Medical History, 1996, **40**: 415–436

The Making of a Biochemist II: The Construction of Frederick Gowland Hopkins' Reputation

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In our previous paper we showed that the British biochemist Frederick Gowland Hopkins promoted "dynamic biochemistry" as the fundamental science of life in a strikingly persistent way until the end of his career.[1] We also presented Hopkins' construction of dynamic biochemistry as a long-term process, in which new lines of research gave new substance to his vision of dynamic biochemistry, which, in turn, motivated new lines of research, and so on. In this paper, we ask why neither the process of the construction of dynamic biochemistry nor Hopkins' persistence in promoting it are prominent features of existing accounts of Hopkins; we also consider the origins of the tensions between and within these accounts.

We begin by considering how Hopkins' contemporaries viewed him, what they considered his strengths and weaknesses, his achievements and failures. In the first section of the paper we show how Hopkins became renowned as a brilliant scientist, not for his work in dynamic biochemistry, but for a relatively minor sideline of his research which came to assume great significance: the discovery of vitamins. We also show how those of Hopkins' friends and colleagues who considered him to be a poor administrator tried to relieve him of his administrative duties. We examine the construction of these images of Hopkins in relation to the uses to which they were put by the people under discussion. In other words, we aim to show to what extent people constructed, or attempted to construct, a particular image of Hopkins to serve their own ends. In some instances Hopkins himself chose to contest the reputations that were ascribed to him; in other cases he encouraged the dissemination of images of his work which, while they did not accord with his personal priorities, none the less served his interests. We shall discuss both Hopkins' resistance to and collusion with the ways in which he was viewed during his lifetime.

In the second section of the paper we show how different versions of a "Hopkins tradition" were created in the 1940s by his younger colleagues and by his successors. Some of these traditions drew upon, and perpetuated the reputations ascribed to Hopkins during previous decades; others challenged them. We show the ways in which differing

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Harmke Kamminga thanks The Nobel Committee for Physiology or Medicine and the Royal Swedish Academy of Sciences, Stockholm, for permission to quote from their records regarding nominations for the 1929 Nobel Prize in Physiology and Medicine and for the 1927 Nobel Prize in Chemistry.

[1] H Kamminga and M W Weatherall, 'The Making of a Biochemist. I. Frederick Gowland Hopkins' Construction of Dynamic Biochemistry', *Med. Hist.*, 1996, **40**: 269–92.

interpretations of the "Hopkins tradition" were used by those who strove to define the nature of Cambridge biochemistry after Hopkins' retirement in 1943, in particular how the electors to his Chair tried to impose order on his department by the appointment of a more conventionally managerial successor.

In the final section of the paper we show how historians of biochemistry have presented aspects of received views of Hopkins depending on the motivations underlying the writing of their histories. We comment on the uses that historians can make of the self-image projected by an individual and the images constructed by others of that individual, in conjunction. In the case of Hopkins, we conclude that existing accounts provide a mosaic of different views (including competing, even inconsistent views), each of which was constructed for a reason which it is the historian's job to make visible. While we do not claim to have arrived at "the truth, the whole truth, and nothing but the truth" about Hopkins, by separating out his own programme from the way in which that programme was understood by his contemporaries, we present a more coherent account of the "making of a biochemist" and, more generally, we hope to have shed light on the intricate ways in which scientific reputations are made.

The Making of a Scientific Reputation

The Discoverer of Vitamins

Ever since their inauguration in 1901, Nobel Prizes have been regarded by scientists and public alike as the pinnacle of scientific achievement. The prizes do not create scientific reputations; quite the converse, because a good reputation among one's peers is necessary if nominations are to be made and supported.[2] But they can reinforce reputations by focusing attention on a particular portion of the scientific work done by the recipient. Such was the case with Frederick Gowland Hopkins who shared the 1929 Nobel Prize for Physiology and Medicine with the Dutch hygienist Christiaan Eijkman for their discovery of vitamins.[3]

Both Hopkins and Eijkman had been nominated for the Nobel Prize in Physiology and Medicine many times before:[4] Eijkman was among the nominees for eight Prizes from 1914 onwards, and Hopkins for five, beginning with the 1923 award. In addition, Hopkins had been nominated for the 1927 Chemistry Prize, but the Nobel Committee of Chemistry declined to evaluate his research, judging that its significance "pertains to the science of physiology". Eijkman was consistently nominated for his investigations of beriberi, leading to its understanding as a vitamin deficiency disease. In letters nominating

[2] One's reputation among the Swedish scientists who sat on the committees that decided upon the awarding of the Prizes was, of course, particularly important, especially in the years before the Second World War. For a review of some of the issues involved in the award of the Prizes, see R M Friedman, 'Text, context, and quicksand: method and understanding in studying the Nobel sciences prizes', *Hist. Stud. physical Sci.*, 1990, **20**: 63–77.

[3] The work for which Nobel Prizes were awarded was not necessarily that for which the scientist was best known. Einstein, for example, was awarded the Prize for Physics for his law of the photoelectric effect, and not for his theories of relativity. See Friedman, op. cit., note 2 above, pp. 66–8.

[4] Material from the Nobel Archives relating to the Nobel Prizes in Physiology and Medicine and in Chemistry, respectively, was kindly provided by The Nobel Committee for Physiology or Medicine and by The Royal Swedish Academy of Sciences, Stockholm.

Hopkins, his work on accessory food factors invariably received mention, either alone or in conjunction with other contributions, such as his work on lactic acid production in muscle or on glutathione.

For the 1929 Prize, Hopkins was nominated by Edward Sharpey-Schäfer and by George Barger. Schäfer's nomination was for an undivided award to Hopkins for his investigations of glutathione, or tryptophan, or vitamins; Barger recommended that the Prize be awarded for contributions to the understanding of vitamins, either jointly to Eijkman and Hopkins, or jointly to Hopkins, Lafayette B Mendel and T B Osborne. In a long report on these nominees, written for the Nobel Committee by G Liljestrand in July 1929, special attention was paid to Eijkman's work relating to beriberi in the 1890s, and to Hopkins' work on accessory food factors between 1906 and 1912. Mendel and Osborne were considered to have used investigative approaches similar to those introduced earlier by Hopkins. In his justification for an award so long after these seminal studies, Liljestrand quoted from Barger's letter: "the far-reaching nature of [Hopkins'] results did not become clear until recent years". His report concluded that a joint award to Eijkman and Hopkins would be appropriate for 1929.

As discussed in our previous paper, Hopkins' vitamin research was for him a digression from his work on the chemistry of proteins. Hopkins found the concept of accessory food factors intriguing, but his failure to isolate them as chemical individuals frustrated him; writing to American colleagues in 1920, Hopkins suggested that if in 1911 he had not become concerned for his scientific credibility after becoming involved in a politically and financially motivated campaign promoting "standard bread" waged by the *Daily Mail*, he would not have published his first paper on the subject in the *Journal of Physiology* the following year.[5] Once this paper had appeared, Hopkins published very little else on vitamins, except in defence of his original findings.[6] Other British and American pioneers of research on micronutrients, many of whom had published their first findings before Hopkins' 1912 paper, pressed on with their work.[7] How, then, did Hopkins come to be regarded as the discoverer of vitamins?

[5] F G Hopkins to L B Mendel and T B Osborne, 2.ii.1920, University of Cambridge Archives (UCA) Add MS 7620/A; F G Hopkins, 'Feeding experiments illustrating the importance of accessory factors in normal dietaries', *J. Physiol.*, 1912, **44**: 425–60. For details about the "standard bread" campaign, see M W Weatherall, 'Bread and newspapers: the making of a "revolution in the science of food"', in H Kamminga and A Cunningham (eds), *The science and culture of nutrition, 1840–1940*, Amsterdam, Rodopi, 1995, pp. 179–212.

[6] F G Hopkins and A Neville, 'A note concerning the influence of diets upon growth', *Biochem. J.*, 1913, **7**: 97–9; F G Hopkins, 'Note on the vitamine content of milk', *Biochem. J.*, 1920, **14**: 721–4.

[7] The leading micronutrient researchers at this time were Thomas Osborne and Lafayette Mendel at Yale, Elmer McCollum and Marguerite Davis at Wisconsin, and Casimir Funk at the Lister Institute in London. It was Funk who coined the term "vitamine", later shortened at the suggestion of Jack Drummond to "vitamin" when it became clear that the factors were not in fact amines. Neither Hopkins nor Walter Fletcher liked the term "vitamine"; in May 1918 Fletcher wrote to Hopkins to suggest that they might be retermed "ergotropic [i.e. growth-promoting] factors" or simply "ergotropes", concluding "The more you laugh at this, the more it behoves you to suggest something better": W M Fletcher to F G Hopkins, 8.v.1918, Public Record Office (PRO) FD1/89. In a 1915 review Hopkins called them "exogenous growth hormones", and in a 1916 lecture to the Chemical Society he called them "food hormone factors": F G Hopkins, 'Progress in physiological chemistry', *Annual Reports of Progress in Chemistry of the Chemical Society*, 1915, **12**: 187–209; *idem*, 'Newer standpoints in the study of nutrition', *Trans. chem. Soc.*, 1916, **109**: 629–49.

The process began towards the end of the First World War, when Hopkins' work was discussed in the historical introduction to the first monograph issued on the subject of vitamins: the *Report on the present state of knowledge concerning accessory food factors (vitamines)* published in the Medical Research Committee's Special Report Series.[8] This monograph was the work of an Accessory Food Factors Committee appointed jointly by the MRC and the Lister Institute. Hopkins chaired this Committee from its first meeting in May 1918 until February 1931. The Report was highly successful: within two years of its publication it had sold over 3,500 copies to government departments and the public.[9] A second edition was prepared in 1924, and a third, completely revised edition appeared in 1932. The historical introduction to this report, written by Hopkins, is a rhetorical masterpiece.[10] After a brief summary of earlier work on the subject, Hopkins' research was introduced and at once distinguished by the sobriquet "classical". This assessment was followed by an extract from Hopkins' earliest published speculations on the subject (his 1906 address to the Society of Public Analysts) and then an extended description of his feeding experiments on rats.[11] The exactitude of Hopkins' technique was stressed throughout, the cessation of growth when components had been *carefully* purified being contrasted with the survival of animals on *crude* mixtures of caseinogen, starch, cane sugar, lard and inorganic salts. Hopkins' contemporaries were criticized on this point: for instance, Franz Röhmann's claim that artificial diets were sufficient was dismissed because there was "every reason to believe that he had failed to use sufficient care in ensuring the purity of the components of his food mixtures".[12]

The message about the significance of Hopkins' experiments was reinforced by the language used to describe his work as showing the "extraordinary effect" of "apparently insignificant" additions to the diet. This usage was different from the language of the rest of the Report, where the effect of minute quantities of active substances was taken for granted. Opposite the description of Hopkins' experiments was a full page devoted to two

[8] Hopkins at this time was also lecturing about vitamins in several London lecture series on wartime conditions and post-war reconstruction, including those at the Royal Institute of Public Health in 1917, at University College, London in 1918, and at King's College, London, in 1919: F G Hopkins, 'On the choice of food in war-time', *J. State Med.*, 1917, **25**: 193–202; *idem*, 'War bread', *Br. med. J.*, 1918, **i**: 157; *idem*, 'Vitamines: unknown but essential accessory factors of the diet', in W D Halliburton (ed.), *Physiology and national needs*, London, Constable & Co, 1919, pp. 27–49, summarized as 'The vital need', *Lancet*, 1919, **i**: 363. The MRC Report, published in 1919 by HMSO, was the 38th volume in the Committee's Special Report Series.

[9] Memorandum for the Accessory Food Factors Committee, 15.ix.1921, PRO FD1/89.

[10] It cannot be assumed that the introduction that appeared in the published Report exactly matches Hopkins' original text. The Report was edited by the biochemist Arthur Harden, and each of the chapters was discussed in detail by the whole committee before a final text was settled upon. Hopkins revised and updated the introduction for subsequent editions.

[11] In his 1906 paper, Hopkins had noted that diets of pure protein, fat, and carbohydrate were insufficient to support life, and speculated that other dietary factors were required; he also linked this point to the possible ætiology of scurvy and rickets. F G Hopkins, 'The analyst and the medical man', *Analyst*, 1906, **31**: 385–97. It is clear from the published discussion which followed the address that this point was lost on his audience, and the importance of this paper lies not so much in its immediate impact, as in its usefulness for Hopkins in his attempts to establish some sort of priority in vitamin research. He quoted at length from it in his 1912 paper in the *Journal of Physiology* (op. cit., note 5 above).

[12] A similar challenge from Osborne and Mendel was countered by the exemplary work of Hopkins and Neville, who were stated to have used "very carefully purified ingredients" in their experiments. This paper was a direct reply to the inability of Osborne and Mendel to replicate Hopkins' findings: Hopkins and Neville, op. cit., note 6 above.

graphs from his 1912 paper. The rhetorical impact of the description was backed up by the aesthetic qualities of the graphical presentation of the work. Indeed, in the long run, Hopkins' graphs may have been the single most important constituent of his claim to priority in vitamin research; they were the simplest, the most direct expressions of the doctrines outlined in the text. In 1933, W R Aykroyd wrote of them:

> The growth curves of the famous Hopkins' rats are familiar to anyone who has ever opened a textbook of physiology. One recalls the proud ascendant curve of the milk-fed group which suddenly turns downwards as the milk supplement is removed, and the waning curve of the other group taking its sudden milk-assisted upward spring, until it passes its fellow now abruptly on the decline. It was the prettiest experiment imaginable. "Feeding experiments illustrating the importance of accessory factors in normal dietaries, *Journal of Physiology*, 1912, xliv, 425," ranks aesthetically beside the best short stories of H G Wells.[13]

There is more than an element of truth in the grudging admission by the American nutrition scientist Elmer McCollum that Hopkins' major service in the discovery of vitamins was to put his views "in such memorable terms that they received wide recognition".[14] The MRC Report had a wide circulation, and immediate effect. New editions of standard textbooks referred to it, and some were subtly affected by it: the 10th (1919) edition of W D Halliburton's *Essential chemical physiology*, for example, changed from describing Hopkins' work as a "recent" demonstration of accessory food factors, to citing it as the "original" demonstration.[15] Many of the hundreds of review articles, monographs, and books on nutrition or biochemistry published by Hopkins' British colleagues referred to the Report, or to his work in particular.[16] Reviewing the second

[13] W R Aykroyd, *Vitamins and other dietary essentials*, London, Heinemann (Medical Books) Ltd, 1933, p. 46. The graphical presentation of average results greatly enhanced the clarity of Hopkins' case. His competitors' papers also included graphs, but by classical aesthetic criteria, they do not evoke as powerful a response as that elicited by Hopkins' graphs. The reason for this is clear; the Americans (Osborne and Mendel, in particular) were as concerned as Hopkins to demonstrate the replicability of their results, but chose to show this as part of their graphical presentation, thus making it crowded and complicated. By relegating the defence of replicability to the extensive tables in the appendix to his paper, Hopkins was able to mobilize aesthetic resources in his support.

[14] McCollum himself always felt robbed of proper credit for the discovery of growth-promoting vitamins. His history of the subject does not refer to the award of the Nobel Prize to Hopkins: E V McCollum, *A history of nutrition: the sequence of ideas in nutrition investigations*, Boston, Houghton Mifflin, 1957. For an account of the priority claims in this field, see N Aronson, 'The discovery of resistance: historical accounts and scientific careers', *Isis*, 1986, **77**: 630–46.

[15] W D Halliburton, *Essential chemical physiology*, London, Longmans, Green & Co, 10th edn, 1919; the 8th (1914) and 9th (1916) editions contain descriptions of Hopkins' work. The later editions follow the format of the 10th. Other textbooks that referred to the Report included W M Bayliss, *Principles of general physiology*, London, Longmans, Green & Co, 3rd edn, 1920; F A Bainbridge and J A Menzies, *Essentials of physiology*, London, Longmans, Green and Co, 4th edn, 1920; vol. 5 of L Luciani, *Human physiology*, London, Macmillan, 1921, in which the translator (the physiologist M S Pembrey) also directed readers to Hopkins' article on 'The practical importance of vitamines', *Br. med. J.*, 1919, **i**: 507. Even Noël Paton, a staunch opponent of the role of vitamin deficiency in the cause of rickets, included an expanded discussion of the subject in his *Essentials of human physiology*, Edinburgh, W Green & Son, 5th edn, 1920.

[16] Perhaps the most important was V G Plimmer and R H A Plimmer, *Vitamins and the choice of food*, London, Longmans, Green & Co, 1922, which went through many editions. See also A Harden, 'Biochemistry and fermentation', in E F Armstrong (ed.), *Chemistry in the twentieth century. An account of the achievement and the present state of knowledge in chemical science*, London, Ernest Benn, 1924, pp. 216–23; J C Drummond, 'The vitamins', in: H H Dale, J C Drummond, L J Henderson and A V Hill, *Lectures on certain aspects of biochemistry*, University of London Press, 1926, pp. 152–72. The

edition of the Report on its appearance in February 1924, Robert McCarrison characterized its influence in the following terms:

> The gathering together into one volume of the observations and experiments which formed the basis of the statements made in the first edition has had much to do with the spread of knowledge in regard to these essential constituents of the food among members of the [medical] profession in general. The report also served as a source of authoritative information and of inspiration to many of those who were devoting themselves to the investigation of problems relating to vitamins. It has become the standard reference book for workers on this subject in nearly every civilized country.[17]

Through the dissemination of the MRC Report, and the widespread reproduction of his graphs, accessory food factors became "Hopkins' stuff",[18] and Hopkins became "the discoverer of vitamins", all at a time when Hopkins himself had long since ceased his research into the subject. Vitamins had been a distraction, a diversion away from Hopkins' central interests, and he believed that the very success of the concept had drawn young researchers away from fields potentially even more fruitful. Reviewing the second edition of H D Dakin's *Oxidations and reductions in the animal body* in 1922, for example, Hopkins wrote:

> Attractive aspects of physical chemistry and the new type of dietary studies which have been so prominent during the last decade have withdrawn a great many workers interested in metabolism from the laborious field which this book surveys.[19]

Nevertheless, although he was no longer interested in working on vitamins, the fact that Hopkins became regarded as their discoverer had its uses, for him and for others. As Kohler has documented, Hopkins' close friend Walter Morley Fletcher, the Secretary of the MRC, used the promise that the discovery of vitamins held for human welfare as a lever to persuade the Trustees of the estate of Sir William Dunn to turn their philanthropic gaze upon Hopkins' department.[20] For Fletcher vitamins provided justification for his broader strategy of devoting MRC funds to fundamental research with the long-term aim of seeking scientific solutions to medical problems.[21] Hopkins and Fletcher agreed that

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influence of the MRC monograph is also clear in popular accounts of vitamins, such as Julian Huxley's essay 'The control of the life-cycle', originally published in *The English Review*, and reprinted in his influential *Essays in popular science*, London, Chatto & Windus, 1926, pp. 75–105, and J B S Haldane's essay 'Vitamins', reprinted in his collection *Possible worlds*, London, Chatto & Windus, 1927, pp. 51–6.

[17] R McCarrison, 'Vitamins. Medical Research Council's Report', *Br. med. J.*, 1924, **i**: 331. While in Britain the MRC Report had indeed become the standard, in the United States this was not so. American textbooks for the most part do not refer to it, instead citing works by Funk, Sherman and Smith, Eddy, McCollum, or Ellis and Macleod. Of these, only Sherman and Smith follow the MRC Report in ascribing credit to Hopkins for the discovery of the growth-promoting vitamin; it is noteworthy that they reproduce Hopkins' graphs in their introductory section. Ellis and Macleod's account is more typical; they concentrate on the achievements of Osborne and Mendel, and relegate Hopkins' contribution to having merely produced "somewhat similar results": C Funk, *The vitamins*, Baltimore, Williams & Wilkins, 1922 [authorised translation from the 2nd German edn by H E Dubin]; H C Sherman and S L Smith, *The vitamins*, New York, Chemical Catalog, 1922; E V McCollum, *The newer knowledge of nutrition*, New York, Macmillan, 1922; C Ellis and A Macleod, *Vital factors of food, vitamins, and nutrition*, London, Chapman & Hall, 1923.

[18] A phrase used by the writer of a leading article on rickets in *Nature*, 1922, **110**: 212.

[19] F G H[opkins], *Physiol. Abstr.*, 1922, **7**: 579.

[20] R E Kohler, 'Walter Fletcher, F G Hopkins, and the Dunn Institute of Biochemistry: a case study in the patronage of science', *Isis*, 1978, **69**: 331–55.

[21] J Austoker, 'Walter Morley Fletcher and the origins of a basic biomedical research policy', in: J Austoker and L Bryder (eds), *Historical perspectives on the role of the MRC*, Oxford University Press, 1989, pp. 23–33.

the role of the MRC should be to support fundamental research, but when it came to biochemistry, they disagreed on what counted as "fundamental". Fletcher was tremendously excited by the potential of research into the biological function of vitamins. He suggested to Hopkins that the MRC should shoulder the full burden of the costs of research into the subject, a sentiment with which Hopkins agreed.[22] Fletcher wanted Hopkins to lead the way and was assiduous in recruiting other friends of Hopkins to reinforce this strategy. Writing to ask the Secretary of the Royal Society, William Bate Hardy, if he would join the Accessory Food Factors Committee, Fletcher told him that he "could help enormously to push on the work at Cambridge":

> Though Hopkins was the pioneer, he has now so many outside jobs that he has little free energy to push this business on, and Cambridge is letting the work slip away to other places. It is one of the biggest new things in biology, and may turn out to be more fundamental than we guess, even now. As for medicine, it is out and away the biggest thing. We have hardly touched yet the effects of deficiency in lowering resistance to disease. Mellanby's Rickets work is showing this clearly already. We moved that to Cambridge, so as to link it with Hopkins's work, but I should like to see Hopkins pushing ahead on a much bigger scale, without special reference to any medical or other applications at the moment.[23]

Within months, however, Mellanby had moved to Sheffield, and Hopkins had turned away from vitamin research for good. Virtually no vitamin research was carried on in the Dunn Institute, and by 1927 Fletcher so despaired of getting any vitamin work done there that he instigated the founding of a separate Nutritional Laboratory at Cambridge. Contrary to Fletcher's expectations Hopkins did not play an active role in guiding the research undertaken in that Laboratory, and it was left to Fletcher to ensure that the Laboratory's director, Leslie Harris, promoted suitably fundamental nutritional research.[24] Fletcher's frustration that Hopkins would not promote work, such as vitamin research, which promised in the foreseeable future to lead to medical applications indicates that he had not fully appreciated the direction in which Hopkins' vision of dynamic biochemistry was taking his research. As Fletcher told the physiologist A V Hill in 1927, he was frustrated with Hopkins' "frightfully learned" colleagues who knew all about "protein molecules and o-r potential and all that", but who all seemed "to run away from biology".[25] If the scientists in Hopkins' Institute were running away from biology, then they certainly could not be running towards medicine.

Hopkins' association with vitamins also gained him a reputation among the interested lay community such as those civil servants and government ministers in whose hands lay many of the final decisions about the funding of research. Hopkins came to be seen as a public authority on the subject of nutrition, and for the rest of his career he used the dietary requirements for vitamins as a major rhetorical resource, drawing attention to the skills and approaches of the biochemist. He used vitamins as a recurring illustration in his lectures of the practical significance of fundamental biochemical research, right into the 1930s. His vitamin work brought him honours from numerous scientific bodies, and

[22] W M Fletcher to F G Hopkins, 22.iv.1920, PRO FD1/89.

[23] W M Fletcher to W B Hardy, 20.iv.1920, PRO FD1/89.

[24] R E Kohler, *From medical chemistry to biochemistry*, Cambridge University Press, 1982, p. 85.

[25] W M Fletcher to A V Hill, 20.i.1927, PRO FD1/1948.

ultimately the highest recognition of all in the shape of the Nobel Prize for Physiology and Medicine in 1929. His introductions to the MRC vitamin reports and his Nobel speech contained strong claims of priority in the discovery, indicating that, despite his alleged disappointment at having been honoured for this facet of his work rather than any other, Hopkins was well aware of the importance that his sideline of two decades previously had assumed in his career.[26] His detailed claims to priority in the discovery of vitamins are a striking instance of Hopkins cultivating a reputation originally ascribed to him by his scientific colleagues.[27] His colleagues, in turn, epitomized Hopkins' discovery of vitamins as an archetypal illustration of the potential benefits of the promotion of fundamental research, at a time when drawing and maintaining the validity of such illustrations was of central importance in attracting funding from government and philanthropic concerns.

The Poor Administrator

In the construction of his reputation as the discoverer of vitamins, Hopkins' own interests converged with those of others who wished to steer biomedical research in Britain in particular directions, especially the Secretary of the MRC, Walter Fletcher. Fletcher also had strong views on how Hopkins should run his Institute; in the process of trying to persuade Hopkins to relinquish the administrative direction, Fletcher and his allies constructed an image of Hopkins as a poor administrator, an image to which Hopkins put up firm resistance. Fletcher's sentiments on the running of the Dunn Institute were shared by an informal network of influential physiologists including William Bate Hardy, A V Hill (professor at Manchester before moving to University College, London, in 1923), Henry Dale (director of the MRC's National Institute of Medical Research from 1928), Thomas Elliott of University College Hospital, and Hugh Anderson (Master of Caius College, Cambridge, and probably the single most influential person in university politics at this time)—all friends of Hopkins who had worked in the Cambridge Physiological Laboratory at some time between 1898 and 1905.

The members of this group believed that if Hopkins could be persuaded to concentrate on research, for example by relinquishing membership of non-scientific bodies such as the General Medical Council, his career would experience a rich Indian summer. As Fletcher told Hopkins after a visit to Cambridge in February 1921:

> It makes me almost indignant with you that you should stand the racket of a single unnecessary train journey or tiresome meeting in London when you have such infinitely more important and inexpressibly more pleasant work in the laboratory. And the urgency of this feeling is multiplied a hundred-fold by the certain knowledge that you can make progress along those lines better than any man in the country, whereas there are scores of us poor idiots who can fuss away and waste time in trains and at meetings.[28]

[26] On his disappointment, see: N W Pirie, 'Sir Frederick Gowland Hopkins (1861–1947)', in: G Semenza (ed.), *Selected topics in the history of biochemistry: personal recollections* (*Comprehensive Biochemistry*, vol. 35), Amsterdam, Elsevier Science Publishers, 1983, pp. 103–28.

[27] Hopkins' Nobel speech, 'The earlier history of vitamin research', is reprinted in J Needham and E Baldwin (eds), *Hopkins and biochemistry 1861–1947*, Cambridge, W Heffer & Sons, 1949, pp. 191–200.

[28] W M Fletcher to F G Hopkins, 8.ii.1921, Archives of the Medical Research Council (MRC) PF106.

In the same way that Hopkins failed to guide his own research in the direction desired by Fletcher and his other friends, so his style of departmental management frustrated them. The physiologists believed that, although Hopkins was a brilliant scientist, he was a poor administrator; as Hill put it succinctly in a letter to Fletcher in February 1927:

> Concerning Hopkins, the trouble is really that Hopkins himself is a genius *provided* that he sticks to his proper job, which is research. As soon as he attempts to become an administrator and organizer he does it badly, his genius is wasted and he wastes his time. He ought to be a full time research professor with only about six able pupils instead of about one hundred ordinary ones. I am afraid, however, that he is quite incurable and that a desire for personal prestige or some of that kind will make him to continue to do what he does badly instead of doing what he does really well.[29]

Of particular concern was Hopkins' apparent willingness to take in anybody who applied to him for bench space. Hopkins was well aware of this worry, and resisted all attempts to portray him in this way. Writing to Fletcher in August 1924, he defended his conduct:

> My good friends, in particular yourself perhaps, realising that I am not doing much personal research, feel: (1) that I am doing too many extraneous jobs, and (2) that, owing to weak good nature, I have allowed the laboratories to become swamped with undesirables, and so have cooked my own goose.
>
> I won't bother you anymore about (1). I have for years been put upon this or that Committee, etc, as a representative of biochemistry, because there were so few of the cloth. Now there are plenty coming along!
>
> But (2) is, for me, rather serious. With yourself, Hardy, Anderson, and others, holding that opinion, the school is likely to suffer.
>
> Much more than my inability to say NO is the rush into Biochemistry the cause of our overcrowding. As a matter of fact I have refused a round dozen applicants in the last six months, almost every one being, for me, a "painful case". To refuse workers is easier in theory than in practice. It would be delightful to have none but A1 workers around one, but I think that an attempt to secure that consummation would result in empty laboratories . . .
>
> . . . In spite too of the over-crowding not a single worker so far has failed to get out at least one decent piece of work. We shall have published fifty papers during an academic year, and very few of them are pot boilers.[30]

Even after this rebuttal, Fletcher's next New Year missive told Hopkins that "All your friends are hoping that 1925 will also mark a new chapter of work in which you will have delegated most of your routine work, and that you will get real leisure for the big new piece of work of your own that you still have to give."[31] This "real leisure" did not materialize, however, and over the next few years the physiologists made concerted efforts to relieve Hopkins of the administrative duties of running what Hill was later to call his "large and rather disorderly department".[32] The first such efforts were made in 1925 by Hugh Anderson who, in response to the concern of the Financial Board of the University of Cambridge that Hopkins' departmental accounts were overdrawn, attempted to persuade Hopkins to reduce the number of research students in the Institute, and to appoint

[29] A V Hill to W M Fletcher, 5.ii.1927, PRO FD1/1948.

[30] F G Hopkins to W M Fletcher, 10.viii.1924, MRC PF106.

[31] W M Fletcher to F G Hopkins, 22.xii.1924, MRC PF106.

[32] Comment by A V Hill: Churchill College Contemporary Archives Centre (CC) AVHL I 3/38.

a senior member of his staff to take on its administration.[33] Hopkins was amenable to the proposed changes, but pleaded that he simply could not afford to make the desired appointment.[34] In the event, although T S Hele was subsequently given some responsibility for administrative affairs, it became clear to the physiologists that nothing less than replacing Hopkins altogether would solve the administrative problems of the Institute.

In 1927 Hill and Elliott conceived a plan to create a research professorship for Hopkins and to bring back another former Cambridge man, medical chemist George Barger, to run the Dunn Institute.[35] Elliott first considered the possibility of putting Hopkins forward for a Royal Society Foulerton Research Professorship, but it became clear that the Royal Society could not provide a sufficient sum to support Hopkins' research. Hill and Elliott turned instead to the Beit Trustees to fund the proposal, and successfully canvassed the support of Dale, Anderson, and the biologist Arthur Shipley (the Beit Trustees' Cambridge representative) for their scheme.[36] Despite further support from eminent scientists such as Charles Sherrington, the Advisory Board to the Trustees were split, particular opposition coming from the bacteriologist William Bulloch.[37] When the proposal was discussed by the Trustees it could not be given the unanimous approval which Otto Beit had demanded because of the unusual nature of the scheme. Hopkins had been favourably disposed towards the plan, as had Barger, but the difficulties which had arisen about the propriety of having two professors in one Institute, and more importantly about the financial commitment needed to guarantee Hopkins' research and pension, proved intractable. Though Hopkins had written to Hill to say that he "should have loved to be freed for research", Hill in fact believed that Hopkins had been reluctant to push for change due to the pleading of his "young friends [who] fearing that a more determined and less pliable head of department would be appointed—so that it would not be a 'free for all' there—persuaded him not to accept."[38]

Why should the physiologists have been so concerned about the direction of the Dunn Institute that they were willing to expend their time and energies in relieving Hopkins of its administration? It is clear that, for those who were heavily or even primarily involved in the administration of science, such as Fletcher, Hardy and Dale, a significant motivation was the possibility of getting a "big new piece of work" out of Hopkins. Writing in reply

[33] Meetings of the Financial Board, 5.iii.1924, 16.vii.1924, 3.xii.1924, 4.iii.1925, 6.v.1925, 13.v.1925, 20.v.1925: UCA Min.II.8.

[34] F G Hopkins to G H A Wilson (Secretary, Financial Board), 12.v.1925, 19.v.1925: UCA Min.II.8.

[35] Barger, an organic chemist of repute, had studied at Cambridge between 1898 and 1901. After a spell as demonstrator in botany at Brussels, he returned to Cambridge in 1903 before becoming (with Dale) a member of the staff of the Wellcome Physiological Research Laboratories. In 1919 he became the first Professor of Chemistry in Relation to Medicine at the University of Edinburgh.

[36] Details of these negotiations are contained in the as yet uncatalogued archives of the Beit Trust held at the Contemporary Medical Archives Centre of the Wellcome Institute for the History of Medicine, London (CMAC). Box 56 of these archives is entitled 'Proposed professorial fellowship for F G Hopkins'. We are grateful to Dr E M Tansey for informing us of the existence of this archive, and to Ms J Sheppard for her assistance in locating the relevant documents. A V Hill's part in the scheme is also documented in his correspondence with Hopkins (CC AVHL I 3/38) and Barger (CC AVHL 4/6).

[37] When informed of Bulloch's opposition, Dale told Elliott that they should "need a cow-catcher to sweep all bovine obstruction from the rails": H H Dale to T R Elliott, 27.vii.1927, CMAC Beit Papers, Box 56.

[38] CC AVHL I 3/38.

to a suggestion by J Kingston Fowler, the Secretary of the Beit Trustees, that the proposal amounted to a "Hopkins Relief Bill", Elliott claimed that:

> There is no 9d for 4d in this proposal, but the very converse. Hopkins would be invited to surrender his own financial advantage for the sake of the scientific work that we, and he, wish to see done for our advantage.[39]

But there was more to the proposal than giving Hopkins more time for research, which could have been achieved simply by providing him with more effective secretarial and administrative assistance. The physiologists, as we have noted, did not like the way in which Hopkins ran his Institute. The almost proprietorial interest of Fletcher and Hardy in the Dunn Institute may be explained by their involvement in procuring the Dunn funds for Hopkins; the interest of the other physiologists may have had more to do with their concerns for the maintenance of the scientific primacy of their university. The Dunn Institute was a highly visible expression of the prestige of Cambridge science; anything that threatened its efficient administration was to be decried by the loyal sons of that *alma mater*, even if that meant replacing as its head the scientist for whom it had been built. The timing of the physiologists' move is suggestive. The biochemists had been in the building long enough for Fletcher to become exasperated with the lack of the kind of work, especially vitamin research, which promised ultimately to lead to medical applications; the early financial returns of the Institute were showing that expenditure was running at unexpectedly high levels;[40] and the Institute's name had recently been associated with the scandal surrounding the Reader in Biochemistry, J B S Haldane. In 1925 Haldane was dismissed from his Readership for acts of "gross immorality" after being cited in the divorce case of his future wife Charlotte Burghes. Haldane, supported by Hopkins and the National Union of Scientific Workers, had taken the case to a private tribunal, where his dismissal was overturned. Haldane had already scandalized the scientific community with his speculative essay *Daedalus, or science and the future*, based on a talk given to the Cambridge Heretics in 1923, and had been the model for striking non-conformist intellectuals in recent novels by Ronald Fraser and Aldous Huxley. His presence in the citadel of Cambridge science, in a position to influence the mores, scientific and otherwise, of a rising generation of young scientists, must have been a source of genuine concern for more conservative figures such as Hill, who espoused the line that science and politics did not mix.[41]

Hopkins' usual defence to criticisms of his administration of the Dunn Institute was to suggest that his critics did not properly understand the difficulties involved in running such a large-scale research school. At the time of the Financial Board's investigations in 1925, he defended himself against charges of extravagance by noting, somewhat acidly, that he "had many years experience to fall back upon of research done under conditions of financial stringency".[42] When Anderson and Fletcher went through his departmental finances in detail in 1925 and 1927, they were forced to conclude that the total expenditure

[39] T R Elliott to J K Fowler, 28.vii.1927, CMAC Beit Papers, Box 56.

[40] F G Hopkins to J Colman and W M Fletcher, UCA BCHEM 3/3 (26); PRO FD1/1836.

[41] G Werskey, *The visible college. A collective biography of British scientists and socialists of the 1930s*, London, 2nd edn, Free Association Books, 1988, pp. 77–100.

[42] F G Hopkins to G H A Wilson, 19.v.1925: UCA Min.II.8.

of the Institute was small in proportion to its size.[43] Hopkins continued to resist change in the administration of the Dunn Institute by the simple expedient of refusing to retire. By the mid-1930s, Hopkins seemed to have settled on 1938 as an appropriate time to step down, but in the event he did not do so until 1943.[44] Even after Hopkins' retirement his disciples in the Institute attempted to resist further change by promoting a positive interpretation of his style of leadership and stressing the breadth of his scientific interests and achievements. Their account of Hopkins' life and work was just one of the competing "Hopkins traditions" which were constructed in the 1940s at the time of the appointment of his successor in 1943, and after his death in 1947. We now turn to these traditions.

Creating Traditions: Images of Hopkins in the 1940s

Hopkins finally retired in 1943.[45] The choice of the electors fell firstly upon Alexander Todd, Professor of Organic Chemistry at Manchester, and subsequently, after Todd's refusal, upon A C Chibnall, Professor of Biochemistry at Imperial College since 1929.[46] No record exists of the deliberations of the Board of Electors, but its composition gives some clue to the reasons behind their choices. Joining the Vice-Chancellor of the University (who sat *ex officio* on all the boards of electors) were the physiologists Hill, Dale (Todd's father-in-law) and E D Adrian, the chemists E K Rideal, H S Raper and Robert Robinson, and the pathologist H R Dean, all of whom were of a like generation.

[43] W M Fletcher to F G Hopkins, 28.vii.1927, PRO FD1/1836.

[44] Note by A L Thomson of meeting with Hopkins, 3.ii.1936, PRO FD1/3794; F G Hopkins to S C Roberts (Cambridge University Press), 29.iv.1936, UCA Pr.A.H.816. Hopkins did not have to retire because his appointment to the chair predated Royal Commission reforms of the early 1920s which instituted a mandatory retirement age of 65 for professors in the University of Cambridge. His appointment also predated the introduction at Cambridge of a superannuation scheme for university teachers; for Hopkins to retire on an adequate pension he therefore had to rely partly on a supplementary pension scheme run by the University which promised (but did not guarantee) a reasonable percentage of his professor's salary, and partly on being appointed to a life Fellowship by Trinity College. As the physiologists had discovered in the late 1920s, however, one had to have been a fellow of Trinity for 25 years to qualify for such an appointment, and Hopkins had only been so honoured in 1910. In 1929 Hopkins told the American biochemist L B Mendel: "I am already somewhat old to occupy my Chair, and yet find it essential that I should postpone resignation for a few years longer. This being so, I must do all that I can to remain efficient.": F G Hopkins to L B Mendel, 14.iv.1929, UCA Add MS 7620/A.

[45] It is not clear whether Hopkins had any influence over who would succeed him. At the time of the proposal to grant him a professorial fellowship, Hopkins told Elliott that if Barger were not to agree to the plan, he did not know who else would be suitable: F G Hopkins to T R Elliott, 14.vii.1927, CMAC Beit Papers, Box 56. The only other scientist whom he mentioned as a possibility was Edward Mellanby. In May 1933 Mellanby was offered a Cambridge chair in pharmacology: 'Report of the Council of the Senate on the establishment of a Sheild Professorship of Pharmacology', *Cambridge University Reporter*, 1932–33, p. 1102. Mellanby refused the offer, probably because he had been invited to succeed Fletcher as Secretary of the MRC: 'Report of the General Board on the establishment of a Sheild Readership in Pharmacology in the Faculty Board of Biology 'B' and of a Sub-Department of Pharmacology in the Department of Physiology', *Cambridge University Reporter*, 1933–34, p. 366.

[46] A Todd, *A time to remember*, Cambridge University Press, 1983, pp. 61–2. Chibnall's interests lay in protein chemistry, but a recently initiated research project which he brought with him to Cambridge—an attempt to understand the relations between the structure and function of insulin—may have seemed to fit in more appropriately with existing Cambridge interest in the control of intermediary metabolism. Chibnall himself claimed that the Cambridge biochemist Shirley Hele had written to him in 1943 to tell him that he was the unanimous choice of "the people here", though it is unclear whether he meant in Cambridge, or in the Dunn Institute: A C Chibnall, 'The road to Cambridge', *Annual Rev. Biochem.*, 1966, **35**: 1–22.

We have seen that Hill did not approve of the way in which Hopkins' "young friends" conducted themselves; it is reasonable to assume that his opinion was shared by many of the electors. For scientists such as Hill, the appointment of an experienced outsider was an ideal opportunity to impose some order on the Dunn Institute. Another interested and influential onlooker—Joseph Barcroft, the Cambridge Professor of Physiology—was also anxious to fill the chair with an outsider with administrative experience.[47]

That outsiders found the administrative condition of the Dunn Institute problematic is clear from the writings of both Todd and Chibnall. Todd recalled that one of his reasons for turning down the appointment was that:

> There was no real unity of purpose in the department. It was a series of little independent kingdoms sharing the departmental budget between them and the only gesture to unity was an almost sycophantic attitude to Hopkins on the part of the leaders in each of them.[48]

Chibnall recalled that when he arrived in Cambridge, he found that the Institute consisted of several more or less independent research teams thrown together under one roof.[49] That he did not wish to maintain this loose federation of research teams is clear from his immediate moves to establish formal sub-departments of enzyme biochemistry under Malcolm Dixon, and of chemical microbiology under Marjory Stephenson. His dislike of the loose administration of his predecessor is also evident from his dealings with the MRC over the question of a stipend for Dorothy Needham, during the course of which he recalled that the high marriage rate in the Dunn Institute—"the hanky-panky that went on in this laboratory between 1923 and 1930"—had been regarded as scandalous, and had led to the MRC changing its policy on the stipends payable to married women.[50] For Chibnall, the "Hopkins tradition" was a purely scientific one. Stressing the importance of the Department's enzyme research in a document on the post-war needs of Cambridge Biochemistry, drafted in January 1944, he wrote of the "Hopkins tradition of the study of *dynamic* biochemistry".[51] On the other hand, when Chibnall resigned the chair in 1949 to concentrate on research, Hill reported to Dale that "Chibnall does not really feel himself to be interested in 'dynamic' biochemistry; which is one of the reasons for giving up the Chair."[52] It is important to stress that for Chibnall (and presumably Hill and the other electors), dynamic biochemistry was not the fundamental science of life that Hopkins had promoted, but was a restricted part of the subject concerned with enzyme action and intermediary metabolism. If this narrow way of understanding dynamic biochemistry was widespread, it would explain why Hopkins continued to promote his vision of dynamic

[47] Chibnall, op. cit., note 46 above. In their Royal Society obituary notice of Chibnall, Synge and Williams state that Robinson may have been keen to have a more "chemical" biochemist appointed: R L M Synge and E F Williams, 'Arthur Charles Chibnall', *Biographical Memoirs of Fellows of the Royal Society*, 1990, **35**: 55–96. Todd recalled that Robinson was one of the people keenest to persuade him to come to Cambridge, and indeed was instrumental in his accepting the Cambridge chair of Organic Chemistry in 1945: Todd, op. cit., note 46 above, pp. 63–4.

[48] Todd, op. cit., note 46 above, p. 62.

[49] Chibnall, op. cit., note 46 above, pp. 17–18.

[50] A C Chibnall to E Mellanby, 5.xi.1945, PRO FD1/362. Norman Heatley has informed one of us (MWW) that the Institute was widely known as 'Hoppy's Dating Agency'.

[51] A C Chibnall, 'Post-war needs of the Department of Biochemistry', UCA BCHEM 1/2* (original emphasis). A draft version of this document with Joseph Needham's amendments is in UCA BCHEM 4/2 (21i).

[52] A V Hill to H H Dale, 28.iii.1949, CC AVHLI 3/12.

biochemistry with such vigour until the end of the career: his vision was misunderstood, and the "Hopkins tradition" as presented by Chibnall was but a pale shadow of Hopkins' approach to the subject.

Chibnall and Todd's recollections of the loose nature of the administration of the department have been taken as evidence of Hopkins' failure to keep the Institute under control, but they could equally well be regarded as evidence of precisely the opposite point of view: that is, that it was Hopkins' skill as an administrator that allowed his researchers the maximum amount of intellectual freedom without allowing them to stray too far from the basic tenets of dynamic biochemistry. Hopkins' "young friends", to whom he stood *in loco parentis*, or as a revered elder, embodied their feelings for Hopkins within their own version of the tradition, one which we term the "Hoppy tradition" to distinguish it from the restricted scientific tradition outlined above.

The "Hoppy tradition" consistently stressed the breadth of Hopkins' scientific interests, and the sheer number and fertility of the areas of research which he opened up, only to allow others to exploit them; it also emphasized that he did not impose his ideas on his staff, but instead let talented researchers follow their intuition and eke out their own areas for investigation. But those who constructed the "Hoppy tradition" were not just interested in the content of Hopkins' science; they were also concerned to show the value of the style of Hopkins' scientific leadership. The "Hoppy tradition" stressed the warmth and generosity of Hopkins' personality, and noted that his "courage and tenacity of purpose" was "somewhat at variance with his gentle, slightly hesitating manner".[53] The "Hoppy tradition" placed Hopkins *in loco parentis* not just to the Cambridge scientists who were closest to him, but to all British biochemists. Malcolm Dixon, for example, dubbed Hopkins "the father of British biochemistry",[54] an epithet later used by Ernest Baldwin, and by several of those who delivered the Biochemical Society's Hopkins Memorial Lecture, established in 1958. Rudolph Peters, delivering the first of these lectures, said:

> Though other countries and other societies will have their heroes, to us British biochemists in the Biochemical Society Sir Frederick Gowland Hopkins will always be our Master biochemist, and it is fitting that his name and influence should be commemorated in a lecture.[55]

The Needhams dubbed Hopkins the *Fundator et Primus Abbas* of biochemistry in England and when, in October 1961, the Royal Society held "A Symposium on Biochemistry and Nutrition to celebrate the centenary of the birth of a former President of the Royal Society, Sir Frederick Gowland Hopkins, OM, 1861–1947", Joseph Needham stated in his opening address that:

> . . . our meeting today symbolizes the feeling of discipleship which we all have for Frederick Gowland Hopkins, essentially the founder of modern biochemistry in our country.[56]

Hopkins' colleagues had good reason to be grateful for his administrative skill: he had allowed them to work more or less unhindered on whatever interested them, he had

[53] M Stephenson, 'Frederick Gowland Hopkins, 1861–1947', *Biochem. J.*, 1948, **42**: 161–9.

[54] M Dixon, 'Sir F Gowland Hopkins, OM, FRS', *Nature*, 1947, **160**: 44–7.

[55] R A Peters, 'The faith of a master in biochemistry. The First Hopkins Memorial Lecture', *Biochem. J.*, 1959, 71:1–9.

[56] J Needham, 'Opening Address [of a Symposium on Biochemistry and Nutrition to celebrate the centenary of the birth of Sir Frederick Gowland Hopkins]', *Proc. R. Soc. B*, 1962, **156**: 289–94.

worked hard to acquire their grants from diverse sources including Cambridge colleges, government departments, and international agencies such as the Rockefeller Foundation, and he had defended their interests when others had demanded increased contributions from them in the form of bench fees.[57] Hopkins' gentle approach, combined with the presence of ebullient and controversial characters such as Haldane, made the Dunn Institute a highly unusual scientific environment in the 1920s. It was often those who came to Hopkins' laboratory from outside who were best placed to appreciate the special nature of Hopkins' leadership; for instance, in his *Reminiscences* Hans Krebs recalled that "Hopkins was the central figure, beloved and respected as a natural leader, exercising leadership from within and not from above, utterly humble, modest, gentle, but by no means weak."[58] Colleagues such as Baldwin and Stephenson noted that Hopkins was at his best in informal discussions, rather than in formal settings such as lectures.[59] Bill Pirie recalled:

> He subtly directed our interests towards processes rather than substances. The word *subtly* should be stressed. He did not direct research but influenced it by obvious interest in some aspects rather than others.[60]

Pirie also suggested that Hopkins set an example of the economical use of reagents and equipment which was emulated throughout the laboratory, and that "there was strong social pressure on anyone who seemed to be extravagant".[61] It was perhaps the way in which Hopkins relied on social emulation and people's good natures which so infuriated his friends at centres where financial and other constraints meant that they did not dare allow such latitude.

The driving forces behind the "Hoppy tradition" were Joseph Needham, who of all the Cambridge biochemists probably had the most acute awareness of the importance of tradition and history in achieving particular contemporary ends,[62] and Stephenson, whose unusual scientific enterprise had flourished in the environment fostered by Hopkins' leadership.[63] Needham had already instigated a highly successful *Festschrift* for Hopkins, presented to him in 1936 at his 75th birthday celebrations. This volume—*Perspectives in biochemistry*—was an exercise in comprehensiveness rather than selectivity, its purpose being to demonstrate the breadth of Hopkins' influence by presenting as many short essays as possible written by those of Hopkins' friends and former students who had risen to prominent positions in science and polity.[64] *Hopkins and biochemistry*, a volume

[57] See, for example, Hopkins' correspondence with the Financial Board of the University: UCA Min.II.8.

[58] H A Krebs, *Reminiscences and reflections*, Oxford, Clarendon Press, 1981, p. 92. Krebs is here paraphrasing the comments of Joseph Needham in *Hopkins and biochemistry*. Holmes, in his biography of Krebs, suggests that the latter's reluctance to desert Cambridge for a planned research institute in Palestine expressed "not just a contractual obligation, but a personal one": F L Holmes, *Hans Krebs. Volume II. Architect of intermediary metabolism*, Oxford University Press, 1993, p. 110.

[59] E Baldwin, 'Hopkins, Frederick Gowland', *Dictionary of Scientific Biography*, vol. 4, pp. 498–502; M Stephenson, 'Sir F G Hopkins' teaching and scientific influence', in: Needham and Baldwin (eds), op. cit., note 27 above, pp. 27–38.

[60] Pirie, op. cit., note 26 above, p. 124.

[61] Ibid, p. 125.

[62] Needham was closely involved with the development of the history of science in Cambridge, and himself was the author of several historical works and the instigator of the massive *Science and civilisation in China* project.

[63] R E Kohler, 'Innovation in normal science. Bacterial physiology', *Isis*, 1985, **76**: 162–81.

[64] J Needham and D E Green (eds), *Perspectives in biochemistry*, Cambridge University Press, 1937.

prepared by Needham and colleagues for distribution at the First International Congress of Biochemistry, held in Cambridge in 1949, was an altogether different affair.[65] Hopkins' many papers pronouncing on the nature of biochemistry were scattered throughout the literature; he had never written a book on the subject. What better strategy for his colleagues to adopt than to write it for him? Stephenson herself wrote:

> There comes a time in the life of every science when it requires a midwife before it can emerge and start an independent existence. What Lyell did for geology and Claude Bernard for physiology Hopkins did for biochemistry. It is a matter for deep regret that he left behind no single book embodying his conception of the subject he brought to life. His views have, therefore, to be assembled from lectures, addresses and publications belonging to various periods of his life and witnessing to his thought and teaching.[66]

The way in which the process of Hopkins' "thought and teaching" was assembled and presented provides a rather different view of the Dunn Institute from that presented by Chibnall and Todd. Interwoven with the presentations of the scientific tradition of dynamic biochemistry were accounts of the personal tradition of Hopkins' leadership and influence. There was no mention of any administrative problems. The volume contained an autobiographical piece written by Hopkins in 1937, reminiscences of Hopkins written by Stephenson (who herself died shortly before the book's publication) and by the Needhams, excerpts from Hopkins' scientific papers with a commentary by Leslie Harris, selections from the humorous in-house annual *Brighter Biochemistry*, a roster of those who had worked with Hopkins, several photographs of Hopkins, and fifteen of his public addresses, some previously unpublished, in some of which the editors italicized the passages they considered most important. *Hopkins and biochemistry* constantly harked back to a golden age in the 1920s and 1930s; the flavour of the period was clearly conveyed by the quotations from *Brighter Biochemistry*, "precious copies" of which, as it was stated in the introduction to the selections:

> . . . still bear witness to the spirit of the Institute during the years which were perhaps the zenith of Sir F. G. Hopkins' life, when he was at the head of a large Institute which he had himself created, and which was full of brightness, not only of intellect and experiment, but of comradeship, alive awareness of the world outside biochemistry, and warm inspiration owed and universally acknowledged to the leader and founder.[67]

With *Hopkins and biochemistry* given to the 1,700 biochemists from 42 different countries who attended the 1949 International Congress, the global community of biochemists from then on could not fail to acknowledge the breadth of Hopkins' interests and the successes of his style of scientific leadership. But if those who constructed and disseminated the "Hoppy tradition" hoped thereby to influence future elections to the Cambridge chair, perhaps even to install one of their own, they were to be disappointed. Even before *Hopkins and biochemistry* was published, Chibnall had been succeeded by

[65] *Hopkins and biochemistry* was prepared by an editorial committee comprising Joseph and Dorothy Needham, Vernon Booth, Malcolm Dixon, Leslie Harris, and Marjory Stephenson.

[66] Stephenson, op. cit., note 59 above, p. 37. Hopkins did in fact sign a contract with the Syndics of the Cambridge University Press to produce a textbook of biochemistry in the early 1920s, but he never even began to write it: F G Hopkins to S C Roberts, 11.xi.1924, 28.x.1930, UCA Pr.A.H.816.

[67] Needham and Baldwin (eds), op. cit., note 27 above, p. 321.

Frank Young, whose biochemical interests in metabolism and its hormonal control were consistent with the narrow interpretation of the scientific "Hopkins tradition", not with the broad view of dynamic biochemistry. Nor did he prove to be an administrator in the "Hoppy tradition". As Philip Randle recalled in his biographical memoir of Young:

> There was a strong sense of tradition that some sought to sustain. Matters came to a head with the revival of the Dunn dinner. The reaction of younger members was expressed in some ribald lines in the annual Christmas pantomime. That was the end of the Dunn dinner and, thus fortified, Frank proceeded to drop Sir William from the notepaper. Thus was born the Department of Biochemistry and a new era.[68]

For Needham and his colleagues, the brightness went out of biochemistry when Hopkins retired. Needham himself moved out of biochemistry altogether to concentrate his attention on his studies of Chinese science and civilisation, although he retained his Readership in Biochemistry until 1966.[69] Their construction of a "Hoppy tradition" did not bring back the good old days of the 1920s, but as an accessible and apparently comprehensive account of Hopkins' life and work, *Hopkins and biochemistry* became an important resource for historical accounts of the development of biochemistry, and Hopkins' role within it. The final section of this paper, which examines how Hopkins has been treated by historians of biochemistry, shows that although those who constructed the "Hoppy tradition" lost the battle of succession, they won the war of history.

Hopkins and the History of Biochemistry

During his lifetime Hopkins acquired a reputation as a brilliant scientist (as epitomized by his work on vitamins and the subsequent award of the Nobel Prize), and as a poor administrator. As we have shown, these reputations were variously reinforced and challenged in the "Hopkins traditions" constructed in the 1940s. We now aim to show how historians have drawn both on Hopkins' contemporary reputations, and on the traditions constructed in the 1940s, to site Hopkins within the history of biochemistry.

The influence of the "Hoppy tradition" can be seen in a series of works on the history of biochemistry, originating in Cambridge and Yale, including studies by Joseph Fruton, F L Holmes, and Mikuláš Teich.[70] These works, which have generally had as their starting

[68] P Randle, 'Frank George Young', *Biographical Memoirs of Fellows of the Royal Society*, 1990, **36**: 581–99.

[69] Needham recapitulated his construction of the "Hoppy tradition" at length in an extensive appreciation published in 1962: J Needham, 'Frederick Gowland Hopkins', *Perspect. Biol. Med.*, 1962, **6**: 2–46.

[70] J S Fruton, *Molecules and life. Historical essays on the interplay of chemistry and biology*, New York, John Wiley & Sons, 1972; *idem*, *Contrasts in scientific style. Research groups in the chemical and biochemical sciences*, Philadelphia, American Philosophical Society, 1990; *idem*, *A skeptical biochemist*, Cambridge, MA, Harvard University Press, 1992; F L Holmes, *Hans Krebs. Volume I. The formation of a scientific life, 1900–1933*, Oxford University Press, 1990; *idem*, op. cit., note 58 above; *idem*, *Between biology and medicine: the formation of intermediary metabolism*, Berkeley, Office for History of Science and Technology, 1992; M Teich with D M Needham, *A documentary history of biochemistry 1770–1940*, Leicester University Press, 1992; David Keilin's *The history of cell respiration and cytochrome*, Cambridge University Press, 1966, also belongs in this category, as does the volume of Cambridge biochemists' historical lectures edited by Joseph Needham entitled *The chemistry of life. Eight lectures on the history of biochemistry*, Cambridge University Press, 1970. In his introduction to this volume, Needham wrote that it should be considered as "one more laurel-wreath for 'Hoppy'".

point the internal development of biochemical science, have presented Hopkins as a brilliant biochemist with a wide range of interests and a sympathetic, effective style of scientific leadership; in other words, as one of the men (and a few women) who forged the field of biochemistry. These historians have tended to concentrate on those of Hopkins' scientific achievements—particularly his work on protein chemistry and on the chemical dynamics of muscle—which proved with hindsight to have been the starting point for other, more famous researches into the pathways of metabolism. Hopkins' work on glutathione, which appears retrospectively to have been a scientific dead end, receives only sporadic comment, as does his micronutrient research which lies largely outside the stories being told in these works.[71] None of these studies take Hopkins as their sole object; by looking at his work in a piecemeal fashion, the force of Hopkins' propagation of dynamic biochemistry has been lost, as have the intricate links between Hopkins' research and his vision of the subject as a whole.

Among those historians who follow the "Hoppy tradition" closely, the most extensive discussion of Hopkins' administrative abilities and style of leadership appears in the work of Joseph Fruton. Fruton considers "the role of the group leader in the education of his junior associates no less important than the research achievements for which he gained fame", and has extolled Hopkins' virtues of "vision, tolerance, and the ability to recognize and encourage scientific talent".[72] Fruton does not give a detailed account of Hopkins' leadership, however, and is content to follow the "Hoppy tradition", a reading facilitated by his preferential citation of accounts of Hopkins written by Needham and Pirie.[73] In his extensive biographical account of the early career of Hans Krebs, Holmes also provides a positive interpretation of Hopkins' style of scientific leadership, and is clearly echoing Krebs' own views on the virtues of Hopkins' approach.[74]

Almost the only work seriously to challenge the primacy of the "Hoppy tradition" is that of Robert Kohler in his book *From medical chemistry to biochemistry*, and in related articles. Kohler relates Hopkins' success in establishing biochemistry as a separate discipline to the unusual institutional environment of the University of Cambridge.[75] Kohler's account marginalizes Hopkins' scientific research, claiming that his distinctive programme of "general biochemistry" was derived from his reading of the scientific literature, and was put into practice by his colleagues.[76] Kohler also portrays Hopkins as

[71] In Fruton's *Molecules and life*, for example, there are three references to Hopkins and Cole's work on tryptophan, two to further work by Hopkins on proteins, two to Hopkins and Fletcher's work on muscle, and only one to glutathione, which serves as a lead into a discussion of Szent-Györgyi's elucidation of the chemical nature of vitamin C: Fruton, *Molecules and life*, op. cit., note 70 above, pp. 113, 119–20, 432; 129, 146; 340–41, 364–65; 329–30. Holmes' books about Krebs largely concern the latter's elucidation of the metabolic cycle that bears his name, and so it is unsurprising that Hopkins' work with Fletcher is emphasized both in these volumes and in his work on the formation of intermediary metabolism, a book which Holmes notes grew out of his investigations into the scientific background to Krebs' researches: Holmes, *Between biology and medicine*, op. cit., note 70 above, pp. 8–9.

[72] Fruton, *Contrasts in scientific style*, op. cit., note 70 above, p. 275. In another account he does not give a reference for this viewpoint; this is particularly curious since he later quotes Todd's unfavourable view of the Cambridge Institute in 1943 as an instance of how critical study of autobiographical accounts can furnish historians with important information about noted scientists: Fruton, *A skeptical biochemist*, op. cit., note 70 above, pp. 192–3, 225–6.

[73] Fruton, *Contrasts in scientific style*, op. cit., note 70 above, p. 109fn.

[74] See works by Holmes cited in notes 58 and 70.

[75] Kohler, op. cit., note 24 above, pp. 47–55, 73–92.

[76] Ibid, p. 74.

a poor administrator, a view based on his extensive research in the archives of the MRC and of the Rockefeller Foundation. Given the prominent role of Hopkins' physiological colleagues in running the former and advising the latter, it is perhaps unsurprising that their opinion of Hopkins' administrative skills should predominate in the archives and hence in Kohler's account. We are presented with a paradox: Hopkins was a poor administrator, he was personally unimpressive, and his research was unimportant—and yet he was a supremely successful discipline builder. How is this to be explained?

An explicit agenda drives Kohler's work on disciplinary history. Kohler clearly states the usefulness to the historian of science of the study of discipline building:

> The intellectual benefits of discipline history are congruent with the strategic needs of historians of science to consolidate their discipline and win greater support from social, economic, and intellectual historians.[77]

Kohler's aim of integrating the history of science with broader social, economic, and intellectual history is, of course, admirable. In his work on Hopkins, however, he does this at the cost of marginalizing the very enterprise which formed the centrepiece of Hopkins' life: his scientific research. By neglecting Hopkins' science, Kohler fails to appreciate the central driving force behind Hopkins' promotion of dynamic biochemistry. In its place, he puts entrepreneurship, a concept which, as we have shown, sits awkwardly with any detailed account of Hopkins' character and actions.[78]

Ironically, there is a sense in which the true entrepreneurs were Hopkins' physiological colleagues who, in their attempts to provide a space for him to pursue his research, actually built around him those institutional trappings which Kohler looks for as evidence of the discipline of biochemistry. Hopkins was not a passive figure in this process; he was, as we have shown, assiduous in ensuring that there would be as little interference as possible in his work and that of his colleagues. With the benefit of hindsight, it may be tempting, though not necessarily justified, to conclude that the outcome of this process—a separate discipline of biochemistry in Kohler's sense—was what Hopkins had been striving for all along. The key element of Kohler's retrospective reconstruction of Hopkins as a discipline builder is the establishment of the Dunn Institute. When one looks at the nature and extent of the work being done in the Institute, however, it is not clear that its establishment made much of a difference to Hopkins' programme of dynamic biochemistry, or to the personnel who carried it out. The roster of those who worked alongside Hopkins in the years between the end of the First World War and the move to the Dunn Institute contains many of those on whom the reputation of the Institute was

[77] Ibid, p. 2.

[78] Kohler's methodological approach to the study of discipline building privileges the role of the scientist-entrepreneur, a concept derived from the work of Charles Rosenberg on the American agricultural research stations. To apply this post-World War II American concept to fundamental research of the late nineteenth and early twentieth centuries is problematic, to say the least. We here share the concern of Joseph Fruton that "those social historians of science who have projected into the nineteenth and the early twentieth century the 'political economy' of the present-day biochemical sciences may have been unduly influenced by the current social climate of these sciences": Fruton, *A skeptical biochemist*, op. cit., note 70 above, p. 193. More recent historical scholarship has begun to provide us with a more thorough understanding of what the notion of a scientific discipline actually connoted in the past. An excellent recent study is K M Olesko, *Physics as a calling: discipline and practice in the Königsberg seminar for physics*, Ithaca, Cornell University Press, 1991.

built: Malcolm Dixon, J B S Haldane, Dorothy Moyle (later Needham), Marjory Stephenson and Joseph Needham, for example.[79] Although the number of papers published each year by the members of the Department rose from an average of 8.6 between 1918 and 1922 to an average of 38.6 a decade later (1928 to 1932), the main jump occurred in 1923, before the move to the Dunn Institute, when 38 papers appeared as opposed to the 11 that had appeared in 1922.[80] Work on the subjects which accounted for most of this expanded output—protein chemistry, carbohydrate metabolism, and the control of biological processes—had begun to appear in 1921, also well before the move to the Dunn Institute.[81] Dunn money was of course crucial in shoring up departmental finances, and the physical presence of the Dunn Institute a powerful symbol of the importance of biochemistry, but it is none the less likely that had the Dunn bequest not been directed to Hopkins, much of the work that later came to be associated with the Institute would have been carried out regardless.

We suggest that the institutional development of biochemistry in the context of the University of Cambridge is necessary but not sufficient to explain Hopkins' actions. As our first paper showed, Hopkins did not stop promoting dynamic biochemistry as the fundamental science of life after he had acquired a secure institutional footing. There was more to Hopkins' biochemistry than the bricks and mortar that made up the Dunn Institute. As we have shown in this paper, Hopkins was always more concerned about people than about buildings. Dynamic biochemistry was about simplicity, unity, individuality and "brightness", and it does not appear from our discussions of the rhetorical and historical strategies that Hopkins employed to promote his subject, that he believed a complete institutional demarcation of biochemistry from other sciences was fruitful, or indeed possible. His move to the Institute of Animal Nutrition attached to the University's School of Agriculture in 1912 shows his willingness to go where bright students and research funding seemed most likely to be available.[82]

Our first paper demonstrates that in Hopkins' case the development of the institutions for which he was responsible cannot be understood without reference to his particular vision of dynamic biochemistry, which in turn cannot be understood without reference to Hopkins' own research. Understanding this may help resolve the paradox in Kohler's account. Kohler's statement that Hopkins' programme developed out of his reading rather than his own research is simply wrong. Hopkins' view of dynamic biochemistry acquired new depth, as we have shown, as his own research progressed. General biochemistry, which Kohler cites as the characteristic of the Cambridge school, was, we have argued, a later stage of Hopkins' construction of dynamic biochemistry. His vision was dynamic biochemistry: a vision crafted from the very training and the very times in which Kohler claims Hopkins was captured. Hopkins was active in promoting dynamic biochemistry as a meeting-place for biologists and chemists. It was left to others—to his colleagues, his

[79] Details of those working in the laboratory can be gathered from the annual reports of the Department's activities submitted to the General Board of Studies, and published in the *Cambridge University Reporter*.

[80] Bound volumes of the collected papers of the Department of Biochemistry are held in the Library of the Department.

[81] Ibid.

[82] The almost complete absence of Hopkins' brief alliance with the agriculturists from Kohler's account suggests that Kohler has unduly privileged Hopkins' problematic relationship with the Cambridge physiologists in order to cite it as an example of a more general schism.

students, and to historians—to set that meeting-place apart and call it a "separate discipline".[83]

Conclusions

In these two papers we have shown how it is possible to reach novel, nuanced views of the life and work of scientists by considering not only how they viewed their own enterprise, but also how others viewed it. To understand the making of Frederick Gowland Hopkins, for example, it is necessary not only to follow him at work in and out of his laboratory, but also to watch those around him, to see where they were going and what they were doing, and to understand why they were interested in him, and why they attempted to influence him. It is only then that one can begin to understand why the process of the construction of dynamic biochemistry, and Hopkins' persistence in promoting it, do not appear in existing accounts.

There are of course questions that we have not been able to address in detail in these papers. One particularly important issue which has only been touched upon is the question of the relationship of Hopkins' dynamic biochemistry with medicine, and with clinical science. We have chosen to portray the construction of Hopkins' reputation as the "discoverer of vitamins" as an instance in which, in the 1920s and 1930s, Hopkins acquiesced in, and indeed actively promoted, the dissemination of an image of him that did not accord with his actual research activities. It may be the case, however, that this sobriquet caused Hopkins more trouble than we have indicated; to be identified so closely with the discovery of substances with such medical importance may have actually created problems for Hopkins when he came to argue for continued support for research which seemed unlikely to offer immediate medical applications. Hopkins was certainly ambivalent about clinical research, and clinicians generally. His recollections of his time as a research student at Guy's Hospital in the 1890s vary; in his autobiography, he states that the clinicians there were interested in his work and sympathetic to it,[84] but in an address entitled 'The clinician and the laboratory worker', dating from 1931, he stated that to anyone "who became stamped as a laboratory worker the wards were closed and there was definite antagonism to his attempting to make any contact with cases".[85] This is clearly an issue which needs further attention.

It is not until one separates out Hopkins' own programme from the way in which that programme was understood by his contemporaries that one can begin to resolve the tensions between, and indeed within, existing accounts of this intriguing scientist. The received view of Hopkins—that is, that view which would be formulated by a careful

[83] Of Hopkins' obituarists, only two (Marjory Stephenson and Henry Dale) suggested that he had attempted to establish biochemistry as a "separate discipline". Both clearly understood "discipline" in terms of scientific approach rather than in any institutional sense: H H Dale, 'Hopkins, Sir Frederick Gowland (1861–1947)', *Dictionary of National Biography 1941–1950*, pp. 406–08; Stephenson, op. cit., note 53 above, p. 168.

[84] F G Hopkins, 'Autobiography', in Needham and Baldwin (eds), op. cit., note 27 above, p. 18.

[85] F G Hopkins, 'The clinician and the laboratory worker', in Needham and Baldwin (eds), op. cit., note 27 above, p. 206. These comments should be set against the background of the ongoing debate of that period about the control of medical research, as should be Hopkins' vehement denunciation of clinical science in his 1934 Presidential Address to the Royal Society: F G Hopkins, 'Address of the President', *Proc. R. Soc. B*, 1934, **116**: 403–27.

survey of the existing literature on Hopkins—is, as we have shown, full of such tensions.[86] The "Hopkins" of Needham and Baldwin is not the same person as the "Hopkins" of Kohler. We have shown in this paper that these tensions may in part be explained by the sources used by different authors, and the purposes for which they wrote their accounts. In their place we present a novel interpretation of Hopkins, which teases out his own intentions from those of his colleagues and pupils. If there is a paradox within our own work, it derives from the fact that in order to understand the "making of a biochemist", we have had to separate strands of thought and action that were, historically, inextricably intertwined. We hope that, when both papers are read together as one account, these separate strands are brought back together to give the reader a new perspective on Hopkins and, more generally, on the making of scientific reputations.

[86] This received view can be found in our previous publication dealing with Hopkins: M W Weatherall and H Kamminga, *Dynamic science: biochemistry in Cambridge, 1898–1949*, Cambridge, Wellcome Unit Publications, 1992. This booklet was prepared to accompany an exhibition on Cambridge biochemistry; although it contains some original work on the period up to 1924, it is otherwise largely derived from accounts by Hopkins' colleagues and by historians.