
by

JAMES TABERY¹,* AND SAHOTRA SARKAR²

¹Department of Philosophy, University of Utah, 215 South Central Campus Drive, Salt Lake City, UT 84112, USA
²Department of Philosophy and Department of Integrative Biology, University of Texas at Austin, 2210 Speedway, No. C3500 (WAG 316), Austin, TX 78712, USA

From 1930 to 1937 Lancelot Hogben FRS occupied the Chair of Social Biology at the London School of Economics and Political Science. According to standard histories of this appointment, he and R. A. Fisher FRS both applied for the position, but Hogben was selected over Fisher. The episode has received attention in large part because of the later prominence of the two figures involved. The surviving archival records, however, tell a remarkably different story. Neither Fisher nor Hogben was ever an official candidate for the chair. Indeed, Fisher seems not to have applied for the position at all, and Hogben was approached only behind the scenes of the official search. The purpose of this paper is to correct and complete the history of this episode.

Keywords: Eugenics; Ronald Aylmer Fisher; Lancelot Thomas Hogben; London School of Economics and Political Science; social biology

THE PROBLEM

From 1930 to 1937 Lancelot Hogben FRS occupied the Chair of Social Biology at the London School of Economics and Political Science (LSE). According to histories of this appointment, he and R. A. Fisher FRS both applied for the position, but Hogben was selected over Fisher. For instance, in her biography of Fisher, his daughter, Joan Fisher Box, wrote:

In 1929, while [Fisher] was fighting for better census and registration forms, he was attracted by the advertisement of a research professorship in social biology at the London School of Economics. The opportunity to direct his own research was obviously appealing, and he wrote to Major [Leonard] Darwin about it, wondering what assistance the professor would have. For his research program, he stated, he would want a geneticist and a research psychologist to work with him. Darwin protested he should ‘first catch his hare before fattening it,’ and used Charles Darwin to exemplify what a man can do without

*Author for correspondence (james.tabery@utah.edu).
institutional support. He wanted Fisher to get all his mathematical contributions out of his head before becoming entangled with directing research. Fisher, in contrast, felt the limitation to the amount of theorizing any man can do and the great need for well-conducted experiments. He applied for the professorship, to which L. Hogben was appointed.\(^1\)

This version of the story has since been repeated by several scholars who have discussed the appointment of the chair, including Sahotra Sarkar,\(^2\) Anthony W. F. Edwards\(^3\) and James Tabery.\(^4\) The episode has in large part received attention because of the later prominence of the two individuals involved. By 1929 Fisher and Hogben were both already rising stars in the community of British scientists—Fisher because of his contributions to statistics and evolutionary genetics, and Hogben because of his contributions to experimental biology and physiology. The two also shared strong personalities and were known for their predilection for cantankerous interactions. Most importantly they differed profoundly and publicly about eugenics. Fisher was a leading voice in the British eugenics movement at the time, and Hogben was one of its most forceful critics. They were thus among each other’s most critical disputants. What made the appointment of the chair at the LSE of historical interest, therefore, was its creation of a moment in time when the two men apparently competed for a position in social biology, the first appointment of such a kind anywhere (to the best of our knowledge), that only one of them could win. And, so the legend goes, Hogben won out over Fisher.

The surviving archival records, however, tell a remarkably different story. Neither Fisher nor Hogben was ever an official candidate for the chair. Indeed, Fisher seems not to have applied for the position at all, and Hogben was approached only behind the scenes of the official search. This short paper corrects and completes the history of this episode.

**THE CHAIR**

The idea for a Chair of Social Biology at the LSE came from its then Director, Sir (later Lord) William Beveridge.\(^5\) Beveridge, when he arrived at the LSE in 1919, sought to raise the credibility of the school by improving the legitimacy of the social sciences. He saw a clear ordering of the sciences, from the physical, through the life, to the social, based in large part on how rigorously they employed what he took to be the scientific method, and so Beveridge believed that the social sciences practised at the LSE would benefit from exposure to the detached observation of facts and unbiased experimentation found in a field such as biology.\(^6\) Beveridge saw an opportunity to make his vision a reality when, in 1925, personnel from the Laura Spelman Rockefeller Memorial expressed interest in funding novel approaches to scientific investigation. In reply, Beveridge proposed a programme in ‘the natural bases of the social sciences’, which consisted in ‘the study of the borderland between various natural sciences . . . on the one hand, and the social sciences . . . on the other hand’.\(^7\)

The key to making the natural bases of the social sciences a success was, as Beveridge saw it, selecting the ideal person to oversee the borderland. The perfect candidate would bring expertise, methodologies and a strong reputation from biology, and would apply those elements to problems of social importance. The Chair of Social Biology was officially established in October 1929, and a Board of Advisors was set up to oversee the appointment.\(^8\) In the next month, on 21 November, the Deputy Academic Registrar at the
LSE, W. S. Angus, publically announced the chair and invited applications. ‘It is desired that the Chair shall be held by a person of high qualifications as a biologist, who is prepared to devote himself principally to the study of these subjects in co-operation with the teachers of the Social Sciences.’ Interested individuals were told to submit to the registrar by 23 January 1930 an application, as well as testimonials and a list of up to three names of references who would speak on the candidates’ behalf. As a foreshadowing of things to come, the call for applications concluded by noting, ‘Under the University Regulations the Board of Advisors are not limited in their choice to the persons who have applied to the post.’

THE SEARCH

A week after the call for applications went out, Fisher wrote to Darwin. At the time, Fisher was a statistician for the Rothamsted Agricultural Research Station, and so the chair offered Fisher an opportunity to enter more traditional academic life. ‘Would you advise me putting in for it?’ he asked. ‘They speak of testimonials and references. Would it do to send in a copy of my Royal Society testimonial from last year, and refer to my chief here, Sir John Russell, and perhaps to you for personal repute?’ If I put in I should like to gain some support towards the extravagant claim to “high qualifications as a biologist”!’ Darwin wrote back the next day with encouragement: ‘I am certainly of opinion that you should have a shot at the enclosed. I see no argument against it.’ But then he also issued a word of caution, continuing, ‘You must not mind failure. They are, I think, a cranky body, and one cannot guess what line they will take. Certainly refer to me if you like, as I shall be very glad to help.’

Several days later Fisher brought Darwin up to date, saying, ‘I decided therefore to send the enclosed to Sir William, and his answer which I have not yet received, ought to make it clear, what “High qualifications” are intended to mean.’ He then began considering whom he might surround himself with if he was hired. ‘I suppose I ought to raise the question of subordinate appointees if things go any further. I should like to get a geneticist, an experimental psychologist, if they will go so far.’ Fisher even began thinking about research projects that he could undertake at the borderland of biology and the social sciences. Returning to a favourite hobby-horse, on the relation between fertility and income and other societal measures, which could only be analysed if census forms collected much more extensive data than was empowered by the Census Act of 1921, he added:

Here is a scheme which has occurred to me. A first class re-organisation of our registration system seems far out of sight, owing to the registrars being poorly paid, perhaps insufficiently skilled, not whole time employees, etc., Suppose an organization with money to spend on research were to put up one of its employees for a vacant post as registrar, equip his office with card indices, and clerical assistance, and set him to make a proper study of one registration district. Could any busybody make trouble about it? The advantage would be to provide an ideal framework for further bio-economic enquiries. What do you think?

Darwin’s answer warned Fisher that Beveridge’s reply would probably be tempered. ‘I fear that Beveridge will only reply that he cannot in any way prejudge any decision of the Appointment Board in regard to any appointment at his College, but that there can be no harm in applying, since the more candidates the better. If he does reply in this
Darwin then went on to say that Fisher was getting a bit ahead of himself with all the talk of appointments and projects. ‘As to the staff under you, would it not be wise to catch your hare before trying to fatten it? I suppose it takes £30,000 to endow a professorship like this one. Would it not take another £30,000 to make it into the kind of department you have in mind? . . . Seriously I advise beginning slowly.’ When Fisher replied, he made the case for thinking through the issues earlier rather than later. ‘The value to me of the hare—unfattened—consists of two items’, he explained. The first was a slightly increased salary, and the second was the ability to apply his statistics explicitly to research on humans. At the same time he told Darwin that he was surrounded by a large staff at Rothamsted: two research assistants, four laboratory assistants, and four voluntary workers from Australia, Denmark, India and the USA. ‘I have to consider’, he explained, ‘whether a smaller organisation would make any useful headway in the problems proposed for the new research Professor. Do you not think this should be considered early, if not before applying for, at least before accepting such an appointment?’ Darwin now had a better sense of what Fisher was concerned about, and so he replied:

The intermediate raise in salary is less than I had understood, and I did not know that your department is as big as it is. All that makes a difference. I have been on 2 or 3 of these boards, when candidates have been interviewed. The chairman has often asked the candidate if he wants any more information about the job, and I think it appeared that they had always enquired beforehand. On this point I may have misunderstood you a little. It seems to me that it would be a wise precaution before applying or before the interview, it would not matter which, to write to the person to whom the application is to be sent to ask what is the size and nature of the personnel, if any, of the department over which you would have to preside.

According to the Fisher Archive, there is no mention again of the chair by either man for three months.

In the meantime the official search at the LSE was under way. The deadline for applications was 23 January 1930, and the board received ten applications: from William B. Brierley, Wilfried R. B. Brooks, William B. Crow, Valentine Davis, Harold B. Fantham, Walter P. Kennedy, Eldon Moore, John A. Fraser Roberts, Joseph H. Woodger and Solly Zuckerman. There was no application from Fisher, and no application from Hogben. The board met on 4 February and selected five candidates for interviews that would take place on 11 February, summoning Brierley, Kennedy, Fraser Roberts, Woodger and Zuckerman; Fantham’s application was also to be considered, but he was abroad and so unable to participate in an interview. At that time Brierley was Head of the Department of Mycology at the Rothamsted Agricultural Research Station, Kennedy was a lecturer in the Department of Physiology at the University of Edinburgh, Fraser Roberts was Head of the Biology Department of the Wool Research Association at Leeds (he was elected FRS in 1963), Woodger was then Reader in Biology at the Middlesex Hospital Medical School (now University College London Medical School), Zuckerman was Prosector or Anatomical Research Fellow at the London Zoo (he was elected FRS in 1943), and Fantham was Professor of Zoology at the University of the Witwatersrand in Johannesburg. The applicants thus came from diverse biological backgrounds, reflecting the fact that social biology had yet to be defined as a field.
Even though the board proceeded with a round of interviews, it was clear beforehand that Beveridge was not excited about any of the candidates. On 27 January, just days after the call for applications had closed, Beveridge wrote to Angus (who received all the applications and then circulated them to the board):

In reply to your letter of the 24th of January enclosing applications for the University Chair of Social Biology, I do not think that any of the candidates need be summoned for interviews; the only one whom I should propose to summon if he were in England, is Professor H. B. Fantham, whose name should be considered. I shall, however, have three or four other names of persons who have not applied to bring before the Board for consideration. 23

Beveridge sent a similar message to Vice-Chancellor Gregory Foster on 31 January, bringing him up to date on the process, and also saying who the ‘three or four other names of persons’ were that he had in mind.

As you will realise the Chair is of a somewhat special character and though it was thought well to advertise it, I have also been making enquiries as to a number of persons who might be prepared to consider an offer of the Chair if made by the Board of Advisors, but who cannot well be expected to make formal application. As a result of this, there are four names which I should like to bring before the Board of Advisors for consideration, namely those of:– Dr. D. A. E. Crew, Mr. J. B. S. Haldane, Professor Lancelot Hogben and Professor Julian Huxley, with each of whom I have discussed the Chair and the possibility of their considering an invitation to it. In naming these gentlemen, I do not imply that each of them would be prepared to accept the post, and in view of the confidential nature of my talks with them, I cannot well put more in writing about their views, but I shall be able to tell the Board of Advisors more when they meet. Meanwhile, I hope you may think fit to circulate this letter to the members of the Board so that they may have notice before they actually meet and come prepared to consider these names in addition to those of any candidates who may be summoned for interview as a result of application. Some of the Advisors may, of course, have other names of their own to suggest. 24

The official search, then, seems to have been merely a formality as far as Beveridge was concerned. And, indeed, Beveridge had spent the previous months feeling out F. A. E. Crew, Haldane, Huxley and Hogben (both in terms of their interest in the chair and in terms of the biological community’s interest in their research). According to Dahrendorf, that short list was soon reduced to just Haldane and Hogben; when Haldane dropped out of the search, Beveridge focused his energies on Hogben. 25

As early as October 1929 (that is, before the chair was even officially announced), Carl Frederick Pantin from the Zoological Laboratory at Cambridge wrote to Beveridge:

I expect you know that [Hogben] is one of the most brilliant (perhaps the most brilliant) of the younger biologists. . . . There are few with so wide a knowledge of Biology, and such a unique philosophical outlook and there are, I think, none at present time who combine these with a deep interest in sociology, except Hogben. . . . He is a rather strange person and many people do not get on well with him. Personally I like him. . . . If you chose him you would certainly get an original and first class person. I would indeed be glad to see him with you for I think he is being wasted in his present position. 26

Hogben’s ‘present position’ at that time was at the University of Cape Town in South Africa. Although he was well supported there in terms of research, he also was appalled
by the political system in place and the institutionalized racial discrimination (that eventually led to the official policy of apartheid in 1948), and he was eager to return to England. As a result of this tension between research support and personal dissatisfaction, Hogben explained to his colleague at Cape Town, Arnold Plant, that he would not apply for the chair at LSE but would take very seriously the opportunity if it was privately offered:

This is, because I am known in biological circles to have an exceptionally well equipped laboratory: for experimental zoology, I think the best in the British Empire at the moment, and angling for appointments that might seem outside my proper beat would give the impression that I am more keen about my geographical location than on progressing in my research work . . . .

Plant relayed this information to Beveridge and also gave the LSE Director his own assessment of the brilliant but irascible Hogben:

It is difficult to write, even in confidence, one’s personal impressions of Hogben because of his many-sidedness. Perhaps it will be best for me to say, firstly, that, in spite of anything that follows, my considered view and that of most of his colleagues is that whatever disturbances and annoyances he has caused us we do not regret his appointment. . . . I have been, and still am, one of his closest friends here, but there are long periods during which we avoid each other, and I can never be sure whether I love him or loathe him more. Of his ability as an experimental biologist there is no question, and many of us have learned much from him concerning the standpoint of the modern school of biologists and indeed of natural science as a whole. . . . No institution will ever be dull while he is connected to it. He has disturbed the University of Cape Town greatly, but it is nevertheless a better place and its academic reputation has been increased by his connection with it.

By January 1930, Beveridge and Hogben were corresponding directly. Hogben explained to Beveridge that he could only undertake the socially oriented research at the LSE if he could simultaneously keep his experimental work up and running. Experimental work, of course, was exactly what Beveridge wanted to bring to the LSE, and Hogben both practised and represented it well, having co-founded (along with Crew and Huxley) the British Journal of Experimental Biology and its associated Society for Experimental Biology in 1923. Beveridge was thus willing to look past Hogben’s difficult personality in exchange for the experimental approach he would add to the programme.

So on 11 February the Board of Advisors interviewed Brierley, Kennedy, Fraser Roberts, Woodger and Zuckerman (and discussed Fantham). But by that time Beveridge was already positioning Hogben as the choice by circulating to members of the board copies of his publications as well as copies of confidential correspondence speaking to his talent. As a result, on the same day that the official interviews took place, the board voted unanimously to recommend offering Hogben the chair, and the university senate voted later that month in favour of the board’s recommendation.

**CORRECTING THE LEGEND**

On 9 March, just over a week after the LSE offered Hogben the chair, Darwin wrote to Fisher to say ‘how sorry I am that you have not got the London job. No-one wrote to me, so I had no chance of speaking in your favour, which I should like to have done. My regret is only
lessened by what you told me as to its financial advantages. I hope other and better things will turn up.'

Darwin’s letter to Fisher is strange. It is strange because it implies that Darwin thought Fisher had applied for the chair and, as they had discussed, listed Darwin as a reference. Darwin has not been alone in this belief; for, as was discussed in the introduction, several historians who have told the story of this episode also thought Fisher applied for the post. This is not surprising, given that those historians were united in relying on the Fisher–Darwin correspondence to make that claim wherein Fisher did indeed express interest in the position.

Fisher’s expressed interest, however, did not translate into an actual application. On this, the archival record is clear—Fisher was not among the ten candidates who submitted applications by 23 January. What is not clear is what happened between November/December 1929, when Fisher seemed genuinely interested in the opportunity, and January, when he had clearly changed his mind. Recall that on 3 December Fisher told Darwin that he sent Beveridge ‘the enclosed’ to find out what ‘high qualifications’ meant. Unfortunately, there is no record of what Fisher enclosed for Beveridge, but we may assume from his earlier letter to Darwin that it could have been Fisher’s Royal Society testimonial from earlier that year. There is also no record (in either the Fisher Archive or the Beveridge/LSE Archive) of that letter from Fisher to Beveridge, or of any reply from Beveridge to Fisher. There seem to be at least three possibilities for what transpired between December and January. First, it is possible that Beveridge replied to Fisher’s 3 December letter in such a fashion that Fisher took Beveridge to imply that Fisher should not apply, and this letter has been lost to history. Second, it is possible that Fisher took Darwin up on his suggestion to correspond with Angus (who was receiving the applications at the LSE) to ask about the size of staff he might oversee as chair, and when Angus told Fisher that the staff would be smaller than Fisher liked, Fisher decided against submitting an application. As with the first scenario, though, there is no record of Fisher’s having written to Angus or of his having received any letter from Angus about the chair.

The third possibility, and the only one that does not require making assumptions about missing archive documents, is simply that Fisher changed his mind. It could be that, as he was weighing the benefits of a slightly increased salary and the ability to write about humans against the costs of what would probably be a smaller staff than he had at Rothamsted, he simply decided that the chair was not a good fit for him. The other historical data point that speaks in favour of this third possibility is the fact that Brierley and Fraser Roberts were two of the ten official applicants. Brierley was at Rothamsted with Fisher at the time. More personally, Fisher and Fraser Roberts were collaborators and shared an interest in genetics and eugenics. So it is possible that Fisher decided not to submit an application when either/both Brierley or/and Fraser Roberts informed Fisher of their interest in the position. There is no record of Fisher and either Brierley or Fraser Roberts discussing the chair before the deadline for applications. But in April (two months after it was announced that Hogben was hired), Fraser Roberts mentioned to Fisher that he was interviewed for the chair and then said that he thought Hogben was an excellent fit for the position. In response, Fisher wrote:

I hope Hogben will do well at the London School; his work seemed extremely promising when he left England, and he is obviously better placed in this country than overseas. On the statistical side I think your credentials were the stronger, and it is said, though I do not know with what truth that his Socialistic leanings tipped the balance.
Sir William explained his policy at the Galton dinner, but I judged that he had no conception of the difference between biometric and economic problems. So it does not seem that Fraser Roberts knew of Fisher’s early interest in the chair either. On this third possibility, then, Fisher, although initially interested in the chair, eventually decided against applying, perhaps because the smaller staff was not worth the advantages of the position, perhaps because a colleague or collaborator expressed interest in the position and Fisher wanted to give him the best chance, or perhaps for some other reason not recorded for history. (Although speculative, it is worth mentioning that, even if Fisher had applied, he probably would not have fared well in the competition: Beveridge was seeking to add an experimental biologist to the LSE, which meant that individuals such as Crew, Haldane, Huxley and Hogben were more attractive at the time than Fisher, who focused more on statistical approaches to biology.)

What remains a mystery is why Fisher held off on telling Darwin that he did not apply for the chair. There is no record of Fisher’s having corrected Darwin’s misapprehension, so it is not clear why Fisher would have let Darwin remain misinformed when, on any of the possibilities above, Fisher had reason not to proceed with an application. In any event, Darwin’s line to Fisher about hoping ‘other and better things will turn up’ was soon realized. In 1933 Fisher left Rothamsted to assume the Galton Professorship of Eugenics at University College London where he had been appointed with the full support of Haldane, who had already been appointed Professor of Genetics there.

CONCLUSION

When it comes to the appointment of the Chair of Social Biology at the LSE, there is a sense in which the legend of Hogben winning out over Fisher is true. Hogben was actively pursued by Beveridge, whereas Fisher was not. Moreover, Fisher apparently expressed a personal interest in the chair to Beveridge, but there is no evidence that Beveridge encouraged that interest. There is another sense, however, in which the legend does an unjust and untrue disservice to Fisher. Fisher, for whatever reason, never applied for the chair. So it is unfair to say that Fisher lost against Hogben in a competition that he never really entered.

ACKNOWLEDGEMENTS

Thanks are due to Cheryl Hoskin of the University of Adelaide for help with locating material at the Fisher Archive, as well as staff at the London School of Economics for assistance with the Beveridge/LSE Archive. We are also grateful to Steindór Erlingsson, Chris Renwick and an anonymous referee for reading and providing us with feedback on an earlier draft of the manuscript.

NOTES

The ‘competition’ for the Chair of Social Biology


‘Report of the Meeting on the Natural Bases of the Social Sciences’ (LSE Archive, File 101, Natural Bases of the Social Sciences, Correspondence Re). Both Dahrendorf (see note 5) and Renwick (see note 6) point out that what the ‘natural bases of the social sciences’ meant actually evolved substantially as Beveridge and other professors at the LSE wrangled both to make sense of the idea and to mould it in a fashion that best suited their particular interests.

The board consisted of Sir Gregory Foster, Dr Edwin Deller, Dr E. J. Allen FRS, Sir Peter Chalmers Mitchell FRS, Dr H. M. Vernon, Sir William Beveridge, Professor A. L. Bowley, Professor H. J. Laski, and Sidney Webb, 1st Baron Passfield.

‘University Chair of Social Biology Tenable at London School of Economics’ (LSE Archive, File 237, Chair of Social Biology, Hogben, L. T. Prof).


Fisher was elected to the Royal Society earlier that year, in May 1929.


Fisher to Darwin, 3 December 1929 (R. A. Fisher Archive, University of Adelaide Library Special Collections; http://digital.library.adelaide.edu.au/dspace/bitstream/2440/67636/285/1929-12-03.pdf). In the Fisher Archive, ‘the enclosed’ is not included with the correspondence, so we cannot be positive about what this document was. One likely possibility is that, in response to the chair advertisement, Fisher sent Beveridge his Royal Society testimonial that he mentioned in the first letter to Darwin about the chair. This missing data point is discussed below.


‘Report of Board of Advisors in Connexion with University Chair of Social Biology Tenable at London School of Economics’ (LSE Archive, File 237, Chair of Social Biology, Hogben, L. T. Prof).


23 Beveridge to Angus, 27 January 1930 (LSE Archive, File 237, Chair of Social Biology, Hogben, L. T. Prof).
24 Beveridge to Foster, 31 January 1930 (LSE Archive, File 237, Chair of Social Biology, Hogben, L. T. Prof).
28 Hogben to Plant, 14 November 1929 (LSE Archive, Professor Lancelot T. Hogben File).
29 Plant to Beveridge, 15 November 1929 (LSE Archive, Professor Lancelot T. Hogben File).
30 Hogben to Beveridge, 18 January 1930 (LSE Archive, Professor Lancelot T. Hogben File).
32 Beveridge to Bowley, 7 February 1930 (LSE Archive, File 237, Chair of Social Biology, Hogben, L. T. Prof).
35 There is an important contrast to make here between those scholars who have discussed Fisher’s interest in the position (Box, Sarkar, Edwards and Tabery) and those scholars who have not (Dahrendorf and Renwick). The former came to the episode by way of an interest in either Fisher or Hogben, and so were drawn to the personal archives of those men, where the Fisher–Darwin correspondence guided the analysis. The latter came to the episode by way of an interest in the chair at the LSE itself, and so were drawn to the LSE Archive, where the Beveridge papers (showing no sign of Fisher) guided the analysis.
36 Fraser Roberts to Fisher, 12 April 1930 (R. A. Fisher Archive, University of Adelaide Library, Special Collections).