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BIOLOGICAL WARFARE WARRIORS, SECRECY AND PURE SCIENCE IN THE COLD WAR: HOW TO UNDERSTAND DIALOGUE AND THE CLASSIFICATIONS OF SCIENCE

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SUMMARY

BIOLOGICAL WARFARE WARRIORS, SECRECY AND PURE SCIENCE IN THE COLD WAR

This paper uses a case study from the Cold War to reflect on the meaning at the time of the term 'Pure Science'. In 1961, four senior scientists from Britain's biological warfare centre at Porton Down visited Moscow both attending an International Congress and visiting Russian microbiological and biochemical laboratories. The reports of the British scientists in talking about a limited range of topics encountered in the Soviet Union expressed qualities of openness, sociologists of the time associated with pure science. The paper reflects on the discourses of "Pure Science", secrecy and security in the Cold War. Using Bakhtin's approach, I suggest the cordial communication between scientists from opposing sides can be seen in terms of the performance, or speaking, of one language among several at their disposal. Pure science was the language they were allowed to share outside their institutions, and indeed political blocs.

On the night of 12 August 1961, Berlin was divided east from west by the sudden appearance of the wall which gave physical reality to the Iron Curtain. A week earlier, the Soviet vessel MV Estonia, sailing from London, had docked in the harbour of Leningrad, USSR.

Key words: Biological warfare - Pure science - Cold war - Secrecy

Disembarking, the board of the International Union of Biochemistry was met by the organisers of the conference and taken by train to Moscow. Six thousand biochemists would take part in a meeting, beginning on 10 August, and made famous by Nirenberg's announcement that he had managed to synthesise a protein in a cell-free environment of RNA[1]. To bring the American delegation, three DC7 planes had been chartered by the American Society of Biological Chemists. The atmosphere among the scientists seems to have been jovial, in part because their number was so huge they were ungovernable. Informal dinner meetings between guests and hosts took place both there, and afterwards. The mood in town was celebratory following the successful circumnavigation of the earth by the Soviet Union's second astronaut, Titov. Scientific achievement was being used both inside the country and worldwide to promote the international standing. It was a time, apparently, of détente and because of a news blackout, the delegates were unaware of the construction of the Wall.

The Cold War was however not alien to all the delegates. The British delegation included four senior members of staff of Britain's secretive but very large Microbiological Research Establishment at Porton Down, part of the country's War Office, dedicated to preparing against the eventuality of biological warfare with the Soviet Union. The scientists at Porton were working on plague bacillus, producing large quantities to model an attack, for defensive purposes, but, truth to tell, the line between defence and offence in this technology was narrow. The group included the distinguished biochemist, Dr L. H. Kent, who was also the Establishment's distinguished Deputy Director, with the rank of Major. There was no secret as to who they were or, indeed, on their return, that they had gone. Early in the visit, Kent and his wife invited a Russian colleague, the plague specialist Evgeny Gubarev to meet in their hotel[2]:

> *After a few minutes the floor porter came in and sat down at the other end of the room and quite obviously listened in. The ladies were discussing*

> *family and domestic matters in Russian, the men science in English. After about 5 minutes the porter evidently decided there was nothing of interest and disappeared*[3].

Through the efforts of Gubarev and the wife of Dr Kent, eight visits to Soviet biochemical and microbiological laboratories were arranged. The institutes visited included the New Antibiotics Unit of the Academy of Medical Sciences, the Institute of Industrial Fermentation and the anti-plague institute.

This was not the first encounter between Porton scientists and their Comintern counterparts. In 1958, the Czechs had hosted the first of what became a series of meetings on the topic of continuous fermentation. This is a methodology of cultivating micro-organisms ideal for producing very large quantities of an homogenous fermentation product, such as a biological warfare agent, either for offensive use or, in a defensive role, to model an attack. That is why the British at Porton Down were working on it. On the other side, it had become known in the west, that the Czechs were also using just this technology to manufacture biological warfare agents. The peaceable Swedish microbiologist at the Karolinska Institut, Carl-Gören Hedén, a former student of the instrument pioneer Tiselius, who had developed a continuous fermenter was horrified when he was told that the Czechs, having bought one of them, were using it for just this purpose at the Institute of applied microbiology[4]. Indeed Hedén was so shocked that he changed subject fields. Thus the very public and official circulation of these people and this knowledge between agents of opposing sides is particularly surprising at a time of Cold War.

Certainly, at a time when Bell was proposing the end of ideology, and the development of even more lethal nuclear weapons which arguably rendered obsolete the use of biological warfare, the time seemed to be propitious for communication between biological experts[5]. Indeed, scientists working at Porton thought it would be civilianised[6]. The microbiologist John Postgate had taken a sabbatical from the centre, to the US, confident he would return to a civilian organisation – a plan reversed in the wake of the subsequent Cuban missile crisis. More generally, despite the Berlin Wall, the atmosphere was of détente. Since 1958, civilian exchanges had been undertaken between US and Soviet bioscientists[7].

However this visit seems very different in tone from the observations of Anne Geltzer who has reviewed the American visits to the Soviet Union. Kent was not engaged in a stiff diplomatic visit but rather in a visit to people he counted as friends. Of course the Cold War was a frame for the meetings, as it was for the Americans, since Kent himself was employed as a warrior. Yet what is striking is the level of communication as well as its obvious limitations. Moreover, to describe this event in terms of détente would be too simple, as the erection of the Berlin Wall demonstrated. The tours had a geopolitical consequence, and the high level support they required suggests that this was also intended. The West was acquainted in this way with Soviet capabilities and military potential in this area. A decade later, in 1972, the biological weapons convention would lead to the ending of western biological weapon programmes as a quid pro quo for ending their Soviet counterpart.

Brian Balmer has studied secrecy at the biological warfare laboratory at Porton as a case study in the historical sociology of biological warfare[8]. He points out the complex negotiation between the scientists and security at Porton, which was extremely complex. Recently other historians of science have seen the interest in the issue of secrecy. A special issue of the *British Journal for the History of Science* addressed the topic[9]. Several of the papers discussed the way secrets are part of the ordering of science, since their availability enables such issues as 'whom do I trust with what' to explain what could be discussed. John Krige has reflected on how the sharing of 'basic science' was an important part of the process by which the

US exercised its post-war scientific hegemony[10]. This literature is a valuable pointer to the interest that lies in the negotiation of secrecy and openness during the early Cold War. In this paper I shall reflect on the process of knowledge circulation and what it means for our understanding of pure and applied science at this time.

My argument will proceed in three stages. First I shall look at the moral definition of science as it emerged early in the Cold War, and the role of secrecy and openness in that moral discourse. Then I shall look at the ways the old phrases, pure and applied science, were adopted as terms within this moral discourse. Finally, I shall return to the question of pharmaceuticals, chemical warfare agents and the remarkable tour of the British Cold Warriors.

Secrecy has been particularly interesting to the historian because it so runs against what might be called the moral interpretation of science, whose values were defined during the course of the Second World War by Robert Merton[11]. This had been expressed in terms of opposition to fascism and conservative Christianity. However in the postwar years, what David Hollinger has called a Kulturkampf turned against Communism. The defining moment for this moral interpretation of science was the meeting in Hamburg in 1953, the proceedings of which were published as science and freedom. The location was carefully chosen, for the city itself was still visibly a victim of war, criminality and of the abuse of science, having been one of the first victims of intensive firebombing. The main theme of the meeting was that the pure scientist was accountable to neither the state nor industry but only to the scientific community which was international[12]. This internationalism and the cosmopolitan nature of science were essential in the thinking of this community. It resonated with Merton's norms expressed a few years earlier. Just a few years before Conant had spoken, Robert Merton had articulated what he saw as the four norms of science[13]. In addition to organised scepticism and disinterestedness, he identified communism and universalism which both grew out of a sense of science as an unimpeded international endeavour.

Elena Aronova has traced the relationships between the Hamburg meeting and the ongoing development of the Congress of Cultural Freedom (CCF)[14]. This had a clear agenda she argues, pointing out how its leader, the sociologist Ed Shils, wanted to develop a theory of the new postwar 'technical age'. The tendency to a sober post-ideological time had its advantages but also its risks as unlike the arguments put in 1953, Aronova argues that the CCF emphasised its non-ideological character. 'Minerva', Shils said in 1962 when the journal was founded, 'will be concerned with the indirect as well as the direct influence of the Cold War on the role of science and scholarship and on the performance of their true calling'[15].

Of course, subsequently, the non-ideological quality of the CCF has been disputed particularly after CIA funding was revealed. Moreover post-Mertonian sociology of science has questioned the historical accuracy of Merton's descriptions. The expressions in Hamburg should nonetheless not be seen just as anti-Soviet propaganda during the depths of Cold War. Merton, as Hollinger has pointed out, worked at Columbia where Hofstadter and Metzger were using the valuation of science to combat right-wing McCarthyism. Moreover the internationalism of science was fostered by proponents of the Left. Unlike the aftermath of the First World War, the years following the Second World War saw an attempt by the victors to reincorporate Germans into the worldwide scientific community. UNESCO was conceived in 1944, initially without the inclusion of science but accepted the addition through the forcible arguments of Huxley and of Joseph Needham its first head of science. Although these men are generally associated with the left, and with views opposing those of the CCF, they shared the same sense of the global quality of science[16].

At the heart of such understanding of science was its openness. It was well known of course that science was not, in fact, all open. The obvious counter-examples were its practice in the military and in commerce. My suggestion is that the distinction between these apparent distortions and proper science was equated with the distinction between the two traditionally differentiated discourses of pure and applied science, terms which had been used for one hundred and fifty years.

Classically it has been hard to distinguish between pure and applied science in terms of inherent knowledge qualities. Certainly the arguments made by functionalist sociologists in the 1950s – that scientists in industry would suffer 'role strain' and moral qualms – are hard to sustain and have been dismissed recently by Shapin[17]. To the historian there is the question of what was therefore then meant and what was at stake at that time, early in the Cold War. Calvert has studied attitudes to Basic science at the beginning of the twenty-first century. She found six kinds of interpretation: 1. 'Epistemological (Nature of knowledge produced); 2. Intentional (Aims of the research); 3. Distance from application; 4. Institutional (Where carried out); 5. Disclosure norms (How disseminated); 6. Scientific'[18]. From the early twenty-first century and an era of 'technoscience' there has been a tendency to wonder what the fuss was about. Categories of science, pure and applied are interwoven into such an intricate embroidery of technoscience that such words seem gross and, to some, meaningful only for statistical purposes[19].

Nonetheless, sixty years ago these categories seemed really to matter to many. Confusing pure with applied, or mistaking the boundaries were the topics of polemics and politics. In his book *Understanding Science* about a proposed course for American undergraduates on the 'tactics and strategy' of science, Harvard President J. B. Conant wrote:

> *I shall not attempt to prescribe how the instructor should balance the contending views. To my mind it is important, however, that he should point out*

> *that some modern writers have declared that "Science is the product of economic conditions of society, and its social function is to benefit the ruling classes of society"; and this group have minimized any distinction between pure and applied science or between science and technology. On the other hand, such contentions have been vigorously attacked as representing a false interpretation of history and a pernicious ideal for the future*[20].

The influential chemist and philosopher Michael Polanyi was adamant that the two categories had to be kept separate[21]. If he could be described as idiosyncratic, his emphasis on the synergy between pure science and its enabling community was widespread. An article in the journal Philosophy of Science published in 1956 and fairly well known at the time (receiving thirty citations since its publication), identified pure science with the community who provide intellectual sustenance and dialogue[22]. In the past, the distinction between two types of science had been understood in other terms; often it had been associated with any research conducted in industry, but that association had become increasingly insupportable, and came under particular challenge through the postwar support for research from military-industrial sources.
Whereas academic and industrial contexts were becoming increasingly confused, the distinction between openness and secrecy as two distinct cultures seemed much clearer in the 1950s. At the Hamburg conference, the physicist Samuel Allison, though emphasising the openness of science and a well-known opponent of secrecy had to admit that the visitor '…may, in some of our universities, find physicists working behind closed doors'[23]. Wang has pointed out how the outrage of such physicists as Allison or Merle Tuve, at such Cold War conditions was sincerely felt[24]. The careful classification of scientific findings, and the locking of offices, provided the foil against which scholars such as Shils and Merton could promote the idea of an open cosmopolitan science, and indeed the reality of certain academic people and journals circulating the world. When the two

cultures collided, and secrecy was seen to have been violated, the response could be extreme. The flight of nuclear physicist Bruno Pontecorvo to the Soviet Union caused uproar in 1950[25]. Not just nuclear issues prompted secrecy.

The realm of pharmaceuticals proved to be among the highest profile cases in which secrecy, openness and circulation of science were debated. Antibiotic development had been precipitated by the wartime journey of two British academic scientists with knowledge and samples of penicillin to the United States. Funded by the Rockefeller Foundation, there is no evidence that in July 1941 any concern other than getting a large amount of penicillin made for their research was behind the flight, nor was there any governmental objection[26]. Subsequently penicillin proved impossible to patent, either by British or American interests. This experience and the low price engendered by the easy flow of information about its manufacture encouraged much more careful attention to Intellectual Property (IP) in the next generation of tetracyclines. However, it was market failure and the suspicion that knowledge about prices was circulating among just a few companies that encouraged first the Federal Trade Commission and then the Kefauver Committee to look into it[27]. Their officers wanted to reduce radically the period of protection to the circulation of ideas given by patents.

Coinciding remarkable with the Biochemical Congress in Moscow, the Kefauver committee's assault on the pharmaceutical industry was interrupted by the unfolding in the courts of a remarkable story, worthy of a movie, behind the theft of achromycine (the American Cyanamide trademarked version of tetracycline). In the period 1958 to 1964, two employees of Lederle borrowed company documents and copied them, subsequently returning them. Some cultures were stolen[28]. The organisms and copied documents were smuggled to Italy where they were used as the basis of substantial international businesses in their own right, supplying for instance Britain's

National Health Service. When caught defendants argued that they had not stolen anything, because they had returned original documents after copying. Here was the classic locus of secret applied science, associated with the secretiveness of big business. Although the Kefauver Committee was fundamentally sceptical about the ethics of corporations such as American Cyanamide, it could not question the propriety of the company keeping secret its science, and the impropriety of sharing it.

Clearly knowledge of achromycine cultures and manufacturing techniques was a kind of scientific knowledge that was not meant to circulate. It was subject to what Galison has called antiepistemology[29]. Talk about such secrets, was also classically 'applied science' discourse. My suggestion here is that it was not the inherent nature of the information which could directly differentiate it from pure science. Rather the applied quality was intimately interconnected with the secret status of talk about it.

How then does this relate to the journey of the British scientists to the Soviet Union? Clearly, the information that was shared formally was not applied science. The nine volumes of *Proceedings* published by Pergamon gave away information that was sensitive neither economically, politically or commercially. To understand the report on the Soviet visit which was written by Denis Herbert, a microbiologist and member of the team, it is important to understand the nature both of his techniques and his own role. After working at Porton Down during the Second World War he was demobilised and moved to London's National Institute of Medical Research. There he heard a paper read by Jacques Monod on the development of the chemostat, a device in which the rate of growth of micro-organisms was controlled by the limitation of a single nutrient[30]. By a managed supply of the nutrient, the micro-organism could be produced continuously and homogenously. The mathematics were interesting, the technique challenging and its potential application wide-ranging. Subsequently it would be applied to a wide range of industries including brewing, the production of single-cell protein and the production of the anti-cancer drug asparaginase[31]. A team of about a dozen members was built up in the 1950s, and despite the uses in biological warfare, there was a rich published output from the laboratory. By 1976 its members had published almost a hundred papers on various aspects of continuous culture[32]. The atmosphere within the laboratory was collegial with ideas shared at tea-time in the mess, and even those deemed to have military rank not wearing uniform.

To us, the reading of Denis Herbert's report on his visits to four fermentation research centres in the Soviet Union evokes an equivalent sense of the demarcation of information[33]. In his visit to the new antibiotics unit which he undertook with his colleague Dr Kent, he was allowed to see research on the production of antibiotics in the pilot plant run by Russia's leading expert, Professor Gauze of the Academy of Medical Sciences. He reported on the size of the building and on the organisational structure of five divisions 'microbiology, biochemistry, organic chemistry, experimental chemotherapeutics, and a pilot plant.' He was told, 'The major problems under investigation are (I) search for new antibiotics (II) methods for attacking resistant strains of micro-organisms (III) anticancer antibiotics.' He learned that there was an elaborate screening programme and that about 100 antibiotic producers a year were selected for further investigation. They were grown first in shake flasks and then in 10l fermenters. Up-to-date separation techniques including paper chromatography and gel diffusion and ionophoresis 'solvent partitition (in Russian made Craig Apparatus)' were in use. Nothing he was allowed to see would have been unknown in the west and indeed he was impressed by the range of Russian made instruments. Similar information was gleaned at other laboratories.

The account of the bemused guard routinely observing and then departing, highlights the distinction from a 'normal' encounter with foreigners. Instead it expresses the mutual trust of each other and differentness from ordinary society, in the popular imagination associated with ‘the ivory tower’. Even the account of ‘All-Union Institute for Research on Wood Hydrolysates and Sulphite Liquors, Leningrad’ whose remit was described as ‘strictly applied’ was purely scientific. The area of greatest shared interest was continuous culture. Thus it was commented that the section head who took Herbert around was described as having ‘a better understanding of continuous culture, both in theory and in practice, than most of the ‘academic’ Russian scientists in this field.’ Without being told details, or himself being made privy to secret information, Herbert could conclude his report of this visit:

> *A fact that was rather impressive was the good co-ordination that seemed to exist between the research institute of VNIIGS and its factories, and in particular that a scientist, having developed a process in the laboratory, would go to the factory to see it brought into production and then return to the Institute. The transition from the research laboratory to the factory manager's office tends in England to be a one-way process, but in VNIIGS at least it appears to be readily reversible.*

Here even where the difference between UK and Soviet practice is described, it is in terms of Soviet superiority. In other places the report is more ironic:

> *There appeared to be no shortage of money for internal purchases and Dr Gauze informed us that there was sometimes difficulty in spending all the annual apparatus grant before the end of the financial year – it was important to do this, or the grant might be cut in the following year. (The remarkable similarity between British and Soviet arrangements in this respect should perhaps be brought to the notice of the Treasury).*

The visit seems to have caused no undue offense and meetings of the international continuous fermentation workshop continued for several years, indeed until the Soviet invasion of Czechoslovakia.

On the British side, there was also satisfaction: 'high praise indeed', noted Porton's director in passing on comments from Whitehall[34]. The general report had ended with the comments:

> *Nothing that we have seen throws any light on the effort devoted to B.W. [Biological Warfare], but we see no reason to doubt that they could man, equip and engineer a programme in this field if they wish to do so*[35].

No doubt, that was the conclusion it was intended they should reach. The report combined its military conclusion with the language of collegiality and, in certain areas, the free exchange of information with Soviet peers. While it might be argued that this does not necessarily equate with pure science seen as an academic pursuit, it might be similar to what Polanyi had been contemplating. After all he had worked, as Mary Jo Nye pointed out, on a mixture of commercially important work and on science that we would today call fundamental, and widely achieved at the Kaiser Wilhelm Gesellschaft[36]. In both the early career of Polanyi and the work of Denis Herbert one sees a mixture of different kinds of performance. Later Polanyi would add the role of science commentator to his repertoire and it is in that role he is perhaps most famous. To use the language of performance of Stephen Hilgartner, the two were performed entirely differently. In his study of expert advice Hilgartner looks at the careful presentation of a finished knowledge product as expertise, and the careful exclusion from the public gaze of the processes by which that knowledge was generated[37].

An alternative to the metaphor of various acts enables us to engage directly with the careful balancing of language and discourse chosen by the men of Porton. This is the linguistic interpretation of Bakhtin who noticed the different registers of communication, and suggested the work 'heteroglossia' to describe the various tones in which a writer could communicate. A 'representative anecdote' of this world was the nineteenth-century novel moving with delicate agility parodying, translating and skating across different language usages. Bakhtin points illuminatingly to the radical shift between forms of address in a passage from Dickens' *Little Dorrit*:

> *[The conference] had reached this point when Mr Merdle came home from his daily occupation of causing the British name to be more and more respected in all parts of the civilised globe capable of appreciation of worldwide commercial enterprise and gigantic combinations of skill and capital. For, though nobody knew with the least precision what Mr Merdle's business was, except that it was to coin money, these were the terms in which everybody defined it on all ceremonious occasions….*[38].

The parallel between the heteroglossia of Dickens and of Herbert is very clear

In conclusion, the visit of the Porton microbiologists to the Soviet Union in 1961 and their subsequent reports can be seen to testify to flexibility and multilinguality of scientists. Those scientists successfully engaged with Soviet colleagues, sharing values and evaluations. I have suggested, therefore, we invert Merton's definition of the qualities of the scientific community and the values of science, to suggest that the subjects about which scientists could talk outside their institutions and indeed countries, even with potential enemies, worked as 'pure science'. That interpretation does not exclude at all the ability of the same people at other times to be engaged in the most institutionally driven work and concerns, even leading potentially to mass killing of the people with whom they also had such good relations.

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Acknowledgements
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