Observation and Detachment:
William Beveridge and "The Natural Bases of Social Science"

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In June 1937 the social reformer and economist William Beveridge (1879-1963) delivered his farewell address as director of the London School of Economics and Political Science. During his 18-year tenure the LSE had been transformed from a relatively small university into one of the world's leading social science institutions, home to the likes of Lionel Robbins, Friedrich Hayek, and Bronislaw Malinowski. Beveridge, however, was immensely disappointed with what he had achieved during his directorship. In particular, he was frustrated with his failure to change the way social science was practiced at LSE. To illustrate this point, Beveridge asked his audience to consider what an outsider might make of economics in its current state. According to Beveridge, the outsider would be forced to conclude that economics was not a science but "a survival of medieval logic" (Beveridge 1937: 466). Nevertheless, Beveridge explained, there were two keys that economists could use to unlock the door to a better science: "observation and detachment" (Beveridge 1937: 479).

Beveridge's appeal to observation and detachment was quite familiar to his audience. Indeed, he had made them the heart of a program called "The Natural Bases of Social Science" (hereafter TNBSS), which had helped make the second half of his directorship immensely controversial. Featuring a department of social biology led by one of the UK's leading geneticists, TNBSS had been conceived as an ambitious means of "cross-fertilising" natural and social science

and enabling economists to develop new observational practices. This paper throws light on not only what Beveridge was trying to achieve when he brought a geneticist to the LSE during the 1930s but also what he thought was at stake when the project failed.

As we will see first, TNBSS was rooted in an agenda Beveridge outlined to the LSE in 1920 during his inaugural lecture. According to Beveridge, social science generally, but economics specifically, had two main faults: a poor evidential basis for its laws and a tendency to be closely connected with political ideas. In his view, the route out of these problems involved closer relations between natural and social science research, which he endeavored to facilitate in TNBSS by bringing to the LSE Lancelot Hogben, whose population genetics research was meant to set a methodological example to social scientists and suggest ways they might broaden their work. However, as the third section shows, prominent members of the LSE faculty, most notably Friedrich Hayek, were skeptical of Beveridge's aims and turned against him.

In tracing this history, a number of interrelated points will be discussed. The first can be grouped under a single heading: "the politics of observation." Beveridge saw the reform of observational practices as a means of taking the ideological heat out of economic arguments. However, in the midst of economic problems of the 1920s and then the depression of the 1930s, that aim was not straightforward. Although Beveridge certainly believed he was advancing a methodology of observational detachment, his critics were not so sure. This paper therefore explores how methods of observation can and do have political meanings, both within disciplines and the wider world, and, in so doing, draws

on perspectives that have recently been developed with respect to social sciences other than economics (Porter 2011; Savage 2010).

Following on from these issues is a second and related discussion about observation that is framed in counterfactual terms. As we will see, the debate at the LSE echoed other episodes from the history of economics, in particular the *Methodonstreit*. However, TNBSS was not simply the *Methodonstreit* played out in an English context. Paying particular attention to his focus on the relationship between economics and biology, the conclusion therefore explores what Beveridge hoped the department of social biology would achieve at the LSE—a discussion that will draw attention to questions about the role of experiment in economic science. Nevertheless, for us to see these points, we must first return to the beginning of Beveridge's tenure at the LSE and his ideas about reforming social science.

1. The Origins and Establishment of "The Natural Bases of Social Science"

When Sidney Webb recruited Beveridge to replace the politician William Pember Reeves as the LSE's director in 1919, it was a significant moment for all concerned. Established in 1895, the LSE's Fabian founders wanted to challenge the UK's ancient universities and, in particular, what they saw as their failure to be rigorously scientific about society and its problems (Dahrendorf 1995). Consequently, the LSE had quickly become the UK's leading social science institution, with the country's first chair of sociology, which was awarded to L. T. Hobhouse in 1907, and a developing faculty of economics that was seen by many

as an alternative to Alfred Marshall's school at Cambridge (Koot 1982). By the late 1910s, however, the LSE desperately needed to expand if it was to fulfill its founding aims.

Beveridge's decision to take up this challenge was motivated by his own ambitions. After an impressive undergraduate career at Oxford during the 1890s, he had become a sub-warden at Toynbee Hall, a university settlement in the east end of London, where he had witnessed the problems of modern industrial society. From that point onwards, Beveridge focused on economic and social reform, in particular unemployment, about which he wrote one of the standard works: *Unemployment: A Problem of Industry* (1909). Moreover, Beveridge held a variety of civil service posts, including roles in the development of labor exchanges, social insurance and wartime food policy. However, after World War One, he resolved to spend more time on research, in particular an historical study of prices and wages that slowly became his obsession (Harris 1997: 260; Beveridge 1939). Thus, when Webb promised that directors of the LSE had plenty of time for research, Beveridge took up the offer.

As he made clear in October 1920 in his inaugural lecture, which was published as the first article in the LSE's new journal, *Economica*, while Beveridge saw the directorship as an opportunity to pursue his own research projects, he also believed it was a chance to promote a broader social science agenda. Specifically, Beveridge wanted to use his position at the LSE to address what he had identified as a major obstacle confronting social scientists: earning the same respect as natural and physical scientists. Although this problem had many aspects, Beveridge believed the main challenge was methodological. Social scientists tended to overlook the importance of a rigorous approach to their

subject matter, he argued, and this undermined their aspirations to impartiality, reliability, and, most importantly, scientific authority.

To tackle this problem, Beveridge believed social scientists should look to the biological sciences, which had overcome similar challenges during the mid-nineteenth-century debates about evolution, and, in particular, the writings of "Darwin's Bulldog," T. H. Huxley. Drawing on an 1854 essay, entitled "The Educational Value of the Natural Sciences," Beveridge set out for his audience the four principles by which Huxley had suggested science could be distinguished from other enterprises:

> "1. Observation of facts, and experiment, which is only a special form of observation.
> 2. Comparison and classification of the facts observed, leading by induction to general propositions.
> 3. Deduction from general propositions to facts again, so as to foretell these latter in advance of observation.
> 4. Verification of deductions by fresh observations." (Beveridge 1921: 4)

While Huxley had insisted that each of these principles be present for an investigation to be classed as scientific, he also suggested the balance between them need not always be the same. The reason was the complexity of biological and social science phenomena, which made it difficult for their practitioners to copy everything mathematicians and physicists did. Nevertheless, Huxley had argued, by sticking to these four principles as closely as possible, natural and social scientists could be sure they were on the right track.

Addressing his new colleagues in the faculty of economics, Beveridge elaborated on what he thought was particularly relevant about this final point.

> "There was a time when the natural sciences had to be freed from the purely deductive method. The chief discredit of political economy in the past has been due to premature deduction on too narrow a basis of fact and induction. Thus we had the 'Malthusian principle of population', which is not based on any accumulation of facts at all. We have the assumption that higher wages necessarily mean a higher cost of production. We had the 'Wages Fund Theory' and the argument that it was mathematically impossible for Trade Unions to raise the wages of the working classes as a whole. All these are exploded doctrines based on insufficient previous investigation of the facts. Now economists, if they are to get full recognition for their science, must treat it as a science based like others on observation of facts, never forgetting that their ultimate aim is the making of general propositions, not the collection of facts, but content if necessary to postpone for a while the attainment of that aim." (Beveridge 1921: 8)

Consequently, when Beveridge began to oversee a significant expansion of the LSE two years later, he endeavored to put the observation of facts at its heart. As Martin Bulmer and others have documented, that expansion, which was facilitated by grants of around £450,000 from the Laura Spelman Rockefeller Memorial (hereafter LSRM) and the Rockefeller Foundation, helped create the LSE we know today by doubling the size of its premises and providing for research and teaching throughout the 1920s and 30s (Bulmer 1982, 1984;

Bulmer and Bulmer 1983; Fisher 1980, 1983, 1984; Ahmad 1987).[1] For instance, while Rockefeller money paid for large-scale projects such as A. L. Bowley's *New Survey of London Life and Labour* (1930-1935), it also provided for new departments, such as International Relations, and new chairs, including one in political economy, which was awarded to Allyn Young in 1927. Furthermore, Rockefeller money paid for teaching relief that enabled the publication of landmark books including R. H. Tawney's *Religion and the Rise of Capitalism* (1926), Robbins' *An Essay on the Nature and Significance of Economic Science* (1931), and Lilian Knowles' *Tudor Economic Documents* (1924).

While these developments were set in motion, however, Beveridge was busy negotiating behind the scenes with Beardsley Ruml, director of the LSRM, for money to fund a project called TNBSS. Inspired by the message he had outlined in his inaugural lecture, TNBSS was intended, as it was put in a memorandum to the LSRM in July 1925, to "complete the circle of the social sciences" at the LSE and, in so doing, develop social science both methodologically and intellectually. To do this, Beveridge wanted to build on the "existing departments of the London School of Economics and Political Science," which corresponded "roughly to the two parts of its title," by creating a

> "third group of studies… dealing with the natural bases of economics and politics, with the human material and with its physical environment, and forming a bridge between the natural and social sciences. On the side of human material there should be included here such subjects as Anthropology, 'Social Biology' (genetics, population, vital statistics,

[1] The Laura Spelman Rockefeller Memorial was a philanthropic foundation dedicated to social welfare. Initially independent, the LSRM was merged with the Rockefeller Foundation in the late 1920s.

> heredity, eugenics and dysgenics), Physiology so far as it bears on problems of fatigue and nutrition, Economic Psychology, and Public Health. On the side of physical environment come Geography in its widest sense as a study of natural resources, Agriculture and Meterology… [T]he addition of the missing third to the existing third of the existing structure of the School… would be perhaps the most important step that could be taken now for the development of the social sciences. Apart from its direct value in advancing the borders of human knowledge, it would confer a great indirect benefit by bringing the natural and social sciences into contact and importing the methods of the former into the latter." (Beveridge 1953: 382-383)

Trained in experimental psychology and sharing Beveridge's taste for empirical science, Ruml fast tracked this proposal and by January the following year he was able to report that the LSRM had approved payments totaling $150,000 to support it (Craver 1986).[2] However, despite some immediate benefits for anthropology, which received money to support Bronislaw Malinowski and his students, overall progress was slow.

In one particular case, social biology, the reason for the delay was Beveridge's very clear vision for the project. "What is wanted," he told the LSRM, "is to get a man of biological training to learn economics and politics and then and only then apply himself to economic and social problems. Actual investigation would be mainly statistical" (Beveridge 1953: 383). Consequently, Beveridge told Ruml in November 1927, "progress [was] necessarily slow" because the success of social biology was dependent on "finding just the right

[2] Ruml to Beveridge, 7th January 1926, Laura Spelman Rockefeller Memorial, Series 3, Box 55, Folder 593, Rockefeller Archive Centre, New York.

people."[3] Indeed, such was Beveridge's commitment to this principle that a social biology appointment was not made for another three years. Yet in the biologist Lancelot Hogben, who took the chair of social biology in 1930, Beveridge found not only a kindred spirit when it came to scientific methodology but also someone who helped push his project further than he had originally envisaged.

2. Lancelot Hogben and the Department of Social Biology

The LSE's offer of the chair of social biology came at an opportune moment for Lancelot Hogben (1895-1975). After graduating from Cambridge in 1915, he had moved from one university job to another, including posts in London, Edinburgh, and Montreal. In 1929, however, Hogben was Professor of Zoology at the University of Cape Town. Having been raised a Methodist, gone on to embrace socialism during his teenage years, and been imprisoned as a conscientious objector during World War One, Hogben was deeply uncomfortable with the acceleration of racial segregation in South Africa during the late 1920s (Hogben 1998: ch. 12). He therefore welcomed the opportunity to leave South Africa and continue his research in the UK.

From Beveridge's perspective, Hogben was an attractive candidate for the chair of social biology for a number of reasons. Most important was Hogben's place in the intellectual ecology of biology—a field that embraced a range of often-diverse methodological practices (Bowler 2009: chs 7 & 9). Along with

[3] Beveridge to Ruml, 2nd November 1927, LSRM, Series 3, Box 55, Folder 593, RAC.

Julian Huxley, J. S. Haldane and F. A. E. Crew, under whom he worked at Edinburgh, Hogben was a key member of the group that established the Society for Experimental Biology and the *British Journal for Experimental Biology* during the early 1920s (Erlingsson 2005).[4] Frustrated by the main British forums for biological science, in particular the Royal Society, Hogben's group had established these new institutions to support and promote the kinds of experimental practices they believed were essential to not only answer the most pressing questions about genetics and evolutionary development but also put biology on the same footing as the physical sciences (Mazumdar 1992: ch. 4; Bud 1993: chs 3 & 4).

However, what also signaled Hogben's suitability for the chair of social biology was his critical interest in relating biology and society. While he was not unsympathetic towards "*Eugenics* as defined in general terms by [Francis] Galton," Hogben did think "the general tendency of eugenic propaganda ha[d] been to exaggerate, and grossly exaggerate, the applicability of genetic principles to the analysis of human society" (Hogben 1930: 193-194). On the one hand, Hogben believed the problem stemmed largely from defective methodologies, in particular the simplistic pedigrees used by many eugenicists, such as those associated with Britain's Eugenics Education Society (Mazumdar 1992: ch. 4). However, he also saw those methodologies as symbolizing the political bias of most eugenic research, which identified most social problems with the breeding rates of the working classes.

[4] Indeed, F. A. E. Crew claimed he had been offered the chair of social biology first but turned it down. Interview with F. A. E. Crew, Saturday May 11, 1935, Rockefeller Foundation Archives, R6, 12.1, Diaries – Warren Weaver, pp. 66-67, Rockefeller Archive Centre, New York.

As he told Beveridge soon after joining the LSE, Hogben believed it was necessary for the new department of social biology to tackle these issues:

> "What is especially needed… is a new disposition to lay the foundations of our inquiries upon a more secure basis by devising methods appropriate to the complexity of the problems, pursuing our enquiries upon a scale suitable to the magnitude of the theoretical conclusions at which we aim and checking as far as possible the relevance of any conclusion derived from the study of lower animals to the social behaviour of human beings. Eugenists and psychologists have been too content with inquiries of which the results are at best plausible and suggestive. It should be the aim of the department of Social Biology to direct its attention to the examination of data of which the interpretation involves no ambiguity."[5]

As he explained in his inaugural lecture at the LSE in October 1930, Hogben believed these problems concerned scientists of all types and political persuasions because, in the absence of an accepted method, biological ideas were used to support almost every political viewpoint. In this respect, Hogben went on,

> "my main concern [is] to emphasise that the first task of the social biologist is not to advocate the sterilisation of the unfit, but to undertake the sterilisation of the instruments of research before operating on the body politic… In many directions the social biologist must co-operate with the sociologist, the economist and the statistician, if we are to ascertain

[5] Lancelot Hogben, "Memorandum by Professor Lancelot Hogben Concerning the Development of a Science of Social Biology," Lancelot Hogben's Human Resources File, Human Resources, London School of Economics and Political Science, no date, p. 5. Although this memorandum is undated it is kept with other letters and documents from the mid-1930s and relates closely to correspondence from the period around Hogben's arrival in London.

> the significant factors which determine the growth of human populations… [S]ocial biology will not develop fruitfully if it isolates itself from the methods of experimental inquiry." (Hogben 1931a: 23)

Hogben's insistence that social biology be anchored in experimental practices helped sharpen its identity within TNBSS. Although Beveridge had initially told the LSRM that he did not envisage needing a biology laboratory at the LSE, Hogben was clear he would only move to the LSE if he could continue with his experimental work on animal biology.[6] Hogben was therefore given a laboratory, equipped with benches, chemicals, specialist equipment such as spectrographs and centrifuges, a laboratory assistant and accommodation for around 1,500 animals, which was located in 19 Houghton Street, at the heart of the LSE. Moreover, Beveridge helped Hogben assemble a distinguished team of researchers to work in the three floors of teaching and research space that the department of social biology occupied. Though many people passed through the department, the core of the team were demographers, including Hogben's wife, Enid Charles, René Kuczynski, who joined the LSE as a refugee from Nazi Germany in 1933, and David Glass, who was given his first postgraduate job by Beveridge in 1931.

The work these researchers carried out during the early 1930s was mainly of two different types. On the one hand, there were statistical investigations. For example, in one of the department's first studies, Louis Herrmann, working under Hogben's supervision, sent questionnaires to over 4,000 twins of school age in the London area. The returns were then analyzed

[6] Hogben to Arnold Plant, 14th November 1929, and Hogben to Beveridge, 18th January 1930, Lancelot Hogben's Human Resources File, Human Resources, LSE.

with the aim of understanding more about the relationship between heredity, environment, and intelligence, as well as the value of psychological testing (Herrman and Hogben 1933). On the other hand, Hogben and his postgraduate students, many of whom came to London from Cape Town, continued with his experimental biology. The initial focus for this work was "The Physiology of Reproduction": a study by Hogben and Charles of the ovarian cycles of animals, in particular rabbits and *Xenopus*, a South African toad, which led to a new pregnancy test (Hogben 1946). Furthermore, and building on recent advances in German mathematical genetics, Hogben focused on the interaction of nature and nurture in evolutionary development: issues that were at the heart of two books he published soon after taking the chair of social biology (Hogben 1931b; Hogben 1933; Tabery 2008).

By the mid-1930s, the LSE therefore had a presence across a range of different fields, with journals such as *Annals of Eugenics*, *Biochemical Journal*, *British Journal of Experimental Biology*, *American Naturalist*, and the *Proceedings* of the Royal Societies of Edinburgh and London publishing research by the department of social biology. Beveridge's intention, however, had been for the department to "cross-fertilise" natural and social science. In this respect, the department gravitated towards a major set of issues: differential fertility and population decline, social mobility and educational opportunity, and intelligence testing. Indeed, the department carried out so much work on these issues that it was able to publish a 500-page edited collection, entitled *Political Arithmetic* in an effort to locate the book in the tradition of William Petty (Hogben 1938b).

Although the department used statistics to investigate these topics, in particular fertility and marriage rates across different classes, Hogben made

clear in his introduction to *Political Arithmetic* that experimental approaches could and should be utilized in future. For instance, he suggested it would be possible to select "random groups of parents with large and small families in the same occupational groups and [apply] an arbitrary method of scoring to the social circumstances of their lives" in order to better understand the incentives and disincentives behind human fertility (Hogben 1938a: 35). Indeed, given that eugenicists built arguments out of such subject matter, Hogben believed it was essential his experimental work in genetics was used to build a comprehensive picture of human behavior in these areas. And it was exactly this kind of thinking that Beveridge hoped the department of social biology would encourage among the LSE's social scientists, specifically the economists, whom he had suggested in his inaugural lecture rushed to conclusions without sufficient evidence to support them. However, as we will now see, far from helping achieve Beveridge's aim of neutralizing social science arguments, Hogben's department actually brought to light the political dynamics of debates about observation.

3. "It takes more than two empirical swallows to make a social science summer": The decline and fall of the department of social biology.

By the mid-1930s the department of social biology and its associate members were developing and publishing their work at an impressive rate. However, TNBSS soon began to break down. As one Rockefeller Foundation officer explained in late 1934, while Beveridge believed he could "still make a success of the Social Biology experiment," it was "now widely recognised in most quarters

as a failure. Hogben himself is not happy in his work, and both biologists and social scientists are now sceptical of the whole lay out of Social Biology at the School."[7] For this reason, the Rockefeller Foundation's social science officers started to consider whether they should continue funding the project, especially as a separate molecular biology division had recently been established (Kohler 1976).

Aside from a growing sense of personal resentment towards Beveridge's style of administration (Harris 1997; Dahrendorf 1995), what helped make TNBSS an issue were changes in the intellectual ecology of economics at the LSE. With retirements, such as Edwin Cannan's in 1926, and, in the case of Allyn Young three years later, deaths, Lionel Robbins had been promoted to a university chair in economics in 1929. Unlike most other economists at the LSE, as well as in Britain more generally, Robbins was widely read in European economic literature and this reading had brought Friedrich Hayek's work on the business cycle to his attention. At a moment when he was entering his famous and heated controversy with Keynes over UK government policy during the Great Depression, Robbins recognized Hayek was a fellow traveler in terms of both policy prescriptions and theoretical leanings (Howson and Winch 1977). Via an invitation to deliver a series of lectures in 1931 on the work that would subsequently appear as *Prices and Production*, Robbins therefore helped engineer a situation in which Hayek was offered the LSE's Tooke chair in economics in 1932 (Caldwell 2004: ch. 8; Robbins 1971: 127). As a consequence, the LSE was established as an anti-protectionist alternative to Keynes and

[7] N. F. Hall to Tracy B. Kittredge, 13th December 1934, Rockefeller Foundation Archives, R6, 1.1 Projects, 401 S, Box 71, Folder 941, RAC.

Cambridge—a situation that represented a complete switch from earlier in the century (Koot 1982).

However, and notwithstanding areas of agreement with Beveridge, particularly with respect to tariff policy, Hayek's arrival also marked an escalation of the debate about TNBSS (Beveridge 1931). The reason was Hayek's objections to the methodological principles motivating the project. On the one hand, for Hayek, an economist educated in the Austrian tradition, Beveridge's arguments for an empirical turn in economics would have reminded him of the late nineteenth-century *Methodonstreit,* which still cast a shadow in both Europe and North America (Caldwell 2004: ch. 9, 2006; Shearmur 2010). On the other hand, and largely because of Otto Neurath, effectively the social science representative of the Vienna circle of logical positivists, Hayek had come to see the *Methodonstreit* in political terms. More specifically, Hayek identified historical economics, which he saw in Neurath's application of positivism to economics in the name of a planned economy, as the methodology of socialism and state expansion.

Though Hayek (1983: 1-18) discussed these points explicitly later in his life, he addressed them in 1933 in his inaugural lecture to the LSE, entitled "The Trend of Economic Thinking," in which he made a strident case for the importance of theoretical analysis in economics. He constructed this argument by depicting economics as a science that was born as an "investigation and refutation" of "Utopian proposals," by which he meant "proposals for the improvement of undesirable effects of the existing system, based on a complete disregard of those forces which actually enabled it to work" (Hayek 1933: 123). In his view, the heart of economics was the unintended but beneficial order,

known since Hume and Smith onwards, that sprang out of masses individual actions. Economic science was therefore about interrogating suggestions that interference and compulsion could produce something better.

Theoretical analysis was key to the identity of economics, conceived in that way, Hayek explained, because it was the most effective way of seeing the interconnections and interdependencies that constituted the unintended economic and social order. In this sense, historical economics, which had been presented as an alternative to theoretical analysis since the mid-nineteenth century, was flawed because its reliance on description and analyzing events in terms of specific circumstances made it incapable of grasping how individual phenomena fit together and make a whole. As a consequence of these faults, historical economists were more susceptible to interventionist ideas, in particular socialism and planning. This, Hayek explained, was something that could be observed in the history of economic thought, during which time liberal theorists and socialistic empiricists had see-sawed back and forth in popular opinion.

> "That it is not the experience of one or two individuals is perhaps somewhat difficult to see if one looks at a single country; but it becomes fairly clear if one compares countries in different phases of development of economic thought. If one compares, for example, Germany, where the influences which led to the decline of analytical insight originated, with, say, the United States, where they have been felt only in comparatively recent times, or even with England—which, in this respect, occupies a kind of intermediate position—one cannot help noticing how far the cycle has already swung round in Germany and how completely the relative position of the intellectual radicals and popular opinion have changed. In

> Germany—and to a certain extent in England also—the people who call for a further extension of governmental control of economic life have certainly ceased to be in any way intellectual path-breakers. They are most definitely the expression of the spirit of the age, the ultimate product of the revolutionary thinking of an earlier generation. To recognise their position in this respect, of course, does nothing to decide the question whether the future belongs to them—as it well may. But it throws an interesting light on the role played by the progress of knowledge in this development." (Hayek 1933: 136)

On account of these opinions, it was therefore no surprise that Hayek found himself unable to embrace the ideas motivating TNBSS or the leadership offered by Hogben, a known socialist. Indeed, Beveridge's problems gathered pace at the LSE from the moment, in mid-1934, when Hayek learned from John Van Sickle, assistant director of the Rockefeller Foundation's social science division, that "it was improbable that the Foundation would consider support of research work [at the LSE] as long as this state of affairs [with social biology] continued."

Given that he had constantly assured skeptical staff, including Hayek, "that the work in social biology was particularly popular with the Foundation," Van Sickle's revelation forced Beveridge to defend his program.[8] As he explained in May 1935 to Warren Weaver, director of the Rockefeller Foundation's natural science division, Beveridge continued to believe the department was essential for two reasons. On the one hand, "the social sciences must understand the biological organism which forms the unit of society… [O]n the other hand, biology likewise needs the contact with the social sciences to help avoid the

[8] John Van Sickle to JMP, 19th January 1937, Rockefeller Foundation Archives, R6 Series 401 S, Record Group 1.1 Projects, Box 71, Folder 944, RAC.

'errors involved in arguing from mice to men'."[9] Moreover, as Hogben explained in a memo to the Rockefeller Foundation, the department of social biology could only achieve these aims in an expensively equipped laboratory because of not only "the necessity of laying firmly the foundations of an exact science of human inheritance" but also "the danger of divorcing a body of purely statistical enquiries from contact with experimental research."[10]

The Rockefeller Foundation, Hayek and others were unconvinced by these arguments, though. Frustrated by this situation, and convinced that it was hindering his ability to recruit postgraduate students, Hogben began looking for a new job and by late 1936 he had secured the Regius chair of natural history at the University of Aberdeen. As he put it in a letter to Beveridge shortly after leaving the LSE, Hogben went to Aberdeen with the intention of "busily (and rapidly) salvaging [his] reputation as a experimental scientist after besmirching it with six years of association with economists and such."[11] Reflecting on his time at the LSE, Hogben later recalled "few with whom [he] had much in common" but remembered two people for all of the wrong reasons: "I regarded… as a mental exercise comparable with astrology, economics as taught by Frederick von Hayek and Lionel Robbins in what an early Fellow of the Royal Society called 'the notional and disputatious manner of the ancient philosophers'" (Hogben 1998: 130). Indeed, for Hogben, the LSE's department of economics was "the last stronghold of the most ultra-individualistic

[9] Interview with Beveridge, Friday 3rd May 1935, Rockefeller Foundation Archives, R6, 12.1, Diaries – Warren Weaver, pp. 66-67, RAC.
[10] "The Fifth Year in the Work of the Department of Social Biology," Rockefeller Foundation Archives, R6, 1.1 Projects, 401 S, Box 73, Folder 959, RAC.
[11] Hogben to Beveridge, 1st November 1937, Beveridge Papers, Beveridge/5/19, LSE.

metaphysical nonsense masquerading as economic science west of Vienna" (Hogben 1998: 120).

As Hogben's comments indicate, the social biology experiment left a rather bitter taste in the mouths of everyone involved. In fact, while Hayek (1984: 111-113) continuously ridiculed Beveridge's capacity for anything other than administration, even Robbins (1972: 136-137), who enjoyed a cordial personal relationship with Beveridge, believed the episode showed how his severe limitations as an economist were a threat to the field's development at the LSE. It should therefore be no surprise that Hogben's departure for Aberdeen spelled the end of the whole social biology experiment. Indeed, armed with new powers granted during a dispute about how Beveridge ran the LSE, the Professorial Council blocked efforts to appoint a new professor of social biology. Thus, like Hogben, Beveridge sought a way out, which came when he accepted the mastership of University College Oxford, in 1937. Nevertheless, after 18 years service, Beveridge was given a final opportunity to address the LSE.

4. Conclusion

Opening his farewell lecture in June 1937, Beveridge told his audience it was "inevitable… [he] should seek to measure the results" of his directorship "against [the] hopes" he had outlined at the beginning (Beveridge 1937: 459). In so doing, his conclusion was brutal: there had been little progress towards a genuinely scientific economics. This much was demonstrated, Beveridge argued, by the

ongoing debate about John Maynard Keynes' *The General Theory of Employment, Interest and Money*:

> "Mr. Keynes sets out to challenge established economic theory in its foundations… The parallel to the challenge presented to Newtonian physics and Euclidean geometry by the new theories mainly associated with the name of Einstein is obvious and suggested by Mr Keynes himself. But the parallel does not apply either to Mr Keynes' own methods or to the nature of the debate by other economists that has followed the publication of his book… Einstein went back to facts or told others where they should go, to confirm or to reject his theory... Mr. Keynes neither starts from facts nor returns to them." (Beveridge 1937: 463-464).

As he later explained to the economist Roy Harrod in a letter about his farewell address, Beveridge saw Keynes' work and the debate about it as merely highlighting what he saw as the general problem with economic science.

> "Of course I agree that what Keynes writes arises out of his interpretation of the world as he sees it, and obviously also, to all his other gifts he adds a very penetrating power of understanding the world as he sees it; but I feel that scientific observation implies something rather more systematic than this, particularly on the side of observation… [W]e really have to try to get the resources to make something like a new start in economics, based on the combination of statistics, field work and reasoning; particularly we have to develop the technique of observation appropriate to sciences which cannot dissect or experiment."[12]

[12] Beveridge to Roy Harrod, 1st November 1937, Beveridge Papers, Beveridge/5/19, LSE.

In concluding, it is therefore important to consider what that fresh start was supposed to be; or, to put the issue another way: what did Beveridge think he was offering as an alternative to the economics represented by Keynes, which was emerging in the late 1930s?

While it is certainly important to note that Beveridge seemed to be far from clear about every aspect of TNBSS, the program's general thrust was well defined enough to enable us to make a number of points in response to this question. The first is that Beveridge's vision for reforming economic practices at the LSE placed great emphasis on natural and social scientists occupying the same physical space. Instead of simply arguing for particular practices to be adopted, he insisted on bringing a biologist to the LSE and then providing him with a laboratory. As he later explained, there were specific reasons for this:

> "Economists, political scientists and sociologists, if they are to be scientific at all, must have intimate co-operation with those engaged in the study of man as an individual, that is to say with biologists, anthropologists and psychologists. In the conditions of London, with the Colleges so widely separated, it seemed to me that co-operation could be made certain only by having both the social sciences and their natural bases in the same College. And with my views as to the kind of science that economics should be, based on observation rather than analysis of concepts, the bringing of a least one first-rate natural scientist, into the School had special attractions." (Beveridge 1953: 251)

Those special attractions were two fold. First, Beveridge believed the physical presence of a research biologist would make it more likely that the LSE's social scientists would learn about the methodological principles he was

promoting, as they would come into contact with each other more frequently, at both work and, no doubt, socially. Second, through their close proximity, social and natural scientists could share not just methodology but also the actual findings of their work, which they might not know about otherwise, given the increasing specialization of academic disciplines.

In these respects, what Beveridge seems to have envisaged at the LSE was a situation in which the department of social biology and social scientists would initially conduct their research in parallel but, importantly, within sight of each other. Then, as he outlined during the early stages of the project, he seems to have wanted these different researchers to come together in official forums dedicated to issues on which their interests overlapped, for example the questions about social structure and mobility that the department of social biology had started work on. The aim would be to not only make data of mutual interest freely available but also to facilitate useful communication about methodology and practices; that is, a discussion about how particular ideas or arguments were supported and related to one another. Long term, Beveridge saw this kind of ideal being perpetuated by the free movement of personnel across the different disciplines involved, with students, for example, training in the department of social biology and then moving to economics, and vice versa.[13] In Beveridge's view, such interaction would establish a connection between economics and experimentation and, hopefully, encourage ways of developing forms of experimentation appropriate for economic phenomena. In so doing, economics could develop a new identity: one in which there was a clear

[13] See "Report of the Meeting on the Natural Bases of the Social Sciences," Minutes of the Professorial Council, 1st December 1926, LSE/Minutes/7/1/5, pp. 42-43, London School of Economics.

relationship between its theories and a wide range of different observations, which would eliminate the problem of ideological heat in social science arguments.

In closing, it is worth reflecting on this vision in light of a phrase used at the start of the paper: "the politics of observation." In a rather direct way, this phrase sums up two crucial aspects of the debate about TNBSS that speak to often hidden dimensions of the history of observation in economics. The first is that debates about methods, in this case observational practices, can and often do align with political beliefs and programs. As Hayek's identification of TNBSS with socialism showed, methodologies can be seen to lead inexorably to particular political conclusions. The second, and in some ways related, issue is that "the politics of observation" also has meanings within disciplines. Arguments for one method of observation involves conflict between practitioners and, aside from their intellectual meanings and significance, those conflicts also have very real material consequences in terms of the allocation of resources. In the case of TNBSS, this point was demonstrated by the revelation that Hayek, Robbins, and others would not be able to apply for Rockefeller Foundation funding until the argument with Beveridge had been resolved. Combined with the apparently ideological ends of Beveridge's plans, the debate at the LSE during the 1930s therefore shows how observation is an act with political significance, even when the aim is detachment.

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