Working worlds and British science, 1900-1939

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Introduction

'Working worlds' was a concept that I devised and found useful in making sense of twentieth century science.[1] I have taken the opportunity of my invitation to the 'Institutionalisation of Science and the Public Sphere in the Modern Britain' seminar to investigate in more detail the working worlds of British science in the first four decades of the century. What I do in the following is, first, describe what working worlds are, identify the five prominent working worlds of twentieth-century science, and discuss the series of steps whereby working worlds call forth science.

Second, I summarise some research I have undertaken that aimed to identify how, and how often, 'problems' were raised in the public sphere and science was suggested as a solution, or part solution, to these problems. In this research I took the letters and editorial pages of *The Times* as a central forum for the public sphere in Britain between 1900 and 1939.

Third, I review the secondary historical literature on science in Britain in the first half of the twentieth century in order to understand who (in public, or in the public sector) was promoting science as a solution to working world problems, the recurrent features of public debate about science, and which sectors (public and private) were of particular importance.

What are working worlds?

Working worlds are arenas of human action that generate problems. The intuition was that science does not operate in a featureless, level environment; rather it responds to an uneven, given landscape of which working worlds are the major features. Our lives, but especially our sciences, have been organised by our orientation towards working worlds.

In *Science in the Twentieth Century and Beyond*, I identified four working worlds as being of particular magnitude and significance. These were: (1) projects to build technological systems, in the Hughesian sense: expanding networks of human and material components orchestrated by systems builders.[2] The (2) preparation, mobilisation and maintenance of fighting and defence forces, I suggested, was sometimes of overwhelming importance for twentieth century science. Its contender was my third working world, (3) the maintenance of human (and other organisms) bodies, in sickness and in health. Fourth, (4) civil administration offered many problems that called for a response from sciences, especially social sciences. On reflection, I think I would now add a fifth, which might be described as (5) the monitoring and maintenance of global order, especially global environmental stability and economic dynamism. Working worlds can be distinguished and described, although they overlap considerably. Military systems are, for example, often technological systems. Furthermore, sciences can respond to several working worlds at once. Military medicine, for example, is clearly emergent from working worlds (2) and (3). This promiscuity is not, I think, an analytical problem; rather it captures a significant feature of the working world-science relationship that should be recognised explicitly, not buried.

Identification of working worlds is only the first, necessary step in my framework of analysis. It is my contention that the sciences solve (or aim to solve) the problems of working worlds in a distinctive way. Crucially, working worlds are far too complex to have all their problems solved directly. (Some problems are solved directly; these do not implicate, or call into action, the sciences.) Instead there are, typically, a series of steps: problematisation, making representatives, representative science, and solutionisation.

First, problems have to be recognised as such. This *problematisation*, I must stress, is an active achievement. Problems are articulated, given particular form and name. A death is not a problem until it is spoken as such. One strategy is aggregation. Deaths, let us say of cholera in nineteenth-century London, can be aggregated as a kind that can be presented as a problem. Likewise, the movement of an individual is a mere journey, but the aggregated movement (internal migration, say) of people is a problem for civil administration. Another strategy is selection: a characteristic of a working world is picked out as especially significant. In the 1920s and 1930s, for example, the speed and invisibility, at a distance, of enemy bombers was spoken of as an immense threat. "The bomber will always get through", in the words of Stanley Baldwin. A third strategy is projection, a future-oriented discourse in which prediction and imagination both play a part. Anthropogenic global warming as a problem is an achievement of the last third of the twentieth century. Note here that science is implicated in the articulation of predicted problems. Some potential problems, however, are not articulated, or not heard; others that are can be contested. Who is able to state problems, and with what authority, is a pertinent question. The public sphere can be an important space for the articulation of problems.

Second, science responds to working world problems in a distinctive – arguably definitional - way. Because many working world problems cannot be solved directly, science makes manageable, manipulable, abstracted *representatives*. Across disciplines, across the decades, science has sought such representatives, that stand for the problem but are simplified and tractable in comparison. In 1850s London, John Snow's maps of cholera cases are vastly simpler than the diseased bodies and the busy metropolis, that is to say the working worlds (3) and (4). In my second example, the problem of movement of people, the response of social science has been to take censuses. The representatives in this case are tables of data, once paper now electronic spreadsheets and databases. The effort and expense to build these representatives might be immense, and involve further technological innovation – for example 1901 census of the United Kingdom strained to the limits conventional clerical methods of tabulation, and the ambitious 1911 census was only made feasible because of the application for the first time in a national context of punched card machinery[3] – but are justified because their manipulation offers knowledge of vital importance to civil administration. Again, large as the census database might be, it is simpler than its subject; indeed, it is qualitatively different in its epistemology: it can be manipulated and known in a way that is impossible for its working world subject.

Likewise, The response to the speed of the twentieth century war called forth one of the most influential projects of the 1930s and 1940s: radar. The detection of radio echoes from incoming aircraft was only the beginning of a technological system that worked by 'filtering': reports were collated, wheat separated from chaff in 'filter' rooms, and represented in simplified form across tables (literal tables in this case) in operations rooms. The view of an air marshal looking down at such a table is of the working world of warfare in the form that minimises complexity at the same time as maximising relevance.

Notice that all these representatives, whether for the social or natural sciences, are artificial. There are living representatives – the model organism – but these too are, in an important sense, artificial. The *Drosophila* fruit fly of the 1910s and 1920s (and likewise later twentieth-century organisms such as the *Arabadopsis* plant and the laboratory mouse) was standardised into many strains that were bred for certain, repeatable and stable characteristics.[4] Furthermore, of course, these model organisms were steps towards more artificial (and certainly dead) representatives: tables of genetic maps, sequenced genetic code, or sets of measurements of rates of cancer, say.[5]

The third stage is, simply, *science*. If science operates in a landscape given by the making, manipulation and contest of abstracted, simplified representatives of working world problems, then the form of this operation has been the subject of a much history of science. The more 'internal' a historiography, the more it has focussed on this stage. There is no one-to-one relationship between working worlds and representatives (it is potentially, and often realised as, a many-to-many relationship). Different representatives can be made, and they can be made sense of in different ways. The representatives are, in modern science, discipline-bound, and are manipulated, contested and used for knowledge-claims in ways that are regulated by disciplinary expectations. For one discipline this might mean different combinations or selections of experiment, theory-building, hypothesis-testing, fieldwork, data collection, observation, and so on. In the case of radar, in the 1940s the operations rooms called forth its own science – operational research – precisely to solve problems within the working world of the military.[6]

The fourth stage is '*solutionisation*'. Conclusions drawn about representatives need to be translated into a consequences that are relevant, and accessible, to the working world. Like problematisation, solutionisation is an achievement, and a hard-worked and precarious one at that. In the case of John Snow, the cholera maps revealed a pattern of disease, but the consequences of this knowledge still had to be made relevant and compelling. As the story goes, Snow dramatically achieved this translation by removing the handle of the water pump, thus interrupting the cycle of infection. In the case of the census, the General Register Office (and more recently the Office of National Statistics) did most of the work of manipulating the representatives, and its conclusions are fed in digestible form back to policy actors through summaries and reports. In the case of radar, scientists ran experimental tests of the ability to receive echoes from aircraft, most famously in Robert Watson-Watt and Arnold Wilkins' experiment of 26 February 1935 using the BBC transmitter and Daventry.[7] But these experiments were also demonstrations, witnessed by the military brass who might approve development. Later, as scientists were mobilised in great numbers, and ever more sophisticated variants of the radar techniques were invented, so this entrepreneurial phase of demonstration, a staged enactment that translated the consequences of representative knowledge and practice, remained crucial. The results were concrete radar solutions. Likewise, the conclusions of operational research reshaped military interventions.

I found this pattern – of problematisation, construction of representatives, science, and solutionisation – to be common in the history of twentieth-century science. Turning now to early to mid-twentieth century Britain, I wanted to investigate the extent to which 'problems' were articulated in the public sphere, and to what extent science was linked to solutions.

Science, the Public Sphere and *The Times*

If we take the public sphere to be space where public opinion is expressed and formed, and where problems are identified and their solutions discussed, then there is no doubt that letters pages and editorial content of newspapers were a major constituent of the public sphere in the early twentieth century.[8] In Britain, the letters pages of *The Times*, in particular, were regarded as a central, if establishment, forum for the airing of public opinion and debate.

I have therefore conducted some broad, qualitative and quantitative research on the letters to the editor and editorials in *The Times* between 1900 and 1939 in order to gauge the extent that problems identified and discussed in the public sphere in Britain were seen to have solutions in the world of scientific expertise.[9]

My first question was: what proportion of editorials (anonymous leader articles expressing the editors' views of a national issue) directly called on science to aid the solving of problems? Of the 45,552 editorials published in the *Times* between the turn of the century (1 January 1900) and the outbreak of the Second World War (1 September 1939), nearly a quarter contain the word 'problem' (or associated words such as 'problems', 'problematic' and so on). I then inspected a random sample.[10] Less than 3% of editorials that identified a 'problem' mentioned a solution coming from the application of science. For example, the editorial of 28 July 1928 on 'Imperial communications' discussed the merits of wireless beams versus cables as means of communication in the Empire; 'scientific coordination' was suggested as part of the solution.[11] Such instances, however, were rare. Most editorials, on topics as diverse as the situation in the Punjab, the fiscal policy of the Empire, Russian cruisers stopping and searching British ships in the Red Sea, the opening times of public houses, or post-war reparations, did not mention science.

For my second experiment I asked the same question of the letters sent by readers of the newspaper to the editor: what proportion of letters to the editor mention a 'problem' and then suggest that science might contribute to the solution? Of the 143,616 letters to the editor, less than 8% of the sample were letters in which the authors sought scientific (or partly scientific) solutions to problems that they identified. For example, in 1933 Digby Solomon wrote a letter in which he noted the problem of the fast spread of fire on-board ships and in country houses; he then argued that scientific research needed to applied, and called on the government, in particular the Department of Scientific and Industrial Research, to investigate.[12] Another example: Lord Allen of Hurtwood proposed in a 1936 letter that an international commission be established that would ascertain facts (including psychological facts); on the basis of such sound scientific evidence, competing claims to colonies – he cited German claims in particular – would be rationally settled.[13] However, again, such cases were unusual.[14]

These overall findings, of an <u>uncommon</u> association of problems with scientific solutions, are perhaps unsurprising: <u>most</u> political (or other) issues were <u>not</u> seen as having directly scientific solutions. However, the finding is a useful, general fact to keep in mind as a corrective when we turn to examine the history of early twentieth-century science and its relationships to politics and the public sphere.

Nevertheless, there are still the interesting cases of the letters to the editor and editorials that *do* call on science to investigate. How did the authors articulate the relations of science to perceived problems, and did such relationships map on to particular working worlds? This part of the research therefore explores the operation of working worlds within the public sphere of Britain in the early to mid-twentieth century.

In *Times* editorials, 1,224 (of 45,552, or 2.7%) mention both 'science' and 'problem'.[15] I investigated a sample of these editorials further, examining whether the connection between 'problem' and 'science' was a direct one, and if so whether it could be related to a particular working world. Of the sample (40 editorials), half showed no direction connection, while just under half spoke of a direction connection of a problem to science (the small remainder were ambiguous). Of the editorials raising a direct connection, the problematic issues were (in descending order of importance):

Imperial management (6)[16], Health (6)[17]

Industry/Technological systems (4)[18]

Food supply (3)[19]

Military (1)[20], Pedagogy (1)[21]

Finally, I asked the same question of letters to the editor of *The Times*: how did letter writers, who mentioned 'science' and a 'problem' relate the two, and in what areas did they see a desired connection? Among this group of 1,586 letters to the editor, again the split was roughly half and half. That is to say 17 out of 40 letter writers identified a problem and saw science as being part (or all) of the solution; the remainder mentioned 'science' and 'problem' but drew no connection.[22] Of those that did connect problems and science, the problematic issues, as seen by letter writers to *The Times*, were (in descending order of importance):

Industry/Technological systems (7)[23]

Military (3)[24], Health (3)[25]

Civil administration (2)[26]

Food supply (1)[27] Origin of Man (1)[28] Artwork provenance (1)[29]

So what do we conclude? I have taken editorials and letters to the editor of *The Times* as a proxy for the public sphere. While, in general, editors and letter writers rarely directly linked the problems they identified with solutions from science (question 1 and 2), when they did do so the problems that they articulated (questions 3 and 4) matched broadly with the working worlds I suggested were important for science globally across the twentieth century. In Britain, between 1900 and 1939, the problems articulated in the public sphere of *The Times* included those from the worlds of technological systems and industry, the maintenance of human health, civil administration, and the maintenance of military systems.

But there are also some qualifications to be made with respect to these findings. Editorials, in particular, linked problems with the British Empire to scientific solutions. I am unsure how to think about 'Empire' as a working world. Since imperial projects certainly generated problems, of which some were seen as a having scientific solutions, then 'Empire' could qualify as a working world of its own. Or, alternatively, 'Empire' is a combination of working worlds, including military, human health, civil administration, and the maintenance of global order. On balance, I think it is a weakness of the working worlds model that 'Empire' does not fit easily within it.

Second, there are problems identified and scientific solutions anticipated in very narrow areas, especially by letter writers, who were often motivated by very local or narrowly focussed concerns. (An example is the provenance of the Duke of Westminster's Rembrandt paintings, a problem solvable, the letter writer suggested, by scientific scrutiny.) This empirical finding does not cause problems for the working worlds model; small working worlds are allowable. It would only have been an issue if these small-scale working worlds predominated.

Third, food supply – agriculture, fisheries, and associated science – may be a working world of its own, although I see it as nested within the working world of the maintenance of human and animal bodies in sickness and health.

Fourth, I am intrigued by the general finding that most problems raised in the public sphere are not linked to scientific solutions. Presumably some of these negative cases may not have been linked by editors or letter writers but nevertheless still received scientific attention. But there may also be a significant class of problems that are never seen as open to scientific inquiry and application. We may ask: why not? Why don't some problematic areas generate science? Is it perhaps professional occupation of niches that explains the pattern? Does the pattern look similar at different times, or in different countries?

Finally there are some methodological issues that should be acknowledged. I have used a relatively small sample (inspecting 160 editorials/letters in total), which is enough to suggest indicative trends but not to demonstrate statistical significance. Nevertheless, the results could be made more robust relatively easily by extending the sample. Second, the letters to the editor are only those which the editor has chosen to publish. The letters page was not a perfect public sphere where access to debate was unrestricted (anyone was free to write, but not everyone was published). Nevertheless, editors acted to hold the ring rather than steer debate. Furthermore, this methodological problem could be overcome if a sample of all letters sent to the editor could be obtained from the unpublished archives. Third, *The Times* is, of course, a particular newspaper. Before 1922 it was owned by Lord Northcliffe (who also owned the *Daily Mail* and *Daily Mirror*), and was virulently anti-German; after 1922 it was owned by Lord Astor and was pro-appeasement in the 1930s. The editorials follow this lead. Nevertheless, it also maintained a reputation as the 'paper of record'. The letters page was undoubtedly a public forum, read daily by opinion formers. It would nevertheless be interesting to conduct similar, comparative research of other newspaper letter pages to get a less partisan sense of the public sphere and science.

Who (in public) was calling for science to respond to problems?

In the section above I surveyed the letters and editorials of *The Times* in order to gauge how often science was promoted as a solution to problems. I also categorised some of the science that letter and editorial writers promoted as originating in certain working worlds. In this section I will explore in more detail which individuals and groups were publicly calling for science to respond to problems. These include: individual authors, co-ordinated pressure groups, political parties and government departments. There was, in addition, of course considerable articulation of problems in private, notably within private firms.

(i) Individuals

The letter writers to the *Times* were individuals identifying problems in public, although sometimes they wrote on behalf of private interests (such as industrial associations, or, less frequently, individual firms). A second category of individuals is authors.

We can get a strong, general sense of how the relationship of science to 'problems' was conceived around the turn of the century by examining one of the most read manifestos for science of the period: Karl Pearson's *The Grammar of Science* (first published 1892, second edition 1900, third edition 1911). Even now Pearson's book is one of the most cited books on science published in the forty years before 1940.[30] Pearson opened his survey of science with four 'claims of science' that, he argued, justified its support:

> (a) the efficient mental training it provides for the citizen; (b) the light it brings to bear on many important social problems; (c) the increased comfort it adds to practical life; (d) the permanent gratification it yields to the aesthetic judgment.[31]

The first of these claims – (a) – was the proposal that in a period of turmoil and conflicting opinions only a citizenry possessing knowledge of the scientific method would be able to identify the facts necessary for informed debate and thereby social stability. The fourth – (d) – classed science as the product of human imagination at its highest and at its most critical level. Science, for Pearson, was the 'sole possible method' of meeting humanity's 'insatiable desire' of an understanding of the universe. Both (b) and (c) address how science relates to problems, and Pearson sets this relationship up in a way that I would describe as typical of the Victorian/Edwardian period. So, under (b) he notes that 'science can on occasion adduce facts having far more direct bearing on social problems' than anything produced by political philosophy. His case study is eugenics, where the social problems are those committed by society's so-called 'degenerate members'.[32] Pearson concludes that 'the laboratory experiments of biologists may have greater weight than all theories of the state from Plato to Hegel'. Indeed, this problem-solving capacity alone, for Pearson, provided 'sufficient justification for the national endowment of science, and for the universal training of our citizens in scientific methods of thought'.[33] But this promise must be tempered by the recognition that the steps from pure science to practical application were long and winding - (c):

> Ultimately the direct influence of pure science on practical life is enormous… It is impossible to say of any result in pure science that it will not some day be the starting-point of wide-reaching technical applications. The frogs legs of Galvani and the Atlantic cable seem wide enough apart, but the former was the starting point of … the latter.[34]

In other words, science could have the greatest importance for solving social and practical problems (and deserved lavish state funding), but it was also unreasonable to demand immediate applications, which in turn justified the autonomy, as well as the ultimate promise, of pure science.

(ii) <u>pressure groups</u>

Pearson was one of many individuals classed by Frank Turner as 'public scientists'. Turner argued that nineteenth-century British scientists found that

> …they must justify their activities to the political powers and other social institutions upon whose good will, patronage, and cooperation they depend[ed]. The body of rhetoric, argument, and polemic produced in this process may be termed *public science*, and those who sustain the enterprise may be regarded as *public scientists*.[35]

Turner identified three periods of British public science. The first, from 1800 to 1851 saw public scientists such as Humphry Davy, David Brewster, and Charles Babbage, promoted 'the importance of science as a mode of useful knowledge, an instrument of self-improvement, an aid to … economic activity, and a pillar of natural religion'; in all of this, the 'scientific enterprise, like economic enterprise, was to be private'. In the second period of public science, from the mid-1840s to the late 1870s, Victorian publicists for science challenged the cultural dominance of the clergy, forging 'a genuinely self-conscious professional scientific community' in the process.[36] They sought, and failed to secure, a national endowment for science. 'Scientists, like other groups of intellectuals who during the 1860s had hoped to participate broadly in public life', notes Turner, 'found themselves able to exert relatively little direct power or influence in the civic arena'. Therefore, in Turner's third period, after the 1870s the public scientists turned ever more critical of 'politicians and complacent manufacturers', now seen as enemies of the 'progress and application of scientific knowledge'.[37] The public scientists now attacked the political system, in which party politics rather than science guided policy. They promoted science education as means of instilling desired virtues of truthfulness and endurance in citizens (and eventually politicians) and eugenics, as a means by which science could deliver 'direct civic benefits to the nation-state'.[38] An alliance between public scientists, pre-eminently Norman Lockyer, the editor of *Nature*, and sympathetic social imperialist politicians, was institutionalised in the British Science Guild in 1904. The Guild lobbied hard for science. In particular, it viewed science as a solution to political problems:

> [the purpose of the British Science Guild] is to stimulate, not so much the acquisition of scientific knowledge, as the appreciation of its value, and the advantage of employing the methods of scientific inquiry, the study of cause and effect, in affairs of every kind. Such methods are not less applicable to the *problems* which confront the statesman, the official, the merchant, the manufacturer, the soldier, and the schoolmaster, than those of the chemist or the biologist; and the value of a scientific education lies in the cultivation which it gives of the power to grasp and apply the principles of investigation employed in the laboratory to the *problems* which modern life presents in peace or war.[39]

'Edwardian public science centred on [the] technocratic BSG/Nature axis' argues Hull, agreeing with Turner, it 'continually pressed for executive influence over government policy for scientists, arguing both that scientific method was transferable to social problems and that science was the key component in national power in a modern state faced with constant economic competition which might at any time become war.'[40] From the turn of the century through the First World War the public scientists continued to argue publicly – including in *The Times* - that 'politicians were ignorant of scientific matters', and that the Government neglected to support or use science properly.[41]

(iii) political parties

The intensity and frequency of the public scientists' complaints about party politics should not distract us from the fact that science policy was discussed within a party context, and that politicians and their party's political programmes had direct influence on science. The influence of social imperialist politicians on the development of sciences (such as tropical medicine and ecology) of direct interest to imperial projects is well known to historians.[42] Olby accounts for the establishment of the Development Commission in 1909, one of the first mechanisms for substantial state-funded science, as part of David Lloyd George's vision as Liberal chancellor of the exchequer (an instance of the institutionalisation of science discussed further below).[43] Likewise, Roy and Kay MacLeod have argued that the 1918 Labour Party manifesto 'which bore the distinctive hand of Sydney Webb, made a particular appeal for the support of scientists, on the grounds that [quoting the MP Arthur Henderson]: "Essentially, in the complexities of politics... the Labour Party stands for increased study, for the scientific investigation of each succeeding problem, and for a much more rapid dissemination among the whole people of all the science that exists. And it is ... the Labour Party that has the duty of placing the Advancement of Science at the forefront of the political programme"'.[44] This particular political promise to apply science to social problems had little immediate effect.

(iv) government departments

Turner reminds us that public rhetoric may not reflect actual practice. Historians are now very aware that the British state invested heavily in science in the early twentieth century, especially in support of its army, navy, and, later, air force (via its relevant government departments, such as the War Office, Admiralty and Air Ministry).[45] The civilian scientists – such as Bragg or Rutherford - who were mobilised into war work complemented existing support of research. When they returned to civilian, university work the military and industrial research continued. Nevertheless, the war did precipitate the formation of the Department of Scientific and Industrial Research (DSIR) in 1916. Its peculiar form – 'operating under the loose supervision of the Lord President [a minister without direct departmental interests] and the Privy Council Committee on Scientific and Industrial Research' – was, as Andrew Hull has noted - was 'mediated by the state's desire to extend the idea of a national science policy without publicly ceding executive authority over that policy to scientists, and the desire of the Royal Society to continue to be the informal scientific authority which influenced national science policy without formal involvement in the machinery of government, which would have tainted the objectivity on which its scientific authority was founded'.[46]

The DSIR assumed responsibility for some government laboratories (notably the National Physical Laboratory), supported industrial research (through matching funding from a Million (Pound) Fund to support cooperative, sector-specific 'research associations'), and provided grants to postgraduates. Much of government research remained outside of its control, notably defence science but also agricultural, medical and meteorological research. Furthermore, industrial research funded directly by firms (such as at Imperial Chemical Industries) was greater than that funded through the research associations.

Nevertheless, the DSIR is a prime example of the institutionalisation of expertise in Britain in relation to the state. I am interested in this paper in the articulation of problems and the framing of solutions as likely to be found in science. Sabine Clarke has demonstrated that the use of the term "fundamental science" – pure science with a practical aim - by the DSIR had immense rhetorical value, enabling the department to, on the one hand, placate the public scientists for whom government should be supporting "pure science" and, on the other hand, satisfy industrialists that the research would contribute to improvements in industrial methods and not merely "subsidising a corps of scientific dreamers".[47] Pure science promised solutions in due time, but only if science was properly endowed and left autonomous – this was the position of Pearson (above), or Thomas Henry Huxley[48], or *Nature* editor Richard Gregory.[49] But "fundamental science", 'a term flexible enough to work with two different groups', enabled the DSIR to tweak pure science so that it *was* connected to problem-solving, as these two quotations from DSIR Annual Reports makes clear:

> Pure science has in the past owed much to observations, suggestions and difficulties which have come from activities external to the laboratory or the study.[50]
>
> The problems of industry draw attention to gaps in scientific knowledge which it is the duty of the industrial researcher to fill. The acquisition of such knowledge may be called fundamental research as applied to industry for, without it, far-reaching changes and improvements in industry are almost impossible.[51]

While Clarke notes that it 'would be wrong … to assume that the DSIR issued grants according to their potential relevance to industrial problems', this applied to academic grants. (Even then, while the criteria might be "quality of research", this did not of course stop such research being relevant to the solution of working world problems.) On the industrial research side of the DSIR's activities, plenty of grants were made available to institutions associated with industry. For example, the Advisory Council of Scientific and Industrial Research in its first year (1915-1916) immediately conducted interviews with a wide range of institutions[52], and began issuing grants (firstly to continuing research, and then to new applications for funds), albeit with an aim to produce 'researches of a general nature, the results of which … should be made fully available'.[53] An inspection of the first set of grants (table 1) illustrates their industrial, problem-solving orientation:

Institution applying for grant	Subject of investigation	Amount of grant recommended
Institution of Mechanical Engineers	(a) Hardness test for journals and pins (b) Properties and composition of alloys	(a) £100 (b) £200
Institution of Electrical Engineers	(a) Heating of buried cables (b) Properties of insulating oils	(a) £840 (b) £250
Manchester Association of Engineers	Tool steel experiments	£440
Institute of Chemistry	(a) Laboratory glass (b) Optical glass	(a) £400 (b) £500
Institution of Gas Engineers	Refractory materials	£130

Table 1: First grants recommended by the ACSIR, 1915. DSIR 1/1. Minutes, ACSIR, 10 December 1915.

Likewise special attention was paid in this first year to subjects of practical, wartime importance, for example research into hard porcelain, which was needed for scientific and hospital equipment but which, like optical glass, had largely been sourced pre-war from Germany.[54]

Of course, we might expect a problem-solving orientation during wartime, but we find a similar pattern if we inspect the activities of the DSIR in interwar years. The Scientific Grants Committee funded PhD students and research assistants through its four sub-committees (chemistry, physics, biology and engineering), although a finer grained analysis would be needed to determine whether such academic research was related to specific working worlds.[55] The Million Fund matching grants to industrial research associations, however, were clearly oriented towards particular industrial sectors, and their associated problems (see table 2).

Research Association	Maximum commitment
Boot and Shoe	£4,500
Cast Iron	£29,000
Cocoa	£7,500
Cotton	£45,000
Cutlery	£13,000
Electrical	£24,000
Food	£25,000
Launderers	£9,750
Leather	£7,000
Linen	£21,000
Millers (Flour)	£13,000
Motor	£9,000
Motor Cycle	£3,000
Non-Ferrous	£39,625
Paint, Colour and Varnish	£25,000
Photographic	£9,250
Refractories	£4,125
Rubber	£6,000
Scientific Instrument	£50,800
Silk	£5,250
Shale Oil	-
Wool	£19,200

Table 2: Statement of amount of Million Fund commitments as of 31 March 1926. DSIR 1/24, 'DSIR Advisory Council. Million Fund Committee. Memorandum on commitments of the Million Fund', 21 April 1926.

In the previous section, I explored the articulation of problems in (part of) the public sphere, and demonstrated that while the link with science was in fact rarely made, when it was made then the certain working worlds were important. In this section, I have examined who was arguing that science provided solutions to problems in the period. I have agreed with Turner that the public scientists framed the debate in a particular way: criticising the lack of use of science, but also presenting a particular late Victorian/Edwardian social contract, that relatively autonomous ('pure') science would deliver solutions in the long term if endowed and left to self-organise. But I also argued, noting the arguments of Clarke, that public bodies (such as government departments) could finesse this arrangement, through flexible terms such as 'fundamental science' that did link more closely with problem-solving, especially in industrial and military settings. Sometimes this link was suggested in the public sphere, such as the *Times* letter writer who argued that problems of fires could be tackled with DSIR research.

What does the secondary (and some primary) literature say about particular sectors?

To finish this paper, I will review further some of the secondary literature on science in Britain in the first half of the twentieth century, drawing out some further points about particular sectors (working worlds) and the sciences associated with them.

Industry

Even before the First World War, many British firms engaged in research, in sectors such as chemicals and dyestuffs (eg United Alkali, British Alizarin, Levinstein, Read Holliday and Brunner, Mond), food (eg Cadbury), textiles, telegraphy, steel (eg Brown-Firth), and arms (eg Vickers, Nobel).[56] More scientists still were employed in non-research roles, such as analytical control of production. During and the First World War this research intensity increased, especially in chemicals and dyestuffs (where mergers starting in 1915 created the leviathan Imperial Chemical Industries in 1926), electrical industries (ITT, GEC and Metropolitan-Vickers), glass (Pilkington), and arms and aeroplanes (eg Bristol Aeroplane, Rolls-Royce). Many of the 'largest British R&D performers [of the interwar period] were foreign-owned and/or were members of international and national cartels'.[57]Overall, the features of British industrial research before the Second World War were, argue Edgerton and Horrocks, that relatively high amounts of R&D was performed (more than historians had given credit for), much was in-house (compared, say, to the DSIR's support via the research associations), it was concentrated 'within a few firms and sectors', much of it was war-related, it had considerable international links.[58] Therefore, the point for this paper is that much of the articulation of problems for which scientific solutions were sought, in Britain between 1900 and 1939, were articulated within *private* settings, specifically private firms.

Contacts between industrial scientists and government (such as with institutions such as the NPL, or through committees) and between industrial scientists and academia were important. Universities and technical colleges supplied trained experts (although there was also in house training). Divall, who discusses engineering in universities and technical colleges show that sometimes industrial problems could be articulated by academics, and in curriculum formation, vice versa, although the link was fraught with issues of academic independence and commercial secrecy.[59] Donnelly also reminds us that the public articulation of problems and knowledge in engineering was also constrained by industrialists' understandable desire for commercial secrecy. [60] This secrecy acted as a significant constraint on the articulation of engineering problems in the public sphere.

Military

David Edgerton reminds us of the centrality of defence concerns within British science policy:

> the state's funding of R&D was concentrated overwhelmingly in defence – the key government funders of R&D before 1914 were the navy ministry (Admiralty)

> and the army ministry (War Office); in the Great War the Ministry of Munitions funded the bulk of R&D. In the interwar years, the Admiralty, War Office and a new service ministry, the Air Ministry, responsible for the most technically-oriented of the services, the Royal Air Force, spent much more on R&D than civil ministries. The Air Ministry was the largest single spender on R&D in interwar Britain by a considerable margin. All of these ministries, furthermore, spent much of their R&D budgets in industry.[61]

Within this context, 'problems' were articulated inside the public sphere (as we have seen in the case of the *The Times*), but also outside, in the management of private firms, and joint advisory bodies that brought together service, political and civil scientific expertise. An example of the latter was the Advisory Committee for Aeronautics, established in 1909, and which eventually (from 1920) became the Aeronautical Research Council. Its task was to articulate problems, as one of its founding documents makes clear:

> It is not the general duty of the Advisory Committee for Aeronautics either to construct or invent. Its function is not to initiate, but to consider what is initiated elsewhere, and is referred to it by the executive officers of the Navy and Army and Construction Departments. The problems which are likely to arise in this way for solution are numerous, and it will be the work of the committee to advise on these problems and to seek their solution by the application of both theoretical and experimental methods of research.[62]

The ACA/ARC encouraged work on such matters as surface friction in air, turbulence, flutter, and propeller thrust, and, more rarely, airborne instruments[63], and it acted to link theory with practical problems, sometimes from former to latter[64], sometimes from latter to former.[65] The origin of the problem could be 'a firm's design shop', or government experimental station, or academia. ARC acted as a clearing house, referring the problem on to RAE, NPL or academic aeronautics scientist. It is an important institutionalisation of expertise – perhaps more important than the DSIR – one that was not part of the public sphere, although was occasionally a matter for public sphere commentary.[66] The research generated in this manner was described by its internal historian as 'basic science', but it is more akin to the DSIR's 'fundamental science', ie pure science with a strongly practical aim, and the 'interest of the Committee in time of peace was seldom diverted from such work'.

In times of war, however, the connection between problem identification and research was necessarily much more direct: it was 'impossible, in the stress of war, to avoid being drawn into the urgent problems which were usually of a development rather than of a purely research character'. This was true for aeronautics research both during the First World War and in the preparation for war in the 1930s. It was also true more generally, from the First World Warm when 'Europe's best men of science, soon followed by those of America, mobilized for the solution of military, naval and munitions problems,'[67] and during 1930s rearmament.

Maintenance of human bodies, in sickness and health

The Medical Research Committee (established in 1913, becoming the Medical Research Council in 1920) had its origins in a governmental response to a particularly problematic disease: the recommendations of the Royal Commission into the Relations of Human and Animal Tuberculosis, which investigated between 1901-1911.[68] The proposal that a fund be set up was generalised and folded into the National Insurance Act, under which a penny per insured person would be devoted to general medical research. The MRC has always had a more stronger defined function and organisation than other major innovations in the state-funding of science, such as the Development Commission and the DSIR. This integrity has been crucial in how relations between medical scientists and the working world of the maintenance of human bodies have been managed. In particular, there was the question of how close the relationship should be between medical scientists and the clinic (and its problems). 'In a similar way to the DSIR, the concern of these state bodies [such as the MRC] … was often to distinguish research from other more routine types of laboratory investigation such as standardization and testing', notes Clarke, adding that the 'MRC did not deny that a focus on immediate practical problems could generate important knowledge about the body and disease, rather the concern it expressed was that some scientists were subject to undue pressure to devote time to routine tasks [ie the immediate problems of the clinic]'.[69]

Desirée Cox-Maksimov has offered an interesting argument that connects the MRC's desire to solve practical problems (ie disease) with an evolving definition of clinical trials of therapeutic substances.[70] In particular, she argues that the MRC, especially as guided by its secretary Walter Fletcher and Henry Dale 'tried to create a new class of citizen with a duty to define the meaning and nature of the "practical" needs of society, to determine public policy and find solutions to national concerns through "medical research". The MRC encouraged relationships with trusted physicians (whom she labels "noble scientists"), and sough to exclude maverick outsiders, notably Major Chas Stevens, who promoted his Umckaloabo treatment for tuberculosis from the 1910s to 1940s. To fight him off the MRC progressively redefined the clinical trial (notably in the insulin trials of the 1920s), so that it became a system (a 'machine') based on trusted individuals and manufacturers. With randomisation (the introduction of which is associated with Austin Bradford Hill in the 1940s) we have the modern randomised controlled trial (RCT). The RCT is invented in the space between medical scientist and working world. The RCT is a representative (in my working world theory sense) of the problem of disease. The RCT is also a major development in the institutionalisation of science in the twentieth century.

We see all of these issues at play in the other major component of the working world of the maintenance of organisms in sickness and health: the securing of an adequate food supply. Historians of science have recently paid increasing attention to British agricultural science. In this final section I review some of this literature.

The 'first government initiative to establish State-funded and planned scientific research on a substantial and national scale in the United Kingdom', argued Robert Olby, 'was taken in the Edwardian period, to be precise, in 1909, when David Lloyd George presented the Development and Road Improvement Bill.'[71] This set up the Development Commission (DC). Lloyd George had initially envisaged seven possible areas where the state could take a longer investment view, compared to the individual: 'afforestation, scientific research, the improvement of stock, agricultural co-operation in marketing, experimental farms, and equipment for agricultural instruction'.[72] As Winston Churchill had criticised at the time, this looked ill-thought out. It expressed Lloyd George's general anxieties (such as the degenerative effects of cities, and the consequent desire to restore rural development) rather than a response to a clear, articulation of agricultural problems being matched with investment in the science to respond. Nevertheless, in practice, guided by Daniel Hall (the most active DC member and director of Rothamsted laboratory), the DC did take 'a strong managerial role' (anticipating, says Olby, the style of the Rockefeller Foundation under Weaver), and 'formulated comprehensive schemes which together formed a coherent plan'.[73] Therefore: the DC represented an investment in science to solve agricultural problems, but this came with the explicit recognition that solutions would not be immediate. The DC 'twice warned "…against expecting immediate results from the expenditure of money on scientific research … it may be long before the investment produced a visible return"'.[74] Olby doesn't say this, but one cause of this expectation for a long time period for a return by state-supported scientific work may be the justification offered by Lloyd George that the state could and must take the longer-term view compared to individuals. On the other hand, as Turner argues, the social contract (of endowment and autonomy for science in return for solutions in the longer term) was the credo of the public scientists after 1870.

The Development Commission provided the funds for the establishment of a large number of research institutes.[75] The Commission's influence was broad, spreading from its core concern of agriculture to experimental zoology (via its support for the Plymouth marine biology laboratory) for example.[76] Despite Hall's supposed aim to ensure that the agricultural research funded through the DC was not unduly influenced by immediate economic concerns (and the immediate problems of farmers), ' the Development Commission's policy on scientific research was still regarded by many academic scientists as excessively dominated by practical considerations', for example the geneticist William Bateson declined Hall's offer of Development Fund money to the John Innes Horticultural Institute in 1918, 'on the grounds that it would drive research in an economic direction'.[77]

Indeed, when Hall reflected broadly on the interaction of pure science and practical concerns, he saw relationships that are close to those expected from the working worlds model:

> What we have to try to ensure is that all our workers are making contacts with the practical men. We do this first of all to ensure that their work will have some immediate practical purpose; of course we must ever remember that the purest scientific work may suddenly bloom into something utilitarian . . . . None the

> less, I think we are more likely to get even our pure science work vital if we can persuade the workers to be agriculturists, horticulturists, and, above all, naturalists, who have their eyes upon the living, growing organisms . . . . Often it is only when you turn to the practical man, who is working upon a very large scale with plants or animals, that factors are disclosed, exceptions are thrown up, and difficulties are discovered, which do provide a lead, perhaps into the ocean of pure science itself; and, therefore, it is good, from both the pure science point of view, and the immediate needs of the practical man, that all our research workers should have their ears to the ground.[78]

Berris Charnley shows that Rowland Biffen, the key figure building up research institutions such as the Plant Breeding Institute in Cambridge, advocated that the correct relationship between agriculture and science was that agriculture was 'best aided by conducting scientific research unfettered by specific practical concerns', while simultaneously being 'deeply committed to practical outcomes'.[79] This 'serendipitous' view (in Charnley's apt label) is the one that maximises professional autonomy and status, useful (essential?) for a Cambridge academic. It also reserved the right to define problems to the scientist rather than the farmer, say. Biffen saw the main problems as both national and international (or rather imperial): or bringing stability and prosperity, even "civilisation" back to rural regions destabilised by the "grain invasion" of cheap American wheat (the national problem) or, closely conceived, the development of white Kenyan wheat farming in the highlands (the imperial problem). We certainly have problems being articulated and sciences responding, but what sort of working worlds are these? They are not simply the maintenance of human health by bolstering food supply. They have an element of maintenance of global order and economic dynamism (or at least reinvigoration)

Furthermore, there is an interesting – and, in a general history of science sense, significant – dispute about what are good representatives of these problems. Specifically, it was in the context of UK agricultural science in the first half of the twentieth century that the question of what makes a good experimental trial was debated (in parallel with, but also largely prior to, the medical articulation of trials discussed above). Dominic Berry shows how the dispute was between advocates of the half-drill strip method, such as William Sealey Gosset and Edwin Sloper Beaven, and the randomised controlled trial, such as R.A. Fisher at Rothamsted.[80] Berry argues that if the full range of epistemic and social aims is examined, the half-drill strip is more successful in some than the RCT.[81] Both the half-drill strip and the RCT are representatives in the working world sense: they are microworld surrogates of the problem that enable, through abstraction, quantification, and statistical analysis, science to act. Most interesting in the context of the working worlds question is that one of advantages that the half-drill strip had over the RCT was in the social aim of enabling 'mutual understanding' between farmer and scientist. Both could 'see' what was happening in the field, and both sides could contribute therefore to the framing of the science. That was less true of the more abstruse RCT, whose meaning emerged only later when the statisticians had finished their work. What we see, then, as RCTs become the gold standard and other experimental designs are downgraded (as was the case with half-drill strips by the 1930s), is a diminishing of the ability of those experiencing the problem to shape the design of the science that is being proposed to be of help.

Like earlier agricultural research (such as Biffen's) the Agricultural Research Council, established in 1931, 'had been conducted on the principle that the production of practical benefits from research would be most effective in a system in which scientists were free to pursue research problems without the constraints imposed by immediate economic needs and agendas set by the government.' [82] The 'pure and strategic side of the range could be legitimated as no less practical than the applied side', notes Tim DeJager, 'Pure research, controlled by scientists, was regarded as the best way of ensuring that practical applications would emerge from their endeavours'. This autonomy was hard won, and arose from arguments that surrounded the early years of the ARC, in particular 'disputes were about the constitution of the Agricultural Research Council, the issue of appointments to its council, and the control of a new research institute' (the Institute for Research on Animal Diseases, which opened in 1938). On the one side were scientists arguing that agricultural science should be under the administrative control of scientists (they cited the MRC as the model, in which medical research funding was autonomous from doctors); on the other were politicians (such as the Minister of Agriculture) and civil servants (such as Harold Edward Dale) who thought that research must be part of an agricultural policy and therefore not autonomous.

The scientists largely won their argument for control (*de facto* from 1935, and formally and belatedly acknowledged in the Agricultural Research Bill of 1956). Even then the ministry argued that it was in a better position to articulate farmers' problems which should be the 'main inspiration for research', while farmers themselves agreed that the ARC was "too theoretical and not sufficiently acquainted with the practical needs of farmers".[83] I propose that such negotiations can be seen as boundary work within the working worlds diagram – where the line might be drawn between the articulation of problems and the work of science.

I have said, following Turner, that one reason the autonomy of science was defended was for reasons of securing professional status (or academic respect). Another way this was expressed was in how science self-regulated in order to protect this autonomy and status. For example, in 1905-1906 a controversy blew up over Thomas Jamieson's claims to have discovered the capacity of all plants to fix nitrogen from the air.[84] Jamieson ran the experiments at the Agricultural Research Association in Aberdeen, Scotland. He already had a reputation for clashes with leading English agricultural researchers. A. D. Hall viciously criticised what he called Jamieson's "illusory discovery" in *Nature*, explaining, significantly, that it

> … would be amusing were it not so dangerous and discreditable to the cause of scientific research, Mr Jamieson has a following … there is a body of solid farmers and landowners who sit under him and take advice on practical matters which they suppose to represent the last word on science.[85]

In other words, it was the close contact between Jamieson and local articulators-of-problems that was 'dangerous'. Hall's peer criticism (in *Nature*) of Jamieson was simultaneously science's self-regulation (and therefore an example of science's autonomy in practice) and policing of the boundary between working world and science.

According to David Smith, when Jamieson's ARA folded, a second new agricultural institute sprung up in the Aberdeen area. The famous Rowett Institute for animal (and human) nutrition was built up under John Boyd Orr using matched Development Commission and philanthropic funds. Whereas the ARA was guided by a committee of landowners, farmers, factors, manufacturers of manure and a seedsman, the Rowett was loosely answerable to a board of academic appointees and its research programme was very much chosen by Orr. This is just one example of a distancing from local working worlds.[86]

The case of the Rowett institute prompts me to propose a hypothesis: increased funding for science encouraged science by larger groups, but increased funds also meant the political justification of research had to be tied to outcomes (economic, problem-solving etc). The team-work-style science was therefore most compatible with problem-oriented science, which was also likely to be more interdisciplinary (which in turn was compatible with team-work). But tying research to outcomes threatened control of research, so in turn autonomy was emphasised, which is done by stressing how free fundamental research will 'ultimately' have practical value. This process captures some of dynamics of the relationship between working world and science in the period of "scaling up" so distinctive of the mid-twentieth century.[87] So, for example, in Smith's account of John Boyd Orr's Rowett we find all of the features of this dynamic.[88] The Development Fund (and philanthropy) was the source of dramatically larger funds. The problem-oriented science was "animal nutrition". Orr explained:

> So soon as any experiment in Animal Nutrition is pushed beyond the elementary stage problems arise in the branches of science [the traditional disciplines of chemistry, bacteriology, physiology] named. The solution of these problems, though offering *no immediate economic return*, must be attempted for the sake of elucidating the problems of nutrition. The more the work is calculated to be of permanent scientific value, and *ultimately* of practical and economic value, the more it is necessary to attack these subsidiary problems…[89]

And to do so, said Orr, an interdisciplinary team must be constituted ("a staff of experts in those branches… [which would] render possible the collaboration on one problem of several men trained in the different branches of science necessary", a "communal method of research… likely to be characteristic of research work in the future"). In practice, as Smith notes, 'the "communal method of research" at the Rowett soon became closely linked to the direction of research towards areas thought likely to produce practical results'.

Finally, some of this debate played out in the public sphere. The "team work" issue was the subject of a debate in letter pages of the *Times* in 1919.[90] *The Times'* own medical correspondent had called for the "appointment of teams of workers, specially chosen, and well versed in modern methods. These teams must be given facilities and must be set to attack definite problems". In reply, H.C Ross, director of the Clinical Laboratory of the Ministry of National Service, objected, saying that researchers must be "free to follow his own ideals, for it is by new methods that discoveries are made". W.S. Lazarus-Barlow, director of the Cancer Research Laboratory at Middlesex Hospital, also replied, distinguishing top-down, supposedly Germanic "team work" from egalitarian, supposedly British "group work". Smith sees this exchange as a comment on whether the direction from above – ie from the director of an institute – was fruitful or not. I would add that also at stake here is whether the science is being directed by a problem-orientation.

Another interesting example of the public sphere at play here is given by David Smith in his account of animal nutrition at the Rowett institute. Orr favoured mineral deficiency explanations of disease, and colleagues who disagreed were forced out. One was the pathologist John Pool McGowan. As the issue came to a head, McGowan criticised Rowett research on sheep. Frustrated, McGowan (almost certainly) wrote an anonymous letter to the local Aberdeen newspaper, the *Press and Journal*, in which he asked 'practical but informed questions that "sheep men would like to see answered".[91] Orr's side of the Rowett replied, in the same newspaper letter pages, inviting this supposed farmer in to discuss matters, but adding "I do not think it probable that he will have to forsake the fastnesses of a remote hill grazing to do so: I imagine that he is to be found much nearer to home". This is amusing enough, but is relevant to this paper in two ways. First, as an example of how appeals could be made through the public sphere. And second, that the framing of this appeal was made by pretending to be the voice of the person who experienced the working world problems (ie the sheep farmer).

Conclusion

In this paper I have made an inquiry into the relationship between problem articulation and the sciences in Britain between 1900 and 1939. I started with an overview of what I call 'working worlds', a general pattern I found in my synthetic survey history *Science in the Twentieth Century and Beyond* (2012). In that book I noted that several working worlds, arenas of human action that generate problems, seemed to be of particular importance to twentieth century science, ones associated with military, maintenance of the body in health and sickness, civil administration, technological systems, and global order. In this paper, I asked whether this picture would also characterise British science between 1900 and 1939.

I explored the articulation of problems in the public sphere, taking the letters pages and editorials of The Times as my case study. I found that – surprisingly? - most problems that were articulated were <u>not</u> linked to solutions from the world of science. But where they were, the working worlds of British science were broadly as found in the larger, wider study.

I then investigated further who (in public, or through the public sector) was calling for science to respond to problems during the period. Examining a range of bodies, drawing on secondary literature, I drew attention to the recurrence of rhetoric that embodied a prominent Victorian/Edwardian social contract for science, namely that in return for science being properly endowed, respected and granted status and social autonomy (and protection from short-term routine problem solving for clients), solutions from 'pure' science would be delivered in the medium to long term. I argue that this formation can be related to working worlds: the pressure of immediate problems needed to be managed and relieved, while at the same time promising ultimate contribution to solutions made a politically viable offer. Working worlds may not have been the origin, but they were the context for sustaining and deepening this phenomenon.

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[1] Jon Agar, *Science in the Twentieth Century and Beyond*, Cambridge: Polity Press, 2012. The first section of this paper draws on this publication.

[2] Thomas P. Hughes, *Networks of Power: Electrification in Western Society, 1880-1930*, Baltimore: Johns Hopkins University Press, 1983.

[3] Martin Campbell-Kelly, 'Information technology and organizational change in the British census, 1801-1911', *Information Systems Research* (1996) 7, pp. 22-36. Edward Higgs, 'The statistical big bang of 1911: ideology, technologcai innovation and the production of medical statistics', *Social History of Medicine* (1996) 9, pp. 409-426.

[4] Robert E. Kohler, *Lords of the Fly:* Drosophila *Genetics and the Experimental Life*, Chicago: University of Chicago Press, 1994. Sabina Leonelli. 'Weed for Thought: Using *Arabadopsis thaliana* to Understand Plant Biology', PhD thesis, Amsterdam University, 2007. Donna Haraway, *Modest_Witness@Second_Millennium.FemaleMan_Meets_OncoMouse: Feminism and Technoscience*, London: Routledge, 1997.

[5] The making of representatives has, of course, attracted considerable attention from historians, philosophers and sociologists of science. Joseph Rouse labelled them 'microworlds', a particularly apt name in the sense of this paper. Much of Bruno Latour's early work can be seen as describing, naming and analysing the construction of representatives. Representatives are partly 'immutable mobiles', putting complex worlds on paper where they can be seen at a glance. A special, but central, case of the abstractive effort concerns phenomena: natural effects that are deemed important and interesting but are difficult to manipulate in their wild state. The powerful place of laboratories, especially in the sciences since the mid-nineteenth century, can be explained because they were places where 'phenomenal microworlds', in Rouse's sense and in Latour's account, were made. James Scott says radical simplification is the way that states see; more accurately it is the way the state's scientific experts make representatives of state-defined problems. There is a great literature on model organisms, maps, models, classifications, databases, and other representatives.

[6] Patrick M.S. Blackett, 'Scientists at the Operational Level' (1942), reprinted in Blackett, *Studies of War*, Edinburgh: Oliver and Boyd, 1962, pp. 171–176. Maurice W. Kirby, *Operational Research in War and Peace: the British Experience from the 1930s to 1970*, London: Imperial College Press, 2003.

[7] A.P. Rowe, *One Story of Radar*, Cambridge: Cambridge University Press, 1948. Robert Buderi, *The Invention that Changed the World: How a Small Group of Radar Pioneers Won the Second World War and Launched a Technological Revolution*, New York: Simon & Schuster, 1996.

[8] For summaries of arguments for and against the existence of a functioning public sphere in Britain, see: Brian McNair, *Journalism and Democracy: an Evaluation of the Political Public Sphere*, London: Routledge, 2000. Much of the historical literature on the public sphere, following Habermas, is on its emergence in the early modern period.
[9] I used the *Times Digital Archive*, which contains searchable content from the newspaper from foundation (1785) to 2011. The search engine is here: http://find.galegroup.com/ttda/
[10] The sample was randomly distributed across the sample, using random number sequences generated via www.random.org/sequences.
[11] 'Imperial communications', *The Times*, editorial, 28 July 1928, p. 13.
[12] Digby L. Solomon, 'Spread of fire', letter to *The Times*, 30 January 1933.
[13] Allen of Hurtwood, 'German claims to colonies', letter to *The Times*, 30 December 1936.
[14] Letters to the editor were even broader in range of theme than editorials. Topics included: street traffic, Army reform, the interpretation of a Schubert symphony, Indian racial relations, unemployment, coal strikes, the Admiralty Arch, housing, Church politics, the appropriate education of children on Empire Day, ceramic figures in Bechuanaland, a by-pass for the town of Epsom, the gold standard, gambling, the age of Peter Wimsey (a fictional detective), and the question of settlements in the Palestine.
[15] 'Science' here includes related terms such as 'scientific' but also a few artefacts such as '-science' where 'conscience' has been split across lines.
[16] 'We publish this morning…', *The Times*, editorial, 15 June 1903 (on Empire and doctrines of 'scientific economy'); 'In view of the culminating battle over Chinese labour', *The Times*, editorial, 21 March 1904; 'The plague in India', *The Times*, editorial, 5 December 1907; 'East Africa', *The Times*, editorial, 8 May 1925; 'Migration from the North', *The Times*, editorial, 11 January 1930; 'A lead to the conference', *The Times*, editorial, 25 September 1930.
[17] 'The ventilation of the House of Commons…', *The Times*, editorial, 14 August 1906; 'The plague in India', *The Times*, editorial, 5 December 1907; 'Cancer and tuberculosis', *The Times*, editorial, 22 July 1911; 'Medical research', *The Times*, editorial, 19 January 1923; 'East Africa', *The Times*, editorial, 8 May 1925; 'Cancer research', *The Times*, editorial, 20 November 1925;
[18] 'The publication this morning of the conditions…', *The Times*, editorial, 8 June 1901 (on Carnegie's philanthropy as a factor in winning future industrial contests); 'Physical research', *The Times*, editorial, 27 August 1921; 'The International Air Congress', *The Times*, editorial, 25 June 1923; 'New light on steel', *The Times*, editorial, 15 May 1934.
[19] 'East Africa', *The Times*, editorial, 8 May 1925; 'British fisheries', *The Times*, editorial, 31 August 1927; 'Migration from the North', *The Times*, editorial, 11 January 1930.
[20] 'British optical glass', *The Times*, editorial, 19 October 1925.
[21] 'Science and education', *The Times*, editorial, 7 September 1937.
[22] As an example of the latter, a 1900 letter proposed a new way of indexing the Library of the British Museum, and 'science' is mentioned merely as one of many topics that could be indexed. In this case science is not expected to provide a solution. "A scholar", 'The proposed subject-index to the Library of the British Museum', *The Times*, letter to the editor, 15 October 1900.
[23] Henry Armstrong, 'Research chemists', *The Times*, letter to editor, 23 February 1910; C. Edward Sage, 'Technical chemists and manufacturers', *The Times*, letter to editor, 4 June 1913; Charles Harford, 'The demand for more beer', *The Times*, letter to editor, 24 May 1919 ('national efficiency'); Jocelyn Thorpe, 'Science and industry', 2 February 1915; C.E. Ellington Wright, 'Industrial trusts', 29 October 1926; Colin Buist, 'Oil from

coal', *The Times*, letter to editor, 22 August 1931; J.A.A. Pickard, 'Road accident figures', 17 August 1936.
[24] H.O. Arnold-Forster, 'The emperor's new clothes', *The Times*, letter to editor, 10 June 1908; Alex Holle, 'Air power', *The Times*, letter to editor, 8 August 1922; Sydney Tatchell, 'Defence works policy', 6 February 1937.
[25] E.J. Russell, 'Cotton and irrigation', *The Times*, letter to editor, 11 January 1926; Dawson of Penn, 'Diet and health', 11 January 1934; W. Langdon Brown et al, 'Malignancy in cancer', 21 November 1934.
[26] Geoffrey Drage, 'Poor law reform', *The Times*, letter to editor, 13 May 1911; Sydney Tatchell, 'Defence works policy', *The Times*, letter to editor, 6 February 1937.
[27] E.J. Russell, 'Cotton and irrigation', *The Times*, letter to editor, 11 January 1926.
[28] E.O. James, *The Times*, letter to editor, 7 September 1927.
[29] A.P. Laurie, 'Duke of Westminster's Rembrandts', *The Times*, letter to editor, 27 February 1929.
[30] Google scholar, albeit a crude but comparative measure of impact, records 933 citations. Other comparable books from the period would include A.N. Whitehead, *Science and the Modern World* (1926, 729 citations), Henri Poincaré, *Science and Hypothesis* (1905, 3293 citations), J.D. Bernal, *The Social Function of Science* (1939, 1483 citations), T. Veblen, *The Place of Science in Modern Civilisation* (1919, 1393 citations), H.G. Wells, Julian Huxley and G.P. Wells, *The Science of Life* (1931, 154 citations).
[31] Karl Pearson, *The Grammar of Science*, 2nd edition, London: Adam and Charles Black, 1900, p. 37.
[32] Alison Bashford and Philippa Levine (eds.), *The Oxford Handbook of the History of Eugenics*, Oxford: Oxford University Press, 2010. G. R. Searle, *Eugenics and Politics in Britain, 1900-1914*, Leyden: Noordhoff, 1976.
[33] Pearson, op. cit., pp. 25-28. Greta Jones, 'Eugenics and social policy between the wars', *The Historical Journal* (1982) 25, pp. 717-728.
[34] Pearson, op. cit., p. 29.Jones, op. cit.
[35] Frank M. Turner, 'Public science in Britain, 1880-1919', *Isis* (1980), pp. 589-608, p. 589. The documents of public science included: presidential addresses of the British Association for the Advancement of Science, the columns of Nature, 'and the major public utterances of men of science as they appeared in the leading journals and newspapers of the day'.
[36] Turner, op. cit. For physics as a case study, see: Russell Moseley, 'Tadpoles and frogs: some aspects of the professionalization of British physics, 1870-1939', *Social Studies of Science* (1977) 7, pp. 423-466.
[37] Three specific factors, suggests Turner, account for this shift: the identification of Germany as an industrial and military threat, the political success of anti-vivisectionism, and the perceived 'unresponsiveness of the political system'.
[38] Turner, op. cit., pp. 596-597.
[39] The announcement of the British Science Guild in *Nature* (1904) 72, p. 586. My emphasis. Quoted in Turner, op. cit., pp. 601-602.
[40] Andrew Hull, 'War of words: the public science of the British scientific community and the origins of the Department of Scientific and Industrial Research, 1914–16', *British Journal for the History of Science* (1999) 42, pp. 461-481, 466-467.
[41] Turner notes the new Committee on the Neglect of Science criticising the government at length in *The Times* on 2 February 1916, for example.
[42] Joseph Morgan Hodge, Triu*mph of the Expert: Agrarian Doctrines of Development and the Legacies of British Colonialism*, Ohio: Ohio University Press, 2007. Helen Tilley, *Africa as a Living Laboratory: Empire, Development, and the Problem of*

Scientific Knowledge, 1870-1950, Chicago: University of Chicago Press, 2011. Peder Anker, *Imperial Ecology: Environmental Order in the British Empire, 1895-1945*, Cambridge, MA: Harvard University Press, 2001. See also: Mark Harrison, 'Science and the British Empire', *Isis* (2005) 96, pp. 56-63. Paolo Palladino and Michael Worboys (1993) 'Science and imperialism', *Isis* (1993) 84, pp.91-102.

43 Robert Olby, 'Social imperialism and state support for agricultural research in Edwardian Britain', *Annals of Science* (1991) 48, pp. 509-526.

44 Roy MacLeod and Kay MacLeod, 'The contradictions of professionalism: Scientists, trade unionism and the First World War', *Social Studies of Science* (1979) 9, pp. 1-32, p. 14. The reference in the Labour manifesto was Arthur Henderson, *The Aims of Labour* (London: Headley Bros., 1918), Appendix II, 'Labour and the Social Order', p. 112. This Labour policy had little effect (the disappointed Hyman Levy saw this moment as the end of the first phase of social relations of science movement).

45 David Edgerton, 'British Industrial R. and D. 1900-1970', *Journal of European Economic History* (1994) 23, pp. 49-67.

46 Philip Gummett, *Scientists in Whitehall*, Manchester: Manchester University Press, 1980, p. 24. Andrew Hull, 'War of words: the public science of the British scientific community and the origins of the Department of Scientific and Industrial Research, 1914–16', *British Journal for the History of Science* (1999) 42, pp. 461-481, p. 464. See also: Roy MacLeod and E.K. Andrews, 'The origins of the DSIR: reflections on ideas an men, 1915-1916', *Public Administration* (1970) 48, pp. 23-45. Peter Alter, *The Reluctant Patron: Science and the State in Britain, 1850-1920*, Oxford: Berg, 1987.

47 Sabine Clarke, 'Pure science with a practical aim: the meanings of fundamental research in Britain, circa 1916-1950', *Isis* (2010) 101, pp. 285-311.

48 Huxley: "What people called Applied Science is nothing but the application of Pure Science to particular classes of problem", quoted in Clarke, op. cit., p. 291.

49 Gregory, in *Discovery, or the Spirit and Service of Science* (1916) argued that, after listing seven great modern inventions, "each one of these things had its foundation in purely scientific work and was not the result of deliberate intention to make something of service to humanity", quoted in Clarke, op. cit., p. 292. See also: Graeme Gooday, '"Vague and artificial": the historically elusive distinction between pure and applied science', *Isis* (2012) 103, pp. 546-554.

50 *DSIR 1st Annual Report*, Cd. 8336 (1915-1916), p. 16. Quoted in Clarke, op. cit., p. 294.

51 *DSIR 7th Annual Report*, Cmd. 1735 (1921-1922), p. 30. Quoted in Clarke, op. cit., p. 299.

52 Institution of Mechanical Engineers, Institution of Electrical Engineers, Institute of Metals, Iron and Steel Institute, Institution of Gas Engineers, Institution of Mining and Metallurgy, Institution of Mining Engineers, Engineering Standards Committee, Institution of Civil Engineers, Royal Society, Chemical Society, Faraday Society, Institution of Marine Engineers, Institution of Naval Architects, British Engineers Association, Manchester Steam Users Association, Manchester Association of Engineers, Silk Association.

53 DSIR 1/1. Minutes, ACSIR, 10 December 1915, Appendix A.

54 DSIR 1/1. Minutes, ACSIR, 17 August 1915, and onwards.

55 DSIR 1/13. Minutes, DSIR, SGC, 19 October 1927.

56 David Edgerton and Sally Horrocks, 'British Industrial Research and Development before 1945', *Economic History Review* (1994) 47, pp. 213-238, pp. 219-220.

57 Edgerton and Horrocks, op. cit., p. 227.

58 Edgerton and Horrocks, op. cit., p. 214.

[59] Colin Divall , 'A measure of agreement: employers and engineering studies in the universities of England and Wales, 1897–1939', *Social Studies of Science* (1990) 20, pp. 65–112.
[60] J. F. Donnelly (1991) Science, technology and industrial work in Britain, 1860–1930: Towards a new synthesis, *Social History*, 16:2, 191-201, p. 197: 'In the universities, embryonic academic disciplines were visible around the turn of the century in fields such as chemical, electrical and mechanical engineering, ceramics and the food industry. These technologies imitated the practice of the 'pure' sciences, with the creation of a public domain of knowledge to which academics contributed. Public curricula were also constructed in these technical fields. However, the tension with private, commercially sensitive technique was frequently apparent. … In most fields academics undertaking technical work for firms were frequently not allowed to publish, and conflicts occurred at other levels. Such tensions ensured that the influence of private industry on academe was heavily mediated even in technical spheres'.
[61] Edgerton, 'British industrial R&D 1900-1970', op. cit., p. 52. See also: Edgerton, *Warfare State: Britain, 1920-1970*, Cambridge: Cambridge University Press, 2006.
[62] 'Technical Report of the Advisory Committee for Aeronautics, 1909-10', quoted in DSIR 24/85. 'The Aeronautical Research Council', undated (1946). Senior figures on ACA/ARC included the 1st Lord Rayleigh, Sir George Greenhill, Sir Napier Shaw, Lt. Col. Mervyn O'Gorman, F.W. Lanchester, Sir Horace Darwin, Sir Joseph Petavel, Sir Richard Glazebrook. There were representatives of service departments, and, between 1920-1924, firms, before the representation of "trade" became an issue. There were also many sub-committees.
[63] Of 344 papers published alongside the ARC annual reports from 1925/26 to 1929/30, only 3 dealt with aircraft instruments.
[64] 'From the earliest days the effect of this theoretical work quickly showed itself in the field of practical developments. The design of stable aircraft was noteworthy case in the early history of the Committee's work. The Advisory Committee for Aeronautics … took up in 1912 the work which had been started by Professor G.H. Bryan on the theoretical side of the stability of aeroplanes, and published in March 1913, a paper in which an attempt was made to seek practical applications. The celebrated work then undertaken by Busk at the [Royal Aircraft Establishment] was carried out with the encouragement of the Committee, and led to the RE.1 – the first really stable aeroplane'. DSIR 24/85, 'Aeronautical Research Council', 1946, p. 23.
[65] An example is accidents from engine failure, raised as a problem by the Accidents Investigation Sub-Committee of the ARC in 1924, which not only resulted in changes in maintenance practice but also led the Air Ministry to begin to compile and analyse statistics of accidents routinely – the basis for an operational science of engine failure. DSIR, op. cit., pp. 23-24. The 'investigation of engine failures is thus an example of a whole field of problems which the Committee was particularly well equipped to study in an exhaustive manner'.
[66] For example: 'Aeronautical research', *The Times*, editorial, 10 February 1921
[67] Roy MacLeod (1993) The chemists go to war: The mobilization of civilian chemists and the British war effort, 1914–1918, *Annals of Science*, 50:5, 455-481, p. 456. MacLeod notes: In discussing science and the Great War, Alex Roland and other historians have argued a restrictive thesis--that science contributed little to the outcome of a conflict which was fought with weapons mostly in existence at its outset. The problems of manufacturing current technologies outweighed those of developing new ones. Much of this, military historians and historians of science will readily accept. The war did little to draw British scientists and the military into operational collaboration. Indeed, many leading officers remained sceptical of the uses of science. But there are important

qualifications. Particularly in the explosives and munitions industries, but also in the medical, metallurgical and agricultural fields, the war of 1914-1919 was replete with science-based applications and several of them in anticipation of things to come'.
[68] Joan Austoker and Linda Bryder, *Historical Perspectives on the Role of the MRC: Essays in the History of the Medical Research Council of the United Kingdom and its Predecessor, the Medical Research Committee, 1913-1953*, Oxford: Oxford University Press, 1989.
[69] Clarke, op. cit., p. 307.
[70] Desirée Cox-Maksimov, 'The making of the clinical trial in Britain, 1910-1945: expertise, the state and the public', PhD thesis, University of Cambridge, 1997.
[71] Robert Olby (1991) Social imperialism and state support for agricultural research in Edwardian Britain, Annals of Science, 48:6, 509-526, p. 510.
[72] Olby, op. cit., p. 515.
[73] Olby, op cit., p. 520.
[74] "Second Report of the Development Commissioners', Parliamentary Papers (1912-13), XVII, 837-911
(p. 846), cited in Olby, op. cit. p. 520.
[75] See Olby, op. cit., Table 2, although compare this with the very large number of labs found by Paul Smith, 'The development of horticultural science in England, 1910-1930', University College London PhD thesis, 2016.
[76] Steindór J. Erlingsson, 'The Plymouth Laboratory and the institutionalization of experimental zoology in Britain in the 1920s', *Journal of the History of Biology* (2009) 42, pp. 151-183. In supporting work at the Plymouth laboratory, the Development Fund recognised two categories worthy of support: "fundamental research" and research "throughout purposefully directed to definite ends". Erlingsson, op. cit., p. 166.
[77] Tim DeJager, 'Pure science and practical interests: The origins of the agricultural research council, 1930–1937', Minerva (1993) 31, pp. 129-150, p. 133.
[78] A.D. Hall, A.D., 'The future of agricultural science', *Journal of the Ministry of Agriculture* (1933--34) 40, p. 1024, quoted in DeJager, op. cit., p. 134.
[79] Berris Charnley, 'Experiments in empire-building: Mendelian genetics as a national, imperial, and global agricultural enterprise', Studies in History and Philosophy of Science Part A (2013) 44, pp. 292–300.
[80] Dominic Berry, 'The resisted rise of randomisation in experimental design: British agricultural science, c.1910–1930', *History and Philosophy of the Life Sciences* (2015) 37, pp 242–260.
The resisted rise of randomisation in experimental design: British agricultural science, c.1910–1930
[8181] The five aims were: A) investigate varieties according to site B) find the best of new varieties C) manage environmental conditions D) mutual understanding between scientists and farmers, E) compare the old with the new.
[82] DeJager, op. cit., pp. 129-131.
[83] The ministry's and farmers' views (from 1954) are referenced in DeJager, op. cit., pp. 148-149.
[84] David Smith, 'The Agricultural Research Association, the development fund, and the origins of the Rowett Research Institute', *The Agricultural History Review* (1998) 46, pp. 47-63.
[85] Quoted in Smith, op. cit., p. 52.
[86] One final point of difference noted by Smith, op. cit., p. 63: Jamieson was a loner and 'expressed the view that scientific advance was most likely to be the product of hard work by solitary scientists'; in contract, Orr was a networker (making use of of contacts in medical science, agricultural science, and politicians) and a believer and promoter of

teamwork. This meant he was more able to 'take advantage of the opportunities offered by the increasing state support for science of the twentieth century.

[87] Agar, op. cit., for "scaling up", and a summary of the literature on emerging "Big Science".

[88] David Smith, 'The Use of "Team Work" in the Practical Management of Research in the Inter-War Period: John Boyd Orr at the Rowett Research Institute', *Minerva* (1999) 37, pp. 259-280. Smith suggests that there is a close parallel in the rise of team-work (or "corporatism") and laboratories in the medicine as described in: Steve Sturdy and Roger Cooter, 'Science, scientific management and the transformation of medicine in Britain, c1870-1950', *History of Science* (1998) 36, pp. 421-466.

[89] Smith, op. cit., pp. 261-262. My emphasis.

[90] Following quotations from Smith, op. cit., pp. 277-278.

[91] Quotations from Smith, op. cit., p. 272.