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examinations in Padua and elsewhere. The German universities followed slavishly their Italian models. Eberhard Knab, active in Heidelberg from 1451 to 1480, was not alone in leaving memorials of his teaching in the form of *Quaestiones* (see the *Verfasserlexikon*, s.n.), and the orations delivered by Melanchthon's pupils in the Wittenberg of the 1540s and 1550s, printed among his collected works, would not have been out of place two hundred years earlier in Siena. True, they cite more authorities, and use a little less scholastic logic than their medieval precursors, but they frequently debate the same questions and use the same arguments.

In short, while Dr Lawn has again drawn attention to an unfashionable area of medieval teaching, he has barely scratched the surface. He has raised interesting questions, and made some suggestive connections, but much still remains to be done before one can see exactly what role was played in the medieval and renaissance university by the *Quaestio disputata*.

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ARTHUR STILL and IRVING VELODY (eds), *Rewriting the history of madness: studies in Foucault's Histoire de la folie*, London and New York, Routledge, 1992, pp. x, 225, £35.00 (0-415-06654-9).

This collection of papers, most of them from volume 3 of *History of the human Sciences*, is a historiographical achievement. Based on a "challenging thesis" from Colin Gordon, published in 1990 in that same journal, in which he launched a vigorous counter offensive at the critics of Michel Foucault's *Madness and civilisation* (or *Histoire de la folie à l'âge classique*, to use the original 1961 French title), it comprises various responses to that initiating article, and a new final review by Gordon himself entitled 'Rewriting the history of misreading'. This somewhat complex background may reflect rather accurately the nature of studies in the world of Foucault, but should not be allowed to detract from the essence of the arguments enclosed.

First of all it should be noted that, given that the contributions are by individuals such as Robert Castel, Jan Goldstein, Roy Porter, Andrew Scull, and Eric Midelfort, this is no minor skirmish. The smell of cordite, as well as perhaps some more metaphorical form of nerve gas is heavy in the air. Since the essays are largely written in English, with numerous quotes from the French original, the psychological grammar of English ideas is to the fore. Arguments about the meaning of individual words, of brief phrases, and of whole sentences abound. Drifting through the narrative is the persisting image of the "Ship of Fools", in particular Foucault's alluring line "les fous alors avaient une existence facilement errante". Does this mean "the mad led an easy wandering life", or, could "the existence of the mad of that time . . . easily be a wandering one"? Although much debate concentrates on this one sentence, it has to be agreed in the end that nobody knows, least of all, unfortunately, Michel Foucault himself.

As Geoffrey Pearson, Professor of Social Work at Goldsmith's College, rather succinctly points out "something goes wrong in any translation". More forcefully the linguist and translator Anthony Pugh, in a winningly erudite piece full of tropes and catachresis, feels that the work *Histoire de la folie* makes us "aware of the madness of figures and the folly of translation". He is also brave enough to admit that reading Foucault is like "listening to a very long unfinished joke: there is not just an ever increasing time-lag between reading and comprehension, but no cathartic release at all, no revelation of meaning that might restore sense to the world of appearances and misnomers". To the historian Allan Megill the work in question derives from "a machine for the generation of ambiguity from decentred antitheses", while to Roy Porter, the doyen nowadays of broad-brained English common sense, Foucault's strength lay in his linking the history of madness to the history of reason. However, Porter does also point out quite clearly the mistakes in the Great Confinement theory in relation to the English experience at least, and the meaninglessness of certain statistics. The non-applicability of the specific French experience has also been noted with regard to other countries.

But as one reads through this multivariate collection, a number of troubling thoughts intervene. Should we, already, be treating the Foucaultian oeuvre as some kind of sacred text? To be sure, Michel Foucault woke us all up from the grey slumber of accepted history. Was that a particularly

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difficult task, or was it more an accident of co-terminosity? The anti-psychiatrists who adopted his themes, the “Great Confinement” and so forth, happened to be in the ascendant at the time (the early 1960s) and embraced his anti-institutional analysis with fervid zeal despite the confusions engendered in both directions. There seems to have been little acknowledgement either that asylumdom was, in itself, the necessary precursor to current critiques, given that the putting of mad people into the “bricks and mortar” solution was an eventuality that was always going to be tried out by someone, at some time, once economically and architecturally possible. There is also no contribution here from a professional psychiatrist, although Colin Gordon decides that a suggestion from Peter Barham (a psychologist) is “typical evidence” of the psychiatric profession’s “intellectual insecurity and its propensity to moral blackmail”. This may be true, but to equate psychiatrists with psychologists, particularly in the field of anti-psychiatric social history, is, to say the least, thoughtless. This is a kind of “easy wandering lie”, that perhaps points to the existence of someone whose mind might “easily be a wandering one”.

Perhaps psychiatrists, rather than historians or sociologists or related academics, will profit most from this oddly stimulating collection. It provides a useful introduction, warts and all, to modern historiography and the forms of socio-historical analysis now enriching their past. It reveals the danger of attempting to translate ideas (let alone “discourses”) across time, culture, and language. Thus, should we translate the French “*deraison*” as “unreason”? What about “*dysreason*”, since something untried seems in order? Is there any satisfactory word at all? Should we go on arguing about Michel Foucault, or should he be decently interred with the historical plate and armour of his time? Is clarity of expression honourable, or is that just the impossible objective of the dull old English empiric? Some will welcome the spectre of “endless rewriting” generated by such questions. Others will wish to close the book, glad to have done the reading, but glad to be back in open country, clear of the foliage.

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JONATHAN HARWOOD, *Styles of scientific thought: the German genetics community, 1900–1933*, Science and its Conceptual Foundations series, Chicago and London, University of Chicago Press, 1993, pp. xix, 423, illus., £17.96 UK and Eire, \$27.50 all other countries (paperback, 0–226–31882–6).

This book is directed at two disparate audiences: historians of genetics, who will be interested in the detailed information it provides on the German genetics community, and sociologists of science, who will be interested in its wider message. I suspect that it will establish itself as a reference text for both constituencies and I also believe its conclusions to be deeply flawed.

Harwood has two major theses. The first is that German and American geneticists had different research agenda which in turn reflected different national styles: German breadth *versus* American specialization. The second is that within Germany research agenda were determined by class and education: “educated” middle-class breadth *versus* “industrial, commercial or lower” middle-class pragmatism. What both theses have in common is Harwood’s distinction between broad and narrow research agenda. Transmission genetics and cytogenetics count as narrow (or pragmatic); evolutionary and developmental genetics as broad.

My problems start when Harwood assigns these research agenda to different social and national styles, and the reason is that the agenda were largely sequential. Thus the two German schools which Harwood takes as his prototypes for broad and narrow agenda were established at quite different periods: Erwin Baur (his transmission geneticist) was running his first genetics course in 1905; Alfred Kühn (his developmental geneticist) did not enter genetics until 1920. By then the problems of the day had changed, as various German quotes affirm. And although Baur was the son of a pharmacist while Kühn was the son of a doctor, and Baur apparently gardened while Kühn read Goethe, I am not persuaded that these were the factors which determined the differences in their research agenda—particularly when counter-examples are scattered throughout the text.

These counter-examples fall into two categories: individuals who do not fit (but then there are exceptions to every rule), and those who change their research agenda (but not presumably their