

bacterial cells.⁷² The following year a chance observation on one of these enzymes revealed that it required a cofactor similar to respiratory cofactors from animal cells. Stephenson and Gale found themselves in the middle of the hottest biochemical game in town with the right system and the right skills to compete with Otto Warburg and other top biochemists.⁷³ The challenge was irresistible, and the uncertainties of variability were set aside for "pure" enzymology.

The coming of war in 1939 disrupted all ordinary routines. Stephenson's group became involved in various industrial and medical projects,⁷⁴ and she never returned to bacterial variability. After the war she busied herself with organizing her new Research Unit in Chemical Microbiology and became active in the MRC committee that oversaw and funded the unit's work. Stephenson played a leading role in organizing the Society of General Microbiology, rewrote her 1930 monograph, organized a special degree course in bacterial biochemistry, and ran a successful summer school at Cambridge to train workers in the newly fashionable field.⁷⁵ Bacterial biochemistry had come of age, in large part because of the intellectual program that Stephenson had evolved in the 1920s and 1930s. Her ideas, however, went out of fashion after 1945. She died in 1948, just as the new bacterial genetics was beginning to change radically the way biologists thought about bacterial physiology.

V. CONCLUSION

It is clear in retrospect how important Stephenson was in defining a new research specialty. But as with much innovation in normal science, the full measure of her achievement was not obvious even to informed and sympathetic observers. When Stephenson returned from New York in the fall of 1931, she sent Fletcher a glowing report on the bacterial physiologists at the Rockefeller Institute. A lament came back:

 All this significant work in bacterial chemistry in [the] U.S.A. is what I had expected. It is the sort of activity and progress some of us fondly hoped ten and more years ago might be made in England, and not least in Cambridge. But our bacteriologists were not ready for it then, and the biochemists in various ways have got segregated. We might have taken the lead, but now we must try to catch up after a slow start. It saddens me to think that there is no work at all of this kind even beginning in either of the two great palaces for bacteriology in Cambridge and Oxford. Let us hope that this will rapidly change. Your own work . . . is among the really bright spots that relieve the gloom elsewhere.⁷⁶

Shortly before his death in 1933, Fletcher again revealed his sense of failed hopes for bacterial physiology at Cambridge and Oxford, despite the most advantageous conditions for cooperation between biochemists and bacteriologists.⁷⁷ Although few of the MRC's projects in chemical microbiology had be-

⁷² David Green to Edward Mellanby, 21 July 1936; Mellanby to Stephenson, 5 Aug. 1936; and Stephenson, Report to the MRC, July 1939; MRC 2036/2.

⁷³ Stephenson, Report to the MRC, 7 Sept. 1938, July 1939; MRC 2036/2. Stephenson and E. F. Gale, "1-Malic Dehydrogenase and Codehydrogenase of *B. coli*," *Biochem. J.*, 1939, 33:1245-1256.

⁷⁴ Stephenson to Mellanby, 4 Oct. 1939, 20 Mar. 1940, 27 Oct. 1941; and Stephenson, Report to the MRC, 6 Sept. 1941, 8 Sept. 1943; MRC 2036/2.

⁷⁵ Stephenson to Mellanby, 30 May 1944, MRC PF 106; Mellanby to Charles R. Harington, 11 Feb. 1944, MRC 497, and Robertson, "Stephenson" (cit. n. 15), pp. 574-575.

⁷⁶ Fletcher to Stephenson, 9 Nov. 1931; Stephenson to Fletcher, 8 Nov. 1931; MRC 2036/2.

⁷⁷ Walter Fletcher, "Researches in Bacterial Chemistry," 19 May 1933, MRC 1300/1.