Beveridge and the Brief Life of ‘Social Biology’ at the LSE

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Introduction²

Sir William Beveridge, 1879–1963, was a distinguished figure in the history of public policy. His 1942 report *Social Insurance and Allied Services* — widely known as the Beveridge Report³ — played a key role in the development of the British welfare state. He was earlier the Director (equivalent to a Vice Chancellor of a British or Australian university) of the London School of Economics. This role throws interesting light both on Beveridge himself, and on issues relating to the financial support of academic research in Britain in the 1930s.

Beveridge was a strange man (for ample documentation of this, see Harris 1997). At Oxford he studied Mathematics and then Classics, and also read a lot of popular science — including the work of Thomas Henry Huxley, by whose inductivist writings about scientific method he was greatly impressed, and upon which he later lectured the staff at the LSE at every opportunity. He was essentially self-educated in the social sciences, having strong practical interests in the empirical investigation of issues to do with unemployment and social security. He worked at Toynbee Hall — an Oxford University Settlement in London’s East End — and as a leader-writer on the conservative *Morning Post*, and also got to know, and was influenced by, Beatrice and Sydney Webb, who had played a key role in the founding of the LSE. Beveridge had wide-ranging, but somewhat inchoate, views on most issues, and was impatient with anyone who disagreed with him. He subsequently worked in the public service, and came increasingly under the domination of a cousin, Jessie Mair, who followed him to the LSE, where she became Secretary of the School, and in effect a kind of unofficial co-director.

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2 The present account is of necessity preliminary, in that it is based on research at the Rockefeller Foundation Archive and printed sources. More research is called for, both at the Rockefeller Foundation Archive and in the LSE Archive, which latter I have not been able to work at in connection with the present project. Since writing a first draft of this paper, I have had the opportunity to consult two studies which give a good account of the Department of Social Biology: Salma Preveen Ahmad’s ‘Institutions and the Growth of Knowledge’, (University of Manchester Ph.D. thesis, 1987), and Chris Renwick’s *Completing the Circle of the Social Sciences? William Beveridge and Social Biology at London School of Economics during the 1930s*, Philosophy of the Social Sciences DOI: 10.1177/0048393113480782; XX(X) 1–19.

3 <http://www.ncbi.nlm.nih.gov/pmc/articles/PMC2560775/>

Beveridge was an intelligent and capable man, but his concerns were out of tune with what was actually going on in the social sciences at the LSE as they developed. At one level he was effective as an administrator — he attracted massive resources to the School. Yet his personal style was dictatorial, his substantive views were idiosyncratic, and Mrs Mair — whom he was later to marry — was a major influence; she was opinionated, and seems to have been motivated largely on the basis of personal prejudices.⁴

Another key aspect of our story is the Rockefeller Foundation — or, more precisely, with people connected with the Laura Spelman Rockefeller Memorial. This had been set up — with a massive endowment — in 1918, in commemoration of Rockefeller's wife. It was initially concerned to provide funding to various of the causes to which she had personally given support — including African-American education. But there was a shift of policy on the part of the Memorial under the Chairmanship (from 1922–29) of the young and entrepreneurial Beardsley Rummler. It shifted its policy from the funding of more obviously charitable activities, to the encouragement of the development of centres for serious research in the social sciences. Here, it gave massive assistance to a small number of Centres — of which one was the London School of Economics. Beveridge had spent time at the Memorial as an International Fellow, and had had many conversations with Rummler and other members of staff (he was well-known to them as 'Bill, the drink' [as in 'beverage']).

Beveridge got on well with Rummler, and shared strong preferences with him for atheoretical, empiricist work in the social sciences. Beveridge seems to have almost re-discovered the younger Historical School's agenda for himself — proposing, at one point, to undertake a large empirical study of trade cycles, and spending a lot of time on an empirical study of price fluctuations. His own style of operating fitted well that of the Memorial, for it was un-bureaucratic, entrepreneurial and willing to give large amounts of money for the needs of a few selected research centres in the social sciences, with only a minimum of accountability. (Things were to change dramatically when the Memorial was wound up and, in its place, its programs were administered by the Social Sciences division of the Rockefeller Foundation — hereinafter 'Foundation'.)⁵ Under the Memorial, funding was granted to the LSE for buildings, for improving the Library, and for the promotion of research in the social sciences. In part,

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4 See, for example, the Prologue to Harris's *Beveridge* — which is striking in that it clearly aims to offer something of a middle path between her son's account and that of her critics, while having the consequence of damning her completely.

5 Compare, for example, a memorandum from John van Sickle re the renewal of a grant to the LSE, Folder 596, 15 October 1932: 'On the whole the LSRM grant has been used wisely. [Van Sickle goes on to note a difference of interpretation as to what the grant should have been used for]...My task upon returning to Europe will be to call upon Sir William and bring to his attention as tactfully as possible our views upon the subject and to see to it that if the LSRM grant is renewed, the distinction between the purposes of the two grants shall be ['rigidly'] adhered to.'

this was in line with what the LSE was already undertaking — including the funding of a Chair in Economics (Memorial support was used to attract Allyn Young to the LSE). But it also involved the development of a distinctive agenda, which was put together by Beveridge.⁶

Beveridge, the Memorial and Social Biology

The idea of 'social biology' looks to have been thought up by Beveridge himself — although there may well have been discussions with Ruml about the idea. It formed part of a package for work which might be undertaken with Memorial funding, in an area which was referred to as 'Natural Bases of Social Sciences' (and more specifically of economics). Beveridge and Ruml were in correspondence about these ideas between July 1925 and May 1926. In December 1926, Beveridge wrote to Ruml, referring to work in anthropology, geography, psychology and sociology, and also to 'Social Biology'. In his letter, Beveridge quoted from a Memorandum (Harris: 280) which had been drawn up at the LSE. It might be useful to quote briefly from it, to give a flavour of the document. It starts as follows:

The natural Bases of the Social Sciences

By the study of the 'natural Bases of the Social Sciences' is meant the study of the borderland between various natural sciences, in particular Anthropology, Biology, Psychology, Physiology on the one hand, and the social sciences, in particular those concerned with Economics, with Social Institutions and with Political Organisation on the other hand.

The study of this borderland involves familiarity with the territory on each side. Theoretically this knowledge of both fields can be secured in either of two ways. Men who have trained first in natural science may proceed to study the appropriate social science, or vice versa economists may make themselves familiar with the appropriate natural science. The latter course has been followed in the teaching of Sociology at the School of Economics. While continuing and even extending this method, it is desired now to initiate and develop the former method as well. If, however, the working of the borderland is to be undertaken

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⁶ Beveridge represented this as something that was favoured by the Rockefeller Foundation. I have not yet been able to discover if there is evidence about Ruml's support for it; but there would seem no reason to suppose that he was not sympathetic. However, from the takeover of the Memorial by the Rockefeller Foundation onwards, it would seem as if no one outside the Department, apart from Beveridge and Mrs Mair, favoured the idea that the LSE should house a department of Social Biology. Indeed, as I shall explain, the Foundation became progressively more hostile, while Beveridge argued to the senior faculty at the LSE that it was something in which the Rockefeller Foundation had a particular interest.

increasingly by men whose original training is in natural science, it is all the more important that their latter work should be carried on in an institution predominantly devoted to the social sciences, against a background of Economics, History, Political Science and Laws, and in the light of the general study of Sociology. There must be no doubt of their full equipment in social as well as in natural science.

The document includes more supporting detail concerning Social Biology.

Social Biology

This subject may be defined broadly as the application of Biology to human Society; it would cover such topics as variation and heredity in man, selective immunity, relative importance of environmental factors in social structure and changes, questions of race and class in relation to hereditary endowment, economic and biological tests of fitness. Many of these topics have their psychological as well as their biological aspects, as is indicated below in dealing with Social Psychology. Through Vital Statistics 'Social Biology' would connect with 'Public Health', which is also mentioned below [in material not quoted here].

The subject probably does not involve the setting up of a biological laboratory, but does involve that the teachers in it have the run of such a laboratory and that one at least of them is a trained biologist.

Ultimately a department of Social Biology would need a Chair with subordinate staff and provision for research. In the first instance, however, it is possible that no suitable candidate for a Chair would present himself if it were established; there should be liberty to suspend or not at once establish a Chair and to use the income set free for the establishment of temporary Research Fellowships, for, say, two trained biologists who would undertake to study Economics and Social Science, to research into some specific topic on the borderline of Economics and Biology and gradually to qualify for teaching.

On 29 December 1926, Beveridge wrote further to Ruml, asking for a capital sum of \$500 000 as an endowment to promote the study of 'the natural bases of the social sciences', and other work referred to in his Memorandum. However, on the same day, Beveridge wrote a further letter to Ruml in which he asked instead for \$200 000 for 'international studies'. On 14 January 1927, Ruml wrote to Beveridge indicating that the Trustees had agreed to grant the LSE: (i) \$175 000 for the library catalogue and building extension; (ii) \$500 000 for general endowment, and (iii) \$200 000 for international studies.⁷ He further indicated:

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⁷ Folder 594, Ruml to Beveridge, January 14th, 1927.

You will note that the capital sums for endowment herein provided are designated for the general endowment of the School and that the allocation of income is in the discretion of the School. The appropriations, however, were made in view of the circumstances and plans outlined in your letters and memoranda.

Two points are worth making immediately about this. The first, is that while the proposal may have received endorsement from the LSE's Professorial Council, the ideas about 'Social Biology' were criticised by Leonard Hobhouse,⁸ who complained that Sociology had not been consulted, and that he had been linking sociology to biology for some 20 years (Harris: 280). Second, what Beveridge has to say about social biology seems highly problematic, if thought of in terms of the organisation of academic professions — and, thus, of the career structure of someone who might pursue this approach.

At one level, it is surely the case that one might hope that there could be interesting cross-fertilisation between disciplines. But the way in which Beveridge puts his ideas suggests a complete naïveté concerning how academic research and academic departments might usefully develop. A key issue, here, is this. While the notion that the social sciences might benefit from knowledge from biology is indeed interesting, Beveridge seems to me to have failed to think about what this would mean in terms of 'paradigms' or 'research programs' or in terms of institutional organisation. That is, if one had such a department, what, in fact, would the people within it do, and where would they publish their results: what colleagues would form their audience? Beveridge, as far as I can see, failed to give any thought to this at all. But clearly his ideas, as set out in the Memorandum and in other statements⁹ would pose a real difficulty. For what person of any ability in biology would essentially abandon the area in which they had been trained and, instead, re-tool in another discipline? And, if they did, to what kind of work might their efforts be expected to lead?

It is striking that, in an undated memorandum that would seem to date from some time during 1926,¹⁰ Beveridge had written:

In Social Biology there is no post. Special lectures on problems of heredity have been given in recent years with marked success and meet an obvious demand. In this field it is not desired of course to establish

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8 Leonard Hobhouse was a proponent of 'new liberalism', and occupied the Chair of Sociology at the LSE from 1907 until his death in 1929.

9 Such as the following from a letter to Ruml, 2 November 1927: 'We have heard of a possible brilliant biologist who might be prepared to become a research fellow and learn economics with a view to studying sociological problems' (Folder 595; Beveridge to Ruml, 2 November 1927).

10 Folder 596, undated 'Memorandum as to some requirements of the London School of Economics'.

a biological laboratory. What is wanted is to get a man of biological training to learn Economics and then and only then to apply himself to economic and social problems.

As we shall see, the outcome here was interesting — but also problematic.

The Department of Social Biology

The person hired as Professor of Social Biology was Lancelot Hogben (1895–1975). Hogben was a brilliant if awkward man. His father had run a seaside mission and Hogben was brought up with a formidable knowledge of the Bible, but became sceptical about religious matters. His parents had hoped that he would train to be a medical missionary but, in the event, he became a biologist. He studied at Cambridge, where his social background contrasted strongly with that of most of his contemporaries. He became an independent-minded socialist — and very much an intellectual loner: while he associated professionally with other socialist biologists of the period, such as Bernal and Haldane, who might be thought to be close to him politically, he was highly critical of their espousal of Soviet-style dialectical materialism (see for a most interesting discussion, Werskey 1978). Hogben, by contrast, was a more orthodox materialist and a behaviourist, and also a firm empiricist with regard to methodology.

His socialism was also unorthodox for the time, in the sense that he was critical of modernist urbanism, and instead responded to the more romantic and rural strand in British socialism represented by Ruskin and Morris. It was his hope that the application of science might make it possible for people to lead pleasant lives in a rural setting (he was as critical of the modernist apartment blocks of the Vienna socialists as he was of the products of consumer society). But this, in his view, all meant that ordinary people needed an appreciation of the science and mathematics which was re-shaping their lives. To this end, he was to write best-selling popular works on science and mathematics, and to collaborate with a linguist and a historian on further volumes in the same series — the aim of such work being explicitly political.

Hogben had married a feminist scholar, Enid Charles, and was involved in various forms of social activism, as well as in the pursuit of his own career. This took them to South Africa, but he found the ‘scientific racism’ that started to become popular there difficult to take. Hogben had a sharp wit and tongue, and was not someone to leave things unsaid which in his judgement needed to be said. He was critical in print of such ideas, including the eugenic ideas which were at the time popular among many ‘progressive’ people. At the same time,

he also undertook extensive work in biology, including work which made use of material from toads in order to produce a human pregnancy test. However, things became increasingly difficult for him in South Africa.

Hogben was thus attracted to the Social Biology position at the LSE. But among the conditions which he required were that his wife was also employed there, and that he had a biological laboratory. This, and the presence in it of toads and other small animals, was a point of controversy at the LSE. In part, this was simply because of the very idea of having such a laboratory at the LSE and, in part, because running it was expensive (and took a sizable portion of the Rockefeller funding). Hogben also held a research-only position, so that, unlike his colleagues, he was under no obligation to offer undergraduate teaching, although in fact he gave lectures on popular science, which became the basis for his *Science for the Citizen* (1938).

Hogben's position at the LSE would have been difficult under any circumstances. However, his problems were compounded by the fact that he was not good at personal relations.¹¹ In addition, he chose to pick a quarrel with the Department of Economics, headed by Lionel Robbins. Hogben — here in full sympathy with Beveridge — was aggressively in favour of empirical approaches in the social sciences. Not only was the Department's economics strongly theoretical in its orientation, but Robbins was strongly attuned towards developments in Europe and had an interest in the approach of the Austrian School. Robbins was also the author of a volume on the scope and methods of political economy, *An Essay on the Nature and Significance of Economic Science* (1935, 1940), in which he favoured ideas drawn from the British and Austrian approaches which were deductivist and, in certain respects, anti-empiricist in their orientation. Of all of this, Hogben was critical and he was not someone to keep his criticisms to himself. He delivered a lengthy public lecture — which was subsequently published as a small book (Hogben 1936) — at the Red Lion Hall, not far from the LSE, in which he was explicitly critical of the ideas of his colleagues.¹² In addition, when it became clear that the fate of his Department was uncertain, he published a large book, *Political Arithmetic*, which put its work on show. Hogben wrote, as an introduction, a long article in which he again took issue with the methodological ideas of the LSE economists, drawing a parallel between his critique of their work and figures in early modern science who had criticised the obscurantism of their non-empiricist forebears. He was also thought to be behind an attack on the LSE economists which was published as an editorial in *Nature*.¹³

11 It is, for example, striking that he did not show the kind of deference towards Foundation staff that they were obviously used to.

12 See my 'The Battle of Houghton Street', paper presented to the 2010 conference of the History of Economic Thought Society of Australia.

13 RF RG 1.1 Series 401S Box 71, London School of Economics 1938; 25 February 1938, TBK to SHW.

The work that was put on show in *Political Arithmetic*, however, was in fact in demographics. (This contrasts, say, with issues raised in a report to the Foundation of November 1933, where the emphasis was much more on biological issues.)¹⁴ Enid Charles had been joined by other scholars who had undertaken empirical studies which were certainly interesting. David Glass worked for the Department as a Research Assistant, and went on to become a Reader and, subsequently, Professor of Demography. Indeed, somewhat ironically, after the Second World War, his department was to receive financial support from the Rockefeller Foundation, and to have a significant role in the development of both demography and sociology in the UK.

Problems with the Rockefeller Foundation

The Department of Social Biology had been one of the products of the Memorial's funding of the LSE. As mentioned earlier, in 1929 the Memorial was closed down, and its activities were taken over by the Social Sciences branch of the Foundation. There was continuity in respect of many personnel; but Ruml was soon to leave. Further, the period of large-scale entrepreneurial activity was over: one might see the Foundation as engaged in a much more bureaucratic operation. Certainly the wide discretionary activity with regard to expenditure, which Beveridge had initially been given, was no more: applications had to be made for specific projects, and institutions were scrutinised to make sure that they stuck much more closely to the terms for which their funding had been awarded. (Beveridge, for example, was to run into some difficulties in respect of a Foundation award. He had interpreted it, as in the past, as being simply to provide support for the research of senior staff — such funding was, indeed, about the only show in town — but the Foundation had understood it as being primarily oriented towards the training of graduate students. Beveridge was taken to task over this, although the officers of the Foundation were understanding, and did not press him as hard as they felt about this matter.)

It was clear, however, that under the new regime, things started to look difficult for Social Biology. While Ruml personally might well have been enthusiastic about Beveridge's ideas, this enthusiasm was not shared by the Foundation officers, who could not really see what the LSE was doing spending a relatively large amount of money on a biological laboratory. In addition, while Ruml had been an entrepreneur himself, the approach of the Foundation was rather different. They had a program of Fellowships of various kinds, and they used the people who made recommendations to them of possible Fellows as more

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14 See Folder 401S LSE October–December 1933; 14 November, report of work by Hogben (which also discusses work undertaken by other members of his department).

general sources of information (and they sometimes made use of the Fellows themselves for similar purposes). Officers met with these people, were briefed by them about the state of the different academic disciplines, and then circulated synopses of the briefings in the social science section of the Foundation. This meant that the officers of the Foundation were able to make judgements on proposals, not just on the basis of their own views but on those of a range of well-informed people whose opinions could be cross-checked with those of others. Clearly, the result of this was that the Foundation's judgements could have a degree of objectivity to them of a sort which contrasted with those of the Memorial (where the funding of the LSE and of Beveridge's agenda has the feel of a deal done by mates). But it would also mean that the basis of the judgements was more that of an elite consensus and less open to entrepreneurial initiative.

This, however, was not half of the issue. For the Foundation, after a period of what could be called 'caretaking' of the agenda that had been set for them by the Memorial, took a decision, at the level of the Trustees, to change course in quite a radical manner. Essentially, it abandoned the funding of research in the social sciences, in favour of a program which was much more closely targeted at what were seen to be the needs of the US in a period of depression, and in effect shifted their activities to international affairs and to issues relating to social security. (It is striking that there is a marked decline in the quality of the material with which the Social Sciences people were dealing, and in the view of the present writer, if one compares what was achieved by the Memorial and subsequently by the Social Sciences division of the Foundation, it represented a terrible misjudgement of what was academically important.) At the same time, the Foundation recognised that a number of institutions depended upon their support to a significant degree, and bridging funding was provided to assist them wind down these programs or to find alternative sources of support. In addition, the officers of the Foundation took considerable trouble to try to find ways in which those with whom they had been working might still be able to receive limited support under the new programs, and there was some continuing funding for exceptional cases.

All this, however, meant that the writing was on the wall for the Department of Social Biology. The advice that the Foundation were getting from outside the LSE — for example, from Allen at University College, London — indicated that while they had real respect for Hogben, there was little sympathy for the idea that he should be conducting biological research at the LSE. An application that Hogben made to the Foundation for funding was unsuccessful — which seems to me not a judgement on the quality of his work, but a product of the fact that the Foundation was winding down its support for work at the LSE and other significant centres, and also that the Social Sciences section of the Foundation

was (in the light of its own judgements, supported by feedback from Allen and others) less open than the Memorial had been to the idea that there should be funding of such things.

Beveridge was dogged in his support of the Department of Social Biology, and tried to do everything he could to have it continue. But he was his own worst enemy.

First, as indicated earlier, it seems to me that a major problem was that his view of the program was itself intellectually incoherent. In saying this, I am not wishing to criticise the idea that there can be fruitful work across disciplines; my argument here is simply that Beveridge's approach was not the way to go.

Second, Beveridge seemed oddly unaware of what, in fact, the Department of Social Biology was doing. He was very proud, and understandably so, of the fact that Hogben was admitted to the Royal Society. But this clearly related to his work, which was narrowly biological in its character and which thus depended on access to facilities which the LSE could not continue to provide once Rockefeller support dried up. But, in fact, this was only a small proportion of what was going on in the Department. One of Hogben's key activities was an extended critique of eugenics. His department became perhaps the key centre for the development of demography. While Hogben was not himself working in demography,¹⁵ it is striking that Grebenik's article on David Glass — an important figure in the development of the academic study of demography in Britain — emphasises the significance of Hogben's influence upon him, and credits Hogben with encouraging him to work on population problems.¹⁶ In addition, the distinguished demographer R. R. Kuczynski joined the department; and the work that was done in the department was in line with Hogben's predilection for empirical work — thus contrasting with the older, more theoretically-oriented approaches to demography associated with Malthus and Herbert Spencer.

It was the work on demography which, on the face of it, should have been the selling-point for the department; yet there is no sign that, until it was too late, Beveridge had any idea of what was going on, and of its importance. A problem, however, was that it is not clear that this work — important though it was — could have really been made the basis for supporting a Department of Social Biology as it had developed (that is, including Hogben's laboratory and activities in biological research). Hogben would seem to have been important as head of department, and initially to have played an important role intellectually.

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15 Although note the reference to Hogben's 'Introduction: Prolegomena to Political Arithmetic', in Borrie.

16 'The development of his thinking was greatly influenced by his association with Lancelot Hogben, who was professor of social biology at LSE, and his collaborators. From them he acquired an interest in the relationship between social and biological problems and a conviction, which was to last throughout his life, of the importance of quantitative research in the social sciences. Hogben encouraged him to work on population problems and his work turned increasingly in that direction.'

But clearly, he was not himself involved in demographic research, and obviously required a biological laboratory for his own activities. Once his own application for continued funding was turned down, he resigned and took up a position in Aberdeen. However, in due course, when David Glass was running his own department at the LSE, concerned with demographics and social research in the early 1950s, it was able to attract Rockefeller Foundation support.

The twilight of the gods

Beveridge was not, however, someone who would readily take no for an answer, and did what he could to retain the department.

It was clear to the officers of the Foundation from 1934 that things were not going well for Beveridge at the LSE; and Kittredge, a Foundation officer, reported on 'a general feeling that he has not a sufficient personal command of the situation to permit him always to make the wisest decisions'.¹⁷ Beveridge was also starting to look for alternative employment opportunities. He was, however, strongly supportive of the Department of Social Biology, which Kittredge noted was planning to concentrate on issues of 'genetic psychology and on the qualitative aspects of population problems'.¹⁸ Kittredge added, 'I think there is a case to be made for maintaining the Department of Social Biology in the School', but he noted that 'Hogben's salary and part of the expenses of his department are paid at present from [a] seven-year Foundation grant' which would need to be renewed. To this, however, Miss Walker, an RF officer, responded that 'ultimately the question of future support should come before the NS [natural sciences] rather than the SS [social sciences], since the program is rather definitely classified under experimental biology'.¹⁹ On 13 December 1934, Noel Frederick Hall, at University College, offered an overview of various issues relating to the LSE, which said, *inter alia*, that Hogben should go back to biology, and that he would be readily hired elsewhere.

In 1935, the Foundation scaled down its research support for the LSE, with allocations tapering off from \$15 000 in 1935 to a final \$3000 in 1939–40.²⁰ When an officer from the Foundation met with Beveridge, on 3 May 1935, Beveridge was still defending the Department of Social Biology, along the following lines (as summarised by the officer): 'The argument is, in skeleton form, that the social sciences must understand the biological organism which forms the unit of society, and that, on the other hand, biology likewise needs contact with the

17 401S LSE 12 November 1934; Kittredge to Day.

18 401S LSE 27 November 1934; Kittredge to Miss Walker.

19 RF RG 1.1 Series 401S LSE, Box 71, December 1934; Walker to Kittredge.

20 RF RG 1.1 Series 401S Box 71, LSE 1935; Thompson to Beveridge, 24 April 1935.

social sciences to help avoid the “errors involving arguing from mice to men.”²¹ On 8 May, Day, from the Foundation, wrote to Beveridge, indicating that the ‘Trustees decided to terminate as soon as practicable the general program in the social sciences initiated by the Laura Spelman Rockefeller Memorial in 1923–24 and supported since the reorganization of the Boards by the Foundation’. He went on to refer to the tapering grant, and indicated that there was no chance of renewal of another grant that had provided research support.

On 19 June, Alan Gregg from the Foundation wrote a memo on Hogben which indicated that in his judgement, while Hogben was a distinguished biologist, his appointment to the LSE had been something of a mistake, and that while Beveridge was enthusiastic about him, he seemed out of place at the LSE.

Problems, however, were also arising for Beveridge. On 8 November 1935, Day had a meeting in Oxford with Dr Adams, the Warden of All Souls. Adams was in charge of the preparation of the final draft of a report into the LSE which had been commissioned by the Senate of the University of London. Day indicated that the report would be highly critical of the organisation of the School and, in effect, of how Beveridge was running it. He confirmed that ‘[Beveridge] would be invited into some other position’ which would enable him to concentrate upon his current interests. Day added that: ‘Dr Adams feels strongly that such activities as the Department of Social Biology should be eliminated from the School’s program...’.

Beveridge, however, had not given up. In November 1936, Kittredge wrote to a couple of officers at the Foundation:

The report of the inspection by the London Senate seems to have been unexpectedly favourable to the School. I learned later at Oxford that [Beveridge] had spent several days with Dr Adams when the latter was preparing the final draft. This perhaps accounts for the disappearance from the report of the recommendation concerning the elimination of the Department of Social Biology...²²

During 1936, Hogben submitted a proposal to the Foundation specifically on the topic of population; but, in a letter to Mrs Mair, Kittredge indicated that it was difficult to see how they could fund it under the new rules, and also that funding it would involve their continuing to fund the Department of Social Biology — which they felt unhappy about funding under the Social Sciences program.²³

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21 RF RG 1.1 Series 401S Box 71, LSE 1935, WW Diary, 3 May 1935. It is worth noting that whatever the merits of this defence might have been, they do not relate to the work in demographics which, by this point, was playing a key role in the Department of Social Biology.

22 RF RG 1.1 Series 401 S Box 71 London School of Economics 1936; TBK to SHW-JVS, 18 November 1936.

23 RF RG 1.1 Series 401 S Box 71 London School of Economics 1936; Kittredge to Mrs Mair, 17 March 1936.

Beveridge *still* did not give up, and Kittredge later reported to Miss Walker at the Foundation that at a meeting on 24 April Beveridge and Mrs Mair had impressed on him the importance of Hogben, the Department, and its work on population. Kittredge suggested that while Beveridge and Mrs Mair seemed obsessed with the future of the Department, he had communicated what had now become the Foundation's standard line: that Hogben's work would have been more appropriate in a faculty of biology, while under the new program for the Foundation there was no possibility of funding of new activities outside the re-defined areas of concentration.²⁴

This, really, was the end of the line for Beveridge and the Department of Social Biology. While Beveridge continued to press the issue at every opportunity, Hogben resigned to take up a new position. While Beveridge did now start to stress demography, it was connected up with what the Foundation came to see as his obsession with the Department of Social Biology, which they were unwilling to fund.

During 1937, issues became even more difficult for Beveridge. It had become clear to the Foundation that Beveridge had been telling the staff at the LSE that social biology needed to be supported because of the Foundation's interest in it. The Foundation put the word about informally among the senior LSE staff that it would not support research work LSE 'as long as this state of affairs [that is, Beveridge's support of social biology] continued'.²⁵ Hogben left to take up his position in Aberdeen, and a Foundation officer reported that Beveridge and Mrs Mair had now finally understood the views of the officers of the Foundation and had expressed regret that 'the more enlightened view expressed fifteen years ago by Beardsley Ruml had not prevailed'.²⁶ It was the expectation of the Foundation officer that, if Social Biology continued, it would be only in name, and that all that would continue would be work on 'demographic considerations and... the social, economic and political consequences of population changes'.

Beveridge resigned in 1937, still pressing the merits of Social Biology upon his successor, Carr-Saunders (who himself worked on population issues). But in an interview with an officer of the Foundation, Carr-Saunders indicated that he did not think that Hogben's biological work should be continued.²⁷ On 16 November 1937, Kittredge of the Foundation wrote a memorandum on a meeting with Carr-

24 RF RG 1.1 Series 401 S Box 71 London School of Economics 1936; Kittredge to Miss Walker, 18 May 1936.

25 RF RG 1.1; Series 401S; London School of Economics; Box 71, 1937. See JVS to EED, 1 December 1937, and also the material cited in the next footnote.

26 RF RG 1.1; Series 401S; London School of Economics; Box 71, 1937; excerpt from letter from TBK to JVS, 28 January 1937.

27 RF RG 1.1; Series 401S; London School of Economics; Box 71, 1937; SHW interview with Carr-Saunders, 21 June 1937. See also TBK memorandum on a conversation with Carr-Saunders, 1 July 1937; and JVS Memorandum 24 September 1937.

Saunders, in which he indicated that the Department of Social Biology had been abolished, but that a member of its staff had been appointed to a Readership in Demography — with a saving to the School of some \$2000 a year.

Concluding reflections

I would like to conclude this paper by suggesting a few lessons that might be drawn from this story; ones which in part seem to me also to speak to our current situation in Australia.

First, there is a lot to be said for the approach to the funding of research of the Laura Rockefeller Memorial under Ruml: there were huge benefits to simply giving money to the LSE and to a few other centres of excellence, and to distinguished people within them, for them to do pretty much what they wished with the funds. Clearly, there had to be financial accountability, and funding would be withdrawn if nothing worthwhile was produced. But to insist that what was to be achieved should be spelled out in detail in advance, or to demand tight accountability on an intellectual basis seemed to be counter-productive. While for the historically inclined researcher, the fact that there were detailed reports made on what people thought about the Department of Social Biology is very useful, there seems everything to be said against the idea of funding being given on the basis of an academic consensus. The reason for this is that, on such a basis, one will get the funding of work which is, indeed, good as judged on the basis of a current, and therefore backward-looking, perspective. It also seems fairly clear that the increased pressure for detailed management of the funding, and especially the turn towards more practical and policy-driven concerns, was a disaster. The decline in the intellectual interest of the applications to the Social Sciences section of the Rockefeller Foundation, once it directed its efforts to more practical concerns, has to be seen to be believed. There are, here, surely some lessons for Australia in all this. (Think only of the character of, and procedures for, ARC discovery awards.)

Second, there is the problem of inter-disciplinarity. Here, the situation is complicated, in part because of the spread of the division of labour within academic work. As Adam Smith (1776: II9) noted:

In the progress of society, philosophy [at that time, also used to refer to what we would call science] or speculation becomes, like every other employment, the principal or sole trade and occupation of a particular class of citizens. Like every other employment too, it is subdivided into a great number of different branches, each of which affords occupation to a peculiar tribe or class of philosophers; and this subdivision of employment in philosophy, as well as in every other business, improves

dexterity, and saves time. Each individual becomes more expert in his own peculiar branch, more work is done upon the whole, and the quantity of science is considerably increased by it.

But this, obviously, points towards Kuhnian normal science. This in turn raises issues concerning whether the assumptions on the basis of which work of different kinds is being conducted are, in fact, coherent. Smith himself poses an interesting problem for us here, in the sense that, in his own view, it is clear that at some points it is necessary that someone can take an overview of what is going on, and suggest on the basis of that the kind of policy measures that are appropriate. But as specialisation increases, who will be in a position to do this? Indeed, one might wonder whether what Smith says (elsewhere) on the intellectually debilitating effects of an advanced division of labour may not apply to academic and policy issues, too!

Beveridge's own approach — not uncharacteristic of a senior administrator's perspective — points to real difficulties in trying to encourage inter-disciplinarity; namely, that he was led to call for a distinguished biologist who would, in effect, give up his own area of research and would, instead, re-tool in economics. This did not happen. Instead, they got Hogben — who not only did significant work of his own but also played an important role in the development of demographics at the LSE. (This, through the influence of David Glass, had a wider influence, too, on the development of British empirical sociology.) Perhaps, if inter-disciplinarity is wanted, the way to go is to encourage people from within a discipline to develop knowledge of other fields, such that they can contribute within their own discipline and also collaborate with other people in theirs. Again, this would suggest the wisdom of giving resources to senior and accomplished people who wish to try out something new, rather than trying to set the agenda for them from above, or in effect promising them funding if they will do once more the kinds of things that they have done successfully in the past.

Finally, there is the interesting issue over methodological issues raised by the controversies between Beveridge and Hogben, on the one hand, and the LSE economists on the other.²⁸ Here, Beveridge seems to have made little impact. But even those who disagreed with Hogben recognised his personal intellectual worth.²⁹ Hogben would, I think, have learned something from the LSE economists if he had stopped to try to understand their arguments — for example, relating to the problems of economic calculation under socialism (see Hayek 1997). But he was able to bring something interesting to the table: demography. There was

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28 I have discussed these in my 'The Battle of Houghton Street'.

29 Hayek expresses such a view in comments from an unpublished interview with Bartley, and Malinowski is reported as having expressed a similar point, in material in the Foundation papers.

certainly an interesting issue as to how questions raised by demography are to be integrated with lessons from economics — in the development of social policy, and also with individual freedom, for example. But that is clearly an issue for another occasion.

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