History and Technology
Vol. 24, No. 2, June 2008, 173–190

Upheaval in the moral economy of science? Patenting, teamwork and the World War II experience of penicillin

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The development of penicillin can be seen as marking a decisive break with prewar biomedical research but not as launching the new world of 'Mode 2' science. The patenting of vitamin D enrichment by Steenbock in the 1920s and the administration of these patents by the Wisconsin Alumni Research Foundation (WARF) had deepened resistance in the UK, and in some US institutions, to the patenting of life science innovations. The initially negative reaction of the Medical Research Council (MRC) to penicillin patenting must be seen as a response specifically to the vitamin D experience. However MRC came to accept the patenting of innovations in penicillin technology. Similarly teamwork in wartime penicillin development went to unanticipated lengths. Such scientific styles were accepted and seen as paradigmatic for the positive potential of certain kinds of science. Postwar basic science policy can be seen, however, as an attempt to protect some scientific work from the impact of such innovations.

Keywords: penicillin; vitamin D; team work; patents; World War II

Introduction

Shock over the patenting of DNA sequences in recent years has symbolized responses to a fundamental shift in the process of research. That decoding the book of life had become just another money-making activity[1] was just one aspect of a widely perceived change in the practice and context of science. Undeniably, there is a considerable distance between contemporary practice and an idealized world of pure science; but hot disputes still surround the historical significance of current practice, the extent of the discontinuity with the past and on the process of change.[2]

In framing the 'modern' historical period, the importance of World War II as a turning point in the organization of scientific knowledge has proved particularly problematic. Highlighted by such revolutionary enterprises as the Manhattan project, and overwhelming in the memory of many participants, 'The War' was cataclysmic for science as for so many other aspects of society and culture. It is also, however, becoming clear how challenged traditional images of science had already been during the interwar era. Nicholas Rasmussen for instance has shown how science and scientists were already weaving between scholarship and commerce, and institutions were responding. To several scholars, it was the period between 1945 and the 1970s that was anomalous in the emphasis granted to basic science.[3]

In this paper we look at both prewar and wartime experience of science. This paper examines the interwar controversy over the patenting of Vitamin D production by irradiation by WARF. Although today largely forgotten, the debate was very fierce and widely cited, rousing many who resisted the very idea of patenting medical science. To the historian it provided a valuable prewar counterpart to the challenge faced in wartime with the development of penicillin that, famously, entailed large multidisciplinary team projects, integration between life sciences, natural sciences and engineering, Anglo-American cooperation, great public interest on both sides of the Atlantic

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ISSN 0734-1512 print/ISSN 1477-2620 online

DOI: 10.1080/07341510701810955
http://www.informaworld.com

and substantial patent innovations. We will explore whether the later development is best considered a mere sequel to the earlier, or whether it marked a new cultural turn in the history of science.

Mode 2

It is worthwhile first revisiting, briefly, the controversy over the significance of modern changes in science. This dispute conducted in terms of such accusation and denial, has resembled, as Thomas P. Hughes has pointed out, a disciplinary conflict between historians and policy analysts.[4] In 1994 a team of six distinguished historians and students of science policy articulated the view that science was being subsumed within a technological domain.[5] In their book, *The New Production of Knowledge*, the team cleanly divided the history of twentieth-century science into two styles. The former Mode 1 had been individualist, specialist, pure, publicly funded and isolated from the public. It was being replaced by Mode 2 said to be interdisciplinary, team driven, transgressive of traditional boundaries, privately funded and open to public debate and based on the linkage with application and innovation. The authors were rather unclear about time boundaries, but the beginning of Mode 2 is traced to World War II and the development of such large technological systems as the Manhattan Project. The book was itself not unique in its argument that science was changing in form, as well as content. Thus, writing as a policy analyst, physicist John Ziman for instance saw the transformation of science in terms of the transition from a focus on 'Cudos' to one on 'Place'.[6]

The New Production of Knowledge received very different treatments from historians and policy analysts. Within policy circles, the book and its sequel, published in 2001, have been widely discussed and used. Historical journals did not accord it a single review. Instead historians have discussed the book in sociology journals, and in general have been deeply critical. Terry Shinn suggested there was nothing new in the so-called Mode 2. In a broad-ranging paper in *Social Science Information* he looked at the Mode 1/Mode 2 dichotomy and the related so-called triple helix model highlighting the integration of science, industry and the state. Shinn argued that,

> science is per se characterized by constant but circumscribed flux, and that many of the changes occurring today, which so surprise and titillate some observers, in fact have historical precedents and counterparts.[7]

Historians have therefore, typically, argued that Mode 2 had its roots long before even World War II. Certainly there is nothing new to it, and one should emphasize continuity rather than change.[8]

Current historical research, however, offers progress from such polemical disputes. The historian Dominique Pestre has developed a long view which addresses change in the post-World War II science from the point of view of '*cités de justice*,' the common worlds of citizens' expectations and norms.[9] Increasingly informed discussions of science in the cold war that succeeded World War II have shed light on the particular mechanisms of massive growth in academe, industry and government funding.[10] Thus Glen Asner has argued that the distinction between basic and applied science, and the so-called linear model of scientific development that it underpinned was not just given analytical reality in the postwar years but was institutionally manufactured then, in response to challenges which had been posed by the incorporation of science within wartime strategy and missions.[11]

Above all in recent years, historians of science have begun to find analytical techniques for describing the changes in science. The concept of the 'Moral Economy of Science' following the example set by E.P. Thompson has seemed, for instance, fruitful.[12] It would seem be a particularly apt tool, for Thompson himself was reflecting on the culture of the working class in the industrial revolution, its collision with mercantile values, and outrage – leading to riot – when the values of the established moral economy were violated.[13] An analogous integration of the economic and the moral was to be seen in 1931, when the British Medical Association discussed with ire, if not violence, the issue of patenting medicine under the title of 'Ethics of Remuneration and Reward for Medical Invention'.[14]

Of all forms of scientific endeavor, the conventions of medical science were particularly threatened, in the prewar years. In part this was because they had been especially insulated from commercial concerns. Thus, even in patent law, historically, medicines had been given special status. Reflecting old established professional claims, the preparation of drugs was seen to be a private art and subject to patent while the drugs themselves were in the public domain. Thus, in France, since the early nineteenth century, and in Germany since the formulation of national patent legislation in the 1880s, patenting of medicines themselves had been explicitly forbidden.[15] These longstanding restrictions would affect regulations, opinion and practice across the industrialized world.

Penicillin – the case study

It is widely recognized that the advent of penicillin was not just a turning point in the use of medicines, but also a turning point in the commercial and public significance of drugs.[16] For the first time a wide range of infections could be cured almost instantly. Subsequently the scientific medicine industry grew enormously, and private medical research shot upwards. The growth of companies such as Pfizer, Merck and Glaxo to the highest rank of global companies can be directly attributed to this development.

Penicillin caused an excitement which meant that institutional hurdles were vaulted and cultural problems put aside, but they were not abolished. Moreover, this period of rapid development was short-lived. It had followed a period of low interest. Famously, little advance had been made in the immediate aftermath of the 1929 publication of Alexander Fleming's discovery that the penicillium mould exuded an antibacterial compound. In 1939 Ernst Chain, Howard Florey and then Norman Heatley in Oxford picked up the problem and successfully isolated the drug albeit in tiny quantities and demonstrated its efficacy. In July 1941 Florey and Heatley flew to the USA where new manufacturing techniques were quickly developed through a major coordinated effort. Overall, as a postwar US study showed, 36 US universities and hospitals, 11 companies, and four federal, state, local and national organizations held antibiotics research contracts from the Office of Scientific Research and Development (OSRD) during wartime.[17]

Two different programs for penicillin production were pursued in parallel. The first, building on the Oxford work, sought to extract penicillin from the natural exudate of fermented penicillium mould. It seemed to many that this was as obsolete an approach as producing aspirin from willow trees. Moreover, even if it had been proven in principle, the implementation to mass production was very challenging. The work required the collaboration of experts in several disciplines, including chemistry, mycology and engineering, and it was rushed. Over the two years after Florey and Heatley's arrival in the USA, the medical potential of the drug was proven but supplies remained very short. Deep fermentation seemed a distant prospect even early in 1943, but by early 1944 a large plant had been installed by the Pfizer Corporation. By Victory in Europe (VE) day in May 1945 supplies were plentiful for US civilians and the British were building the world's largest plant.

The second program was intended to provide a longer term but also cheaper route to penicillin production through finding a strictly chemical synthesis. Both British and US companies and academics made major contributions in this endeavor. Thus, the Oxford chemist Dorothy Hodgkin was the first to model the structure in 1945. Although, as a separate route to production, this program ultimately failed, between 1943 and early 1945, it had seemed to many key players the most likely means of providing cheap penicillin. A thousand chemists were employed in it, and the Merck Corporation alone invested US$800,000 in the prosecution of this campaign.[18]

Meeting the challenge of researching, producing and isolating large quantities of penicillin had been hard. Both approaches had involved practitioners of several disciplines and required them moreover to work together, an experience most of them had never had. This experience had challenged the normal norms of academic life: reward structures, what constituted understanding and adequate evidence, trust and the role of publications. Moreover it related clearly to two of the features often associated with Mode 2: its characteristic teamwork approach and the close linkage to private exploitation.

Penicillin itself was not patented. However, rather than taking this traditional response to a new drug for granted, British folk history of the achievement often revolved around the lack of British-owned penicillin patents. In the post-World War II years it seemed a symbol of an attitude to science simultaneously arrogant and sentimental that was now being smashed in a newly modern Britain and by an ascendant USA. It was remembered that Chain had visited the Secretary of the Medical Research Council, Edward Mellanby, in 1941 to urge that the new development be patented, but he had been quickly dispatched. The English mandarin had rebuked the German refugee on a short-term contract with the advice that he should not contemplate such an idea. Chain's treatment has been put down to snobbery, disdain for the son of the Jewish chemical manufacturer and ethics.[19] An interpretation of penicillin as introducing the patent issue to a naïve and backward establishment is, however, too simplistic. For a dozen years before Chain's encounter with Mellanby, patenting of medicines had been the topic of ferocious dispute both in the UK and the USA.

Patenting the sun

Even before the War, traditional attitudes to patenting had been challenged, particularly in the USA.[20] Those challenges had inspired their own backlash. Although overlooked by later historians, considerable controversy about the principle of patenting in medicine had already been caused by Harry Steenbock's experience of patenting Vitamin D enrichment.

In retrospect, the 1923 discovery by Steenbock at Wisconsin that antirachitic factors could be produced by irradiating such fatty foods as milk has been treated as a triumph, and the transfer of his rights to the newly created Wisconsin Alumni Research Foundation has been praised as an imaginative innovation.[21] By 1940 royalties, which would ultimately benefit the University of Wisconsin, had reached US$7.5 million. In 1933 and 1934, the very depths of the Depression, WARF provided US$325,000 to its university.[22] Analyzing the US situation, David Mowery and Bhaven Sempat have shown how between 1934 and 1939, several publically funded research intensive institutions, did develop patent policies.[23] They had of course been particularly hit by the budget cuts of the Depression years.

However, an extensive historical literature has also highlighted substantial ambivalence over patenting medical advances in many parts of American academe.[24] Great private universities such as Harvard had set their face against any patenting of discoveries made by their staff. The University of Pennsylvania, Harvard, Johns Hopkins and Caltech restricted patents of medical advances. At Columbia the College of Physicians and Surgeons ruled in 1930 that it was inappropriate for a member of its faculty to take out a patent on a medical discovery.[25] In 1933 Britain's distinguished Henry Dale opened Merck's new research laboratories and warned against patenting of medical products. He was quoted warmly by the *New York Times* in an article which was reprinted in *Science*.[26]

Conferences on the patenting of medicines came to be a fashion. When, in 1937, the American Chemical Society held a conference on the topic, under the title 'Are patents on medicinal discoveries and on foods in the public interest?' the presenters were not surprisingly affirmative but the issue was clearly still controversial and the papers were reprinted in full in *Industrial and Engineering Chemistry*.[27] The American Medical Association held a meeting to discuss the issue on 16 March 1939, though by then urgent world events were displacing news of such grappling with chronic problems – the same day Germany took over the whole of Czechoslovakia.[28]

The deep resentment against the patenting of vitamin enrichment, outside academe has been perhaps overlooked. To many at the time, it was seen as patenting the sun. In 1944, the patents were invalidated by a court which found them 'unwarranted and against the public interest'.[29] Assistant Attorney General Wendell Berge published a damning denunciation in his 1944 condemnation of cartels.[30] The chapter on vitamins began with the assertion that, 'The Wisconsin Alumni Research Foundation acts as a screen behind which a group of monopolistic chemical, pharmaceutical and food companies control Vitamin D.'[31]

In Britain, too there was an intense debate from the late 1920s about the proper forms of reward for development and ownership of its results. On the one hand, opportunities for profit encouraged scientists even at the most elite institutions to think of the commercial implications of their research, Thus, Robert Robinson, the Professor of Chemistry at Oxford University took out nine patents in his own name during the 1930s.[32] On the other hand, reserve about medical patents was even greater than in the USA. This reflected both universal values, but also lessons drawn from local history, honed over 20 years, which have not been widely discussed.

In 1919, with World War I over and German's famous chemical industry temporarily disabled, the British self-consciously sought to emulate the past success of the great competitor. A new patent act copied German practice and refused a monopoly on drugs and foods by generally preventing patents on such products and compelling patentees to grant licenses on those rights that had been awarded. This was straightforward public policy. Twenty years later, at the time of penicillin's introduction, experience in general and the particular resentment over the Steenbock patent in particular had strongly reinforced such a position.

To the British, the vitamin D story had particular resonance. The development of vitamin studies had proceeded very rapidly within a few years of the end of World War I, particularly through the mutual inspiration of British and American scientists. First an anti-rachitic factor had been discovered, then that was divided into vitamins A and D. Thus the name of Edward Mellanby would long be associated with the study of vitamins A and D. In October 1927, for instance, Walter Fletcher, Secretary of the Medical Research Council [MRC] wrote to Mellanby on the multiplying experiments with vitamin D: 'All this is of course a great triumph for you, for it all comes from your original work and simply confirms it.'[33] Other British scientists also made a contribution: Hume and Smith demonstrated that the mice were protected against rickets when the sawdust in their cages was irradiated.[34] In 1927 Rosenheim and Webster showed that ergestorol is the parent of vitamin D.

As scientists, such people were pleased that Steenbock built on this observation the discovery that irradiation of sterols led to the production of vitamin D. Steenbock's move to patenting his discovery however caused much alarm in Britain. Initially this was allayed by the protestation that patenting was merely a means of limiting use of the discovery to ethical applications. However the transition to WARF also marked the transition from science to business. Not just scientists but also businesses were bitter that hard-headed US lawyers were charging royalties and limiting use of what they saw as a 'British Discovery.'[35]

Steenbock was awarded a British patent for his process in 1926, shortly after his American application.[36] Nonetheless, the MRC itself waged a campaign against the patent. Since 1919 British law had severely restricted the patenting of chemicals for use as food or medical products, out of a wish to emulate German success in these areas.[37] In November 1929 testimony to the Board of Trade Departmental committee, which looked into changing patent law, the MRC was adamant that the Steenbock patent had set a dangerous precedent. The Council argued that effectively what had been protected was a ‘law of nature.’ The prospect was for research workers to feel that their efforts would be valued for the potential patentable value of the work they conducted.[38] To the argument that patents were allowed outside the medical field, the argument was put that there was no clear line in life sciences between discovery and valuable application. Therefore if Steenbock’s precedent were followed ‘this would bring a drastic and deleterious change in the present conditions of medical research work which the Council must feel it strongly their duty to resist in the interest of the progress of medical research.’

There were also real commercial differences between interests in agricultural Wisconsin and in industrial Britain. Rima Apple has pointed out that behind the Steenbock and WARF patent plans lay a conscious intent to restrict the use of vitamins to enhance the nutritive value of margarine. To Wisconsin’s dairy industry, margarine then and, for a long time thereafter, seemed a challenging threat. However, in Britain the possibility of enhancing the nutritive value of margarine eaten by the poor so that they would be protected against the tragedy of rickets seemed to be in the public interest. Moreover the margarine manufacturer, the Anglo-Dutch combine, Unilever was among the country’s greatest companies and was in any case having difficulty persuading a skeptical public to accept what might seem to be ‘doctored food’ and whose flavor might be tainted if fish oil were used as the vitamin source. As one civil servant wrote,

> It seems intolerable that we are debarred from freely using vitamin D, which is known to be an essential food factor, except on payment of a tribute to a foreigner whose contribution to the isolation and identification of the substance has been relatively small.[39]

The Minister of Health himself was shocked that children’s health was being put at risk by the administration of the Steenbock patent.[40] During the 1930s, discussion continued.

Thus a vigorous debate on medical patents was held at the British Medical Association meeting in 1931 in which the major doubt seemed to be over methods rather than intent.[41] By a large majority the meeting carried the motion supporting the proposition that it was wrong for a researcher to restrict ‘the use and knowledge of each discovery and invention for his personal advantage.’ The following year the Board of Trade invited the Royal College of Physicians, the Royal College of Surgeons and the MRC to an informal meeting. Walter Fletcher emphasized that the MRC’s stand was not based on abstract ethical principles, but was based on public interest grounds alone.[42] In essence his Council believed that patenting medicines would inhibit scientific development. In conclusion, the meeting called for an international treaty banning patents on medicines.

The argument for the special treatment of fat irradiation had been framed in part because it was a biological discovery whose antirachitic benefits were claimed even when they were not understood. However the central part of the MRC argument related to all medical patents, whether or not they depended specifically on biological knowledge.

In terms of the Mode 2 classification, the Vitamin D innovation had proved to be threatening because it had not just represented a distinctive scientific breakthrough but also a new way of managing science which would seem to threaten the process of science itself.

Penicillin patenting

The Steenbock patents acted therefore as a critical, if long-overlooked, part of the background to penicillin patenting. When penicillin appeared, a framework for making decisions about biomedical innovation was already in place. Mellanby could, unproblematically, object to Chain’s proposal – not because of anti-Semitism but because for the MRC, the Vitamin D experience provided a solid basis for resisting the patenting of penicillin developments. Mellanby soon had, however, to contend with the wishes of such British commercial companies as ICI, Boots and Wellcome to obtain penicillin-related patents. This issue might have seemed a straightforward question of industrial policy, but the underlying issue was similar to the general scientific problem: what properly lay in the public domain (to be regulated on behalf of the public by governments) and what should legitimately be considered private. Separate resolutions to this conflict were achieved in the two related but separate wartime penicillin programs.

In the long run, Merck and the other companies could deploy their patents to earn revenue from their pioneering work on fermentation routes to penicillin. Even Merck found it difficult, however, to persuade the British to pay royalties and ultimately did not push the case. A USDA scientist, Andrew Moyer, compelled to transfer any US patents to his employer was nonetheless free to take out British patents on the deep fermentation process. He applied for three British patents, on the use of corn steep liquor and on the use of lactose.[43] Moyer was however constrained by competing Merck patents on the one hand and the moral misgivings of his colleagues on the other. As a result he was unable to force large British corporations to pay him license fees. Compounding the confusion, companies such as Pfizer could charge handsomely for know-how which was not patented. In wartime, therefore, the possible patentability of the fermentation route to penicillin caused anger but not revenue.

As early as 1943 the diplomatic and commercial problems caused by the helter-skelter race to produce penicillin biologically were being observed, and they proved increasingly pressing.[44] As a result, a much more systematic approach to the apparently more significant, if longer term, problem of synthesis was sought. From 1944, there were discussions between the UK and the USA on a formal agreement for the sharing of patent information.[45] On the US side, the lead was taken by Vannevar Bush, on behalf of OSRD. In Britain, while the Ministry of Supply had taken over responsibility for penicillin production by means of fermentation, Mellanby on behalf of MRC kept control of chemical synthesis.

Bush explained his train of thought in a letter, dated January 1945, addressed to Mellanby.[46] In engaging with the British over patents, Bush's main anxiety was that a key discovery might be made giving control of penicillin to a single inventor. The danger was greatest in the event that penicillin would be successfully synthesized through a chemical pathway and that a single company would control that path. Fermentation, Bush argued, was subject to so many possible variations that it was felt that while companies might profit, there was no danger of peoples being held to ransom. Moreover British law would protect British customers (though not necessarily would-be British exporters) from extortionate demands.

Lord Halifax, British Ambassador to Washington, explained the issues to Foreign Minister Anthony Eden.[47] With a large number of companies involved in the penicillin business there was a danger that anyone could restrict development. Halifax expressed himself in very much the same language as had been used about Vitamin D: 'Any one of these patentees may be able to block some important step in production or levy extortionate tribute on a drug of benefit of humanity.' He went on to argue that nonetheless patents were inevitable in the age of corporate invention. Therefore compulsory licensing was the way forward. This of course raised the question of how much reimbursement would flow to the numerous patentees in the penicillin field when a system was installed.

In attempting to manage the potential problems that would be raised by successful penicillin synthesis, the British and the US governments preferred different solutions. The MRC, on behalf of the British government, argued that the two governments should control the inventions made in their two countries and license manufacturers to produce, charging a royalty related to their contribution to early developments. On the other side of the Atlantic, Vannevar Bush, on behalf of the US government, preferred that companies themselves should have rights according to their contribution.[48] In the event a combination of the two schemes was adopted. Bush would allocate patent rights according to organizational contributions to the penicillin project. The US and British governments would be allocated non-exclusive royalty-free licenses to any patent in the project.

Moreover, certain companies would be given rights according to their banding. The only company in band one (Merck) would obtain a royalty free non-exclusive license to use any patents from the whole enterprise for whatever purpose. Companies in band two, such as the British company ICI, would be allowed a royalty-free license to public patents held in either country but only for penicillin production.[49]

It is perhaps striking that the solution of allowing the companies themselves to create a patent pool was not chosen. However patent pooling had moved radically out of favor in US antitrust circles.[50] The case of Hartford-Empire *v.* the United States (1939) marked a radical hardening of the attitudes of the Justice Department against patent pooling. Vesting the ultimate rights in the governments but limiting the range of companies to whom they could license freely provided a way around this control.

The agreement was however very complex and into the 1950s there were international negotiations about penicillin patents, complicated by two factors. The US government's powers with relation to its employees oversees rights was a continuing sore. The second problem was those patents on the borders of bio-synthesis and chemical synthesis. Eli Lilly had identified that phenylacetic acid was a valuable additive to the brew.[51] If this were seen to be purely a result of studies of fermentation then the discovery was not covered by the Anglo-American agreement whereas if it were seen to follow from chemical work on the structure of penicillin then it could be seen to be subject to the agreement and therefore use would be available royalty free to participants.

The resolution seemed complex even to participants. Sir Henry Dale, complained of a confusion between national and commercial considerations and challenged Mellanby in 1945 to explain why his much simpler solution had not yet been implemented.[52] He favored a national holding trust to which all penicillin-related patents would be assigned, irrespective of their origin. It is perhaps ironic that the penicillin-synthesis agreement negotiated with such care proved to have little importance because of the failure of the project to produce a cheaper product. Nonetheless, that such an effort was made at a high level indicates the sense that not just for wartime purposes but also for peacetime, quite new arrangements were needed. Penicillin was seen as forcing the introduction of new patent procedures, breaking with prewar conditions which had already been seen as, in the long term, untenable. In both Britain and the USA the competing models of not patenting at all on the one hand, or control by individual companies on the other hand, had been challenged.

To the British people the moral of the penicillin tale was often that they must, in future, capture the benefits of research for themselves, rather than allowing their country to be hostage to foreign companies. The argument ceased to be, as it had been before the war, that the benefits from medical science belonged to the whole world. Now the state was proposed as the appropriate beneficiary. Public policy came to be oriented towards ensuring that such a lapse between great discovery and industrial application would never happen again. Patent law was changed in 1948 and the National Research Development Corporation with rights to patentable inventions created with public money was formally established to prevent a recurrence of the penicillin story.

As Toine Pieters has shown in his study of the development of interferon during the 1950s, the MRC's wish to preserve a domain for pure science was matched by a determination to ensure that the penicillin story would not be repeated.[53] The postwar scientist was seen as accountable to the taxpayer and to the citizen. Howard Florey wrote to a friend in Australia:

> Largely as a result of this experience, every substance discovered by a medical man or a chemist is now patented in this country as national policy, if it shows the slightest signs of being useful.[54]

So whereas the penicillin crisis showed many structural similarities to the vitamin D crisis, the responses proved rather different. The MRC had responded to Steenbock by attempting to hold the old line and maintaining the old moral economy. The response to penicillin had been to accept the end of the old, and to create a new National agency to act on behalf of the public good. The right of the National Research Development Council and its successor the British Technology Group to university discoveries lasted until 1985.[55]

In the USA, similarly, the benefits of publicly-funded research were reviewed. A three volume study of federal regulations was published in 1946 with a view to standardizing the diverse regulations which had emerged across the public sector.[56] Some agencies allowed exclusive licenses to private contractors – essentially assigning them the patents, others permitted only non-exclusive licenses. The report came down firmly on the side of the latter. Research funded from federal funds was kept in the public domain. It was not as if the turbulent wartime years had never been. The number of university owned patents increased from a handful during the 1930s to about a 100 in 1950, but they did not keep multiplying, and did not exceed 150 until the end of the 1960s.[57] Penicillin development had disrupted the old world, rather than leading directly to the new.

Even US pharmaceutical companies experienced the fruits of ambivalence about patenting. In the 1950s the price of penicillin collapsed as new entrants piled into the industry, whose product had not been patented. However there was a determination that the newer antibiotics, such as the tetracyclines, should be much more closely controlled by US patents and their price was kept from collapse. During the late 1950s the patent and profit mindedness of the industry was challenged by both the Federal Trade Commission and the Senate as prewar concerns were brought to bear on the newly booming pharmaceutical industry. Campaigners who in the 1930s had seen patenting as a cause of the Great Depression continued their struggles through the 1950s, particularly deploying Senate support.[58] Gradually, however, the emphasis moved from a concern with patents to anxieties about safety. Although the outcome would be the strengthening of the Food and Drug Administration as the guardian of the public interest, the right to patent was untouched.

The response to the penicillin experience was different from the earlier denial; it did represent a rejection of the prewar marginalization of the issue of ownership. However, neither in Britain nor in the USA was there a straight line between the wartime experience and the private science of the 1980s. The variety of experiments with managed public ownership of the fruits of medical science, suggested a much more complex process than a mere take-off for the patent flight of the 1980s.

Teamwork

If patenting threatened the moral economy of science by offering novel but seductive financial inducements to some scientists and institutions, the teamwork that was emerging as the characteristic form of scientific research was threatening the more traditional rewards for science. The fame that was earned by authorship was diluted by multi-authorship. This turn provided the backdrop to jealousies and anxieties in the penicillin story but also indicated fundamental threats to the traditional process of academic science, as deep as patenting.

Again, teamwork had been widely familiar in prewar industrial research. Although it was not widely familiar in academe, in the 1930s, the practice had begun to emerge even in basic science.[59] A 1953 study of *Science* magazine showed a growth of number of papers with three or more authors rising from 0 in 1921 to 20 in 1936 to 113 in 1951. Single authored papers had merely doubled in number across the same period.[60] A study of papers in bioscience showed that between 1934 and 1938 there had not been a year in which the proportion of papers authored by three people exceeded 20%. In the period 1940 to 1950 the proportion fluctuated between 20 and 25%.[61] This shift could be attributed in part to wartime experience of such projects as penicillin. In the case of cancer research, for instance, there was a clear link between that success and anticipations of the future.[62] Writing in the *Journal of Personnel Management*, the biochemist and cancer researcher Murray Shear, distinguished between the traditional model of a scientist working with assistants and the new model of a true partnership of equals:

> The experiences of the last decade, especially in industrial laboratories and in some of the war projects, have shown that problems in applied science in particular may be successfully handled by the new kind of research team.[63]

The renewed vigor given to the distinction between basic and applied research served to save science from a full restructuring in the face of such radical shifts. During the 1950s the proportion of multi-authored articles in several chemical journals fell back, only in the applied science journal *Industrial and Engineering Chemistry* did it persist.[64]

Creating science with teams meant the pursuit of quite new work styles, reward systems and prestige structures for researchers in the laboratory. These changes were accepted, sometimes reluctantly, because of the urgency of wartime need for such products as penicillin. In recounting the style of wartime penicillin research at the University of Wisconsin, then graduate student David Perlman described the novelty of the experience of frequent meetings and of 'vigorous discussions,' bringing out the novelty of what would later be a common experience in science.[65] The allocation of credit created by such teamwork was also proving difficult. In an era of letter writing, such an anxiety loomed large in a mass of correspondence between actors across the penicillin project. Florey was constantly aware of the jostling in his team, and in particular of Chain's suspicion that he was being sidelined.[66]

Old debates were reignited, when histories were written after the war. These could be understood in three ways. First there was the normal jealousy of ambitious scientists. Second, there was a very particular wartime context. Scientific results could not be published at all, so the normal moral economy was suspended and there was a problem of reconstruction after the war as wartime results were released. Third, and more profoundly, there were changes in the moral economy of science engendered by the widespread processes of teamwork.

When for instance Chester Keefer who had been responsible for distributing penicillin during the war sent the draft of his article for a volume dealing with advances in military medicine, he got this response from the Northern Regional Research Laboratory that had first entertained Heatley and Florey and where deep fermentation of penicillium mould was pioneered.

> For example, there is a great play made of the fact that Drs Florey and Heatley came to this laboratory and that Dr Heatley worked for some time. No one could question the value of his contribution, but I dare say that it is certainly no greater than that of W.H. Schmidt, R.G. Benedict, and many others. I note that the names of these investigators were deleted; whereas, for example, that of Max D. Reaves was left in the text. Mr Reeves did important work in designing a number of pieces of equipment which were extremely useful to us, but his contribution to the problem certainly could not compare with that of some other individuals whose names are omitted completely.[67]

Even Norman Heatley was criticized for a draft historical account of work at the Northern Regional Research Laboratory to which he had brought penicillin. He was told it was so difficult to identify key players that in would be best to attribute the laboratory as a whole rather than to pick up individuals.[68]

The debate over credit therefore reflected the more fundamental shift from the lone scientist to the team which people such as Heatley or Chester Keefer hardly understood. In the case of streptomycin the question of credit even went to court. A student of Rutger's Professor Selman Waksman, Albert Schatz, claimed a share of the royalties in streptomycin, which he rather than the professor had been the first to observe in 1943.[69] A new moral economy was indicated by disorientation experienced by such men as Waksman who might have been the most worldly-wise. If such scientists had been experiencing the continuation of an existing social code, they would have understood what was happening around them. Their anomic behavior might have indicated that the phenomena they were experiencing was strangely new and bizarre.

The controversy over contemporary teamwork was echoed in historical disagreements over the history of penicillin. In May 1948 the young Harvard historian of science I.B. Cohen wrote to Howard Florey seeking the opinion of the distinguished scientist. He had recently published *Science in the Service of Man*.[70] The scope was as wide as the title implied, but, Cohen complained, criticism had been focused on a single chapter, his treatment of penicillin. There, Cohen had attempted to explain the decade-long lapse between Alexander Fleming's announcement in 1929 of the anti-bacterial power of the juice produced by penicillium mould and the development of a drug. Cohen's explanation of the decade-long delay was that the techniques and understanding of the 1920s was just not up to the job. The overall scientific situation had been wrong. Cohen referred darkly to critics who believed in 'an overall complete planning of science.' He was referring to Waldemar Kaempffert who had criticized *Science Servant of Man* in the *New York Times*.[71] Kaempffert who was the USA's leading science journalist had testified to Congress on behalf of the Kilgore Bill. For him the key reason for the success of the attack on penicillin was the relationship between applied and basic science. He did not see why organization which had proved so successful in wartime should not have been able to deliver advances in quantum physics or genetics.

Cohen might also have complained about the *Lancet*. A leader portrayed the debate between Fleming and Florey in terms of styles of research, ancient and modern.[72] While it did attest to the observations and experiments of 'brilliant individuals' it also pointed out that 'the development and extension of their work calls increasingly for large teams, complex organization, and great financial resources.'

Conclusion

The experience of penicillin highlights the dependence of wartime innovation on prewar cultural resources, but also the challenges posed by its radically new implications. Clearly, even in the prewar era, teamwork was already a well-established form, particularly in industry. Rasmussen's argument that the interface between industry and academe was already an accepted habitus for the prewar scientist is cogently put. However, for most biomedical scientists, teams on the one hand and patents on the other were very much on the cultural margins. The patents for insulin, irradiation to produce vitamin D and Salvarsan had been the frequently attested exceptions to a general rule.

Although the 'novelty' of the moral economy might seem an elusive concept, it was indicated by the anomic responses of the actors as they negotiated their wartime experience. We have examined some here. Others were to be found in the Manhattan project. Hounshell has described the annoyance experienced by physicists in the Chicago-based plutonium project, the so-called 'Met lab' when Du Pont engineers endeavored to tell them what to do. Neither from the engineering nor the scientific sides was there experience of such a collaboration and their occasionally overflowing irritation can be taken expression of the novelty of the relationship.[73] That would change in the years to come. Galison and Krige have written of the adeptness of certain physicists who had learned to negotiate between the different tropes of science and engineering.[74]

In reviewing the experience of scientists working in industrial research during the 1950s, Shapin has recently pointed out the lack of evidence that they were experiencing 'role strain.'[75] He does accept, nonetheless, that the culture of science in academe and industry could differ. Certainly, stresses in wartime science were greater, and the more surprising because they were experienced in academe and government laboratories, and the response more easily detectable.

The scientists caught up in wartime teamwork experienced its culture and moral economy as profoundly different from the earlier academic model with which they had been accustomed. The postwar answer was, for many years, a hardening of the divisions between basic and applied science. Vannevar Bush introduced his defense of elite basic science. *Science the Endless Frontier*, by appealing to the conditions under which penicillin had been discovered:

> We all know how much the new drug, penicillin, has meant to our grievously wounded men on the grim battlefields of this war – the countless lives it has saved – the incalculable suffering which its use has prevented. Science and the great practical genius of this nation has made this achievement possible.[76]

A substantial literature has explored the meaning Bush's paean to 'basic science'. The small scale and late delivery of the National Science Foundation which ultimately resulted has been taken demonstrated the political limitations of the model. However, as such scholars as Hounshell and Stokes have argued, Bush did evoke the importance of what would be called the linear model and the propriety of government funding of its roots.[77] Through the creation of organizations such as the National Science Foundation, the conditions for 'basic science' were formalized and protected, to be recalled in the 1990s as 'Mode 1'.

It is perhaps more interesting than merely ironic, that Bush, leader of the wartime OSRD and ultimately responsible for coordinating penicillin development should use its story to introduce the importance of basic science. The penicillin projects were not just great technical successes. They also exemplified cultural threats to the moral economy of basic science that had also been valued highly. In this light, the postwar regimes of funding may be seen as an albeit, temporary, response to such challenges as the success of penicillin.

This paper has explored how the experience of great wartime projects unsettled old compromises. For the outstanding success of such practical outcomes as penicillin had proved as challenging to the institution of science as they were encouraging to the health of patients. However, it has not suggested that so-called Mode 2 science can be traced in a straight line back to World War II. Both in the postwar Britain of the penicillin scandal and in the postwar USA of Vannevar Bush, it seemed to academic leaders that traditional models of science needed revision so that science could be both protected and exploited. The renewed emphasis on distinctions between 'basic' and 'applied' research, which would be swept away by 'Mode 2,' can be seen as an attempt to manage challenges such as had been posed by penicillin.

Acknowledgements

The author is happy to acknowledge the permission of the Medical Research Council to reproduce quotations from unpublished MRC material. Crown copyright material is reproduced with the permission of the Controller of HMSO and the Queen's Printer for Scotland. Earlier versions of this paper were presented to the Seminar in the History and Philosophy of Science at Cambridge University, and to the annual meetings of the American Association for the History of Medicine and of the British Society for the History of Science. The author would like to express his appreciation for the comments of participants, for the advice of Michael Gibbons, Rima Apple and Nick Rasmussen, for the stimulus of Jean-Paul Gaudillière and for the opportunity to discuss its subject with Paul Forman. This essay complements the broader treatment of the history of penicillin presented in: Robert Bud, *Penicillin: Triumph and Tragedy* (Oxford University Press, 2007).

Notes

1. On this huge topic see for instance, Magnus *et al.*, *Who Owns Life?*; Kevles and Hood, *Code of Codes.*
2. For the ideal see Ben-David, *Scientist's Role in Society*.
3. Rasmussen, 'Moral Economy.' Also see Swann, 'Academic Scientists and the Pharmaceutical Industry.' For the more general context of applied science before World War II, see also Kline, 'Constructing Technology.' For post-war science see Rasmussen, 'Of "Small Men"' and Hounshell, 'Rethinking the Cold War.'

4. Hughes, 'History of Science.'
5. Gibbons *et al*., *New Production of Knowledge*; Nowotny *et al*., *Rethinking Science*.
6. Ziman, *Prometheus Bound*, 99.
7. Shinn, 'Change or Mutation?'
8. Peter Weingart was also withering, complaining of the 'flashy names' given by the discoverers of apparently 'new forms of knowledge-production'. See Weingart, 'From "Finalization"' to "Mode 2".' To Weingart these all constituted rediscoveries in a more politically acceptable manner of the 'finalization' thesis promoted by the Starnberg group in the 1970s.
9. Pestre, *Science, argent et politique*.
10. See for instance Mowery *et al*., *Ivory Tower*.
11. Asner, 'Linear Model'.
12. See for instance Daston, 'Moral Economy;' Kohler, *Lords of the Fly*; McCray, 'Large Telescopes.'
13. Thompson, 'Moral Economy'.
14. See Annual Representative Meeting, 'Medical Ethics.' Similarly, the norms of science formulated by Robert Merton within a few years served not merely as abstract analyses but also as historical documents of scholars attempting to make sense of a world under threat. Merton, 'Normative Structure of Science.' See also Cole, 'Merton's Contribution;' Hagstrom, *The Scientific Community*. On the interpretation of the sociology of science as a response to threats to science see Fuller, *Thomas Kuhn*. See too Hollinger, 'Science as a Weapon.'
15. See Hancher, *Regulating for Competition.*
16. See Bud, *Penicillin: Triumph and Tragedy.*
17. Federal Trade Commission, *Economic Report*, Table 1, 48.
18. Swann, 'Search for Synthetic Penicillin.'
19. Clark, *Life of Ernst Chain*, 56–58.
20. The negotiations over penicillin can be seen as a special case of a more general wartime negotiation between the USA and the UK over patents owned by governments. From 1942 this was managed through the Patent Interchange Agreement that established the rules by inventions in one country could be licensed for the duration of the war to a manufacturer in the other without prejudice to postwar licensing. It applied, above all, to jet engines, radio and electronics. The background is discussed by Zimmerman, *Top Secret Exchange*.
21. Apple, 'Patenting University Research.' However even in the USA at the time there was a recognition that the WARF patenting of vitamin D manufacture by radiation was controversial. See Fischbein, 'Medical Patents'.
22. McKusick, 'Study of Patent Policies,' 213.
23. Mowery and Sempat, 'University Patents,' 790.
24. Rasmussen, 'Moral Economy;' Weiner, 'Patenting and Academic Research;' Etzkowitz, 'Knowledge as Property;' Mowery and Sampat, 'University Patents.'
25. Mowery and Sampat, 'University Patents.' See also Mowery *et al*., *Ivory Tower*; Palmer, *Survey of University Patent Policies*.
26. *Science*, 'Medical Patents.'
27. American Chemical Society, 'Are Patents on Medicinal Discoveries.' See particularly Fishbein, 'Medical Patents.' See also Connolly, 'Should Medical Inventions be Patented?'
28. 'Report of the Conference on Medical Patents'.
29. McKusick, 'A Study of Patent Policies,' 215.
30. Berge, *Cartels*, 82–111.
31. Berge, *Cartels*, 82.
32. Morrell, *Science at Oxford*, 351.
33. Fletcher to Mellanby 29 October 1927, PP/MEL/J.1, Wellcome Library for the History and Public Understanding of Medicine.
34. John Carnwarth, 'The Steenbock Patent,' 15 May 1932, in 'Therapeutic Substances. Vitamins A and D. Steenbock and Other Patents,' MH 58/130, National Archives, UK.
35. 'Notes on the Steenbock Patent,' 16 January 1930 appended to FHC to Mellanby, PP/MEL/B.8, Wellcome Library for the History and Public Understanding of Medicine.
36. Harry Steenbock, 'Improved Manufacture of Edible Products.' GB236197 Publication date: 12 November 1926.
37. 'Memorandum on the Legal Position In Regard to Medical Patents,' in 'Medical Research Council: Medical inventions and Discoveries: Remuneration and Reward: Patents and Designs Bill, 1932,' MH58/260, National Archives, UK.

38. 'Professor Steenbock. Patent and the Photosynthesis of Vitamin D,' paras 12–16 in 'Memorandum by the Medical Research Council on the Patent Law in Relation to Medical Research,' MH58/260, National Archives, UK.
39. Thomas Carnwarth to Mr Beckett 6 May 1932, MH58/130, National Archives, UK.
40. DCLW to the Comptroller of Patents, May 1932, MH 58/130, National Archives, UK.
41. See Annual Representative Meeting, 'Medical Ethics,' 56–61.
42. Statement of Walter Fletcher, 'Informal Discussion on Medical Patents,' in 'Remuneration and Reward. Patent and Designs Bill,' MH58/260, National Archives, UK.
43. See Williams, *Florey*, 308–14; Bud, 'Penicillin and the New Elizabethans'. The patents issued to Moyer were 618415 and 618416 in 1948–49 and 624,411 in 1949–50. Also Neushul, 'Science, Government.'
44. In August 1943 Mellanby floated the idea of a Committee to coordinate work on penicillin synthesis, with Sir Robert Robinson. See Mellanby to Robinson, 20 August 1943, FD1 6835, National Archives, UK. The committee first met on 13 January 1944, under the chairmanship of Sir Robert Robinson who formally proposed terms of reference. On 2 December Mellanby was invited to a meeting of the 'Committee for Coordinating Departmental Policy in Connection with Patented and Unpatented Inventions' at the Board of Trade, specifically to discuss penicillin patents. There Mellanby explained how the synthesis research was being better managed than the early fermentation work. 'Committee for Coordinating Departmental Policy in Connection with Patented and Unpatented Inventions,' 2 December 1944, FD1 6845, National Archives, UK.
45. Mellanby summarized past and current discussion in a minute of a meeting held on 30 January 1945, FD1 6845.
46. Bush to Mellanby, 30 January 1945; Mellanby to Bush, 15 February 1945, FD1/6845 and attached memorandum dated 13 February 1945, FD1/6845, National Archives, UK.
47. Lord Halifax to Anthony Eden, 5 January 1945, BT 305/4, National Archives, UK.
48. Oakley to Dommett, 4 April 1945, BT305/4, National Archives, UK.
49. The only class 1 company was Merck. In the second class were Squibb, Eli Lilly, Therapeutic Research Corporation and ICI. See 'The Anglo-American Penicillin Agreement,' 25 January 1946, Treaty Series No. 4 (1946) Cmd 6757.
50. Shairfer, 'Patent Pools;' Wells, *Antitrust and the Making of the Postwar World.*
51. See 'Statement of Case Submitted by the Therapeutic Research Corporation of Great Britain Ltd to the Medical Research Council 6 March 1953, FD1/5341, National Archives, UK.
52. Dale to Mellanby, 26 February 1945, FD1 6845, National Archives, UK.
53. Pieters, *Interferon*, 59–60
54. Florey to Dr Clive Fitts, 20 May 1952, 98HF 38.3.22, Royal Society.
55. However, the belief in pure science was also to be strengthened. One can see the strong reaction against the patenting approach in the culture of the Medical Research Council's Laboratory of Molecular Biology established in 1963. Even at the time of the debates over monoclonal antibodies in the late 1970s it was said not to have a patenting culture. See de Chadaravian, *Designs for Life*, 358.
56. US Department of Justice, *Investigation*. See also Jaffe and Lerner, 'Reinventing Public R&D' and Eisenberg, 'Public Research and Private Development.' An interesting indicator of shifts in attitudes to patenting science in the USA is provided by Turchetti's study of Enrico Fermi's attempt to profit from his discovery of the use of slow neutrons to stimulate nuclear fission. See Turchetti, '"For Slow Neutrons, Slow Pay".'
57. Mowery and Sempat, 'University Patents,' 798.
58. Bud, 'Antibiotics, Big Business and Consumers.'
59. Derek de Solla Price's book is the best known of a number of studies tracking the growth of multiauthorship. De Solla Price, *Little Science Big Science*, 86–91.
60. At the same time the secretary of the AAAS sounded a slightly sceptical note in his foreword, pointing out that: 'like so many things that acquire sudden prominence, team research may currently be glorified beyond its merits, and there are those who lack the historical perspective to recognise its antiquity' See Thomson, 'Editor's Critique.'
61. Clarke, 'Multiple Authorship Trends.'
62. Bud, 'Strategy in American Cancer.'
63. Shear, 'Teamwork in Scientific Research.'
64. Bush and Hattery, 'Teamwork and Creativity in Research,' 365.
65. Perlman, 'How Penicillin Research.'
66. Florey to Fulton, 7 December 1943, Yale University Archives.
67. Kenneth Raper to Chester S. Keefer, 3 July 1947, Box 2 Liaison Folder, RG227, US National Archives.

68. Raper to Norman Heatley, 15 January 1948, Box 2 Liaison Folder.
69. Wainwright, 'Streptomycin.'
70. Cohen, *Science, Servant of Man.* See also Cohen's letter to Florey, 20 July 1948, HF37.1.39, Florey Papers, Royal Society.
71. Kaempffert, 'On Great Discoveries.' Private communication from the late Professor Cohen to Robert Bud.
72. *Lancet*, 'Penicillin and Modern Research.'
73. Hounshell and Smith, *Science and Corporate Strategy*, 338–40.
74. Galison, *Image and Logic.* In exploring postwar experimentation he foregrounds the development of teamworking in physics and the novel role of people such as Cecil Powell, the experimental particle physicist. See also Krige, 'The 1984 Nobel Physics Prize.' Paul Forman looks at the context of postwar physicists in Forman, 'Recent Science.'
75. Shapin, 'Who is the Industrial Scientist?'
76. Vannevar Bush, *Science: The Endless Frontier*.
77. See Hounshell, 'Industrial Research. Commentary'. Stokes, *Pasteur's Quadrant*, 45–57. Gustorn, 'Critical Appraisal in Science and Technology Policy Analysis.' For a dissenting view see Edgerton, '"The Linear Model" Did Not Exist.' See also, Kevles, *The Physicists.* On the development of conceptions of basic science, see Calvert, 'Idea of "Basic Research"' and 'What's Special about Basic Research?'

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