

pointed in the direction of social and cultural concerns informing the government's scientific appointments. Instead, she treats governmental acceptance, and hygienists' rejection, of Koch's bacteriology as a self-evident example of practical benefit (on the government's side) versus ideological commitment (the hygienists). Yet a very different account has been given of the relations of hygiene, bacteriology and government in France by Bruno Latour in his *Pasteurization of France*. Both French and German bacteriology are conferred with institutional power, but in strikingly different ways. Yet no contrasts are drawn, let alone explanations offered.

Not only does Mazumdar's approach obscure the social and cultural specificities of historical explanation; it also over-simplifies an explanatory framework. Two philosophical interludes—one, a heuristically-effective opening on Kant; the other, a narrative-halting exposition of Mach's ideas of scientific understanding (the whole of chapter 8)—seem to be grafted onto the text. The author relies primarily on direct transmission of scientific styles to explain her subject's continuity: "it is almost impossible to exaggerate the determining effect of this mixture of technology and intellectual patterning that is passed from teacher to student" (p. 380). Her thematic exposition itself serves as proof of this claim. Still, does not such an explanation beg important questions? Why, for instance, was Landsteiner drawn to Gruber's unitarian perspective? Is there evidence, either in personal archives or in published work, that Landsteiner had some position of general philosophical inclination *before* he studied with Gruber (or even with Emil Fisher)? What of the numerous students who passed through the laboratories of Schleiden, Nageli, Landsteiner, etc., *without* being converted to the unitarian doctrine? Is not Mazumdar herself following the "successful progress" of an idea—even if it has been a "losing" idea? To demonstrate the persistence of thematic continuity, the author narrows her focus to exclude the multi-levelled historical complexities that might distract from her narrative's coherence.

This does not mean that the narrative is simple. Sometimes, perhaps carried away by the internal complexities of her theme, Mazumdar plunges head-first into the scientific details of variations. Unfortunately, she often does this without providing the reader with insight into why such detail is significant. To take one example, she describes Landsteiner's chemical training in a style reminiscent of an organic chemistry text. Additionally, several pages are devoted to the ideas of the physical chemists who, by Mazumdar's admission, had no influence on Landsteiner at that time. Only later do we discover that the chemists' ideas link not to the eight previous chapters, but to several subsequent chapters. Without a clear statement of their relevance, these details can quickly overwhelm the narrative. The book could use a few more maps to guide the reader.

Overall, Mazumdar has composed a fine piece that, despite some methodological limitations, will raise numerous questions for historians of science and medicine. Perhaps even for those of the next generation.

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Allan Young, *The harmony of illusions: inventing post-traumatic stress disorder*, Princeton University Press, 1995, pp. x, 327, \$35.00 (0-691-03352-8).

In explicit opposition to the growing body of literature on the historical origins of Post-Traumatic Stress Disorder (PTSD), Alan Young, a professor of anthropology at McGill University, sets out to deny the timelessness of traumatic memory. While a number of recent works have purported to demonstrate the existence of PTSD-like conditions decades or centuries before the American Psychiatric Association accepted PTSD in its 1980 diagnostic manual (DSMIII), Young offers, in contrast, a self-consciously historicist approach to trauma. Revealing major sources of discontinuity in the history of traumatic memory, he argues that the condition we know

as PTSD was not discovered, but rather was created, or “glued together”, “by the practices, technologies and narratives with which it is diagnosed, studied, treated and represented . . .” (p. 5), a claim he convincingly supports in this thought-provoking, though uneven book. Significantly, Young does not deny the “reality” of post-traumatic suffering; rather his goal is to lay bare the construction of that reality through the discourses and practices of contemporary psychiatry.

Composed of three somewhat disjointed sections, which Young fails to integrate adequately, the book documents three ways in which PTSD has been constructed: by the (inaccurate) assertion of a continuous history; in the “political” and diagnostic struggles of post-war American psychiatry; and, most interestingly, through current psychiatric practice and psychiatric science.

The historical section, the weakest part of this book, analyses theories of trauma and memory from John Eric Erichsen’s “railway spine” diagnosis of the 1860s through the so-called war neuroses of the First World War. The highlight of the section is Young’s discussion of W H R Rivers and the treatment of shell-shock, in which he undermines the common view that celebrates the English neurologist as a “progressive” precursor to contemporary thinking on PTSD. However, other than refuting the linear histories written by psychiatric insiders, Young adds little new material to a subject that will be familiar to most specialized readers and was thoroughly covered over two decades ago by the Swiss medical historian, Esther Fischer-Homberger (who, strikingly, does not appear in his bibliography). Furthermore, his historical section pays regrettably little attention to national context and occasionally makes false claims, such as the assertion that the inter-war period saw little medical interest in psychic trauma, when, in fact, the so-called “accident neuroses” inspired vigorous debate among Continental doctors throughout the 1920s.

Skipping ahead thirty years, Young moves onto firmer ground in the second section, which is devoted to the creation of the PTSD

diagnosis in the post-war American psychiatric profession. Recounting the victory of the neo-Kraepelinians in their struggle against psychodynamic psychiatry, he assesses the impact of positivistic psychiatric classification on theories of traumatic memory. Diagnostic technologies, Young suggestively argues, rather than better classifying “real” conditions, actually help form the maladies they purport to identify and describe.

The third section contains a fascinating—and often disturbing—glimpse into daily life at a centre for the treatment of traumatized Vietnam war veterans. By recounting case histories and revealing the dynamics of group therapy sessions, Young shows how patients’ experiences are moulded into narratives that fit the accepted symptomological and chronological criteria for PTSD, and how the centre’s “ideology” functions to produce the desired psychiatric knowledge. His ethnographic method and the absorbing case histories make this the most compelling and persuasive section of the book.

While it is now standard for historians to show how psychiatric knowledge has been constructed by the discourses, professional contexts and social practices of past periods, it is a far more serious challenge to unveil these same processes at work in contemporary settings. That is the achievement of this flawed, but richly provocative book.

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Louis Galambos with Jane Eliot Sewell, *Networks of innovation: vaccine development at Merck, Sharp & Dohme, and Mulford, 1895–1995*, Cambridge University Press, 1995, pp. xii, 273, illus., £35.00 (0-521-56308-9).

People will ask: is this a commissioned company history? Yes and no. The authors, an American business historian and a British medical historian, say they first wrote an “internal” history of vaccine and anti-toxin development in these companies, then expanded their study with wider research and