman, *The Dream Life: Movies, Media, and the Mythology of the Sixties*, New York, 2003, 11–12.

6 T. H. Anderson, *The Movement and the Sixties*, Oxford, 1995, 13.

oriented long 1960s from the growing recognition of the economic power of youth (*c.*1958) to the oil shock (*c.*1974).[7]

Other historians have identified shorter periods of profound flux within the long 1960s. In such formulations, a 'short 1960s' might be sandwiched between the breakdown of the contained buttoned-down societies of the early Cold War and the reconstruction of a different order. Especially pertinent is Hollinger's emphasis on the early 1960s. While rightly rejecting 'The Sixties' as a 'historiographic monster' that obscures or even displaces true historical understanding, Hollinger insists that the early 1960s witnessed a 'number of important transformations, trajectories and tensions' that demand our attention.[8] In particular, he gives a specific contextual argument for the rise of a 'radically reoriented learned discussion of the entire scientific enterprise', not least Kuhn's *Structure* (1962), to be assessed later in this paper.

While these commentators disagree about dates, they agree that a period of unusual transformation was contained within the long 1960s. Set against them, in principle if not always in practice, are commentators who favour a chronological framework that stresses continuity rather than discontinuity. Foremost among these accounts are those that emphasize the Cold War as a continuing and primary organizing process. Still more broadly, Hobsbawm proposes a 'short twentieth century' roughly coterminous with the existence of the Soviet Union, in which the 1960s are demoted relative to the outbreak and end of wars.[9] Nevertheless, the 'long 1960s' form a useful periodization for historians of science and technology. The label draws attention to some continuity of aspirations and attitudes, actions and institutions, that together were seen to be part of a process of change. A plausible account of the long 1960s would credit both continuous and discontinuous features. In particular, the sciences and techniques promoted under Cold War regimes were partly constitutive of long-1960s transformations.

'Crisis' talk

To get a handle on what is at stake, drop in at Friends House, Euston Road, London in November 1970. Over three days an average of seven hundred people per day gathered at a meeting organized by the British Society for Social Responsibility in Science (BSSRS) to discuss the Social Impact of Modern Biology.[10] It is an exemplary gathering for understanding the history of science in the long 1960s. Its overflowing attendance suggests that it asked a question resonant for its audience. Many, perhaps, had come to hear the 'constellation of scientific superstars'.[11] But the conference was also unusual

7 Marwick, op. cit. (2), 7–8.

8 D. A. Hollinger, *Science, Jews, and Secular Culture: Studies in Mid-twentieth Century American Intellectual History*, Princeton, 1996, 4–7.

9 E. J. Hobsbawm, *Age of Extremes: The Short Twentieth Century, 1914–1991*, London, 1994.

10 The politics, style, size and form of the BSSRS changed several times. Rosenhead has argued that until 1970 it can be thought of as a 'classical operation with a distinguished and distinctive pedigree: the establishment of a leftish pressure group to enlighten a liberal elite without alarming it excessively'. J. Rosenhead, 'BSSRS – ten years', *Science for People* (1979), **43/44**, 23–5. The proceedings of the conference can be found in W. Fuller (ed.), *The Social Impact of Modern Biology*, London, 1971.

11 Rosenhead, op. cit. (10).

for its breadth: speakers included not only establishment 'superstars' but young radicals, historians of science and technicians, while the voices from the floor were still more diverse.[12]

The BSSRS was, crucially, both a revival and a neonate. Some of its advocates saw in it a return to 1930s radicalism, while others saw novelty, a split that can only inadequately be mapped on to 'old left' and 'New Left'. In February 1969 a core group of scientists, including Maurice Wilkins, cosmic ray physicist and Nobel prizewinner Cecil Frank Powell, medical scientist R. L. Smith, physicist D. K. Butt and young Imperial College biochemist Steven Rose, were drumming up support for a new scientist–activist movement. In a drive to find one hundred sympathetic 'prominent scientists', they wrote to Joseph Needham:

> Over the last few months a group of scientists brought together by a common concern for the future of science and society have been discussing the need for an organisation which will be concerned with the social responsibilities of the scientist. Many scientists have expressed their concern at the new evidence of the abuse and moral compromise of science that is now occurring. Thus the existence of classified scientific research in Universities, the current application of science to techniques of chemical and biological warfare, the potential abuse of discoveries in molecular biology, have and are giving rise to grave disquiet amongst scientists. There has occurred a decline in morale among scientists and a loss of esteem for science in the community at large. Furthermore, the future of science is threatened by the hostility now felt by young people towards science. These developments we believe originate from the mis-use and abuse of science.[13]

An inaugural meeting was held in April 1969, with addresses by an impressive roster of professors.[14] Over three hundred people attended, including the elderly and ill J. D. Bernal. The audience, 'healthily mixed, embracing "scientists, students and others interested in science"', were challenged by Maurice Wilkins, in what was to become the keynote theme: 'We have to face the fact that there is a crisis in science today.'[15] While debate centred on some familiar concerns such as the status of engineers and the arts–science divide in secondary education, a sign of things to come came from the intervention of a young statistician: 'despite the plushy surroundings of this conference', said Jonathan Rosenhead, 'we have to face the fact that a lot of people are going to be afraid that we are preaching subversion'. The BSSRS would indeed become a broad church and it is precisely because its active membership stretched from elite

12 While some might feel that the meeting already conceded too much room, the radicals felt it nevertheless 'left little scope for those who were inexpert or poorly connected; indeed various steps were taken which had the effect of discouraging grass-roots activity by the membership'. Rosenhead, op. cit. (10).

13 Wilkins *et al.* to Needham, 20 February 1969, Cambridge University Library, Needham Papers. Needham agreed to be a founder member. The archives of his papers are a major source of insight into the changing nature of the BSSRS.

14 Speakers were Wilkins, Essex mathematician Professor G. A. Barnard, Sussex biologist Professor J. Maynard Smith, R. L. Smith of St Mary's Hospital Medical School, Powell, A. N. Oppenheim of the LSE, and Heinz Wolff, who was at NIMR. Chairs included Professor E. H. S. Burlop, Steven Rose, Professor Henry Miller (vice chancellor at Newcastle), and molecular biologist Professor Martin Pollock of Edinburgh.

15 'Inaugural meeting of BSSRS – April 19th Saturday', undated. Cambridge University Library, Needham Papers.

scientists to 'subversives', at least in the early years, that the society provides a good case study of sea-change arguments.

At the meeting on the Social Impact of Modern Biology in 1970, Wilkins reminded the audience again of the 'crisis in science today [which] has not only direct bearing on the question of our survival but is of deep significance in relation to our fundamental beliefs and our value-judgments':

> The main cause is probably the Bomb: scientists no longer have their almost arrogant confidence in the value of science. At the same time non-scientists increasingly and openly question the value of science. There are extremists who go further and object to rational thought as a whole.[16]

Wilkins portrayed the scientific community as deeply split over its response to this growing 'breakdown in confidence in reason' among the many. Following the use–abuse model, Wilkins concluded that it was vital not to 'over-react' lest this lead to 'overall condemnation of science', but to be socially responsible and choose to pursue science that, to borrow Peter Medawar's phrase, provided 'imaginative uplift'. Seventeenth-century natural philosophy had possessed this quality. It was

> still possible today to catch some of that imaginative uplift. Consider for example the branch of science that deals with nervous systems. Such science should not only lead to control of nervous disorders but, by providing understanding of how the human brain works, should throw new light on the nature of mind itself. The understanding should (to use hippie language) expand the mind … [Such] self-knowledge should greatly influence our values. Science is valuable, then, in terms of the self-knowledge that it gives.

Notice how this argument was structured. 'Disturbances', partly originating in 'general student unrest and political frustration', but also originating 'directly … with science, with its organization and social priorities', contributed to a sense of 'crisis', and this 'crisis in science is only part of a larger cultural crisis'. This in turn led to scrutiny of the sciences in a form of self-analysis, leading to the valorization of science's potential contribution to self-knowledge. Wilkins ended by likening this 'very critical phase in the development of science' to another, 'the critical phase of the 17th century'. Where that phase provoked experimental solutions such as the Royal Society, the current phase called for more 'experiments that may produce unexpected results'.

Other elite 'leftish' scientists made similar arguments. Fresh from his widely publicized involvement in the 1968 Paris events, Jacques Monod proposed that science, as a 'strictly objective approach to the analysis and interpretation of the universe … [which] must ignore value judgements', was destroying any and all 'traditional systems of value':

> Hence modern societies, living both economically and psychologically upon the technological fruits of science, have been robbed, by science itself, of any firm, coherent, acceptable 'belief' upon which to base their value systems. This, probably, is the greatest revolution that ever occurred in human culture. I mean, again, the utter destruction, by science, by the systematic pursuit of objective knowledge, of all belief systems, whether primitive or highly sophisticated,

16 M. Wilkins, 'Introduction', in Fuller, op. cit. (10), 5–10.

> which had, for thousands of years, served the essential function of justifying the moral code and the social structure.[17]

This 'revolution', argued Monod, was 'at the very root of the modern *mal du siècle*', especially among 'the young'. When Monod looked out from the Institut Pasteur to see the 'revolution' on the streets in 1968, one has to imagine him thinking the events were caused by science.

The outsider physicist David Bohm presented a different argument, although one fully in tune with the preceding tone. Bohm's involvement in radical student politics while attending Berkeley in the 1930s had rebounded many times on his later life. He had been barred from working at Los Alamos despite being Oppenheimer's protégé, was fired from Princeton after pleading the Fifth Amendment before the House Un-American Activities Committee despite being Einstein's co-worker, and had moved on to Brazil and Israel before settling in England in 1957. Through the 1960s Bohm was professor of theoretical physics at Birkbeck College, London. His paper at Friends House portrayed science as an arrogant priesthood: he drew direct parallels with medieval scholasticism, presumably having read Frances Yates. But he portrayed problems in science as manifestations of a 'general social condition: fragmentation'. Fragmentation had been a key theme, too, of Wilkins's talk, and Wilkins was clearly intellectually indebted to Bohm in the way he thought of crises in science. Bohm prescribed a new science of 'holocyclation' to unite a fractured world.

Wilkins was not the only contemporary commentator to identify 'crises'.[18] Barry Commoner, for example, had written in *Science and Survival* (1966) of the crises of modern biology, by which he meant a science that was being torn apart by the conflict between traditional organismic science, derived from natural history, and an aggressive new molecular biology. But Wilkins's 'crisis' is interesting because he portrayed it as a momentous condition afflicting the sciences more broadly. The point is not whether Wilkins's diagnosis was correct.[19] Rather, it is that nearly all speakers at the conference offered their own, often very individual, folk theory of what was wrong. If we look outside the conference we find even more. A personal favourite is Heinz Wolff's 'container' theory of modern crises. It turns out it is not that we lack theories of what happened in the long 1960s, but rather that we have a diversity of divergent accounts.

I will later examine the two talks, by Jacob Bronowski and Robert M. Young, that were judged at the time, albeit by different audiences, to have had the most electrifying effect. For now, note that the Social Impact of Modern Biology conference was large, encompassed a broad spectrum of positions and attitudes among the speakers, and provided a forum which aired sharply divergent accounts of what was amiss with

17 J. Monod, 'On the logical relationship between knowledge and values', in Fuller op. cit. (10), 11–12.

18 See D. Steigerwald, *The Sixties and the End of Modern America*, New York, 1995, 243–71, for 1960s crises more generally.

19 The quantitative evidence, for example, is problematic. Amitai Etzioni and Clyde Z. Nunn reported the results of the Louis Harris poll that the proportion of public expressing 'great confidence' in the people 'running science' had dropped from 56% (1966) to 37% (1972). A. Etzioni and C. Z. Nunn, 'Public views of scientists', *Science* (1973), **181**, 1123. This, of course, was not a measure of confidence in science.

science. This divergence permitted Wilkins's label of 'crisis' to become a commonplace. The meeting also witnessed divergent views on the very nature of scientific knowledge, ranging from an establishment use–abuse model on one side to a radical critique of scientific knowledge shaped by ideology on the other.

The question is: how did we get to a world where a gathering like the Social Impact of Modern Biology was possible?

Sea change: three waves

> Full fathom five thy father lies:
> Of his bones are coral made:
> Those are pearls that were his eyes:
> Nothing of him that doth fade
> But doth suffer a sea-change
> Into something rich and strange.
>
> *The Tempest*, Act I Scene ii

Something about science changed in the long 1960s. The aim here is to characterize what changed and to analyse accounts of such change. In particular, I will describe three waves of change that, together, amount to a sea change. Wave One can be called the Balogh wave. In his unjustly neglected book, *Chain Reaction: Expert Debate and Public Participation in American Nuclear Power, 1945–1975* (1991), Brian Balogh presented a model that accounted for how and why expert debate moved from a place behind closed doors to become performance in public forums. Furthermore, the dynamic Balogh described provides a clue as to why a diversity of experts was generated and was visible in the period that interests us. Once divergent expert views could be compared in public then a host of critical questions followed. Why did experts disagree? Did they share a 'method'? If not, what could be said of a diversity of scientific methods? Who was right? Who should say who was right? A historian can add to such questions: was such disagreement new or was it the public scrutiny of disagreement that was new?

Balogh's model, which draws substantially on the work of the *éminence grise* of American environmental history, Samuel P. Hays, emphasized an internal organizational dynamic that first deepened reliance on experts, and placed them in private opposition, then dragged the disagreements between experts into public view. But Balogh does not have much to say about how expert knowledge was interpreted, reinterpreted, used and countered within broader society. Specifically, to the first wave we must add a second. Social and cultural historians have appealed to social movements to account for the energy, radicalism and tumultuous change of the long 1960s. Historians of science need to draw on their work. We need to ask how science featured as subject, object and tool of social movements – Wave Two. By doing so we will accomplish two things. We will be able to understand what kind of public might be ready to turn the regard of public disagreements between experts into something approaching distrust in authorities, among a diversity of reactions. This will be where Wave One interferes with Wave Two. But we will also be making a contribution to the general history of the long

1960s, which discusses technology only in an abbreviated form and barely acknowledges science at all. 'Scientology' and 'science fiction' may get index entries but 'science' does so much less often.

Some of the best recent general histories of the long 1960s have stressed that alongside familiar currents associated with the political left there flourished not only an intellectual resurgence on the right, but also, crucially, an entrepreneurialism or individualism that was prominent throughout.[20] Historians of science have demonstrated that science in the 1970s and 1980s, particularly the life sciences, responded in new ways to market demands and the forces of commercialization.[21] The question of Wave Three is simply this: what connections can be drawn between the distinctive individualism and entrepreneurialism of the long 1960s and particular trends in the sciences, including commercialization, sociobiological evolutionary arguments and certain representations of the scientist in the late 1960s, 1970s and 1980s? Some general history has begun to ask parallel questions.[22] But my argument goes further than merely an exercise in identifying and accounting for influences across decades. Wave Three is the hardest to describe but also the most profound. Yet Wave Three can be identified by common features of self-awareness, self-scrutiny, even self-analysis. This was a self-consciousness that, even when inquiry was directed at other subjects, tended towards self-description. Here I will call modern science studies as witness to its own birth.

Wave One: the Balogh model

Balogh's *Chain Reaction* described an institutional dynamic. It is superficially a case study of experts and expertise in and around the Atomic Energy Commission (AEC), the civilian body set up in 1946 to manage the Manhattan Project inheritance of laboratories, nuclear factories and nuclear policies in the US. In its early years, notes Balogh, experts within the AEC designed policy agendas with little reference to public demand. Experts might and did disagree, but debate was contained within AEC committees and boards and was invisible to an outside world. However, faced with flagging demand for the AEC's product – electricity from nuclear power stations – the experts and bureaucrats of the AEC were forced to appeal outwith the AEC in order to build

20 G. Andrews, R. Cockett, A. Hooper and M. Williams (eds.), *New Left, New Right and Beyond: Taking the Sixties Seriously*, Basingstoke, 1999, on the New Right; Marwick, op. cit. (2), for entrepreneurialism across movements in the long 1960s; Brick, op. cit. (2), 117, for entrepreneurialism, and 188–9, for the New Right.

21 S. Krimsky, *Genetic Alchemy: The Social History of the Recombinant DNA Controversy*, Cambridge, MA, 1982; S. Wright, 'Recombinant DNA technology and its social transformation, 1972–1982', *Osiris* (1986), **2**, 303–60; M. Kenney, *Biotechnology: The University–Industrial Complex*, New Haven, 1986; D. Dickson, *The New Politics of Science*, Chicago, 1988, 243–60; S. Wright, *Molecular Politics: Developing American and British Regulatory Policy for Genetic Engineering, 1972–1982*, Chicago, 1994; A. Thackray (ed.), *Private Science: Biotechnology and the Rise of the Molecular Sciences*, Philadelphia, 1998; S. Smith Hughes, 'Making dollars out of DNA', *Isis* (2001), **92**, 541–75.

22 P. Jenkins, *Decade of Nightmares: The End of the Sixties and the Making of Eighties America*, Oxford, 2006.

demand. Two processes were now combined. First, experts as specialists were oriented towards divergent, even contradictory, missions. This divergence in institutional interests lay at the root of the internal disagreements that had taken place behind closed doors. Different positions were now being articulated outside the closed world of the meeting rooms of the past. Second, external bodies were forced to employ more experts in order to make sense of, and judgements on, the expert claims emanating from the AEC. These experts, too, were specialists aligned to their particular bodies' projects and thus made different cases in public, which in turn required further expert interpretation. This institutional dynamic therefore created a demand for increasing numbers of experts (a 'chain reaction'), and, as an unintended by-product, the conditions for the spectacle of expert disagreement in public.

Balogh's case study centred on one body (the AEC) in one country (the United States). But I wish to draw attention to the model, not to its specific application. While the 'chain reaction' seems expressly invented to describe the dynamics of nuclear expertise, Balogh's model is generalizable. Hays has described a very similar dynamic in environmental policy-making over the same period.[23] When the dynamic is placed alongside good cultural histories of environmental science, such as Russell's *War and Nature*, a remarkably similar account can be built. So, for example, there existed expert knowledge about the deleterious effects of DDT from 1946, but assessments and arguments were internal and not readily visible from outside.[24] Only later, by the long 1960s, as we shall see, were there sufficient accessible divergent expert views about the effects of pesticides that they could be orchestrated to become a publicly visible conflict. It is also quite likely that a Wave One-style analysis would explain why large databases were not publicly regarded as a threat to personal privacy in the 1940s but suddenly were so regarded in the early 1960s.[25]

Balogh's model gives the demand-side picture that helps us understand one of the supply-side features of the post-war decades: the rapid expansion and growth of higher-education institutions. This growth entirely complemented the Cold War demand for technical expertise relevant to building missiles, radar, nuclear warheads, eavesdropping networks and jet aircraft. Across the Western world, new universities were established and old ones reformed and expanded to produce expert administrators of what Balogh labels the 'proministrative' state. The proministrators were produced to meet the demand for experts produced by Wave One, the Balogh institutional dynamic. The dynamic is well described as a 'chain reaction', making splits in expertise publicly visible and multiplying their number at each turn.

Though largely absent from Balogh's own account, journalists and the changing nature of journalism were critical to Wave One. First, at a simple level, the media were carriers of expert views. Despite fears that the public was losing interest in science stories, quantitative evidence suggests that readership of science and invention stories

23 See, for example, S. P. Hays, *Explorations in Environmental History*, Pittsburgh, 1998, 185–97.

24 E. Russell, *War and Nature: Fighting Humans and Insects with Chemicals from World War I to* Silent Spring, Cambridge, 2001, 158–63.

25 J. Agar, *The Government Machine*, Cambridge, MA, 2003.

increased during the later long 1960s.[26] Second, journalists could ventriloquize, even replace, expert voices. In this way, journalists would not merely represent experts but anticipate and reconstruct what an expert might say. Finally, journalists became sources of criticism of science and also critics of other journalists insufficiently critical of science. This kind of complaint became articulable: Science journalists 'are a bunch of patsies prone to uncritical acceptance of anything we are told by our authorities – our authorities being doctors and scientists'.[27] In particular, the notion developed that science reporters should report on science just as political reporters report on politics. Daniel S. Greenberg writing in *Science* is perhaps the paradigmatic example. This was an aspect, argues Nelkin, of the rise of the advocacy press, since to reject the received form of objectivity in journalism, granting equal time to each side, was to be drawn into further questioning of objectivity.[28] The relevance to Wave One is that an advocacy press based its authority on its own sources of expertise. Even some traditional science journalists 'adapted their writing to the spirit of the times', becoming critical in the long 1960s.[29]

But why should such different Wave One dynamics all coincide in time? There are two kinds of answer. First, the institutional dynamics producing experts were not independent. The demand for nuclear expertise, for example, created in its wake a demand for ecological expertise.[30] The demand for large computerized databases for cryptanalysis and early-warning systems produced the technologies, such as symbol searching, that were identified as threats to personal privacy. Second, the unparalleled military improvisation of technical projects in the Second World War, further reinforced by the immediate start of the Cold War, provided a common starting point for these institutional dynamics. In short, in the years around 1945 experts were likely to be hidden in internal committees, while by the long 1960s they were more numerous and more likely to be drawn into conflicting positions. The moment of the long 1960s took the form it did because an institutional dynamic softened a rigid enclosure of expertise that contingently and extraordinarily was set in place in the mid-twentieth century.

We can see, however, what is explanatorily missing from Wave One and what is needed from Wave Two. Balogh's model does not indicate where and why the agents emerged who could turn observable discord into observed discord. It certainly does not describe the interests, demands, cultures, motives or lives of these observers. Nor does Wave One explain who the orchestrators of expert disagreement might be.[31] Wave Two

26 C. Z. Nunn, 'Is there a crisis of confidence in science?', *Science* (1977), **198**, 995.

27 Henry Pierce of the *Pittsburgh Post Gazette*, 1966, quoted in D. Nelkin, *Selling Science: How the Press Covers Science and Technology*, New York, 1995 (revised edition; first published 1987), 89.

28 Nelkin, op. cit. (27), 89–93.

29 Nelkin, op. cit. (27), 93. Nelkin offers as an example David Perlman, whose career stretched from the 1950s to the 1980s, but whose tone became critical around 1972.

30 See discussion of the Odums in J. B. Hagen, *An Entangled Bank: The Origins of Ecosystem Ecology*, New Brunswick, 1992.

31 Indeed, institutionalist Wave One literature can actively reject the importance of such orchestrators. Examine the scarcity of references to Rachel Carson, for example, in S. P. Hays, *A History of Environmental Politics since 1945*, Pittsburgh, 2000. Or again: 'The entire subject of environmental affairs is attributed to the writings and ideas of some widely read author such as Rachel Carson or Paul Ehrlich, when, in fact, the source of those affairs is found far more in the immediate human circumstances that people experience.' S. P. Hays,

will tell us that the observers were members of new social movements and the orchestrators key figures in such movements.

Wave Two: social movements

Social movements were highly visible features of long-1960s politics and culture. Studies of Wave Two, the wave carried by social movements, make up the vast bulk of literature on the period. The relevant question here is: what roles must social movements play in an account of changing science in the long 1960s?

'Social movements' were the constructs of social science as much as social movements were its objects of study. In a process reminiscent of Balogh's model, social scientific expertise about social movements proliferated from the 1950s to the 1960s. Studies ranged from collective-behaviour theory, drawing on traditional work on the irrationality of crowds, through Marxist accounts, to New Social Movement approaches, which significantly emphasized the importance of self-identification, Goffman-inspired frame accounts and a variety of other theoretical stances.[32] Among these approaches were those that picked out an oppositional core to social movements, a 'counter-culture'.[33] Nevertheless, for example, the proportion of American students who identified with a 'counter-culture' was tiny compared to those who were sustained and changed by social movements more generally.[34]

This secondary literature and first-hand accounts allow a number of features of social movements to be made out. First, social movements had a distinctive fluid, network form. While there were prominent spokespeople, heroes and revered ancestors, each social movement was a patchwork of sometimes short-lived organizations and campaigns. What gave a social movement cohesion was a rough consensus on ultimate targets, such as nuclear disarmament or the removal of racism. Such targets were boundary objects that enabled coordination within the network-like movement. The presence of targets strongly promoted a polarized culture that structured much of the literature, speeches, actions and identities of the movements. Social movements thus shared an oppositional tone. This matches a cliché: if there were common targets across the social movements of the long 1960s, then they would include opposition to 'authority', the 'hierarchy', the 'establishment', 'technocracy',[35] the 'system', 'the man'.

'Introduction: an environmental historian amid the thickets of environmental politics', in *idem*, op. cit. (23), 8–11, 14–25.

32 D. della Porta and M. Diani, *Social Movements: An Introduction*, Oxford, 1999; M. Giugni, D. McAdam and C. Tilly (eds.), *How Social Movements Matter*, Minneapolis, 1999; A. E. Hunt, *The Turning: A History of Vietnam Veterans against the War*, New York, 1999.

33 P. Braunstein and M. W. Doyle, 'Historicizing the American counterculture of the 1960s and '70s', in *Imagine Nation: The American Counterculture of the 1960s and '70s* (ed. P. Braunstein and M. W. Doyle), London, 2002, 5–14; T. Roszak, *The Making of a Counter Culture*, Berkeley, 1995 (first published 1968).

34 Roszak, op. cit. (33), 40. Anderson, op. cit. (6), 17. Furthermore, only 13% of US college students in 1969 identified themselves as 'new left' (and only 3% outside of college).

35 Roszak, op. cit. (33).

Second, social movements learnt from each other, exchanging and transmitting members, ideas and techniques.[36] The relevant social movements here include, but are not restricted to, civil rights, anti-nuclear movements, anti-Vietnam movements, political activism typified by umbrella groups such as SDS, new environmentalism and feminism. Each movement, but in particular the civil rights movement of the 1950s, became a model for later movements, just as they in turn drew inspiration, techniques and other lessons from even earlier tides of activism, including the anti-slavery campaigns of the eighteenth and nineteenth centuries. Collectively, there was a 'Movement', a term with considerable resonance. It is an actors' category, while as an analyst's category it emphasizes the social foundations of historical change. Some authors map the long 1960s precisely onto the rise and fall of these social movements that made up the Movement. Others insist on a less rigidly institutional analysis. Anderson, for example, insists that 'movement' is a useful term when it 'connotes all activists who demonstrated for social change. Anyone could participate: There were no membership cards. Sara Evans, a civil rights volunteer, later wrote, "Above all the term 'movement' was self-descriptive. There was no way to join; you simply announced or felt yourself to be part of the movement"'.[37]

Equipped with a sense of these terms we can now see how science figures in Wave Two. Science and scientists featured in social movements in three relationships. First, certain scientists and sciences were objects of criticism because they were seen within social movements as tools of their opponents. Second, places where science was done became theatres for social-movement demonstration. Third, scientists-as-activists were contributors to social movements. This third relationship took two forms: their science could be incidental to their involvement in a movement, or, most significantly, it could be the cause, the tool, the object and subject of activism. The cases considered below involve all three of these relationships.

The civil rights and anti-nuclear movements furnish candidates for science-as-an-object-of-criticism. Henry E. Garrett, professor and head of the psychology department at Columbia University, testified in support of segregation in the Davis vs County School Board case of 1952.[38] He argued against anthropological studies of the Franz Boas school, and against the position held by Ashley Montagu, who had led the UNESCO 1950 statement on race which questioned typological conceptions of race and innate racial differences in intelligence. Psychological evidence was also integral to the case that overturned the conclusions of Davis vs County School Board – Brown vs Board of Education (1954).[39] In such cases science-as-the-tool-of-the-opponent (Garrett and others) was countered by expert testimony (such as Montagu's) mobilized by bodies within the civil rights movement such as the National Association for the

36 Mendelsohn, op. cit. (1), 159.

37 Anderson, op. cit. (6), p. x.

38 A. S. Winston, 'Science in the service of the Far Right: Henry E. Garrett, the IAAEE, and the Liberty Lobby', *Journal of Social Issues* (1998), **54**, 179–210. W. H. Tucker, *The Science and Politics of Racial Research*, Urbana, 1994.

39 J. P. Jackson, Jr, 'Creating a consensus: psychologists, the Supreme Court, and school desegregation, 1952–1955', *Journal of Social Issues* (1998), **54**, 143–77.

Advancement of Colored People (NAACP). The environmental–hereditarian controversy continued after civil rights legislation was enacted. Arthur Jensen and William Shockley appealed directly to reactionary public individuals and groups in the 1960s and the 1970s, an appeal rebutted, also in public, by critics. The end effect, as in Wave One, was 'socially visible' disagreement.[40]

At first glance, nuclear science is also a candidate for science-as-an-object-of-criticism. Britain's Campaign for Nuclear Disarmament (CND), for example, chose the UK's nuclear weapons laboratory, Aldermaston, as terminus of its Easter marches. But such an analysis is too simplistic. CND's focus, as a lived experience, was as much on the self as on the products of Cold War science.[41] As we shall see, this is an early intimation of Wave Three. CND was a form of revivalism: religious figures, organizations and language, not least J. B. Priestley's 'moral crusade', framed the protests. These were protests against the immorality of defending affluent society with nuclear weapons.[42] Nevertheless, CND also illustrates social movements' capacity to provide temporary institutions in which to learn what was possible. In the words of one commentator, it was a 'visible social alternative', even an 'imminent counter-culture that merged personal expressiveness with political activism', an exemplary 'march of the dissenting young'.[43]

From the days of the Manhattan Project, scientists offered the 'most serious resistance to the use of the Bomb', but their critique was of use (and abuse) rather than of the science.[44] Nevertheless, there were seeds of a critique of use–abuse instrumentalism. While science was seen by some as a neutral tool that was being abused rather than well used, for CND nuclear science was a tool it would rather did not exist in the world. A case could thence be made that, from Leo Szilard onwards, nuclear control or disarmament campaigns were one source of a major intellectual strand of the sea change because they prompted questions about the neutrality of science. Scientists' arguments could be appropriated and reinterpreted as more generalized critiques.[45] But the moral-crusade rhetoric of CND and aligned bodies supplanted rather than complemented scientist-led critiques of nuclear weapon policy. Before 1958, many

40 Y. Ezrahi, 'The authority of science in politics', in *Science and Values: Patterns of Tradition and Change* (ed. A. Thackray and E. Mendelsohn), New York, 1974, 215–51, 232: 'the principal audience of the debate was not so much the scientific community itself but the lay public, the contestants were naturally led to invest much effort in building indirect evidence through which science is made more socially visible in order to persuade the public that their opinion is more representative of the true scientific consensus than that of their rivals'.

41 F. Parkin, *Middle Class Radicalism*, Manchester, 1968; J. Mattausch, *A Commitment to Campaign: A Sociological Study of CND*, Manchester, 1989.

42 V. Bogdanor and R. Skidelsky, *Age of Affluence, 1951–1964*, London, 1970.

43 Nigel Young, quoted in J. Green, *All Dressed Up: The Sixties and Counterculture*, London, 1999, 24–5.

44 L. S. Wittner, *The Struggle against the Bomb*, Volume 1, *One World or None: A History of the World Nuclear Disarmament Movement through 1953*, Stanford, 1993, 29.

45 A possible example is the accusation made by the Greater St Louis Committee for Nuclear Information that Edward Teller had 'a vested interest in arguing that atomic fallout was not harmful. In response, Teller attacked CNI member Edward U. Condon's claim that fallout was dangerous, claiming that it was politically motivated and suspect scientifically because Condon had been investigated by the House Un-American Activities Committee'. K. Moore, 'Organizing integrity: American science and the creation of public interest organizations, 1955–1975', *American Journal of Sociology* (1996), **101**, 1592–627, 1614.

prominent interventions had been led by scientists. Examples include the Chicago scientists' opposition to the use of the Bomb before Hiroshima, the foundation of the *Bulletin of Atomic Scientists*, the Russell–Einstein manifesto of 1955, the first Pugwash conference of 1957 and the petition organized by Linus Pauling in 1957–8, signed by 11,038 scientists from forty-nine countries, including thirty-seven Nobel laureates.[46] Scientists were not so prominent after 1958.[47]

Social movements learnt from each other. In many ways, environmental activists appropriated the roles and arguments of activist nuclear scientists. For example, Rachel Carson repeatedly drew parallels between radiation and pesticides in arguments in *Silent Spring* (1962). She could be confident that her reference to *Lucky Dragon*, the Japanese fishing vessel contaminated by fallout, would be familiar to her audience. An audience that had sat, terrified, through the Cuban missile crisis, could translate from the effects of one known insidious invisible contaminant to make another unknown meaningful and alarming.

Carson is an exemplary Wave Two orchestrator, and *Silent Spring* an exemplary Wave Two text. In the early 1950s at the Fish and Wildlife Service (FWS), Carson had been able to sit at the centre of three networks. First, the reports of different experts such as oceanographers, marine biologists, ornithologists and ecologists crossed her desk, an obligatory passage point in the FWS's review process. Second, through her contacts with bodies such as the Audubon and Wilderness societies, Carson was in touch with naturalists and nature writers. Finally, through her skilful agent she could tap the resources of the publishing world. This position, combined with an enviable gift of expression, provided the basis for the publishing successes of *The Sea Around Us* (1951) and *Silent Spring*. More importantly, Carson was ideally placed to orchestrate the public display of expert difference.[48] This staging and demonstration of expert disagreement is at the heart of *Silent Spring*. Wave One had produced the experts and divergent expert views. Wave Two presented and observed these divergent views in public and linked them to the causes of social movements. Carson issued a call to arms. The public that 'endures' pesticide effects had the right to know and an obligation to act. The members of social movements, in this case the new environmentalism, provided the core readership and audience for Carson's public orchestration. New readers, in turn, became potential new members, a growing audience that could be upset, concerned and eventually curious about rival expert claims. Social movements were resourceful institutions that could sustain scrutiny of expertise. Barry Commoner

46 L. S. Wittner, *The Struggle against the Bomb*, Volume 2, *Resisting the Bomb: A History of the World Nuclear Disarmament Movement, 1954–1970*, Stanford, 1997, 39.

47 For the criticism of scientist–activists by strategic analysts, such as Albert Wohlstetter and Herman Kahn in the context of 1960s debates, see S. Hong, 'Man and machine in the 1960s', *Techné* (2004), 7, 49–77.

48 For example, Jamison and Eyerman note, 'What made it valuable and useful for the movement that eventually took form around its message was its discussion of the alternative ecological solution, "the other road" [i.e. biological controls] … As she outlined those alternatives, she once again, as in all her writings, let the scientists themselves speak, bringing not only people but dispute, contradiction, difference of opinion into the world of the expert. Perhaps even more important than the particular conflict she wrote about – between chemical and biological insect control – was *the presentation of conflict itself*'. A. Jamison and R. Eyerman, *Seeds of the Sixties*, Berkeley, 1994, 99–100. My emphasis.

addressed this moment explicitly: 'If two protagonists claim to know *as* scientists, through the merits of the methods of science, the one that nuclear testing is essential to the national interest, the other that it is destructive of the national interest, where lies the truth?' The fact that the 'thoughtful citizen' has to ask 'How do I know which scientist is telling the truth?' 'tells us that the public is no longer certain that scientists – *all of them* – "tell the truth".'[49]

This is the model through which the sea change could orchestrate the appearance of crisis. The situation in which a concerned witness is confronted by a spectacle of expert disagreement was replicated many times as Wave One interfered with Wave Two. If asked to choose, the witness was faced with a difficult choice between two experts, each claiming to 'know as scientists'. The slippage identified by Commoner, the slide from challenges to some scientists to doubt in Science ('all of them') was invited by this situation. It is a situation that called yet again for the production of more experts. As we see below, science studies has a self-interest in this moment.

A second slippage was a common feature of social movements. Competition for activists' attention, time and resources, in combination with the loose organizational structure of movements, encouraged movement between movements. For example, there is some evidence that campaigns against war in Vietnam weakened disarmament activism.[50] Alternatively, social movements could run together if the targets, good boundary objects, proved flexible enough to coordinate action among very different groups. Feenberg's account of the May 1968 events can be translated into these terms. Surveying more broadly the 'dramatic shift in attitudes toward technology that occurred in the 1960s', it was 'not so much technology', he notes, 'as rising technocracy that provoked public hostility'.[51] In Paris, in particular, when the university was read as a technocratic society in miniature, the students could make common cause with workers' movements and with French middle strata. In May 1968 student demonstrations closed universities, ten million strikers joined them and opposition to the establishment 'exploded among teachers, journalists, employees in the "culture industry", social service workers and civil servants, and even among some middle lower level business executives'.[52] Here positive notions of 'autonomy' acted as a common coordinating thread. Calls for autonomy were not calls for severance from society but identification with the 'people' against technocratic masters. 'While the May Events did not succeed in overthrowing the state', summarizes Feenberg, the ferment starting on the French campuses 'accomplished something else of importance, an anti-technocratic redefinition of the idea of progress that continues to love in a variety of forms to this day'.[53] When revolutionary ideals were scaled back to 'modest realizable goals', a successful new micropolitics of technology emerged.[54]

49 B. Commoner, *Science and Survival*, London, 1971 (first published 1966), 127. My emphasis.

50 For example, Wittner, op. cit. (46), 455.

51 A. Feenberg, *Questioning Technology*, London, 1999, 4.

52 Feenberg, op. cit. (51), 31.

53 Feenberg, op. cit. (51), 43.

54 Feenberg's examples are client-centred professionalism, 'changed medical practices in fields such as childbirth and experimentation on human subjects', participatory management and design, 'communication applications of computers', and 'environmentally conscious technological advance'.

Like the Paris streets, American campuses became theatres for anti-technocracy protest. At Berkeley, the Free Speech Movement, which began in 1964 and grew from civil rights campaigns, launched a tide of student activism. By the following year Berkeley campus was a centre of anti-Vietnam protest and organization. The Vietnam War, notes Feenberg, 'was conceived by the US government and sold to the public as a technical problem American ingenuity could quickly solve'.[55] Edgar Friedenberg's response to the call by the president of UC Berkeley for universities to be 'multiversities', putting knowledge at the disposal of society's powers (not least the military), was to call instead for the university to be 'society's specialized organ of self-scrutiny'.[56] At Princeton military-sponsored research was fiercely debated from 1967, pitting activist engineers such as Steve Slaby against Cold Warrior scientist Eugene Wigner, who likened the actions of the SDS to those of Nazi students.[57] Protest fizzled after one final summer of strikes in 1970 and a committee (chaired by Thomas Kuhn) reported that Princeton had relatively little military-sponsored research.[58] At Stanford student and faculty protests against secret contracts and classified research began in 1966. 'The extent of Stanford's classified research program', centred at the Applied Electronics Laboratory and the Stanford Research Institute (home to counter-insurgency projects), writes Leslie, 'although common knowledge among the engineers, shocked an academic community still coming to terms with the Vietnam War'.[59] In 1967 a Stanford 'student-run alternative college', the Experiment, called for the indictment of university officials and trustees for 'war crimes', while the Experiment and the local SDS chapter organized antiwar marches and campaigns.[60] April 1969 saw the occupation of the Applied Engineering Laboratory by protesters.

SDS had also organized a small sit-in against Dow Chemical at MIT in November 1967, but it was federal defence contracts at the university that triggered vehement opposition.[61] MIT received more defence research and development grants than any other university. Its Lincoln and Instrumentation laboratories, specializing in electronics and missile guidance technologies respectively, as well as the independent but adjacent MITRE labs, were very much part of the Cold War 'first line of defence'. Nevertheless, at MIT, the conversion in 1967 of the Fluid Mechanics Laboratory, from missiles, jet engines and re-entry physics to environmental and medical research,

55 Feenberg, op. cit. (51), 4.

56 Edgar Friedenberg, 'LA of the intellect', *New York Review of Books*, 14 November 1963, 11–12, discussed in Brick, op. cit. (2), 24–5.

57 M. Wisnioski, 'Inside "the system": engineers, scientists, and the boundaries of social protest in the long 1960s', *History and Technology* (2003), **19**, 313–33.

58 Wisnioski, op. cit. (57), 320.

59 S. W. Leslie, *The Military–Industrial–Academic Complex at MIT and Stanford*, New York, 1993, 242. The implication is that the technicians and scientists working directly on the defence projects were on the whole unsympathetic to the protesters. Likewise at MIT one graduate student told a reporter, 'What I'm designing may one day be used to kill people. I don't care. I'm given an interesting technological problem and I get enjoyment out of solving it.' 'Most [laboratory workers] blamed the trouble on outside agitators with no sense of the laboratory's real mission or accomplishments.' Leslie, op. cit., 238.

60 Leslie, op. cit. (59), 242–4.

61 Leslie, op. cit. (59), 235. Dow Chemical had sought graduate recruits.

provided an exemplar for the protesters.[62] In January 1969 MIT faculty members called a strike intended to 'provoke "a public discussion of problems and dangers related to the present role of science and technology in the life of our nation"'.[63] The protesters' manifesto of 4 March called for 'turning research applications away from the present emphasis on military technology toward the solution of pressing environmental and social problems'.[64] A tense standoff between protesters and Instrumentation Laboratory boss Charles Stark Draper was broken by a riot, featuring police dogs and tear gas, in November 1969. In both the MIT and Stanford cases the moderate protesters won. The Instrumentation Laboratory was divested from MIT to become the independent Charles Stark Draper Laboratory in 1970, while the Stanford Research Institute was also divested and its campus annex, the theatre of protest, closed. The radicals had wanted conversion. All the divested Cold War laboratories prospered under continued defence patronage and with continuing ties with the adjacent, if now formally independent, universities.[65]

The campuses and laboratory plazas were indeed theatres of demonstration. Furthermore, the establishment of new bodies indicates that much else was at stake. MIT students and faculty initiated the Union of Concerned Scientists (UCS) in 1969. In a link back from Vietnam to disarmament, what started as a concern about campus contributions to the war in South East Asia shifted in the 1970s to a critique of nuclear safety issues.[66] The UCS was particularly active in the second Cold War period of the 1980s, organizing a report that in many ways was an echo of Pauling's 1957–8 petition.[67] The UCS, alongside the Scientists' Institute for Public Information (SIPI, formed in 1963) and Science for the People (SftP, founded in 1969 'as a group dedicated to finding ways to take political and social action against the war in Vietnam') have all been studied by the sociologist Kelly Moore, who has offered an interesting general argument.[68] Moore argues that 'public interest science organizations', such as UCS, SIPI and SftP, were an institutional response to a severe quandary posed by the mixture of political activism and the sciences:

> Activist scientists had to be politically critical of science without suggesting that the content of scientific knowledge might be tainted by non-scientific values … More specifically, they faced two related problems. First, their activities and claims threatened to fragment professional organizations that represented 'pure' science and unity among scientists. Second, once the discussion became public, it threatened to reveal the subjective nature of problem choices, methods, and interpretations because it focused attention on the relationship between sponsors of science and scientific knowledge.[69]

62 See Wisnioski, op. cit. (57), 323, for discussion of this conversion as pragmatic rather than ideological.

63 B. Magasanik, J. Ross and V. Weisskopf, 'No research strike at MIT', *Science* (1969), **163**, 517, quoted in Leslie, op. cit. (59), 233.

64 Leslie, op. cit. (59), 233. See also J. Allen (ed.), *March 4: Students, Scientists, and Society*, Cambridge, MA, 1970.

65 Leslie, op. cit. (59), 250.

66 L. S. Wittner, *The Struggle against the Bomb*, Volume 3, *Toward Nuclear Abolition: A History of the World Nuclear Disarmament Movement, 1971 to the Present*, Stanford, 2003, 11.

67 Wittner, op. cit. (46), 172–3.

68 Moore, op. cit. (45), 1592–627.

69 Moore, op. cit. (45), 1594.

Such tensions were reconciled, argues Moore, through the formation of public-interest science organizations. They 'made serving the public interest relatively permanent and durable, obfuscated how political interests affect scientific knowledge, and helped preserve the organizational representations of scientific unity: professional science organizations'. In other words, The UCS, SIPI and SftP functioned to preserve the purity of bodies such as MIT, the American Association for the Advancement of Science and the American Physical Society, respectively.[70] Also clearly revealed in Moore's study is the extent to which this institutional response was provoked by the problem that 'attention was increasingly being drawn to *multiple interpretations of scientific evidence* – certainly not a desirable state of affairs for a profession that relies more so than others on the presentation of unanimity on rules, methods, and interpretations'.[71] While she does not cite Balogh or Hays, Moore's account is clearly compatible with the Balogh model sketched in Wave One. Science for the People is the body that fits Moore's analysis least well. Established as Scientists for Social and Political Action in 1969 (later adding 'and Engineers' to become SESPA), Science for the People produced a bimonthly magazine (reaching a circulation of two thousand), squabbled internally and organized protests between 1969 and 1972, not least at AAAS meetings.[72] After 1972 Science for the People quietened. By the late 1970s it had 'evolved into a moderate, more biology-directed group, focussing on issues such as Sociobiology'.[73] It closed in 1991. Nevertheless, before then it was a direct and tangible influence on the BSSRS. Once these public-interest science organizations had been formed, to preserve the purity of core scientific organizations the protagonists were constrained to deploy use/misuse rhetoric and avoid discussions of the shaping of scientific content by interests.[74]

Wave Three

Wave One was an institutional dynamic that dragged experts into public display. Disagreement between experts, previously private, was now potentially publicly visible. Wave Two concerned the growth and actions of new social movements. The social movements provided the reason, people and resources. They cultivated the skills necessary to turn disagreement between experts into opposition to experts identified

70 The best single piece of evidence is the following response to the call for the APS to take a stand on the Vietnam War: 'It would be unwise and uncalled for to jeopardize the purely scientific nature of the APS and the harmony between its members by introducing politics in any form and of any denomination. Let those who must begin their own society.' Goetz Oertel, letter to editor of *Physics Today*, February 1968, quoted in Moore, op. cit. (45), 1610.

71 Moore, op. cit. (45), 1608. Her emphasis (and an emphasis that works here too). The quotation is discussing Barry Commoner's experience in setting up the Greater St Louis Committee for Nuclear Information (CNI), precursor to SIPI, and is clearly in line with the account of Commoner above.

72 Wisnioski, op. cit. (57), 325–6.

73 Wisnioski, op. cit. (57), 327.

74 Moore explicitly argues that focusing on 'misuse' of science was a ploy to avoid the 'serious problems' raised by 'multiple interpretations of evidence [that] were possible among scientists, undermining the claims of scientists to universal standards of interpretation' that arose in publicly observed controversy. Moore, op. cit. (45), 1613.

with the targets of social movements, and, more profoundly, into questions about the nature of expertise. Wave Three is given cohesion by common features all concerning the 'self' in the long 1960s. It is a commonplace that the post-war baby-boom generation held attitudes in opposition to those of their parents' generation. More significant is the observation that the baby-boom generation identified and analysed themselves as different.[75]

Self-consciousness emerged as a theme in elite intellectual thought partly as a reaction against overbearing systematization. The dominant social science was quantitative and scientistic in method and, in the words of Hollinger, 'triumphalist' in spirit, 'marked by the buzzwords modernization theory and the end of ideology'.[76] But the books of Daniel Bell and Walt Rostow, while governing policy, were not the texts deemed influential among the members of social movements in the long 1960s. The texts and authors that were influential had a common theme of overbearing structural determination and the limits on responses of the individual. The works of Marcuse can be glossed as such.[77] Another text, to be discussed in more detail because it relates to the sociology of science and technology, is Jacques Ellul's *Technological Society*. An academic theologian from Bordeaux, Ellul would not have reached such a wide anglophone audience were it not for a fortunate intervention by Aldous Huxley. The author of *Brave New World* recommended an obscure French text, Ellul's *La Technique* (1954), to the Center for the Study of Democratic Institutions of the Fund for the Republic, Inc., Santa Barbara, and its translation and dissemination became the favoured project of the publisher Alfred A. Knopf. With a foreword by the foremost sociologist of science in the United States, Robert K. Merton, *The Technological Society* was published in 1964.

Ellul's argument concerned the expansion of 'technique', an omnivorous entity defined as 'the totality of methods rationally arrived at and having absolute efficiency (for a given stage of development) in every field of human activity'.[78] Technique did not merely mean machines, a crucial point for Ellul. Indeed, machines were merely one human creation that had been absorbed by technique. Technique had agency beyond human control. It was autonomous; it 'integrates everything'. While technique was as old as human societies, it had particularly fastened its grip as the methods of the 'technical revolution' of the late eighteenth century – economic, mechanical, military, administrative and police innovations – had been assimilated.[79] Ellul's was therefore an

75 Ravetz, too, emphasizes demographic forces – an affluent, marketeered, free youth – underpinning critique. Ravetz, op. cit. (1).

76 D. A. Hollinger, 'Science as a weapon in Kulturkampfe in the United States during and after World War II', *Isis* (1995), **86**, 440–54, 450; emphasis removed.

77 Marcuse's analysis is framed within his concept of an 'advanced industrial society'. Technology was part of this, and science, in turn, part of technology. Marcuse's framework therefore invited critiques of science, particularly from his New Left readership, as part of a critique of advanced industrial society. 'Revolutionary consciousness-raising' was a strategy proposed in H. Marcuse, *One-Dimensional Man: Studies in the Ideology of Advanced Industrial Society*, Boston, 1964.

78 J. Ellul, *The Technological Society*, tr. John Wilkinson, New York, 1964, 25.

79 Ellul, op. cit. (78), 43.

extreme version of an industrial-modernism thesis. 'Modern technique' was rational and artificial: these were two characteristics Ellul admitted other authors had noticed. But it was also 'self-directing' and self-augmenting, and formed a whole in which any differentiations were secondary. The twentieth century had witnessed the further spread of technique into all human affairs, a quantitative but not qualitative development. Ellul's analysis had clear parallels with Martin Heidegger's answer to the question concerning technology, a lecture (and then essay) that also only reached a receptive audience in the long 1960s.[80]

Before discussing how Ellul portrayed science's relationship with technique, one should highlight an aspect of his account that distinguishes it decisively from later sociology of technology. Ellul is often held up as a straw man. His arguments, it is claimed, are as close as sophisticated arguments get to a position of technological determinism. But this caricature depends on confusion between 'technology' and 'technique'. Even if technology is understood broadly as the sum of material devices, know-how and the social systems within which they operate, technique was a still more encompassing concept. Ellul would admit, although he was inconsistent on this point, that humans could choose between technologies. But he dismissed outright the suggestion that humans could reject technique: 'Every rejection of a technique judged to be bad entails the application of a new technique, the value of which is estimated from the point of view of efficiency alone':[81]

> The human being is no longer in any sense the agent of choice. Let no one say that man is the agent of technical progress … and that it is he who chooses among possible techniques. In reality, he neither is nor does anything of the sort.[82]

This should be read as a statement of human impotence in the face of technique. Choosing technologies was a case of shuffling deckchairs on the Titanic.

Ellul offered no way out, except by challenging individuals to strive to 'transcend' technique. This significant exception helps us understand why Ellul was read in the long 1960s:

> At stake is our very life, and we shall need all the energy, inventiveness, imagination, goodness, and strength we can muster to triumph in our predicament … [E]ach of us, in his own life, must seek ways of resisting and transcending technological determinants …
>
> We must look at it dialectically, and say that man is indeed determined, but that it is open to him to overcome necessity, and that this very act is freedom. Freedom is not static but dynamic; not a vested interest, but a prize continually to be won …
>
> In the modern world, the most dangerous form of determinism is the technological phenomenon. It is not a question of getting rid of it, but, by an act of freedom, of transcending it. How is this to be done? I do not yet know.[83]

80 'Die Frage nach der Technik' as a lecture dates from 1949. It was published in a collection of essays in 1954.

81 Ellul, op. cit. (78), 110.

82 Ellul. op. cit. (78), 80.

83 Ellul, op. cit. (78), 32–3.

It is not hard here to detect an echo of personalism, the communitarian philosophy produced within 1930s Catholic theology that shaped Ellul's early thinking and which has been claimed as a major influence on radical thought in the long 1960s.[84]

Ellul's pessimism was therefore leavened by a glimpse of salvation. But human 'choice' between technologies was relegated to minor, negligible status. On this matter of choice, Ellul was utterly at odds with the new sociology of technology that developed alongside the sociology of scientific knowledge in the 1970s and 1980s. This new sociology can be characterized as providing, first and foremost, models of technological change in which human choices are paramount, even if the capacity to choose is not evenly distributed according to social justice. Such new models of technological change could only become convincing in a new context after the long 1960s, in which choice, in different forms, became highly valorized.

So, finally, what role does Ellul assign to science? The answer is simple: 'science has become an instrument of technique'. 'Science is becoming more and more subordinate to the search for technical application.'[85] Like all other human affairs, science had been assimilated, an 'enslavement' that only became entrenched in the twentieth century.[86] When Ellul called for 'all of us' to seek by 'acts of freedom' ways of transcending technique, then he must also have been asking either for science's emancipation or for science, too, to be rejected in the name of a greater salvation.

The call to self-analysis and to 'transcend' the system was also the rousing conclusion to Marshall Berman's historical examination of radical individualism and the emergence of modern society, *The Politics of Authenticity* (1971). Berman, a Harvard postgraduate during the years of protest, concludes,

> The system builds and programs everyone to order. Hence only an 'underclass' which is totally 'outside' the system can even understand it, let alone work to change it. The chances that such an underclass will form are very dim; even if it should form, the chances are that it will be co-opted [recall Ellul] … Hence the self can preserve itself only by totally dropping out – by withdrawing into the woods, or into madness, or into both – by living secret lives and creating invisible communities underground. Montesquieu and Rousseau suggest that even in a thoroughly repressive society, there may be alternatives to the polarities of total revolution or total retreat. They argue that even though everyone is indeed conditioned by an alienated social system, *this conditioning may include a capacity to criticize and transcend the system*.[87]

Berman had retreated two centuries and in the end described himself. The 'modern society' whose emergence he traced was the society around him. Furthermore, what he found through Rousseau and Montesquieu was an instruction to analyse oneself: 'the very powers which enable us to see through others can enable us to see through ourselves'.

84 This case for the influence of personalism in the long 1960s is advanced in J. J. Farrell, *The Spirit of the Sixties: Making Postwar Radicalism*, London, 1997.

85 Ellul, op. cit. (78), 10, 312.

86 Ellul, op. cit. (78), 45.

87 M. Berman, *The Politics of Authenticity: Radical Individualism and the Emergence of Modern Society*, London, 1971, 323. Berman's emphasis, but, again, the emphasis works here too.

This self-analysis mattered for science. Like Berman, Theodore Roszak spun observations of his contemporary, largely academic society into a call for a counter-cultural vanguard. In *The Making of the Counter Culture* (1968) Roszak suggested that objectivity itself was mythological. This mattered, in his analysis, because the technocracy depended on experts who justified their role as being purveyors of reliable knowledge, while 'reliable knowledge' was knowledge that was 'scientifically sound' and science was characterized by 'objectivity'. Working back up the chain of reasoning, deny objectivity and you deny technocracy at its source.[88] Roszak's argument against objectivity was suggestive rather than compelling. From Kuhn he borrowed his scepticism about seeing the history of science as the incremental accumulation of true knowledge. He referred the reader to Michael Polanyi for the full challenge to objectivity. We are less concerned with where Roszak hoped the world was going, than with what he said of the source of critique:

> In the case of the counter culture, then, we have a movement which has turned from objective consciousness as if from a place inhabited by plague – and in the moment of that turning, one can just begin to see an entire episode in our cultural history, the great age of science and technology which began in the Enlightenment, standing revealed in all its quaint arbitrary, often absurd, and all too unbalanced aspects.[89]

The revolution he claimed to identify was one of consciousness, for it was 'the psychology and not the epistemology of science that urgently requires our critical attention'.[90] He illustrated the 'objective consciousness' with a horror-show of an appendix. The counter-culture was equipped for the role of critic of the objective consciousness because, despite its diversity, both its main currents, the New Left activists and the 'mind-blown bohemianism of the beats and hippies', shared an 'extraordinary personalism', a '*consciousness* consciousness' that emphasized a politics of self-examination.[91]

So in Roszak's analysis the contribution of the New Left to the change of attitudes to science and technology was indirect.[92] An infantilized generation, made conscious of itself through market effects and expanded higher education, developed a politics of personalism that, by chance, would allow it effectively to oppose the greater evil of science-based technocracy. Roszak found the New Left's personalism best expressed in the SDS Port Huron Statement of 1962 that opposed 'the depersonalisation that reduces human beings to the status of things', an alienation that 'cannot be overcome by better personnel management nor by improved gadgets but only when a love of man overcomes the idolatrous worship of things by man'. Roszak reckoned this personalism implied a devaluation of authority and hierarchy. He approvingly quoted Kenneth

88 Roszak, op. cit. (33), 208.

89 Roszak, op. cit. (33), 215.

90 Roszak, op. cit. (33), 217. What sorts of psychology is unclear. Perhaps something like C. T. Tort, 'States of consciousness and state-specific sciences', *Science* (1972), **176**, 1203–10.

91 Roszak, op. cit. (33), 56, 62.

92 Rose and Rose note how unconcerned with science were key authors of the British New Left such as Raymond Williams and Perry Anderson: H. Rose and S. Rose, *The Radicalisation of Science*, London, 1976, 13.

Keniston of the Yale Medical School: 'in manner and style, these young radicals are extremely "personalistic", focussed on face-to-face, direct and open relationships with other people; hostile to formally structured roles and traditional bureaucratic patterns of power and authority', a characteristic Keniston traces to the child-rearing habits of the contemporary middle-class family'.[93] What was new in Roszak's argument was the identification of the counter-culture as vanguard and of science as its legitimate target. Crucially, for Roszak, the 1960s self was produced as autonomous and self-examining, perhaps also self-absorbed and self-interested. In generational terms, the effects of the market and higher education intensified the 1960s generation's consciousness of itself.[94] Calls for self-management, notes Feenberg, were also a prominent feature of the events of Paris in 1968.[95]

As practised in new ways in the long 1960s, the sociology of scientific knowledge was another, most profound, expression of such self-analysis. The new sociology of science argued that science's content was open to sociological investigation. In the form of Bloor's strong programme, the explicit intention was to turn the methods of science on science itself. Yet when Frances Yates contrasted Giordano Bruno's underground arts of memory with the hierarchical methods of scholasticism, the society described was interpreted by readers as a long-1960s self-description, just as was the work of Berman.[96] Likewise, Commoner's campaigns were projects orchestrated so that scientists were organizing critically to observe science. Jerry Ravetz nailed this moment: industrialized science was provoking its 'opposite, "critical science"', a '*self-conscious* and coherent force'.[97] Finally, the new breed of science journalists, notably Greenberg, helped others to analyse science scientifically. Steven Shapin recalls reading Greenberg, worrying about Vietnam and being moved to the place, intellectually, where the strong programme started.[98]

Many commentators have observed that the new sociology of science emerged in the context of Wave Two, the social movements. Thus far the debate on this emergence has been limited to definition of precisely which social movement was most productive. For example, Haraway favours new environmentalism and feminism while Feenberg stresses the New Left and the Paris events.[99] Other sources provide further examples. Moore pinpoints sources of the critique in which it is claimed interests shaped content as the publication of *How Harvard Rules Women* (1970) and in the arguments of Commoner's protean Greater St Louis Committee for Nuclear Intelligence against

93 Roszak, op. cit. (33), 60.

94 Roszak, op. cit. (33), 27.

95 'Self-management, one of the goals of this revolution.' Feenberg, op. cit. (51), 39.

96 F. A. Yates, *Giordano Bruno and the Hermetic Tradition*, Chicago, 1964; *idem*, *The Art of Memory*, Chicago, 1966.

97 J. R. Ravetz, *Scientific Knowledge and Its Social Problems*, Oxford, 1971, 10, 423, 424. My emphasis.

98 Read the fascinating but brief biographical sketch in Shapin's introductory essay to D. S. Greenberg, *The Politics of Pure Science*, 2nd edn, Chicago, 1999.

99 'Donna Haraway argues that the emergence of new approaches owes much to the environmental and feminist movements, and, I would add the contributions of thinkers such as Marcuse and Foucault ... It is ironic that the currently dominant social theory of technology seems to have no grasp of the political conditions of its own credibility'. Feenberg, op. cit. (51), 12.

Edward Teller.[100] Similarly, Nelkin's *The University and Military Research* (1971) both was clearly personal and emerged from the activism of the 1969 MIT Science Action Coordinating Committee. Likewise, MIT's Science and Public Policy programme produced Anne Hessing Cahn's *Eggheads and Warheads: Scientists and the ABM* (1971).

Likewise, we need to historicize the new sociology of technology. Feenberg finds that the 'movements of the 1960s created a context and an audience for the break with technocratic determinism that had already begun in the theoretical domain in the works of Mumford and a few other skeptical observers of the postwar scene' and that it was 'in this context that an American school of philosophy of technology emerged' (exemplified by Winner, Borgmann and Ihde).[101] Feenberg also notes that it is 'ironic' that the new sociology of technology has forgotten the politics of its birth. One could, contentiously, suggest that the sociology of technology of the beginning of the long 1960s, typified by Ellul, most starkly differed from the sociology of technology emerging at its end, notably the beginnings of the approach known as SCOT, in its account of the roles available to groups and even individuals as choosers. Technological choice became valorized.[102] Technological logics, paths, trajectories, indeed anything strongly constrained or at limit determined, came to be ridiculed. Is it a coincidence that the same period saw the political celebration of consumer choice, when Hayek's *Individualism and Economic Order* was read over Keynes?

In his review of James Watson's autobiographical account of the determination of the structure of DNA, *The Double Helix* (1968), the biochemist Erwin Chargaff made the pregnant remark that it is

> perhaps not realized generally to what extent the 'heroes' of Watson's book represent a new kind of scientist, and one that could hardly have been thought of before science became a mass occupation, subject to, and forming part of, all the vulgarities of the communications media.[103]

We should take seriously Chargaff's notice of a 'new kind of scientist'. Many commentators noted that Watson's protagonists behaved like ordinary human beings rather than following some higher moral code that regulated scientists. But this was not what Chargaff meant. There are two separate questions: was Watson merely describing how scientists behaved 'in real life' for the first time? If so, what had changed about the world so that Watson, in 1968, was able to be the first do this? *The Double Helix* cannot be read simply as an account of what 'really' happened. As Jacob Bronowski was quick to observe, the structure of the narrative resembles a fairy tale. Second, if there was something new about how scientists behaved, what was it and why had it appeared in the post-war period? As Edward Yoxen has pointed out, one context was

100 Moore, op. cit. (45), 1615–16.

101 Feenberg, op. cit. (51), 6.

102 Note that the processes whereby actors could highlight or downplay the roles of choice were complex and need tracing in detail. Feenberg has given one case study in Commoner vs Ehrlich on population growth.

103 E. Chargaff, 'A quick climb up Mount Olympus' (review of *The Double Helix*), *Science* (1968), **159**, 1448–9.

the fight for disciplinary recognition and power for emergent molecular biology. The author of *What Is Life?*, Erwin Schrödinger was one scientist enrolled as an ancestor to give 1960s molecular biology some genealogical substance.[104] Likewise, Watson's portrayal of his and Crick's lifestyle was an invitation to recruits.

But the invention of 'heroes' invites the question of what exactly was being championed. Watson's protagonists were individualistic, entrepreneurial and willing to bend rules and slight colleagues to get ahead. They are a good illustration of what, counter-intuitively, links Waves Two and Three. Marwick observed that individual cultural entrepreneurship was an underappreciated feature of the long 1960s. Such entrepreneurialism explains why new social movements, especially the counter-culture, took the forms they did. Watson's protagonists are from the same mould. (So are the protagonists of the anti-IBM homebrew computer movements of the early and mid-1970s, notably the Apple founders. The similarity is no coincidence.[105]) The exponent of privately funded biotechnology Craig Venter was in California in his early twenties when he read *The Double Helix*. A recent hagiographical sketch significantly informs us, 'Years later Venter would complain that he had no mentors … If there was one, he said, it was the Watson of *The Double Helix*.[106] The point is not that the Watson persona stands in contrast to that of the cultural movers in the long 1960s. Rather, in 'doing his own thing', he is self-ishly similar.

Perhaps the long 1960s were more accurately typified by the coexistence and contradiction between such individualistic entrepreneurship and communal ideals.[107] Indeed, such a tension is precisely what can be seen in the various first-hand narratives of the DNA story. Watson gives us the individualist–entrepreneur. Maurice Wilkins's *The Third Man of the Double Helix* (2003) has belatedly reminded us of the communal ideal.[108] The communal model is old, and was captured if not frozen in its Cold War form in Merton's CUDOS norms. Just as the Balogh dynamic describes a thaw from fixed private expert–expert relations to experts set against each other in public, so the melting of Merton's norms released a tide of individualism and entrepreneurialism within the sciences. Watson provides just the models of behaviour, the 'heroes', necessary for 1970s commercialization in the biosciences. Wave Three provides the link between the long 1960s and the DNA story and the entrepreneurial professors described by Kenney.[109]

104 E. J. Yoxen, 'Where does Schrödinger's *What Is Life?* belong in the history of molecular biology?', *History of Science* (1979), **17**, 17–52.

105 J. Markoff, *What the Dormouse Said: How the Sixties Counterculture Shaped the Personal Computer Industry*, New York, 2005; F. Turner, *From Counterculture to Cyberculture: Stewart Brand, the Whole Earth Network, and the Rise of Digital Utopianism*, Chicago, 2006.

106 T. Anton, *Bold Science: Seven Scientists Who Are Changing Our World*, New York, 2000, 11. I use 'hagiography' in its true sense: Anton presents us with ideally good lives.

107 Brick, op. cit. (2), makes contradiction the unifying theme for understanding the long 1960s.

108 M. Wilkins, *The Third Man of the Double Helix*, Oxford, 2003. Good X-ray pictures were the result of '*The great community spirit and co-operation in our lab*' (123–4). And elsewhere: 'Francis and Jim asked me whether I would mind if they started building models again. I found this question horrible. I did not like treating science as a race, and I especially did not like the idea of them racing against me. *I was strongly attached to the idea of the scientific community*' (205). My emphases.

109 Kenney, op. cit. (21).

What do we do with Chargaff's substantive claim that the 'new kind of scientist' was a product of the development of science as a mass occupation? Watson's protagonists were useful fictions, useful in providing a semblance of individualism in a far less individualistic pursuit. Science was fully a part of what Brick labels the 'socialization of intellect in the new mass universities' and Watson's individualistic self-portrait emerges as a means of resolving tensions.[110] Another approach would be to do as Chargaff suggests, to relate the processes of individualization and entrepreneurialism to developments in mass media. At least one highly popular if problematic sociological attempt to explain why Americans became more individualistic in the post-war period does precisely that: the reason why Americans bowl alone, says Robert Putnam, was television.[111]

Return to Friends House

We should not regard the new sociology of science as entirely a creature of Wave Two or treat it as the product of one social movement rather than another. Instead, we should recognize science studies as the result of an interference between Waves One, Two and Three. Wave One produced experts and created moments where divergent expert views were publicly accessible. As Commoner observed, if faced with two contradictory expert statements there is an alternative to the assumption that one expert is telling the truth and the other is not. One can instead question what both experts have in common, a claim on truth. What happened next can be seen as another link in a Wave One chain reaction, an institutional context that created a demand for further experts: this time, experts on expertise. Wave Two provided the institutional vehicles that could carry scrutiny. The BSSRS, the Social Impact of Modern Biology conference and the edited proceedings were all examples of Wave Two phenomena that orchestrated and supported such an inquiry. Finally, Wave Three directed this inquiry inwards. The geneticist Jon Beckwith had flown from Berkeley, home of Roszak's 'consciousness consciousness', to provide testimony on the 'scientist in opposition in the United States'.[112] Citing the *Nature* editorial of 27 December 1969, Beckwith noted that (as we might now expect from Wave One) it was publicly visible expert conflict that created intense establishment discomfort:

> The reactions to our statements were strong and bitter from some quarters. There was an outcry from many scientists against publicizing any negative aspects of our work. They felt that the problems and control of science were better handled quietly by leaders of the scientific community.

110 Brick, op. cit. (2), 16.

111 I am only being superficially simplistic. A close analysis of the structure of Putnam's argument shows that he argues that the effects of television were pivotal: R. Putnam, *Bowling Alone: The Collapse and Revival of American Community*, New York, 2000.

112 J. Beckwith, 'The scientist in opposition in the United States', in Fuller, op. cit. (10), 225–31.

In consequence, Beckwith argued that in

> the same way that radical historians or economists expose the way in which most history or economics is taught from a political viewpoint supportive of the system, the radical scientist must expose the way in which science … is politically organised and directed.[113]

The inner workings of science must be revealed. Wave Three was also evident, as we will now show, in the astounding speeches by Bronowski and Young.

The key presentation by the establishment scientists was Jacob Bronowski's proposal for the 'disestablishment of science'. Director of the Council of Biology in Human Affairs at the Salk Institute and at that moment planning and shooting footage for the celebrated documentary series *The Ascent of Man*, Bronowski began by labelling calls for 'a moratorium on science' as the 'favoured daydream of the bewildered citizen'. But behind this dream was something more profound, recognition of the importance of 'a voluntary agreement among scientists themselves': 'If science is to express a conscience', argued Bronowski, it must be *self*-generated, 'it must come spontaneously out of the community of scientists'. In particular, scientists were 'face to face with a choice of conscience between two moralities: the moralities of science, and the morality of national and government power'. These moralities, stated Bronowski, were 'not compatible'. Government patronage of the sciences led to 'moral distortion, a readiness to use any means for its own ends':

> The scientist who goes into this jungle of 20th-century government, anywhere in the world, puts himself at a double disadvantage. In the first place, he does not make policy; he does not even help to make it, and most of the time he has no idea what shifts of policy his advice is meant to serve. And in the second and, oddly, the more serious place (for him) he has no control over the way in which what he says in council will be presented to the public. I call this more serious for him, because public respect for science is built on his intellectual integrity, and the second-hand statements and the garbled extracts that are attributed to him bring him into disrepute.

Experts disagreed with experts in public, said Bronowski, because of misrepresentation by and of government bodies. He offered his solution: 'The time has come to consider how we might bring about a separation, as complete as possible, between science and government in all countries. I call this the disestablishment of science.' By rolling back the state in science in the name of restoring the autonomy, the self-determination, of science, Bronowski's programme would deliver science to private interests. His argument is a clear example of how sea-change rhetoric could prepare the ground for the commercialization of the life sciences in the 1970s. Notice the grounds of the argument in the Wave One problem of public statements by experts bringing scientists 'into disrepute'.[114] Furthermore, the resources necessary for Bronowski to make this argument came from Wave Two in the BSSRS as organizers and audience. Bronowski's Council for Biology in Human Affairs at the Salk Institute had stumped up the cash for the conference. He had paid his way in. Notice, too, the flurry of Wave Three notions

113 Beckwith, op. cit. (112), 226–7, 228.

114 J. Bronowski, 'The disestablishment of science', in Fuller, op. cit. (10), 233–43, 233, 234, 238, 239, 241.

such as self-analysis, self-determination and autonomy. Bronowski's argument finds echoes in one recent, sophisticated study of science as an ideological and political resource. Ezrahi has claimed that a disestablishment of science did indeed take place in the long 1960s and that a wave of reflexivity acted to decouple political action from science as an exemplar of rationality in liberal democracies.[115] Thus the *Ascent of Man* connects to the *Descent of Icarus*.

Bronowski infuriated the radicals on the conference floor. 'I think we've just heard a prize example of liberal clap-trap', said one. Rosenhead turned Bronowski's arguments around: it was not individual integrity that was at issue but the irresponsibility of institutions. Radicals and establishment scientists heard and praised different aspects of the conference. Bronowski's talk may have been the one that the 'national press' chose to 'headline', noted an editorial in the *BSSRS Newssheet* in early 1971, but the most 'eagerly-awaited' was the paper by a historian of science, Robert M. Young.[116] It was also by far the most divisive.[117] In 'Evolutionary biology and ideology: then and now, Young started from the same observation as Monod, Wilkins and Bohm: 'We are struggling to integrate science and values' but 'at the same time we are prevented from doing so by our most basic assumptions'. There followed a masterclass in the new sociology of science: facts are theory-laden, concepts are value-laden, 'knowledge is both a product of social change and a major factor in social change and/or the opposition to it'. 'This', noted Young, was a 'commonplace' (for some), 'but its systematic application has radical consequences for the idea of "objective" science'. A sharp analysis of three case studies followed. The essential point, though, was that

> no one can confidently draw the line between fact, interpretation, hypothesis, and speculation (which may itself be fruitful). It seems to me that it is the social responsibility of science to enter wholeheartedly into this debate and directly answer such works in the non-specialist press. Paradoxically, we must relax the authority of science and see it in an ideological perspective in order to get nearer the will-o'-the-wisp of objectivity. We have won a Pyrrhic victory in establishing the part-reality and part-myth of the autonomy and objectivity of science, and the existence of this Society and its conflicting aims reflects our unsteady position. In one sense science should feel strong enough to stop flailing horses which died in the nineteenth century in their attempts to protect the status and methods of science. But in an other sense, we need – for our own moral purposes – to think seriously about the metaphysics of science, about the philosophy of nature, of man and of society, and especially about the ideological assumptions which underlie, constrain and are fed by science.[118]

115 Y. Ezrahi, *The Descent of Icarus: Science and the Transformation of Contemporary Democracy*, Cambridge, MA, 1990.

116 *BSSRS Newsheet*, 1971, 10. The *Newssheet* was turned later into the journal *Science for People*. For comment on *Science for People* and *Undercurrents* see J. R. Ravetz, 'Anti-establishment science in some British journals', in *Counter-movements in the Sciences* (ed. H. Nowotny and H. Rose), *Sociology of the Sciences* (1979) **3**, 27–37.

117 Jon Beckwith's paper pleased the radicals most, being reportage and reflections on the Berkeley experience. The Roses, too, drew on Kuhn, Marcuse and the Wave Two movements to demolish the 'myth of neutrality of science'. But it was Young's paper which electrified the conference. S. Rose and H. Rose, 'The myth of the neutrality of science', in Fuller, op. cit. (10), 215–24. Beckwith, op. cit. (112). See also J. Beckwith, *Making Genes, Making Waves: A Social Activist in Science*, Cambridge, MA, 2002.

118 R. M. Young, 'Evolutionary biology and ideology: then and now', in Fuller, op. cit. (10), 199–213, 201, 203, 211.

The programme for the new sociology of science was here mapped out. Furthermore, as Young notices, the diversity of accounts of what was happening with science in the long 1960s, as exemplified by the BSSRS ('the existence of this Society and its conflicting aims reflects our unsteady position'), was what prompted the turn inwards to ask for a sociology of scientific content. In a powerful sense, then, sociology of scientific knowledge was self-description. This is a line of inquiry that has already begun. Schaffer, for example, has suggested that Kuhn's *Structure* can also be seen as a description of his immediate intellectual context.[119] We may also recall that 'crisis talk' was a feature both of the subject of Paul Forman's path-breaking study of Weimar physics and of the context of science studies in the long 1960s.[120] We must understand Wave Three in order to historicize SSK and vice versa.

Conclusion

> There are powerful reasons for not making a fuss about the transition from the sixties to the seventies a few days from now. For one thing, time is known to be continuous. For another, attempts artificially to separate one interval from another usually stimulate exaggeration or oversimplification … The truth is that there have emerged in public opinion of science and technology a group of interlocking heresies … [121]
>
> Editorial, 'On which side are the angels?', *Nature* (1969), **224**, 1241–2

In the closing days of the 1960s, the editors of *Nature* chose to attack a fearful array of 'interlocking heresies', prominent among them the 'pollution movement', the 'Doomsday Fallacy' and any linkage of 'fears about genetic engineering with the widespread anxiety about the war in Vietnam'. 'Why', they plaintively asked, 'is there such currency in these fantasies?' As it happens, a particular fantasy they hoped to 'give the lie to' was global warming through the greenhouse effect. What made matters much worse, in the editors' eyes, was that this critique was self-generated and self-sustained: the 'fact that many professional scientists have recently been contributing to this misguided assessment of the risks of modern life is reprehensible'.

The *Nature* editors knew something was happening but did not in truth know what it was. Their rivals over at *Science* also ran editorials that consoled the journal's readers that any supposed 'crisis in confidence in science' might be just a matter of 'ambivalence, not rejection', while doubting both the data and the phenomenon.[122] Are we in a better position to know what happened about science in the long 1960s? What can we say that avoids exaggeration and oversimplification?

This paper has offered a three-wave model which together captures something of the long 1960s as a period of 'sea change'. While I have taken as a case study a handful of papers from just one conference, evidence for the three waves is drawn from a diverse

119 S. Schaffer, paper for STS Workshop, Cambridge HPS, 2 March 2006.

120 P. Forman, 'Weimar culture, causality and quantum theory, 1918–1927', *Historical Studies in the Physical Sciences* (1971), 3, 1–115.

121 This editorial was a direct response to a letter published in the same issue from Jim Shapiro, Larry Eron and Jon Beckwith.

122 Etzioni and Nunn, op. cit. (19); Nunn, op. cit. (26).

secondary literature that had not hitherto been fully brought together. Wave One described an institutional dynamic that drew out experts from closed and hidden disagreement into situations where expert disagreement was open to public scrutiny. This 'Balogh model' also accounted for the multiplication of experts. Wave Two consisted of institutions and audiences that could carry public scrutiny and provide a home for sea-change cultures. In particular, Wave Two could provide stage, audience and theatre directors for the play of disagreeing experts. The writings of activist–scientists and new critical journalism also helped. A necessary condition for public 'ambivalence' about science was that both sides were publicly presented. Wave Three was marked by an orientation towards the self, in diverse ways. Acknowledgement of this self-regard resolves some paradoxical features of this topic. For example, many commentators identified changing attitudes towards science with the 'young', while polling data suggested that 'young people' were '*not* the main source of lack of confidence'.[123] But there is little doubt that the baby-boom generation were more reflexive, more likely to examine themselves, their cohort, their society, and therefore more likely to articulate and consume self-analysis, even if they were not on average more critical.

All three waves need to be framed in the Cold War context. The polarized geopolitical world provided a common container for all sea-change phenomena. The Cold War provided the freeze that formed a common origin for the Balogh-style chain reactions. The influence of the Cold War on social movements went far beyond the obvious provision of targets, such as nuclear weapons and the Vietnam War, against which to organize. Cold War culture and institutions, such as containment, consensus, conformity, extraordinary arsenals of science-based technological systems, and hierarchies of systems were, in a powerful sense, the ocean on which the three waves moved. More specifically, the Cold War shaped the sea change by encouraging the development of techniques that, when turned inwards, became instruments of critique. Sometimes this critique was played out within the sciences, as when Cold War oceanographic data provided ammunition for the plate tectonics revolution.[124] Sometimes the critique was played out on a more traditionally political stage. Cybernetics, for example, contributed its core techniques, the analysis of feedback loops, to the models used by the Club of Rome to identify the limits to growth. James Lovelock built his technical authority on the development of ionization detectors for gas chromatography, which attracted patronage from NASA, before he spent this intellectual capital on promulgating Gaia. The Cold War provided the new environmentalism with critical tools. Even as simple a move as the invention of a new self-critical term, 'big science', partly originated in the reflections of Alvin Weinberg, an administrator of a central laboratory of the Cold War, Oak Ridge, as well as in Derek de Solla Price's science of science.[125] Or, shifting fields again, RAND techniques for assessing the management of research and development contributed to a critique of

123 Etzioni and Nunn, op. cit. (19); original emphasis. The older, the less educated, and, in the United States, the further south you were, the less confidence in science you had.

124 Brick, op. cit. (2), 9.

125 J. H. Capshew and K. A. Rader, 'Big science: Price to the present', *Osiris* (1992) 7, 3–25; P. Galison and B. Hevly (eds.), *Big Science: The Growth of Large-Scale Research*, Stanford, 1992.

top-down hierarchical centralized authority.[126] Radical variants of technocratic tools were proposed.[127]

Hollinger's explanation for the 'little renaissance of "science studies"' of 1962 to 1965 is a special case of this general argument.[128] Hollinger notes the remarkable flourishing of communitarian language about science found in the work of, amongst others, Don K. Price, Warren O. Hagstrom and, pre-eminently, Thomas Kuhn's *Structure*. Scientific communities were, of course, not new. But the emphasis on 'scientific community' as a representation of science, replacing older individualist images, was innovative. The switch in representations, argues Hollinger, happened because of the 'revolutionizing of the political economy of physical science'. The rise of big science confirmed in the Cold War encouraged talk of 'scientific community' not because science was more communal but because its precarious autonomy could better be defended. Hence emerged what Hollinger calls 'laissez-faire communitarianism': science is a self-managing community, so let it be. Likewise, Brick argues that the condition of the socialization of the intellect, the organization of intellectual life in formal institutions relying on public funds and engaged with public policy formation, provoked questions about the consequences of socialization for knowledge.[129] But if science were a community then did it not therefore have communal responsibilities? This was precisely the line of thinking that led to bodies such as the BSSRS.

Critical voices were therefore partly generated from within the Cold War establishment, a feature noticed by several of the commentators previously discussed such as Jacob Bronowski, the *Nature* editors and Maurice Wilkins. In his review of the relations of science, social movements and the long 1960s, Mendelsohn has also emphasized the stranger linkages and sympathies that made and crossed the oppositional culture. Thus Lewis Mumford's despairing *The Pentagon of Power* (1970) found critics in conservative historians such as Gerald Holton, and is contrasted to the upbeat optimistic portrayal of technology in Harvard's Technology and Society programme, funded by IBM to the tune of one million dollars. But Mumford's jeremiad shared concerns with Eisenhower's military–industrial complex speech, the president's farewell address to the nation of January 1961, which in turn echoed C. Wright Mills's arguments in the *Power Elite* (1956), hardly a political bedfellow of Eisenhower.[130] Critical science exemplars were Commoner's St Louis group and the societies for social responsibility in science. As previously shown, Commoner's *Science and Survival*

126 D. Hounshell, 'The Cold War, RAND, and the generation of knowledge, 1946–1962', *Historical Studies in the Physical and Biological Sciences* (1997), **27**, 237–67, 257.

127 Including a 'Guerrilla' and 'alternative Operational Reseach': 'the current techniques of OR can be turned to the use of sections of the community threatened by the OR currently used by the dominant forces ... One can speculate on the development of an OR that doesn't view people in a quantifiable abstracted form'. C. Thunhurst, 'Radical OR?', *Science for People* (1974), **25**, 10–11. An Institute of Critical Operational Research was planned – and a journal, *OR?gasm*.

128 Hollinger, op. cit. (8), 99–110.

129 Brick, op. cit. (2), 23.

130 Mendelsohn notes the immediate, local context of Eisenhower's speech: Eisenhower had been hoping for a nuclear test-ban treaty but had been thwarted by manoeuvres by 'newly powerful scientists (Edward Teller is the obvious figure) ... aided by friends in the military'. Mendelsohn, op. cit. (1), 156.

(1966) was self-directed in the sense that it recognized the need for a study of the study of science in the outcome of Balogh-type processes: experts publicly disagreeing with experts. Ravetz's 'self-conscious' 'critical science' is also a good illustration of how Wave Three concerns were produced by Wave Two interactions.

For the generation growing up in the 1960s, the images of science were 'contradictory'. The generation were free to enjoy benefits (domestic technologies, 'high-tech music', synthetic drugs) while consuming critical texts (Kuhn, Feyerabend, Carson, Ehrlich, Commoner, Illich, Schumacher) and recognizing the 'loss of innocence' of science made vivid by anti-nuclear and anti-Vietnam movements. Howard Brick has emphasized how contradiction was a feature of much of what was distinctive about the long 1960s. Likewise, the notion of a sea change formed by the interference of three waves captures the otherwise contradictory aspects of the long 1960s identified by Mendelsohn and Ravetz: short-term turmoil, a feeling of profound movement, a sense of failure.[131] 'The sixties' may have been a short-lived phenomenon,[132] but the waves that interfered to produce them cannot be confined to one place or one time.

For us the outcome of the interference of Waves One, Two and Three in a self-conscious study of science (to which we have given many names) is the closest to home. Participants at the time noticed that analyses of science had passed from use–abuse models, in which a 'good science' was distinguished and preserved from a 'bad science', to models of how the content of scientific knowledge, good, bad or otherwise, was related to context. In the interference between Wave Two and Wave Three we have an explanation of this passage. As Moore has shown us, Wave Two encouraged the formation of separate bodies to preserve the purity of bodies of the scientific establishment. In Wave Three the institutional pressures to preserve purity of the kind that Moore has pinpointed were overcome. It became possible to talk beyond the use–abuse model. A science of science was encouraged. Self-analysis moved to consider the content of science. In the case of the Social Impact of Modern Biology conference, a speech such as Young's was articulable.

There are other ways of historicizing the emergence of a new study of science. Jamison and Eyerman stress continuity between the 1930s and long-1960s radical critiques, a continuity dependent on a fragile chain of torch-bearers: C. Wright Mills, Hannah Arendt, Erich Fromm, Fairfield Osborn, Lewis Mumford, Rachel Carson and Leo Szilard.[133] Likewise, Steven and Hilary Rose charge that the radical idea that the content of science might be social in character had been suppressed by the mobilization for war.[134] The reactions to the Bomb and to Lysenkoism had encouraged an ideology

131 Both Mendelsohn and Ravetz tot up a record of some successes (environmental regulation, women's 'self-health' movements, alternative medicines) but more failures (the withering of the societies for social responsibility in science, alternative technology).

132 Indeed, the Smithsonian Institution housed a Center for Short-lived Phenomena – a Wave Three entity – which collected and compared data on short-lived phenomena (earthquakes, oil spills, sudden declines in puffin populations, infestations of vermin). It produced a few permanent records, such as *The Pulse of the Planet* (1972), before disappearing. The Center for Short-lived Phenomena was itself a short-lived phenomenon.

133 Jamison and Eyerman, op. cit. (48).

134 Rose and Rose, op. cit. (92).

of science as neutral. Only with the thawing of this mid-century freeze, glossed as a loosening of the ties that bound science to the state, could an alternative ideology emerge. (Notice the parallel with Wave One.) Such accounts frame the long 1960s as the return of submerged 1930s attitudes.

Another interpretation of the sea change would see it as a transition from a preference for pyramidal models of organization to network models, from centralized command to distributed agency without 'exaggeration and over-simplification'. We are faced with partial accounts with enough similarities to suggest the need for a general, synthetic history. The available robust accounts appeal to specific causes such as the transition from centralized hierarchies to dispersed networks in social structures, architecture, computer technology and defence organization;[135] the fall from favour of the large centralized firm in favour of the flexible network of firms;[136] the challenge to doctors' authority from 'self-help' social movements;[137] the retreat of government from the 'commanding heights' of the economy; and the expansion of the market.[138] For any 'node' – whether a self-treating patient, a node in the ARPANET, a firm responding flexibly or a consumer – to act autonomously was also to self-analyse and self-direct. Many of these phenomena were crises in some form of reproduction, not only as reproduction of a generation, workforce or university-educated cadre, but also reproductive crises in the sense that the debate over the Pill marked a reproductive crisis.[139] Just as autopoiesis was being named as a scientific subject, the re-creation of the self seemed problematic. My hypothesis is that these processes all share the features of Wave Three.

Wave Three is the least clearly delineated by historians and also the one that strikes closest to home for sociologists of science and technology and for the historians of science alongside whom they have worked. Waves One, Two and particularly Three opened an intellectual space for the sociology of scientific knowledge. The identification of Wave Three immediately opens up a series of historical research topics that can now be seen as part of a wider whole. How can the changing sciences of selfhood – such as immunology, genetics as informed by triumphant molecular biology, or psychology – be understood as part of these broader changes? In computing and in

135 Note Talcott Parsons's observations on the creation of 'networks of solidarity on much more highly universalistic bases than kinship' discussed in Brick, op. cit. (2), 118. P. Galison, 'War against the center', *Grey Room* (2001), **4**, 5–33.

136 Cf. M. J. Piore and C. F. Sabel, *The Second Industrial Divide: Possibilities for Prosperity*, New York, 1984.

137 Cf. S. B. Ruzek, *The Women's Health Movement: Feminist Alternatives to Medical Control*, New York, 1978.

138 The term is, of course, Lenin's but was recalled recently in D. Yergin and J. Stanislaw, *The Commanding Heights: The Battle between Government and the Marketplace that Is Remaking the Modern World*, New York, 1998, unsatisfying because it presents a Keynesian history of the transition from Keynes to Hayek (a few wise heads belatedly chose Hayek), whereas what is needed is a truly Hayekian history of the transition from Keynes to Hayek (that is to say, history which is the product of many, in which the wise choices of the few do not guide history).

139 Furthermore, they were crises in reproduction of forms, such as the hierarchical corporation, or the modern university, that stabilized in the late nineteenth century. The long 1960s, of course, have also been seen as a crisis in Enlightenment forms.

governance, the long 1960s saw a convulsive and otherwise puzzling debate over privacy. Does this debate now make sense when set in a broader context? In religion, in some but not all countries, the long 1960s were the period of dechristianization.[140] This is often glossed, in Wave Two-type analyses, as part of wider patterns of opposition and distrust of authority. But what should we make of the parallels with science? Quantitative evidence reported by *Science*, for example, held that the 'falling away from science' was 'part of a general lessening of faith in American institutions and authorities rather than a major anti-science groundswell … from religion to the military, from the press to major US companies [a]ppreciation for all of them, without exception, has fallen'.[141]

To answer any of these questions we will need to revisit what happened to 'authority', the 'self', 'choice' and the 'individual' in the long 1960s.[142] We will need to conduct an intensively cross-national comparative study to unpick accounts of Wave Two. Explanatory factors often proposed, such as the Vietnam War, vary immensely in meaning and significance between cultures. We know much more about the convulsions on American campuses than we do about the similarly intense episodes in Japan.[143] We will need to consider contrarian arguments ('there was no crisis in science in the 1960s'). We will need to reassess the New Right as well as the New Left. We will need to jettison some received associations, such as entrepreneurship with the political right and, perhaps, sociology of scientific knowledge as an inherently politically progressive project. And we will need to treat many secondary sources as primary sources.

140 C. G. Brown, *The Death of Christian Britain: Understanding Secularisation, 1800–2000*, London, 2001.

141 Etzioni and Nunn, op. cit. (19).

142 The literature on Western individualism is itself vast in scope. See T. C. Heller *et al.* (eds.), *Reconstructing Individualism: Autonomy, Individuality, and the Self in Western Thought*, Stanford, 1986. R. N. Bellah *et al.*, *Habits of the Heart: Individualism and Commitment in American Life*, Berkeley, 1985. And, lest we forget, F. Hayek, *Individualism and Economic Order*, London, 1949.

143 B. Burnett, 'Locating historical understanding of Japanese and Western resistance in education', paper at AARE 2004 conference, Melbourne.