

tions of knowledge production. El Shakry shows that social science was not imported wholesale into Egypt but took on an Egyptian “nationalist” complexion as it developed, the result of which was the emergence of an indigenous form of social science with “a family relation to that of Europe, but one whose local practitioners self-consciously sought out other sources of inspiration” (p. 4). Indeed, the book narrates the emergence of a new form of inquiry in Egypt that reads social science through a screen of national sovereignty and modernity and according to self-conscious nationalist visions of what Egypt should become. It carefully narrates the ways in which local social science and historical knowledge developed their own trajectories based on local nationalist agendas that were supplemented by readings of figures such as Ibn Khaldun or through critical readings of classical “Arabic and Orientalist historical sources.” For example, in the second chapter, “Anthropology’s Indigenous Interlocutors,” El Shakry shows how the anthropological insights of Egyptians such as Jurji Zaydan, Salama Musa, and Abbas Mustafa ‘Ammar were based on rereadings of the current anthropological canon that ultimately entailed subversions of racialist ideas that dominated European social anthropology at the beginning of the twentieth century. Instead of trying to place Egyptians within a broad racial schema encompassing the whole of humanity, their work sought to demonstrate the unity and uniqueness of the people of the Nile Valley in support of both Arabist and “pharaonic” nationalist visions of Egypt that were current in the first half of the twentieth century. These Egyptians drew on a variety of sources to make their claims, including medieval Arab historians and genealogists and other traditional sources detailing the putative origins of ancient Arabian tribes.

However, the book is more than an intellectual history of the social scientific idea or a genealogy of its practice in Egypt. El Shakry argues that through social scientific inquiry Egyptian social reformers and government experts (who were drawn primarily from Egypt’s new middle classes, or *effendiyya*) came to view top-down social engineering as a national imperative to build a “new Egypt.” Through it they developed paradigms for conceiving of, and treating, what they perceived as social ills, such as the ignorance of the peasantry (the peasant *mentalité*) and the “quantity” and “quality” of Egypt’s population (which entailed paying particular attention to the “social uplift of women” [p. 18]). These arguments lead her to conclude that “ideas, epistemological orientations, and

structures of knowledge” are not only related to, but indeed formed by, the “social and political realities of ‘people on the ground’” (p. 220).

As welcome as it will be for those interested in the institutional and intellectual history of the social science disciplines in Egypt and in the ways that the emergent middle classes (the *effendiyya*) came to exploit this knowledge in building their own hegemonic political vision, El Shakry’s ambitious work offers much more than this. *The Great Social Laboratory* uses the story of the rise of the social sciences in Egypt to argue for a rethinking of the history of social scientific disciplines in general. El Shakry argues that the social sciences were not simply imposed on Egyptians by European practitioners; rather, local intellectuals were actively involved in their “development and transformation” on a global scale (pp. 1–2). In presenting this revisionist account of the history of social science, the book raises fascinating questions about “asymmetrical conditions of power” in the production of knowledge on a global scale but also within Egypt itself. Building on an impressively broad range of scholarship drawn from historians working on similar questions around the world, *The Great Social Laboratory* argues that within “globalized and interconnected forms of knowledge production between Europe and the Arab world,” Egypt functioned at once “as both colonizer and colonized” (p. 3). This sort of nuanced reading of the question of power in knowledge production should draw the attention of scholars not only from the fields of Egyptian and modern Middle East history, but equally from those studying the postcolonial world and the history of science.

MICHAEL GASPER

Allan Franklin; A. W. F. Edwards; Daniel J. Fairbanks; Daniel L. Hartl; Teddy Seidenfeld. *Ending the Mendel–Fisher Controversy.* x + 330 pp., illus., tables, app., index. Pittsburgh: University of Pittsburgh Press, 2008. \$27.95 (paper).

In 1936, R. A. Fisher used chi-square analysis to show that Mendel’s famous results were too *close* to Mendel’s expectations. Fisher said that it “remains a possibility” that “an assistant” falsified the results. Since the mid-1960s, the “Mendel–Fisher controversy” has raged: Are Mendel’s results “too close”? Does this prove fraud? Did Fisher accuse Mendel of fraud?

Chapters 4–7 are four previously published articles selected by Allan Franklin, with their author(s) adding a brief postscript. An overview

(Ch. 1) by Franklin discusses the history of the controversy. Also included are Mendel's and Fisher's articles (Chs. 2 and 3) and an appendix about basic probability theory. The book is aimed at those interested in the Mendel–Fisher controversy who want to read original articles: presumably, mostly students of philosophy and history of science—but others can understand the presentation (as long as they have some statistical background) thanks to the lack of philosophical jargon.

The book claims that it is time to end the controversy: Mendel's results *are* generally too close to his expectations, essentially for the statistical reasons Fisher gave. The reason is still unknown—and, at this date, probably unknowable. However, there was no fraud, nor did Fisher accuse Mendel of fraud. This book conclusively proves its thesis.

The articles (and Franklin's overview), whether historical and biographical (Ch. 5), statistical (Chs. 4 and 6), or a critical overview of the literature (Ch. 7), pay close attention to the facts: Mendel's and Fisher's actual words, the botanical properties of pea plants, and so forth. Also, the authors don't allow their own prejudices to lead them to minimize contrary evidence. Indeed, these qualities seem to be Franklin's main reason for including these essays.

All the authors agree that Fisher's claim that Mendel's data are too close to expectation is not an accusation of fraud and that there isn't any reason to suspect fraud on anyone's part. (Teddy Seidenfeld shows that the data's peculiarities prove that any fraudster would have to be far more "lucky" than Mendel.) Many unperceptive writers have assumed that Fisher accused Mendel of fraud and have jumped to further conclusions: that Mendel committed fraud because he opposed Darwin, that Fisher was "unfair" because Mendel's results are an artifact of some speculative experimental methods Mendel "probably" used, and so forth. The book makes short work of such claims by going back to the facts: Fisher's admission that he had no idea by whom, or by what method, the data were made "too good," biographical evidence that Mendel often agreed with Darwin, and denial on Mendel's part that he used the method suggested.

The book shows that, despite many serious attempts to find flaws in Fisher's analysis, detailed reexamination shows that he was on the whole correct. However, it also shows that Mendel did not misclassify heterozygotic plants as homozygotes. This is ironic, since this was the suspicion that initially prompted Fisher to check

all of Mendel's data; but it does not invalidate Fisher's general conclusion.

The authors disagree as to *why* the results are "too good." Some argue that it is because Mendel (sometimes) used *more* than the number of plants Fisher says, others that it is because he used *fewer*. Seidenfeld suggests as an explanation that pea pollen is "correlated"; A. W. F. Edwards denies this—the plant "is an excellent randomizer" (p. 144). Once more, respect for facts: the authors speculate on the number of plants because Mendel was *not* clear about it. Seidenfeld admits, in his postscript, that tests of his "correlated pollen" model failed to support it. Suspending judgment, the book concludes that we don't know the reason for sure.

Disagreements between articles are to be expected when the selection criteria are respect for the facts and good scholarship, not *a priori* support of any particular view. Also, each of the articles (having been previously published) has its own focus, sometimes narrower and sometimes wider than the Mendel–Fisher controversy. This means that there is no small amount of irrelevant material here. Finally, naturally, the relevant parts show some redundancy. But there is no avoiding such flaws, except by deliberately selecting articles that agree with one's thesis or that fit together neatly, without regard to merit.

Other criticisms, however, can be made. The postscripts disappoint. Presumably written especially for this book, they could have focused on the controversy. Instead, each deals with additions to the original article's conclusions and concerns. The book wastes this opportunity to make itself more of a unified whole. The attempt to make the book "self-contained" is seriously misguided. Chapters 2 and 3 and the probability theory appendix are unnecessary. It is hard to imagine that anyone interested in the Mendel–Fisher controversy won't know the Fisher and Mendel articles or the definition of "binomial probability."

That aside, and returning to the heart of the book (Chs. 1, 4–7), whatever *Ending the Mendel–Fisher Controversy* loses in elegance through repetition or irrelevance is more than compensated by the sheer weight of the evidence and the quality of the articles. I believe it is the only book available that addresses the Mendel–Fisher controversy so conclusively. If this book doesn't end the controversy, nothing will.

AVITAL PILPEL