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 perfervid 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 formative educational and social background) with time. Thus Richard Goldschmidt, who was head of the department of genetics at the Kaiser Wilhelm Institute for Biology until he was thrown out of Germany in the thirties, started as a powerful and enthusiastic supporter of transmission genetics and only later moved into physiological genetics. Hans Nachtsheim, Baur's assistant at the Berlin Agricultural College, initially worked on transmission genetics and later pioneered the developmental genetics of the rabbit. Even Baur took an interest in evolution; although his premature death in 1933 (so sudden that it was rumoured to be murder) prevents us from following his later career. (This episode shows Harwood at his best, recounting for non-German readers Baur's battle with the Minister of Agriculture as the National Socialists remorsely eroded academic autonomy.)

How about national styles then, a thesis which Harwood has been arguing for some time? To my mind exactly the same problem arises. Harwood's American prototype is the school of Thomas Hunt Morgan, which transformed transmission genetics between 1910 and 1915. They too moved on to "broader" questions, but this later research is almost completely ignored. "In view of the grounds on which Morgan *et al.* declined to work on evolutionary or developmental genetics . . .", Harwood states blandly, and my jaw simply drops.

How can he so misread the interests of the Morgan group after 1918? I think there are several reasons. One is that Morgan's *ex cathedra* pronouncements were often at variance with the research his laboratory was actually carrying out. Another is that the success of Morgan's transmission genetics probably obscured the research programmes which carried less immediate rewards. (And of course the gene mapping continued: the programmes were not mutually exclusive.) Both these factors probably influenced the contemporary German perception of the Morgan school (the starting point for Harwood's thesis) although in the case of Goldschmidt it is clear that Morgan's dismissal of Goldschmidt's own theory of development was also a factor. (American developmental geneticists such as Leslie Dunn were well aware of the wider interests of the Morgan school.)

But the main reason for Harwood's failure to acknowledge the later work must be that for America he is relying on the secondary history of genetics literature, which has concentrated heavily on the early history of the Morgan group. This also undermines any direct comparison with Germany, where Harwood's sample population is drawn from his survey of the German primary literature. If historians want to start drawing sociological conclusions then they should take on board the basic principle of statistical analysis which is that valid comparisons can be made only when like is compared with like. This means populations sampled at the same time point and populations identified using the same criteria: neither of these conditions is met here and the result (as Harwood himself points out) is that he is comparing fish with fowl.

This is a pity since the comparison between other statistical populations (the membership of the German Genetics Society and the Genetics Society of America for example) provides some fascinating insights. Who would have thought that in 1930 only 8 per cent of the American membership worked in genetics departments? Or that the membership of the German society in 1929 was over 20 per cent larger than its American equivalent in 1933? Taken together these statistics must undermine any simple correlation between the growth of a discipline and its academic structure and it seems to me that they form an interesting starting point for further investigation.

Finally, I have to say that Harwood has been ill-served by his indexer. To give but one example, two of the major German geneticists listed in Table 4.1 never make it to the index at all.

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