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Institutions and innovation: experimental zoology and the creation of the *British Journal of Experimental Biology* and the Society for Experimental Biology

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Abstract. This paper throws light on the development of experimental zoology in Britain by focusing on the establishment of the *British Journal of Experimental Biology* (*BJEB*) and the Society for Experimental Biology (SEB) in 1923. The key actors in this story were Lancelot T. Hogben, Julian S. Huxley and Francis A.E. Crew, who started exploring the possibility of establishing an experimentally oriented zoological journal in 1922. In order to support the *BJEB* and further the cause of the experimental approach, Hogben, Crew, Huxley and their colleagues decided to found a society, which led to the formation of the SEB. From its inception the journal was plagued with difficulties that led to the merger of the *BJEB* and the *Biological Proceedings* of the Cambridge Philosophical Society in the autumn of 1925. Also discussed are the views that the leading proponents of experimental zoology in Britain in the 1920s expressed towards morphology and how their views further complicate the already much modified 'revolt from morphology' thesis.

'Hitherto zoology has been largely concerned with following out the implications of the theory of evolution, if not exclusively, by observational and morphological methods', observed A.V. Hill in his inaugural lecture as professor of physiology at University College London in October 1923. Hill thought that zoologists needed to reduce their overemphasis on these methodologies, for if they wanted to secure its future 'zoology must inevitably look to experimental methods'. He was adamant that this new approach to zoological research may not have a 'home in the Institute of Medical Science, but it will be physiology none the less'.[1] These remarks reflected a growing self-confidence within British zoology in the early 1920s about the importance of experimental zoology, including experimental embryology, general physiology, physiological genetics and

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1 A.V. Hill, 'The present tendencies and the future compass of physiological science', in *idem, The Ethical Dilemma of Science and Other Writings*, New York: Rockefeller Institute Press, 1960 (first published 1923), pp. 7–23, 16.

comparative physiology.[2] Experimental zoology, which focused on locating the causal factors behind the ontogeny and physiology of individual organisms, and gradually led to the introduction of the experimental techniques of physics and chemistry into zoological research, became an active research field in the 1890s and had initially mainly been developed in Germany and America. For various reasons experimental zoology did not gain a permanent footing in Britain until the 1920s, a process that was initiated in 1920 when the Marine Biological Association (MBA) of the United Kingdom built a new laboratory dedicated to experimental zoology at its Plymouth base.[3] At the time Hill delivered his lecture, the impact of the new physiological laboratory had hardly begun to be felt, but experimental zoology was certainly on the agenda in British zoological circles.

This trend was clearly revealed in the slightly defensive tone of the presidential address of James H. Ashworth to the zoology section of the British Association for the Advancement of Science (BAAS) in 1923 where he, among other things, briefly highlighted the major advances that had taken place within zoology since the last time the BAAS had assembled in Liverpool in 1896. Since then, two major advances had, according to Ashworth, occurred in zoology that were developed partially as a reaction to what was felt to be the overpowering influence of evolutionary morphology. Mendelian genetics was one of these advances, while the other was the rise of physiological approaches within zoology. The latter approach began with Wilhelm Roux and Hans Driesch's manipulations of two-cell-stage embryos of frogs and sea urchins respectively, work that was extended in the United States, in the late 1890s, by the likes of Thomas Hunt Morgan and Jacques Loeb, all of whom Ashworth discussed specifically in his address.[4] Ashworth observed that through 'rash speculations' some students of morphology had 'brought discredit on this branch of our science', but he emphasized that these times of morphological excess were 'long past'. Hence Ashworth pleaded for the 'retention of a sound basis of morphology in our zoological courses'. This was essential because those who wanted to 'specialise in experimental zoology' needed to appreciate the fact that 'morphology must be the forerunner of physiology'. Morphology, as a result, neither excluded, nor was detrimental to, 'experimental methods'.[5]

What is especially noteworthy in Ashworth's brief account of the major advances that had occurred in experimental zoology since the 1890s was that he mentioned no British

2 Prior to the rise of experimental zoology some naturalists had relied on experiments in their work, including Charles Darwin (Lynn K. Nyhart, 'Natural history and the "new" biology', in Nicholas Jardine, James A. Secord and Emma C. Spary (eds.), *Cultures of Natural History*, Cambridge: Cambridge University Press, 1996, pp. 426–443, 432–434) and Ernst Haeckel (Robert J. Richards, *The Tragic Sense of Life: Ernst Haeckel and the Struggle over Evolutionary Thought*, Chicago: Chicago University Press, 2008, pp. 185–188).

3 Steindór J. Erlingsson, 'The Plymouth laboratory and the institutionalisation of experimental zoology in Britain in the 1920s', *Journal of the History of Biology* (2009) 42, pp. 151–183.

4 J.H. Ashworth, 'Modern zoology', *Science* (1923) 58, pp. 231–236, 233. Robert J. Richards argues that Haeckel's experiments on siphonophores, which he performed in the late 1860s, 'prefigured the comparable work of two of his students, Wilhelm Roux … and Hans Driech … some twenty years later'. Richards, op. cit. (2), p. 189.

5 Ashworth, op. cit. (4), pp. 231–232.

zoologist.[6] This fact underscores the claim that Lancelot Hogben made in 1966, when he observed that Ernst Haeckel's *Darwinismus* had exerted such an influence on zoological curricula in English universities that 'by 1910 phylogenetic speculation wedded to descriptive morphology here dominated zoological enquiry'.[7] This was in stark contrast to the fact that British botanists had since the 1870s cultivated the physiological tradition.[8] Arthur G. Tansley and four of his botanical colleagues emphasized this reality in 1917 in a paper where they observed that the 'evil effects of the formal divorce of physiology from morphology are strikingly illustrated in the sister science of zoology'. Due to the medical nature of animal physiology in British universities, zoology had, according to the quintet, 'largely become synonymous with comparative anatomy'. The botanists further noted that recent attempts to revive zoology by emphasizing economic biology or genetics,[9] in spite of the brilliance of these fields, had not successfully managed to elevate the subject as a whole, 'precisely because such attempts do not envisage the study of animals as a living whole, and this cannot be done if animal physiology as the essential basis of treatment has to be left out of the account'.[10]

The rise of experimental zoology in the United States has been studied in detail since the 1970s. Until recently, however, this aspect of the history of British biology has not been of particular interest to historians of science.[11] This is a serious neglect. The key actors in this story were leading figures in early twentieth-century experimental zoology, whose story needs to be told. But, more importantly, there are reasons to think that the rise of experimental zoology in the United States was unusually rapid compared to Europe.[12] This means that a fuller understanding of the factors which favoured or hindered this reshaping of experimental zoology can only come from a comparative

6 Ashworth, op. cit. (4), pp. 232–233.

7 Lancelot Hogben, 'The origins of the society', in M.A. Sleigh and J.F. Sutcliffe (eds.), *The Origins and History of the Society for Experimental Biology*, London: SEB, 1966, pp. 5–11, 5.

8 Bernard Thomason, 'The New Botany in Britain circa 1870 to circa 1914', unpublished PhD thesis, University of Manchester, 1987; Steindór J. Erlingsson, 'The rise of experimental zoology in Britain: Hogben, Huxley, Crew and the Society for Experimental Biology', unpublished PhD thesis, University of Manchester, 2005, pp. 26–44.

9 Alison Kraft, 'Pragmatism, patronage and politics in English biology: the rise and fall of economic biology 1904–1920', *Journal of the History of Biology* (2004) 37, pp. 213–258.

10 F.F. Blackman, V.H. Blackman, F.W. Oliver, A.G. Tansley and Frederick Keeble, 'The reconstruction of elementary botanical teaching', *New Phytologist* (1917) 16, pp. 241–252, 245.

11 Erlingsson, op. cit. (8); *idem*, op. cit. (3); *idem*, 'The costs of being a restless intellect: Julian Huxley's popular and scientific career in the 1920s', *Studies in the History and Philosophy of Biology and the Biomedical Sciences* (2009) 40, pp. 101–108. Previous studies that have treated this issue include Mark Ridley, 'Embryology and classical zoology in Great Britain', in Timothy J. Horder, Jan Witkowski and Christopher C. Wylie (eds.), *A History of Embryology*, Cambridge: Cambridge University Press, 1985, pp. 35–67; Jan A. Witkowski, 'Julian Huxley in the laboratory: embracing inquisitiveness and widespread curiosity', in Kenneth Waters and Albert van Helden (eds.), *Julian Huxley: Biologist and Statesman of Science*, Houston: Rice University Press, 1992, pp. 79–103; Samuel J.M.M. Alberti, 'Amateurs and professionals in one county: biology and natural history in late Victorian Yorkshire', *Journal of the History of Biology* (2001) 34, pp. 115–147; Alison Kraft and Samuel J.M.M. Alberti, '"Equal though different": laboratories, museums and the institutional development of biology in late-Victorian northern England', *Studies in History and Philosophy of Biological and Biomedical Sciences* (2003) 34, pp. 203–236; Kraft, op. cit. (9).

12 Jonathan Harwood, 'The transformation of biology as a political process', unpublished paper presented at the International Society for History, Philosophy and Social Studies of Biology, Seattle, 1997.

work. Thus British zoology is one of several possible venues to make this comparison. In this paper I will throw light on the development of experimental zoology in Britain by focusing on the establishment of the *British Journal of Experimental Biology* (*BJEB*) and the Society for Experimental Biology (SEB) in 1923.[13] The key actors in this story were Lancelot T. Hogben, Julian S. Huxley and Francis A.E. Crew, who started exploring the possibility of establishing an experimentally oriented zoological journal in 1922. In order to support the *BJEB* and further the cause of the experimental approach, Hogben, Crew, Huxley and their colleagues decided to found a society, which led to the formation of the SEB.[14] From its inception the journal was plagued with difficulties that led to the merger of the *BJEB* and the *Biological Proceedings* of the Cambridge Philosophical Society in the autumn of 1925. I will also discuss the views that the leading proponents of experimental zoology in Britain in the 1920s expressed towards morphology and how their views further complicate the already much-modified 'revolt from morphology' thesis.

The birth of the *British Journal of Experimental Biology*

The lives of Huxley, Hogben and Crew intersected in the early years of the 1920s. This process started in December 1919, when Huxley, who earlier that year had acquired a position in the Oxford Zoology Department, asked Hogben to comment on a manuscript he was working on, in which Huxley described how he induced metamorphosis in the axolotl tadpole through thyroid feeding.[15] Hogben, who was at that time a lecturer in the Zoology Department of the Imperial College of Science, responded in early January by noting that he had read through Huxley's 'draft with great interest', which prompted him to repeat Huxley's research during the following months,

13 In 1966 the Society for Experimental Zoology published an essay that dealt in a broad manner with the history of the SEB from 1923 until 1966. Michael A. Sleigh, 'Aspects of the history of the Society', in Sleigh and Sutcliffe, op. cit. (7), pp. 12–32.

14 To date, J.B.S. Haldane has always been considered one of the founders of the SEB (George P. Wells, 'The early days of the S.E.B.', in P. Spencer Davis and N. Sunderland (eds.), *Perspectives in Experimental Biology*, vol. 1, London: Pergamon, 1976, pp. 1–6), which has its basis in Hogben's reminiscence of the early history of the SEB that was published in 1966 (Hogben, op. cit. (7); see also *idem*, *Lancelot Hogben: Scientific Humanist*, Woodbridge: Merlin, 1998, p. 79), and has since then been floating around in the literature (see, for example, Sahotra Sarkar, 'Lancelot Hogben, 1895–1975', *Genetics* (1996) 142, pp. 655–660, 656; James Tabery, 'R.A. Fisher, Lancelot Hogben, and the origin(s) of genotype–environment interaction', *Journal of the History of Biology* (2008) 41, pp. 717–761, 732). It is true that Haldane planned to stay with Crew, Hogben and Huxley in Edinburgh during the summer of 1922 (Crew to Huxley, May 1922, Julian S. Huxley Papers, Woodson Research Center, Fondren Library, Rice University, Texas (subsequently JHP, RU)), but something came up that terminated Haldane's interaction with them. The details of this episode are uncertain, but as Crew indicated in a letter from October 1922 the news that Huxley brought of Haldane was really 'bad' and made Crew sorry, for he argued that 'we are losing hold of one who would have brought much reason to our circle.' Nevertheless he observed that Haldane's 'is a selfish point of view, no doubt, but it is a fact' (Crew to Huxley, 9 October 1922, JHP, RU).

15 Julian Huxley, 'Metamorphosis of axolotl caused by thyroid-feeding', *Nature* (1920) 104, p. 435. See also Erlingsson, 'The costs of being a restless intellect', op. cit. (11), pp. 104–105; Susan Merill Squier, *Babies in Bottles: Twentieth-Century Visions of Reproductive Technology*, New Brunswick: Rutgers University Press, 1994, pp. 35–39.

resulting in a 'complete confirmation'. This was the beginning of research cooperation between Huxley and Hogben that resulted in a paper on amphibian metamorphosis in 1922.[16] While this collaboration was brewing, Crew became in 1921 the first director of the Animal Breeding Research Department (ABRD) in Edinburgh. Crew was mainly interested in physiological genetics and intersexes in various vertebrates, which was an interest he developed after getting acquainted with the work of the German geneticist Richard Goldschmidt on intersexes in moths.[17] Huxley, who had corresponded with Goldschmidt in 1920 and co-authored a paper on intersexes in *Gammarus chevreuxi* the following year, also had a keen interest in physiological genetics.[18] It was their mutual interest in Goldschmidt's work which brought Huxley and Crew together in 1921, when Huxley agreed to cooperate with Crew on research that aimed at looking into the effect of feeding thyroid to fowls, which eventually proved to be 'entirely negative and unconvincing'.[19] Huxley used this new-found friendship with Crew to convince him to hire Hogben, who joined the staff of the ABRD in March 1922.[20]

During these years, Hogben, Huxley and Crew were able to publish experimental papers in *Philosophical Transactions of the Royal Society*, *Proceedings of the Royal Society*, the *Quarterly Journal of Microscopical Science* and the *Journal of the Marine Biological Association*, but they did not have any say in the editorial policy of these journals. Hence there was a need for experimentally oriented zoologists to establish their own journal, where they were in full editorial and policy control. The possibility of establishing such a journal was first suggested in London in the spring of 1922, when Hogben discussed the matter with H.G. Wells. This was immediately followed by Hogben's discussion with Huxley and their mutual friend Guy C. Robson, of the University of London.[21] But Hogben's involvement in this scheme was terminated when he moved to Edinburgh. He was, however, still set on securing an outlet for papers that dealt with experimental zoology. One of the first things that Crew and Hogben seem to

16 Hogben to Huxley, 7 January 1920, JHP, RU. Hogben to Huxley, 12 April 1920, JHP, RU. Hogben's research was published as a short note in the *Proceedings of the Zoological Society* in 1920. Julian Huxley and Lancelot Hogben, 'Experiments on amphibian metamorphosis and pigment responses in relation to internal secretions', *Philosophical Transactions of the Royal Society of London. Series B, Containing Papers of a Biological Character* (1922) 93, pp. 268–293.

17 Lancelot Hogben, 'Francis Albert Eley Crew', *Biographical Memoirs of Fellows of the Royal Society* (1974) 20, pp. 135–153; Michael R. Dietrich, 'Of moths and men: Theo Lang and the persistence of Richard Goldschmidt's theory of homosexuality, 1916–1960', *History and Philosophy of the Life Sciences* (2000) 22, pp. 219–247.

18 Crew to Huxley, October 1923, JHP, RU. Huxley to Alister Hardy, 8 April 1920 and 18 July 1920; A. Hardy's Papers, Special Collections and Western Manuscripts, Oxford University. E.W. Sexton and Julian Huxley, 'Intersexes in *Gammarus chevreuxi* and related forms', *Journal of the Marine Biological Association of the United Kingdom* (1921) 12, pp. 506–556.

19 Crew to Huxley, 5 April 1921 and 25 October 1921, JHP, RU. Crew to Huxley, 27 December 1921, JHP, RU. Crew to Huxley, 9 February 1923, JHP, RU. Francis Crew and Julian Huxley, 'The relation of internal secretion to reproduction and growth in the domestic fowl, I: effect of thyroid feeding on growth-rate, feathering and egg production', *Veterinary Journal* (1923) 79, pp. 343–352.

20 Crew to Huxley, 10 February 1922 and March 1922, JHP, RU.

21 Hogben to Huxley, 19 October 1923, JHP, RU. Hogben, op. cit. (17), p. 79. Hogben to Huxley, 31 October 1925, JHP, RU.

have done after Hogben joined the ABRD was to approach Professor Edward A. Sharpey-Schafer to explore the possibility of them taking over editorial control of the *Quarterly Journal of Experimental Physiology* (*QJEP*) which Sharpey-Schafer owned and edited. The papers published at that time in the *QJEP* reflected Edinburgh's physiology research programme, which was predominantly clinical.[22] Hogben and Crew wanted to move the editorial policy towards experimental zoology, which they knew Sharpey-Schafer was personally keen on exploring. They seem to have been remarkably quick to convince him of the merits of their plan, because in April 1922 Crew informed Huxley that all their arrangements with Sharpey-Schafer were confirmed. They had already sent several papers to Sharpey-Schafer, in an effort to fill the 'present number'.[23]

Having filled this number of the *QJEP* they planned to rename it the *Quarterly Journal of Experimental Biology*,[24] since Sharpey-Schafer was 'willing to hand over the entire responsibility to us after the transition is complete'. But to accomplish this they needed to get all the papers for the transition issue through Sharpey-Schafer's editorial control. Sharpey-Schafer turned out to be a difficult editor, as he had by March 1923 vetoed most of the papers that Hogben and Crew had sent to him. This drove Hogben and Crew to terminate their cooperation with Sharpey-Schafer in the spring of 1923, thus suggesting to Huxley that they should go ahead with the original scheme of starting their own journal.[25]

Following Hogben's departure to Edinburgh in the spring of 1922, Huxley continued to explore the possibility of establishing a new experimental journal with his friend Alexander M. Carr-Saunders, who was a professor of sociology at the University of Liverpool. There were at least two reasons behind Huxley's exploration, both of them tied to Crew and Hogben's *QJEP* project. To begin with, Crew and Hogben for some reason did not include him in their scheme, which Crew acknowledged in early April when he noted his regret that Huxley had been excluded. Crew suggested, however, that Huxley could nonetheless have his papers published in the journal.[26] Armed with this information, Huxley immediately dispatched a paper on hydroid dedifferentiation, along with one on chicken embryology, to his friends in Edinburgh, both of which they vetoed.[27] Huxley was at that time familiar with having his papers rejected.[28] But receiving a rejection from two of his closest friends must have come as a shock, which

22 Hogben to Huxley, 25 June 1925, JHP, RU.

23 Crew to Huxley, 8 April 1922, JHP RU.

24 Crew to Huxley, 29 April 1922, JHP, RU.

25 Crew to Huxley, April 1923, JHP, RU. Crew to Huxley, 29 March 1923, JHP, RU.

26 Crew to Huxley, 8 April 1922, JHP, RU.

27 Crew to Huxley, 29 April 1922, JHP, RU. See also Hogben to Huxley, 10 September 1922, JHP, RU.

28 In November 1921, Huxley's paper on dedifferentiation in the *Echinus* larva was vetoed by the *Proceedings of the Royal Society* (Bourne to Huxley, November 1921, JHP, RU). Earlier that autumn Huxley sent two papers to E.J. Allen, the director of the Plymouth Laboratory of the Marine Biological Association (Erlingsson, op. cit. (3)), with the hope of having them published in the *Journal of the Marine Biological Association*. Allen vetoed both papers. In a letter from the end of February 1922, Allen 'quite frankly' advised Huxley 'strongly not to publish either of them until you can work at the subjects again and get a larger amount of experimental data' (Allen to Huxley, 28 February 1922, JHP, RU). In the coming years Huxley would continue to face difficulties with having some of his scientific papers published. Erlingsson, op. cit. (11), 'The costs of being a restless intellect', pp. 102–104.

might explain why Huxley went ahead with Carr-Saunders to organize his own journal. With Carr-Saunders's support, Huxley planned to have full editorial control:

> it does not much matter what you do – so long as you achieve your object which I take it is: to add weight, years and established reputation to the course of the Journal . . . You can have all the proper people in apparent support, while in fact immobilised – if you call them 'vice-presidents' of an Association or if you get their names as a board of 'referees' – Anything will do so long as the names are in print and the owners of the names without any status that enable them to interfere.[29]

During the summer of 1922, Carr-Saunders and Huxley's plan was well on its way and they intended to have the first issue of the journal out by January 1923.[30] When Crew and Hogben's efforts to take over the *QJEP* proved to be abortive, they decided to join Huxley and Carr-Saunders and towards the end of March 1923 the four of them agreed to launch their own journal.[31]

In April and May 1923, Crew negotiated with the printing firm Oliver & Boyd, which was based in Edinburgh. The publishers needed a £200 guarantee that Crew underwrote himself.[32] In addition to the quartet, Carr-Saunders convinced three other University of Liverpool professors to join the editorial board. These were the zoologist William J. Dakin, the oceanographer James Johnstone and the botanist John McLean-Thompson. With the Liverpool group also on board, Crew was certain that the project would be viable.[33] At Hogben's suggestion it was decided that the journal should also be an outlet for experimental botanists. The reason for this was simple. When Hogben was an undergraduate at Cambridge he had attended advanced lectures on plant physiology and ecology when 'neither animal physiology nor animal ecology had a niche in the zoological curriculum'.[34] This reflected the fact mentioned previously that botany in Britain had a long physiological tradition and as such it might prove a valuable ally. As a result, the plant geneticist R. Ruggles Gates, of King's College London, was invited to join the editorial board, though few botanical papers were ever published in the journal since it became completely zoological as of volume six.[35] The absence of Cambridge representatives on the board was quite conspicuous. This was noted by the Cambridge reproductive physiologist Francis H.A. Marshall, who was, as Crew noted in a letter to Huxley, 'rather grieved that we had no Cambridge man on the Editorial Board'.[36] As a result, Marshall was invited to join the board.

29 Carr-Saunders to Huxley, 8 August 1922, JHP, RU.

30 Carr-Saunders to Huxley, 8 July 1922, JHP, RU. Carr-Saunders to Huxley, 8 August 1922, JHP, RU.

31 Crew to Huxley, 29 March 1923, JHP, RU.

32 Crew to Bidder, 12 May 1925, G.P. Bidder's Papers, Archive of the Marine Biological Association, Plymouth, PBD13 (subsequently MBA, PBD13).

33 Crew to Huxley, May 1923, JHP, RU. This group comprised the initial editorial board of the *British Journal of Experimental Biology*, in addition to Guy C. Robson, and the geneticist John W. Heslop-Harrison, of Armstrong College, Newcastle.

34 Hogben, op. cit. (7), p. 8.

35 Botanical papers were three in a total of twenty-eight in volume 1, three in twenty-eight in volume 2, one in nineteen in volume 3, one in thirty in volume 4, one in thirty-two in volume 5, and zero in thirty-two in volume 6.

36 Crew to Huxley, June 1923, JHP, RU.

With Marshall recruited the editorial board went public with the establishment of the *BJEB* in letters that were published in 1923 in *Nature* and *Science* in July and August respectively.[37] The *Nature* letter began with the acknowledgement that British zoologists had made some of the most important contributions to morphological research, but contemporary British biology compared 'very unfavourably with other countries in facilities for the publication of research in experimental biology, especially on the zoological side'. It was noted that the *Journal of Genetics* was the only British journal solely devoted to experimental work, while American zoologists could choose from the *Journal of Experimental Zoology*, the *Biological Bulletin* and the *Journal of General Physiology*. As a result, experimental biologists in Britain had insufficient knowledge of what was going on in their field. More importantly, it was observed that this lack 'of satisfactory channel of expression' threatened to minimize the influence of the experimental approach which was considered 'essential for the further development of biology in Great Britain'. This influence was important. On the one hand, it was becoming clear that evolutionary problems could not be solved by relying solely on descriptive morphology. On the other hand, experimental zoology had opened up new means to tackle the problems of fertilization, development, sex and heredity, 'which have been too often neglected by traditional physiology'. With this in mind the signatories of the *Nature* letter had decided to establish the *BJEB*. It would accept papers dealing with comparative physiology, experimental embryology, genetics and animal behaviour, in addition to 'cytological, *morphological* and *histological* contributions bearing on current experimental problems'.[38] Crew became the managing editor and the first issue of the journal was published in October 1923.

The establishment of the *BJEB* was almost immediately challenged. Apart from the Plymouth Laboratory,[39] one of the very few institutional centres of experimental zoology in the United Kingdom was at Cambridge. In the Cambridge Zoology Department this research was led by James Gray, who had since 1913 specialized in experimental embryology,[40] while in the Physiology Department Joseph Barcroft,[41] who was known for his work on the oxygenation of blood in humans, had developed an interest in the physiology of invertebrates.[42] Among their publication outlets was the *Proceedings of the Cambridge Philosophical Society*, but in order to facilitate the publication of biological papers, the Cambridge Philosophical Society decided in the

37 F.A.E. Crew, W.J. Dakin, J. Heslop Harrison, Lancelot T. Hogben, Julian S. Huxley, J. Johnstone, F.H. A. Marshall, Guy C. Robson, A.M. Carr-Saunders and J. Maclean Thompson, 'The British Journal of Experimental Biology', *Science* (1923) 58, p. 102; F.A.E. Crew, W.J. Dakin, J. Heslop Harrison, Lancelot T. Hogben, J. Johnstone, F.H.A. Marshall, Guy C. Robson, A.M. Carr-Saunders and J. Maclean Thompson, 'The British Journal of Experimental Biology', *Nature* (1923) 112, pp. 133–134.

38 Crew *et al.*, (1923) 112, op. cit. (37), author's emphasis.

39 Erlingsson, op. cit. (3).

40 Erlingsson, op. cit. (8), pp. 82–85; H.W. Lissman, 'James Gray', *Biographical Memoirs of Fellows of the Royal Society* (1978) 24, pp. 55–70.

41 F.J.W. Roughton, 'Joseph Barcroft', *Obituary Notices of Fellows of the Royal Society* (1948–1949) 6, pp. 315–345.

42 See, for example, J. Barcroft and H. Barcroft, 'The blood pigment of *Arenicola*', *Proceedings of the Royal Society. Series B* (1924) 96, pp. 28–42.

summer of 1923 to attempt the publication of a separate biological journal. This was done at Gray's suggestion and he became the editor of the journal.[43] It is not clear why Hogben, Crew and Huxley did not seek cooperation with the Cambridge experimental zoologists. But the effect of the two groups proceeding independently was immediately felt, as the establishment of the *Biological Proceedings* meant that Cambridge zoologists would not submit papers to the *BJEB*. This division within the small community of experimental zoologists in Britain was to have a serious effect on the *BJEB* that culminated in a crisis, as we shall see.

Regardless of the Cambridge initiative, Crew wanted to get the Cambridge zoologists into an association that the editorial board was planning to establish in order to support the journal. In a letter to Huxley he insisted that the key question was whether the 'Association with us and the Cambridge school will help British biology'. Crew and Hogben viewed Joseph Barcroft as an essential element in this merger, for he was 'an experimental zoologist', and as such Crew considered him 'a fit and proper member of our Association. He has my vote'.[44] In Crew's mind this also meant that he should be invited to join the editorial board. After some discussion in the autumn of 1923 about the desirability of having Barcroft on the board, which Huxley seems to have questioned,[45] Barcroft was asked to join the board at the end of December that year, by which time a major new professional association had been established.

The creation of the Society for Experimental Biology

At a board meeting of the *BJEB* in May 1923, it was decided to pursue the idea of forming an association to support the journal and further the cause of experimental zoology in Britain. To pursue this idea further it was decided to hold a preliminary meeting at the gathering of the British Association in Liverpool at the end of August that year, where, as we have seen, Ashworth had defended morphology in his presidential address to the zoology section. Hogben was given the responsibility of organizing this meeting, as he 'uniquely' enjoyed the advantage of having his 'feet in both camps (zoology and physiology)'. Following the Liverpool meeting Hogben was instructed to 'make arrangements with various people annunciating the possibility of a conference to consider the formation of an Association'.[46] In September, he sent out letters to selected individuals, including the editorial board, to continue exploring the interest in forming such an association at a future conference on experimental biology. He received full support from Huxley, Crew, Ruggles Gates and Robson, and no opposition from

43 'Cambridge Philosophical Society'; MBA, PBD13. Lissmann, op. cit. (40), p. 60. The first volume of the *Biological Proceedings of the Cambridge Philosophical Society* spanned the years 1923 to 1925 and nearly two thirds of its twenty-nine papers dealt with experimental zoology (Wells, op. cit. (14), pp. 2–3). On the editorial board of the *Biological Proceedings* were Professor H.R. Dean, Professor F.G. Hopkins, Professor A.C. Seward, Professor J.T. Wilson, J. Barcroft, J. Gray, T.C. Nicholas, and F.A. Potts.

44 Crew to Huxley, 4 October 1923, JHP, RU.

45 Crew to Huxley, 4 October and November 1923, JHP, RU; Hogben to Huxley, 19 October 1923, JHP, RU.

46 Hogben to Huxley, 31 October 1923, JHP, RU.

Table 1. *The number of zoological and botanical papers presented at the SEB conferences that were held from 1923 to 1928. Papers that I was unable to categorize or that apply to both zoology and botany are listed under 'general'. 'Programs for the SEB meetings 1923–1928', SEB Archive, Archive Management Systems, Reading: AMS 98409, C.Ref 6*

Year	No.	Zoology	Botany	General
1923		13	3	4
1924	1	6	5	1
	2	11	2	2
1925		5	2	6
1926	1	7	1	4
	2	5	3	4
	3	6	3	4
1927	1	7	1	5
	2	6	2	4
	3	5	1	5
	4	8	1	5
1928	1	8	4	3
	2	13	3	2
	3	9	3	3
Total		109	34	52

F.H.A. Marshall.[47] There was also great interest in such a conference in Cambridge and at the Plymouth Laboratory.

Hogben continued organizing the conference all through the autumn. At the beginning of December 1923 an invitation to the conference was sent out. In addition to detailing the programme, which included thirteen zoological papers, three botanical papers and four general papers (Table 1), it stated the following:

> You are invited by the Editorial Board of *The British Journal of Experimental Biology* to attend a Conference at Birkbeck College … 21st and 22nd December 1923 for the purpose of considering and proceeding with the formation of an Association to promote intercourse, discussion and facilities of publication among biological workers engaged in experimental lines of enquiry.[48]

At the inaugural conference of the new Society of Experimental Biology (SEB), Hogben was voted the zoological and physiological secretary and Gates the botanical secretary of the society. By creating a special botanical managerial position the society was directly trying to draw into the SEB botanists, who, regardless of the fact that zoological papers

47 Hogben to Huxley, 15 November 1923, JHP, RU. The Liverpool quartet opposed holding a conference and especially Hogben's management of the project. As a result they did not attend the inaugural conference of the SEB. Carr-Saunders to Huxley, 27 October 1923, JHP, RU. Carr-Saunders to Huxley, 5 November 1923, JHP, RU. Carr-Saunders to Huxley, 2 February 1924, JHP, RU.

48 'Programs for the SEB meetings 1923–1928'; SEB Archive, Archive Management Systems, Reading: AMS 98409, C.Ref 6.

Table 2. *List of individuals who are thought to have been the founding members of the Society of Experimental Biology. M.A. Sleigh, 'Aspects of the history of the society', in M.A. Sleigh and J.F. Sutcliffe (eds.),* The Origins and History of the Society for Experimental Biology, *London: SEB, 1966, pp. 12–32, 13. Sleigh lists A.M. Carr-Saunders, W.J. Dakin and J. Johnstone as founding members of the SEB but they joined the society in 1924 (see Carr-Saunders to Huxley, 5 November 1923, JHP, RU; Carr-Saunders to Huxley, 2 February 1924, JHP, RU), along with nineteen other individuals that are included in Sleigh's list*

E.J. Allen	A.L. Bacharach	J.R. Baker	J. Barcroft
W. Bateson	W. Bayliss	G.R. de Beer	G.P. Bidder
K.B. Blackburn	A.E. Boycott	R. Briggs	F.T. Brooks
R. Burian	D.R.R. Burt	H.G. Cannon	H.M. Carleton
G.S. Carter	F.E. Chidester	A.J. Clark	W. Cranmer
F.A.E. Crew	J.T. Cunningham	D. Ward Cutler	G.C. Damant
F. Deacon	A.F. Dence	A. Dendy	C. Diver
J. Duerden	A.E. Ellis	C.S. Elton	R.A. Fisher
H. Munro Fox	J.F. Fulton	F.W. Gamble	F.S. Garrett
S. Garstang	W. Garstang	R.R. Gates	S.R. Gloyne
E.S. Goodrich	H. Goodrich	G.H. Graham	J. Gray
A.W. Greenwood	J.B.S. Haldane	J. Hammond	A.C. Hardy
J.W.H. Harrison	H.R. Hewer	F. Hindle	A.D. Hobson
L.T. Hogben	O.D. Hunt	G.E. Hutchinson	J.S. Huxley
F. Keeble	E.L. Kennaway	W.P. Kennedy	R.D. Laurie
M.V. Lebour	E. MacBride	A.D. MacDonald	F.H. Marshall
J.S. Martin	R. McDowell	J. Meakins	I. Montagu
J.A. Murray	P.D.F Murray	W.C.F. Newton	C.F.A. Pantin
A.S. Parkes	T.R. Parsons	M. Pease	C. Pellow
F. Ponder	F.A. Potts	J. Priestley	R.C. Punnett
A. Subba Rau	M.V. Rayner	A.D. Ritchie	H.E. Roaf
J.A.F. Roberts	G.C. Robson	E.S. Russell	H. Sandon
J.T. Saunders	W. Schlapp	E.A. Spaul	B. Stracey
J. Strohl	W. Tattersall	A. Walton	D.M.S. Watson
H.G. Wells	F.R. Winton	E. Whitley	

dominated the meeting, 'turned up', as Hogben observed in 1966, 'in strength as hoped'. More than Hogben and his colleagues 'could have dared to hope, Joseph Barcroft with associates from the Cambridge Department of Physiology gave the meeting the full weight of his influential standing'.[49]

In spite of the success of the conference, which was attended by ninety-nine individuals (Table 2), the establishment of the society caused a slight tremor within the established biological community in Britain, not least because the founding members of the society also had natural homes in existing societies. While its formation stirred up things within the Linnean Society, and to a lesser extent in the Zoological Society, the Genetical Society (GS) was initially openly antagonistic to the formation of the SEB. The GS had been founded in 1919 by William Bateson. Immediately following the SEB's

49 Hogben, op. cit. (7), p. 9.

formation key members of the GS expressed hostility towards the new society, which can be explained by the fact that more than half of eligible GS members had joined the SEB within its first year, while among the governing bodies of the societies cross-membership was even higher. With this in mind, it is understandable that Bateson, who was the vice-president of the GS, and Reginald C. Punnett, one of its secretaries, viewed the SEB as a threat, but their proposed solution was surprising. In a letter to Bateson at the beginning of February 1924, Punnett expressed concern and argued that something needed to be done. He spoke of the SEB members as overambitious men who wanted 'to figure more prominently in the public eye than they do at present'. As a result 'there cannot be anything but antagonism' between the two societies. In order to prevent this, Punnett 'proposed that Genetical Society should disband itself'.[50] Bateson expressed similar views in a letter to Huxley in early February, where he stated that in light of the establishment of the SEB, the question whether the 'Genetical Society should be continued ... must be seriously considered'.[51] What seems to have been at stake here was that the GS, which was a specialized society, was in danger of losing some of its non-specialized members to a more general society, where they fitted better. Hogben was greatly upset by Bateson's position as he could not 'see any antagonism between the G.S. and ourselves'.[52] In an effort to appease Bateson, he wrote to Bateson to offer him a seat on the SEB council, which Bateson accepted.[53]

The formation of the SEB also had ramifications for more general biological societies – the Linnean and Zoological Societies – that felt in danger of losing their younger experimentalists. The Cambridge zoologist George P. Bidder,[54] who attended the inaugural SEB meeting, raised this issue at the meeting of the Linnean Society's council in early January. He moved that the council should recommend the annual election of three botanists and three zoologists, not older than thirty-five years, as full fellows without having to pay any entrance fee and with a reduced annual fee.[55] The following day Bidder proposed the same issue at a meeting of the Zoological Society. At this meeting there was a general consensus that one society needed to exist to which all zoologists could belong, 'from fin-ray-counters to chromosome counters and from bug-hunters to frog-teasers'. It was, however, resolved that the Linnean would be more suitable as 'the one Society', and a committee was formed 'to negotiate with the Linnean', which was eventually chosen.[56] Earlier, Huxley had informed Crew about this new development; the latter was not impressed, as Bidder's scheme would 'enable Linnean to compete on unequal terms' with the SEB; a point Hogben raised in a very sarcastic letter to Huxley few weeks later.[57] These worries surprised

50 A.G. Cock, 'The Genetical Society in 1924: a near demise?', *Heredity* (1979) 42, pp. 113–117, 113.
51 Bateson to Huxley, 10 February 1924, JHP, RU.
52 Hogben to Huxley, 16 February 1924, JHP, RU.
53 Cock, op. cit. (50), p. 116.
54 See Erlingsson, op. cit. (3).
55 Bidder to Huxley, 18 January 1924, JHP, RU.
56 Bidder to Huxley, 18 January 1924, JHP, RU. Linnean Society Minute Book 1919–1928, 24 January 1924, Linnean Society's Archive (subsequently LSA). Bidder to Huxley, 30 January 1924, JHP, RU.
57 Crew to Huxley, letter undated, JHP, RU, emphasis in original. Hogben to Huxley, 27 February 1924, JHP, RU.

Bidder,[58] but did not prevent him from pushing the matter in the Linnean Council. But contrary to Bidder's hopes his proposal to make it easier for young biologists to join the Linnean Society did not receive the support he had hoped.[59]

British Journal of Experimental Biology 1924–1925

Regardless of the accomplishment of having established a journal and a society that promoted the experimental approach, the confrontation with the older professional societies was only a minor inconvenience compared to the problems that would face the founders of the *BJEB*. For one thing, as early as 1924, Huxley, Hogben and Crew were not pleased with the scientific standards of the journal. Crew expressed this view in a letter to Huxley, where he stated that many of the papers that had appeared in the journal were 'slight'. Crew had turned down few papers that year and Gates had done the same with two papers, but the rest of the editorial board needed to be 'equally ruthless'.[60] This was not the only problem. In the spring of 1924, Crew observed that he could not get British physiologists to subscribe to the journal until it published papers that 'they must read', and the editorial board would not get these physiological papers while it was 'more useful for the physiologists to publish in a physiological journal'. With this in mind it is no wonder that Hogben wanted to make the 'journal more physiological'.[61] The reason for Hogben's view is clearly stated in *An Introduction to Recent Advances in Comparative Physiology*, which he co-authored with a colleague in 1924, in which they emphasized that the modern zoologist should move away from structural and morphological studies towards studying the functional physiology of the living animal.[62] Crew concurred, and emphasized that it was only by securing physiologists as subscribers that the financial position of the journal could be secured, as well as fulfilling the aim of the journal, which was to 'bring physiologists and zoologists together'. As Crew, in his capacity as managing editor, had been unable to accomplish this goal, he was willing to step down, but he did not see that he was solely responsible for the journal's 'scientific failure'. Hogben, who had not been pleased with Crew's editorial work, agreed with him on this point, for he could not see that the editorial board had any legitimate complaint against Crew, 'because he is limited by the papers he receives'.[63]

Part of the problem was that a majority of physiology in Britain was 'unduly hampered by medical preoccupations'.[64] But more seriously, even the general physiologists at Cambridge were publishing elsewhere. An important move to secure

58 Bidder to Huxley, 30 January, 5 February 1924, JHP, RU.

59 Linnean Society Minute Book 1919–1928, 16 October 1924, LSA. The failure to get his motion adopted might be a measure of the council members' fear that the LS would gradually be taken over by young biologists.

60 Crew to Huxley, letter undated, JHP, RU.

61 Crew to Huxley, letter undated, JHP, RU.

62 Lancelot Hogben and Frank Winton, *An Introduction to Recent Advances in Comparative Physiology*, London: V. Collins & Co., 1924, p. 2.

63 Crew to Huxley, letter undated, JHP, RU. Hogben to Huxley, 18 June 1924, JHP, RU.

64 Hogben and Winton, op. cit. (62), p. 2.

Table 3. *Analysis of papers published in volumes 1 and 2 of the* BJEB

	Experimental			Descriptive	Total number
	Physiology	Sexual Phys.	Genetics		
Volume 1	15	3	2	8	28
Volume 2	16	9	1	2	28
Total	**31**	**12**	**3**	**10**	

physiological papers, however, happened while Hogben was working at the Plymouth Laboratory in 1924,[65] when he managed to convince the Plymouth physiologist Carl F.A. Pantin, who had close Cambridge ties, to submit two papers to the journal, one of which was accepted.[66] This prompted Hogben to observe that they had now 'driven a wedge in the Cambridge school', and he hoped 'that future numbers will see more work of a definitely experimental type'.[67] Pantin's paper was among forty-six experimental papers that were published in the first two volumes of the *BJEB*, while ten papers were of a descriptive nature (Table 3). The decline in their number between volumes 1 and 2 seems to indicate that the establishment of the journal and the SEB had managed to increase interest in experimental zoology in Britain. The simultaneous increase in experimental papers did not, however, secure more subscriptions and the result was a severe financial crisis.

By the spring of 1925, Hogben had concluded that 'driving a wedge in the Cambridge school' was not going to suffice. A possible way out was to join forces with the Cambridge experimental school and the *Biological Proceedings of the Cambridge Philosophical Society*. He decided to discuss this matter with G.P. Bidder, who was well connected to Cambridge. Bidder's interest in experimental zoology was reflected in the fact that in 1919–1920 he had been instrumental in establishing the physiological department at the Plymouth Laboratory of the Marine Biological Association, as well as being a founding member of the SEB. Following their discussion, Hogben asked Crew to send Bidder a letter formally asking for his assistance, where Crew observed that he was willing to do almost anything to secure James Gray's aid. Crew expressed his hope that Bidder would convince Gray to start cooperation with the *BJEB* group, for if the division was to remain, 'both sides must fight' and 'before one can achieve complete success the other must succumb'.[68] Bidder responded favourably to Crew's plea for he 'was greatly encouraged' by Bidder's response. In his reply, Crew reiterated his view that Gray, who at Bidder's suggestion had already 'raised the question of the future policy with regard to the *Biological Proceedings*' at the council meeting of the Cambridge Philosophical

65 See Erlingsson, op. cit. (3), pp. 170–172.

66 C.F.A. Pantin, 'On the physiology of amoeboid movement, II: the effect of temperature', *British Journal of Experimental Biology* (1924) 1, pp. 519–539.

67 Hogben to Huxley, 18 June 1924, JHP, RU.

68 Crew to Bidder, 8 May 1925, MBA, PBD13.

Society,[69] must be made the editor of the *BJEB*. But the main issue that Crew raised in his letter revolved around the future arrangements of the journal. With this settled, Crew insisted that it seemed to him that the Cambridge group and the editorial board of the *BJEB* could 'meet on the common ground of the Society for Experimental Biology and that this Society can have for its organ' the united *Biological Proceedings* and *BJEB*.[70] In Bidder's reply he observed that after showing Crew's letter to Gray and his colleague J.T. Saunders, they were willing to cooperate with the *BJEB* editorial board on certain conditions: first, the journal was to be owned by a limited company, where around sixty biologists would hold a single £10 share; second, Crew, Gray, Hogben (chairman) and Saunders (secretary) were to comprise the first board of directors; third, Gray would be the editor; and fourth, Cambridge University Press was to be the publisher.[71]

Bidder was to handle the business side of the merger. The idea behind the company was to create an owner 'who does not die, and does not become senile and out of touch with the generation'.[72] As the future editor, Gray felt that certain changes needed to be made concerning the editorial policy of the *BJEB* in order to secure the success of the journal. The first point he raised was that papers which already have a natural home in another journal, where they may be fully appreciated by experts, are not suitable for publication. The clearest example of such a publication in the *BJEB* had been Huxley's paper on linkage in *Gammarus*,[73] which should have appeared in the *Journal of Genetics*, as its predecessor had. There were also several other papers that Gray would not have accepted, due either to poor quality or to excessive length. Gray realized that his criticism was harsh but it was needed in order to 'ascertain whether there is any real divergence of view between us as to the principles on which the Journal should be conducted'.[74]

Crew was obviously not pleased with having to take all the blame for the scientific failure of the journal, which made him feel 'a little hurt'.[75] He realized, however, that Bidder's plan was the best solution to the present crisis. With this in mind Crew agreed heartily with the scheme that Bidder and Gray proposed. Crew cited a few reasons for this failure. There had been a limited supply of good papers, which had forced Crew to publish papers that were in his opinion 'poor or unsuitable'. In fact, Crew voted against '50% of the papers that that were sent in'. The editorial board had also been a problem, as he thought 50 per cent of its members 'were useless or worse'.[76] The most revealing of Crew's complaints is the fact that he had voted against all of Huxley's

69 Minute Book of the Council of the Cambridge Philosophical Society, 4 May 1925. Cambridge Philosophical Society, Scientific Periodicals Library (subsequently CPS).

70 Crew to Bidder, 12 May 1925, MBA, PBD13.

71 Bidder to Crew, 18 May 1925, MBA, PBD13. 'The Company of Biologists Ltd.', MBA, PBD13.

72 Bidder to Crew, 18 May 1925, MBA, PBD13.

73 Julian Huxley, 'Further data on linkage in *Gammarus chevreuxi*; and its relation to cytology', *British Journal of Experimental Biology* (1923) 1, pp. 79–96.

74 'Principles to govern the editor of the Journal', MBA, PBD13.

75 Crew to Gates, 3 June 1925, RR Gates' papers, King's College Archive, RG1/2–3 (subsequently King's, RG1/2–3).

76 Crew to G.P. Bidder, 19 May 1925, MBA, PBD13.

papers.[77] In his letter to Bidder, Crew noted that Huxley 'had developed the impression that he was doing his duty in sending me an unending supply of his own papers'. To counteract these problems, Crew recommended that when Gray took over as the editor he needed to be free from the interference of an editorial board and that he would 'have to sit on Huxley's head'.[78]

Bidder appreciated Crew's frankness, but neither he nor his Cambridge colleagues wanted Gray to become a dictator. There were more pressing issues to consider. As Bidder pointed out to Crew, there would be several opponents to the *Biological Proceedings* and *BJEB* merger within the Cambridge Philosophical Society (CPS).[79] In early June, Gray had made the president of the CPS aware of the new scheme, which will fail 'if there is a strong objection to amalgamation with a Journal limited to one branch of biology'. But as Gray pointed out to Bidder, he could 'see no other way out'. Gray's main worry concerning the merger was the papers that the editorial board of the *BJEB* had already accepted for publication. He was resolute that he wanted to see them before the takeover.[80] Bidder did not see any problem there and informed Gray that he only needed to take over those papers he thought were good and 'those that were bad Crew returns "in consequence of the sale of the *British Journal of Experimental Biology*"'.[81]

When the news of the new scheme started to spread, Crew began receiving letters from members of the editorial board of the *BJEB*, in which they raised various objections, 'but none of them are very serious'.[82] The SEB council discussed the new scheme at a meeting on 29 May and expressed its approval of the 'steps which have been taken to ensure the cooperation of Cambridge zoologists in the management of the Journal on the lines advocated by Dr. Bidder'. The next day, the general meeting of the SEB approved Bidder's plan.[83] In late July, Crew informed Bidder that he had spoken at length with Oliver & Boyd, the publisher of the *BJEB*, about the new scheme and they had been 'most sympathetic and helpful'. In light of this, Crew did not think it would be difficult to settle things, hence he proposed that Gray could take over the first number of volume 3, which was to be published in October 1925. Crew had enough papers that had been reviewed by various members of the editorial board to fill the October number, which he could hand over to Gray.[84] Due to various difficulties the company's formation was delayed until early November 1925.[85] Later that month the committee of the CPS reached the conclusion that the *Biological Proceedings*, which was to be renamed

77 Crew to Gates, 3 June 1925, King's, RG1/2–3. See also Crew to Bidder, 19 May 1925, MBA, PBD13.

78 Crew to Bidder, 19 May 1925, MBA, PBD13. For a further discussion of this issue see Erlingsson, op. cit. (11), pp. 102–104.

79 Bidder to Crew, 21 May 1925, MBA, PBD13.

80 Gray to Bidder, 3 June 1925, MBA, PBD13. Gray to Bidder, 16 June 1925, MBA, PBD13.

81 Bidder to Gray, 19 June 1925, MBA, PBD13.

82 Crew to Bidder, 10 June 1925, MBA, PBD13.

83 SEB Council Minute Book, 29 May 1925; SEB Archive, Archive Management Systems, Reading, AMS 98421, C.Ref 18 (subsequently SEB-18). SEB General Meeting Minute Book, 30 May 1925, SEB-18.

84 Crew to Bidder, 29 July 1925, MBA, PBD13. Gray took over the editorial control after the first issue was published (Bidder to Crew, 31 July 1925, MBA, PBD13).

85 Bidder to Crew, 8 November 1925, MBA, PBD13.

Biological Reviews, should focus on the 'publication of reviews of general or special biological interest'.[86] The merger immediately managed to secure the scientific position of the *BJEB*. Now the Cambridge school of experimental zoology contributed papers to the journal, which as of volume 7 was renamed the *Journal of Experimental Biology* in order to assert its international standing.[87] The question that remains is whether Gray's views towards morphology were as benign as those of the old editorial board. But first we have to explore the nature of the debate that has surrounded Garland Allen's 'revolt from morphology' thesis.

Experimental zoology versus morphology

In his *Life Science in the Twentieth Century*, Garland Allen argued that young biologists (born after 1860), armed with experimentalism, revolted at the turn of the twentieth century against the speculative nature of the morphological tradition, Ernst Haeckel's biogenetic law (see below) and the fact that the study of form had sidelined functional studies. According to Allen, Wilhelm Roux's methodology and reductionism had a great appeal to young embryologists in the United States, including Thomas Hunt Morgan, E.G. Conklin, Ross G. Harrison and E.B. Wilson, for it 'appeared as a fresh new wave on the dull and placid ocean of descriptive morphology'.[88] In 1981, Jane Maienschein, Ronald Rainger and Keith Benson each wrote very critical papers dealing with Allen's claims. In a co-authored introduction to their articles, they argued that the change that biology at the turn of the twentieth century went through was of an evolutionary or continuous nature.[89] Maienschein argues that the questions being asked in embryology at the turn of the twentieth century shifted gradually from general evolutionary ones to very specific questions about cell or tissue differentiation. Simultaneously, the methods gradually changed from passive observation, at one extreme, 'through systematic observation and description, comparative description, and use of manipulative experimental techniques or methods, to a fully experimental approach on the other extreme', which is exactly what she sees, to a different degree, in the careers of Morgan, Conklin, Harrison and Wilson.[90] In response to this critique, Allen insists that the revolt was a fact in the period from 1890 to 1920, but that he had mistakenly attributed it to a tension between, for example, phylogeny and embryology, while the main fault line was

86 Minute Book of the Council of the Cambridge Philosophical Society, 23 November 1925, CPS.

87 Saunders to Wells, 25 May 1931, SEB Archive, Archive Management Systems, Reading: AMS 98434, C. Ref 31.

88 Garland Allen, *Life Science in the Twentieth Century*, New York: Wiley, 1975, pp. 9 and 35.

89 Jane Maienschein, Ronald Rainger and Keith R. Benson, 'Introduction: were American morphologists in revolt?', *Journal of the History of Biology* (1981) 14, pp. 83–87, 87. See also Jane Maienschein, 'Shifting assumptions in American biology: embryology, 1890–1910', *Journal of the History of Biology* (1981) 14, pp. 89–113; Ronald Rainger, 'The continuation of the morphological tradition: American palaeontology, 1880–1910', *Journal of the History of Biology* (1981) 14, pp. 129–158; Keith R. Benson, 'Problems of individual development: descriptive embryological morphology in America at the turn of the century', *Journal of the History of Biology* (1981) 14, pp. 115–28.

90 Maienschein, op. cit. (89), p. 91. See also *idem*, *Transforming Traditions in American Biology, 1880–1915*, Baltimore: Johns Hopkins University Press, 1991, pp. 134–135.

Table 4. *Analysis of papers published in volumes 3 to 6 of* BJEB. *As of volume 7 the journal was renamed* Journal of Experimental Biology

	Experimental			Descriptive	Total number
	Physiology	Sexual Phys.	Genetics		
Volume 3	11	7	2	–	**20**
Volume 4	21	10	–	–	**31**
Volume 5	24	9	–	–	**33**
Volume 6	25	6		–	**31**
Total	**81**	**32**	**2**	**0**	

between descriptive and experimental methods. This means that the revolt was directed against the 'naturalist tradition', which included morphology.[91]

As we have seen, when the editorial board announced the launching of the *BJEB* it was emphasized that 'morphological and histological contributions bearing on current experimental problems' would be accepted. As a result of this strategy, ten morphological papers appeared in the first two volumes of the *BJEB* (Table 3). When Crew and Hogben attempted to take over *QJEP* in 1922 they had also decided not to 'divorce helpful morphology from experimental zoology'.[92] This editorial policy led to the collapse of their plan, as Sharpey-Schafer did not 'understand that if we get all or any of the control of policy we shall accept papers which though not experimental themselves yet bear upon the work of the experimentalist'.[93] As Table 4 indicates, no morphological papers appeared once Gray became editor. Since he had insisted upon editorial autonomy, the question that remains is whether the total absence of morphological papers after Gray became the editor reflected his disapproval of morphology, or the fact that, as in the case of Huxley's *Gammarus* paper, they had a natural home in some other journal. In this context it is interesting to look at the only

91 Garland Allen, 'Morphology and twentieth-century biology: a response', *Journal of the History of Biology* (1981) 14, pp. 159–176, 165 and 170. For a further discussion on this issue see *idem*, *Thomas Hunt Morgan: The Man and His Science*, Princeton: Princeton University Press, 1978; *idem*, 'Mechanism, vitalism and organicism in late nineteenth and twentieth-century biology: the importance of historical context', *Studies in History and Philosophy of Biological and Biomedical Sciences* (2005) 36, pp. 261–283; Philip Pauly, 'The appearance of academic biology in late nineteenth-century America', *Journal of the History of Biology* (1984) 17, pp. 369–397; Ronald Rainger, Keith R. Benson and Jane Maienschein (eds.), *The American Development of Biology*, New Brunswick: Rutgers University Press, 1988; Nyhart, op. cit. (2); David Magnus, 'Down the primrose path: competing epistemologies in early twentieth-century biology', in Richard Creath and Jane Maienschein (eds.), *Biology and Epistemology*, Cambridge: Cambridge University Press, 2000, pp. 91–121; Robert E. Kohler, *Landscapes and Labscapes: Exploring the Lab–Field Border in Biology*, Chicago: Chicago University Press, 2002.

92 Crew to Huxley, 8 April 1922, JHP, RU.

93 Crew to Huxley, 29 March 1923, JHP, RU.

two evolutionary papers that were published in the *BJEB*, one of them authored by the Oxford embryologist Gavin de Beer,[94] and the other by Gray himself.

Gray's paper, which appeared in 1928, described various experiments that he performed in order to throw light on the role of water in the evolution of vertebrates. Among other things, they showed that while 80 per cent of the body weight of all vertebrates was water, the skin and eggs of amphibians was in dynamic equilibrium with the environment, as in fish, but the skin and eggs of reptiles was in static equilibrium with the surroundings. Hence the ability to retain water within the body occurred after the first terrestrial vertebrates evolved. In de Beer's paper on the evolution of the pituitary, he began by describing the mammalian pituitary, followed by an account of its development, which led to a detailed discussion of pituitary phylogeny. Having made this survey of the pituitary, de Beer concluded the paper by attempting to 'sketch its evolutionary history'.[95] The contrast between these papers is astounding. While Gray tested all his claims by conducting physiological experiments and even concluding his paper by suggesting that it was, 'perhaps, permissible to note that the evidence available is entirely experimental',[96] de Beer relied solely on descriptive methodology.

One might be tempted to think that these divergent approaches to evolution might indicate that Gray was not as congenial to morphology as the old editorial board of the *BJEB* had been. If that was the case it would feed directly into claims made by the anatomist and anthropologist G. Elliot Smith, in his review of Professor E.S. Goodrich's morphological tome *Studies on the Structure and Development of Vertebrates*.[97] Smith's lengthy review of Goodrich's book was as much a defence of the merits of morphology as it was an assessment of the book. Smith was obstinate that 'every student of zoology and palaeontology should be grateful for this eminently useful book'. He considered it a 'peculiar irony' that the scientific methodology that Goodrich used in his research, and which had, in the nineteenth century, resulted in the greatest revolution in human understanding of oneself and one's place in nature, 'should at present be despised and rejected by so many biologists'. Among Smith's sources for this depreciation of morphology in biological circles was a paper by the American zoologist Herbert Spencer Jennings, in which he argued that if morphological 'phobia' were permitted to dominate zoological work it could have serious consequences for functional studies.[98] Erecting secure morphological foundations was, according to Smith, the duty of all the sciences, and he argued that while physicists, chemists, engineers and palaeontologists did not waste their time castigating morphological studies, the paradoxical phenomena existed that numerous biologists wanted to 'repudiate the particular instrument' that Darwin

94 Ingo Brigandt, 'Homology and heterochrony: the evolutionary embryologist Gavin Rylands de Beer (1899–1972)', *Journal of Experimental Zoology (Part B: Molecular and Developmental Evolution)* (2006) 306B, pp. 317–328.

95 G.R. de Beer, 'The evolution of the pituitary', *British Journal of Experimental Biology* (1924) 1, pp. 271–291, 289.

96 James Gray, 'The role of water in the evolution of the terrestrial vertebrates', *British Journal of Experimental Biology* (1928) 6, pp. 26–31, 31.

97 Edwin S. Goodrich, *Studies on the Structure and Development of Vertebrates*, London: Macmillan, 1930.

98 Herbert S. Jennings, 'Biology and experimentation', *Science* (1926) 64, pp. 97–105, 98.

had used to create his revolutionary theory. Smith continued by observing that, contrary to the common view among zoologists, morphological research had 'not been exhausted' and neither was it 'sterile', because the fact was that 'almost every discovery in physiology creates new problems for the morphologists'. All this led Smith to speculate that 'vast accumulations of erroneous inferences still encumber the [experimental] literature of biology because considerations of phylogeny and homology have been ignored'.[99]

As the editor of the main experimental journal, Gray could not leave this charge unanswered. The views that Smith expressed in his review astounded him. In a letter to *Nature* Gray noted that few biologists would be unmoved by Smith's description of this anonymous group of biologists who suffered from 'morpholophobia'. In Gray's mind, the real situation was not nearly as bleak as Smith portrayed it, because 'in spite of anxious and widespread inquiry, I have failed to discover any genuine case of this unfortunate disease'.[100] Even if this phobia existed in Britain, it would be wrong because the final goals of the morphologist and the physiologist were the same: to understand how the different parts of the animal contribute to the whole being and how it gradually changes through evolution. With this common goal in mind, Gray argued that 'the morphologist (in the classical sense) has contributed, and is continuing to contribute, a more complete record of correlated facts than his younger colleagues in experimental laboratories can hope to do for many years to come'. As a result, Gray hoped that morphology and experimental zoology would never be viewed as mutually exclusive, since there was 'no intrinsic virtue in experiment; it is solely an additional weapon in the biological armament'.[101] Hence Gray was on par with the old editorial board about the mutual importance of morphology and experimental zoology.

The benign views that Hogben, Crew, Huxley, Gray and their colleagues expressed towards morphology, however, are of considerable historiographical interest because they seem to undermine Allen's 'revolt from morphology' thesis.[102] The question that remains is what the British experimental zoologists meant by 'helpful' morphology. De Beer's paper on the evolution of the pituitary seems to have been a good example, as Hogben, who along with Crew had planned to include this paper in the *QJEP* takeover,[103] was at that time working on the physiology of this organ.[104] To throw a

99 G. Elliot Smith, 'The importance of morphology', *Nature* (1930) 126, pp. 341–343, 342.

100 James Gray, 'Depreciated morphology', *Nature* (1930) 126, pp. 567–568, 568.

101 Gray, op. cit. (100), p. 567.

102 Maienschein and Benson argue that some of the protagonists of the experimental method in the United States at the turn of the twentieth century spoke of the usefulness of descriptive methods (Benson, op. cit. (89), p. 125; Maienschein, op. cit. (89), p. 95).

103 Crew to Huxley, 8 April 1922, RU JHP. In addition to this it is worth noting that de Beer thanked Hogben and Huxley for their assistance (de Beer, op. cit. (95), p. 290).

104 Lancelot Hogben, 'The pigmentary effector system, IV: a further contribution to the role of pituitary secretion in amphibian colour response', *British Journal of Experimental Biology* (1924) 1, pp. 249–270. See also Lancelot Hogben and F.R. Winton, 'Studies on the pituitary, I: the melanophore stimulant in posterior lobe extracts', *Biochemical Journal* (1922) 16, pp. 619–630; *idem*, 'Studies on the pituitary, II: the influence of hypophysectomy on the rate of carbon-dioxide production on frogs', *Quarterly Journal of Experimental Physiology* (1923) 13, pp. 309–322.

further light on that question it is instructive to explore the views that the Oxford embryologist Wilfred Jenkinson, who had taught Huxley at Oxford,[105] expressed on these matters in his *Experimental Embryology* (1909). Jenkinson's volume was an open attack on Haeckel's biogenetic law[106] – that is to say, that ontogeny recapitulates phylogeny – but not on morphology as such. He pointed out that zoological inquiry had a dual nature, being either physiological or morphological, but while Jenkinson acknowledged the 'great results' that descriptive morphology had produced, he observed that it had been led astray by the causal claims of the biogenetic law that ontogeny was 'a recapitulation of and therefore explicable in terms of phylogeny'. An understanding of the causes of ontogeny could only be accomplished if they were investigated 'like any other function by the ordinary physiological method of experiment'.[107] Jenkinson revealed here that morphology, as such, was not the problem. It lay in the causal claims that morphology, cast in the mould of the biogenetic law, had made. These views were echoed by Jenkinson's post-war zoological successors. Morphology that had been freed from the causal framework of the biogenetic law, and that thus viewed the study of the individual as an end in itself, was considered by Crew, Huxley and Gray as the flip side of the physiology coin.[108] Even Hogben, who had criticized recapitulation on the pages of *Nature* in 1920 and argued forcefully as to the detrimental effect that Haeckelian-style morphology had had on the development of experimental zoology in the *Nature of Living Matter* in 1930,[109] endorsed the *BJEB*'s tolerant line on morphology in 1923–1925. This seems to indicate that the antipathy that Allen and others interpreted as directed towards morphology per se might only have been directed towards Haeckelian-style causal morphology.[110]

Conclusions

Jonathan Harwood argues that during the late nineteenth and the early twentieth centuries many fields – for example, embryology, cytology and morphology – were in existence for long periods without ever receiving disciplinary status. Even though these fields, which Harwood refers to as specialities, lacked 'a substantial clientele for

105 Witkowski, op. cit. (11), pp. 82–84.

106 Richards, op. cit. (2), pp. 149–156.

107 John Wilfred Jenkinson, *Experimental Embryology*, Oxford: Clarendon, 1909, pp. 12, 16, 13. Jenkins expressed similar views four years later in a book on vertebrate embryology. John Wilfred Jenkinson, *Vertebrate Embryology*, Oxford: Clarendon, 1913, pp. 9–10.

108 Huxley's opposition to the biogenetic law is clearly revealed in a letter he received in 1924 from Walter Garstang, who was professor of zoology at the University of Leeds, following Huxley's visit to the university at the end of November that year. In the letter, Garstang, who was a fierce opponent of recapitulation (Brian K. Hall, 'Balfour, Garstang and de Beer: the first century of evolutionary embryology', *American Zoologist* (2000) 40, pp. 718–728, 723–725), observed that he was 'really astonished at the coincidence of our views' (Garstang to Huxley, 1 December 1924, JHP, RU).

109 Lancelot Hogben, 'Recapitulation and descent', *Nature* (1920) 106, pp. 212–213; *idem*, *The Nature of Living Matter*, London: Kegan Paul, 1930, pp. 121–124.

110 Nicolas Rasmussen has argued similarly. Nicolas Rasmussen, 'The decline of recapitulationism in early twentieth-century biology: disciplinary conflict and consensus on the battleground of theory', *Journal of the History of Biology* (1991) 24, pp. 51–89.

teaching, such fields nevertheless found a place in some universities'. The justification for some of these specialities was sought in their contribution to important theoretical questions.[111] Experimental zoology, which is used in this paper as an umbrella term for specialities such as experimental embryology and comparative physiology, originally seems to have found a niche within British universities because of its theoretical contribution.[112] Through Gray and Huxley, experimental zoology gained a foothold immediately after the Great War in Cambridge and Oxford. A few years later, Hogben and Crew were instrumental in establishing experimental zoology in Edinburgh, while Huxley, after receiving the King's College zoology chair in 1925, blew life into the field in London. As an indication of its limited teaching clientele it was not at all certain whether the Oxford zoology department would continue a course on experimental zoology after Huxley left the department in 1925.[113]

A good indicator of this development is the number of visits of university biologists to the Plymouth Laboratory's Physiology Department, established in 1920. When the department was founded, with the aim of promoting experimental zoology, the field was in its infancy in Britain. This is clearly revealed by the fact that during the department's first four years of operation only eleven visitors worked there, eight of whom came from Cambridge. As of 1924 these numbers started to rise significantly, with eight visits during that year and gradual growth in the coming years.[114] The establishment of the *BJEB* and the SEB in 1923 coincided with an increased interest in the Plymouth Laboratory's Physiological Department, indicating that professional societies and journals may well have been important for the development of specialities,[115] such as those that comprised experimental zoology and others.[116]

As this paper has revealed, there were some critical junctions during the founding of the SEB and the *BJEB* and the years that immediately followed, which could have led the story in a different direction. The first critical point was Hogben and Crew's attempted takeover of Professor Schafer's *Quarterly Journal of Experimental Physiology*. Their failure to secure the editorial control of *QJEP* led them in the spring of 1923 to join forces with Huxley to create a venue for experimental zoology, where morphology that had bearing on experimental issues could be fostered. This resulted in the formation of the *BJEB* and the SEB. The second critical point occurred during that summer when the Cambridge Philosophical Society started to publish its *Biological Proceedings*. This resulted in a crisis in *BJEB* affairs that came to a head in the spring of 1925. The only way out of this conundrum lay in the merger of the *BJEB* and the *BP* under the

111 Jonathan Harwood, 'Universities', in Peter J. Bowler and John V. Pickstone (eds.), *The Cambridge History of Science*, vol. 6: *The Modern Biological and Earth Sciences*, Cambridge: Cambridge University Press, 2009, pp. 90–107, 90. For comparison see Lynn Nyhart, *Biology Takes Form: Animal Morpology and the German Universities, 1800–1900*, Chicago: Chicago University Press, 1995; and also her *Modern Nature: The Rise of the Biological Perspective in Germany*, Chicago: Chicago University Press, 2009.

112 Erlingsson, op. cit. (3), pp. 167–168.

113 Hogben to Huxley, 25 June 1925, JHP, RU.

114 Erlingsson, op. cit. (3), pp. 172–174.

115 Harwood, op. cit. (111), p. 94.

116 Toby Appel, 'Organizing biology: the American Society of Naturalists and its "affiliated societies", 1883–1923', in Rainger, Benson and Maienschein, op. cit. (91), pp. 87–120; Kraft, op. cit. (9).

ownership of the Company of Biologists and Gray's editorial direction, which secured the scientific status of the journal.

Despite the obstacles that the *BJEB* faced during its initial years,[117] under Gray the journal 'progressed remarkably' and by the 1930s it was gaining an 'increasing measure of prestige'.[118] As a measure of this, the circulation of the journal rose from 217 copies in 1924 to 363 in 1932.[119] The same applied to the SEB. It grew steadily in the 1930s – the society's membership numbers rising from 120 in 1924 to 325 in 1933[120] – as did experimental zoology in general.[121] Among other things, this is indicated by the fact that the SEB meeting in September 1928 was held jointly with the meeting of the British Association, where one session was held with section D (Zoology) and another with sections D, I (Physiology) and K (Botany). The president of section I, Charles A.L. Evans, clearly noted this in his address at the BA meeting, when he observed that for several decades the closely related sciences of physiology and zoology had been 'poles apart' in Britain. He continued:

> However, the newly *disinterred* subject of comparative physiology . . . bears witness to returning interest of zoologists in the experimental study of function as against mere morphological classification, as well as of physiologists in comparative function as a valuable means of throwing light on their own special problems.[122]

117 The formation of the Company of Biologists meant that the SEB no longer had any direct control over the journal, even though the society used its profits to support it annually until the early 1930s. As the 1920s drew to an end, this was not viewed very favourably by some leading members of the society, leading to a serious crisis in the interaction of the company and the society in 1931 and 1933 which eventually came to a peaceful conclusion (G.P. Wells, 'Discussions between the SEB and COB', SEB Archive, Archive Management Systems, Reading, AMS 98434, C.Ref 31 (subsequently SEB, AMS-31).

118 Saunders to Wells, 5 December 1933, SEB, AMS-31.

119 J.T. Saunders, 'Journal of Experimental Biology', 7 November 1933, SEB, AMS-31.

120 Saunders, op. cit. (119).

121 Erlingsson, op. cit. (3).

122 Charles A.L. Evans, 'The relation of physiology to other sciences', *Science* (1928) 68, pp. 284–291, 287, author's emphasis.