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historical dimensions of one of these questions; the implications of instrumental diagnosis. He charts the rise of the major diagnostic instruments of the nineteenth and early twentieth centuries, and chronicles their gradual and not always enthusiastic adoption by the medical profession. He vividly illustrates both the profound diagnostic possibilities of such instruments and the more constant and frighteningly large factor of human fallibility. This latter he does not regard as being circumvented; he makes no plea for bigger and better instruments. What the work lacks is a slightly more suspicious approach to group interests. He makes, for instance, no reference whatsoever to the market relations of medical technology. Who are the manufacturers, and retailers? How is medical technology "sold" to the profession? He sees an obvious relation between technology and medical specialization, yet he does not investigate the possibility that specialists might pursue complex technology to advance their claims. Rather he portrays technology as passively calling forth special skills. Nor does Reiser place his findings in the broader context of industrial society. How important, for instance, is medical technology in removing responsibility and insight from the patient? What is the relation of this to the profession's putative role as a channel of social control? These however are methodological questions. Reiser's work has gone a long way to providing the material that makes it possible to ask them.

GERALD L. GEISON, *Michael Foster and the Cambridge School of Physiology: The scientific enterprise in late Victorian society*, Princeton University Press, 1978, 8vo, pp. xxi, 401, £18.40.

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After a slow start in the mid-nineteenth century, English experimental physiology drew level with its continental competitors and perhaps in the photo-finish can be judged to have won. Aspects of this familiar but neglected race have recently received detailed attention from two historians, Richard French and Gerald Geison. French's superb study was, amongst other things, a sympathetic account of the effectual disappearance of the moral issue surrounding vivisection in the face of the interests of the scientific community and such contingencies as the entropic allegations and increasingly crank image of the antivivisectionist lobby.¹ Geison's account of the rise of the Cambridge school sounds a rather different note. His initial chapters are a splendidly lucid narrative of the relations between the institutional framework and substantive content of mid-century physiology with its hinterland of natural theology. The singularity of English medicine appeared to the protagonists of the nascent science of experimental physiology as a check on their prodigy's growth. It is their perspective which Geison adopts, and this is what makes his history both cogent and partial.

Taking his cue from Ravetz's work, and Morrell's study of Liebig's laboratory,²

¹ Richard D. French, *Antivivisection and medical science in Victorian society*, Princeton University Press, 1975.

² Jerome R. Ravetz, *Scientific knowledge and its social problems*, Oxford, Clarendon Press, 1971. J. B. Morrell, 'The chemist breeders: the research schools of Liebig and Thomas Thomson', *Ambix*, 1972, 19: 1-46.

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Geison delineates with great skill those ingredients that constitute the nebula that is a successful research school; a central problem, a set of techniques, a particular training programme, and, most significant in this case, the scientific and “political” abilities of the leader. He develops a convincing argument for the reasons why it should have been Cambridge, and Trinity College in particular, where physiology evolved in the 1870s. St. John’s College was probably the other contender, but Geison remarks only that it had a “scientific laboratory of sorts” (p. 90), though it was here that the young Alfred Henry Garrod was engaged, as an undergraduate, in some brilliant experimental work.

However, there can be little doubt about what constitutes Geison’s major achievement. The book in part has joined the small but growing crop of studies that have transcended the dichotomy between “external” and “internal” history of science. He persuasively suggests that the Cambridge physiologists’ solution to the problem of the origin of the heartbeat came from their particularly English commitment to an evolutionary view of nature. He proposes that Foster’s adherence to the myogenic theory was a twin to his doctrine of the “physiological division of labour” with all its social resonances. The Cambridge approach is forcefully compared to the centralizing neurogenic theory upheld by the Germans in a Bismarckian autocracy. Given this argument, his dismissive and vaguely articulated gibe at “the patently absurd notion that Darwin’s achievement can be explained in any meaningful way by referring it to the economic system of Victorian England” (p. 348) is incomprehensible. There is a “meaningful way” in which he has done just this for Foster—and, by inference, for Darwin too.

Geison’s book is an impressive testimony to meticulous scholarship, for, from a host of detail, perhaps too much for some readers, he is able to sustain some very general arguments about the “style” of English physiology and its particular success. However, his achievement should not seduce us into adopting his particular vision of the past. His field is the “rise” of physiology, and its attendant “obstacles”. His is the metaphor of the scientific race (p. 150). His subject, after all, is as much an artifact of the historian as it is an independent reality. There may well be more than a whiff of teleology in any argument that examines the environment that nourishes and inhibits the growth of an embryonic science. David Allen, drawn to the same theme but from another direction, that of the naturalist, could see it quite differently.³ Babington and Newton, for Geison, were obstructionists “men of the old school . . . not disposed . . . to appreciate the value of laboratory biology” (p. 119); for Allen they were “in a way . . . right”.⁴ For him the “dedicated professionals” were “militant anti-amateurs . . . who robbed . . . field studies for many years of a much needed intellectual dynamic”.⁵ Saying this is not necessarily to advocate more natural history and less experimental physiology. I am merely suggesting that Geison has a perspective—one that sees “the value” of laboratory biology.

³ David Elliston Allen, *The naturalist in Britain*, London, Allen Lane, 1976.

⁴ *Ibid.*, p. 184.

⁵ *Ibid.*, p. 184.