STEPHEN G. BRUSH, *The history of modern science: a guide to the second scientific revolution, 1800–1950*, Ames, Iowa State University Press, 1988, 8vo, pp. xv, 544, $39.95.

It takes a bold man to publish the course he gives to his students; and that is what Stephen Brush has done in this book, which consists of lecture-summaries with reading lists. It is comparable with the Course Units the Open University publishes in Britain, but without the illustrations: it is a dauntingly serious work, and the students must derive much profit from their course. How far anybody else would want to use it is a moot point; there is no doubt that any teacher could derive some benefit and interest from it, but the summaries are written in the style of historical introductions in good scientific texts and are highly compressed; students would find them hard going unless they had heard the lectures too, and so will a general reader. The bibliographies, going up to about two years before the book came out, are useful.

The course includes a bit about Freud, whose science some might consider to be the phrenology of the twentieth century; but it comes to life in dealing with physics and astronomy. The medical sciences do not make an appearance; it is wise in giving a course to stick to what one knows, but this is a little drastic given the importance of physiology, for example, inside and outside medicine; even chemistry has only a walk-on part. The framework is that of Kuhn's paradigms, but actually closer to the way J. T. Merz ordered his great book on the science of the nineteenth century, by themes rather than disciplines; but Merz was more systematic and his coverage wider.

The problem with focusing on modern physics is that one drifts into general science rather than serious history; and although Brush asks the right questions at the end of lectures, his summaries are generally internalist rather than contextual. Only when dealing with American astronomy, a science in which the USA was a centre of excellence long before European refugees arrived in the 1930s, does Brush wrestle with the kind of questions that most historians of science now see as the most interesting ones. There is also surprisingly little about instruments and apparatus, though, of course, experiments and observations are discussed.

In the end, then, while the book is impressive, it has the disadvantages of being rather personal without the advantages; the selection of material is pleasantly quirky, but one does not get to know one's guide, or feel that pleasure is being taken in playing with words: and to call it "the" history of modern science is a trifle misleading, especially for medical historians.

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JAN SAPP, *Beyond the gene: cytoplasmic inheritance and the struggle for authority in genetics*, Monographs on the History and Philosophy of Biology, Oxford University Press, 1987, 8vo, pp. xvi, 266, £32.50.

When I was an undergraduate, we were given a slim volume entitled *Extrachromosomal inheritance* as one of our course books. It was hard to know why; even we knew that inheritance depended on genes and that genes were on chromosomes in the nucleus. By our definition, the title itself was a paradox. What could *extrachromosomal* inheritance be? As far as we could determine, it involved peculiar things like mitochondria and chloroplasts, and kappa of *Paramecium*. No, the real things were in the nucleus. It is the purpose of Jan Sapp's book to show how this state of affairs came about, how a climate of research developed such that it was possible for us to be so dismissive of the idea that there might be such a phenomenon as cytoplasmic inheritance.

Sapp has written useful and interesting accounts of biological research that has been ignored in other studies of genetics. He covers research in pre-World War II Germany and his descriptions of the work of Sonneborn in the United States and Ephrussi in France are particularly valuable. The problems and confusions that can arise when politicians adopt and adapt science for their own purposes are well illustrated by Sapp's account of the Lysenko period. Scientists at loggerheads over the importance or even the existence of cytoplasmic inheritance joined forces to fight a common foe when extracytoplasmic inheritance was used as evidence for Lamarckian inheritance.

But the most interesting aspect of Sapp's book is his analysis of the social relations in science summarized in the chapter entitled 'Patterns of power'. He argues that the successes and failures of the various areas of research within heredity were not determined by an "intrinsic logical necessity of scientific thought". And that while the "technical advantage one research program may have over its rivals in producing results . . . *may* be enough to tip the scales in its favor" (my emphasis), success (or failure) was the outcome of a "struggle for scientific authority" that involved three factors. One is "material reality" by which I take it he means the "facts" that are discovered; the second is a competitive struggle between individuals and disciplines; and the third is the relationship between the scientific enterprise and the nature of the society in which that enterprise is going on. Sapp suggests that these social interactions were the major factors that determined success, rather than the "strength of the 'true' idea". Thus Morgan's chromosome-based analysis of genetics assumed a dominant role in the study of heredity, not only because it was producing results, but because Morgan and his followers controlled the journals, grant awards, and job patronage. The nuclear monopoly of inheritance resulted from the ability of the Mendelian geneticists to form their own discipline, to set their own objectives, and to determine what scientific knowledge came to be certified and accepted as true. In so doing they certified themselves as experts in such a way as to legitimize particular kinds of studies. The result, Sapp says, is that cytoplasmic inheritance was excluded as a legitimate field of study and its proponents were unjustly kept from the rewards given to those who indulge in successful scientific research.

I believe that Sapp is putting the cart before the horse. He suggests that a particular research field becomes successful because of the success of its proponents in establishing themselves in a position of power. What Sapp does not deal with directly is the question of *why* the proponents are able to do this. Why was it the Mendelians and not those investigators of cytoplasmic inheritance that were successful? The reason, it seems to me, is precisely because the latter's research programme was not successful. Sapp notes that the challenge to Mendelian inheritance failed in the period 1920 to 1940, not because of the intrinsic strength of a "true" idea, but because the Mendelian geneticists had an effective technique; that is the Morganists were able to do science while the evidence for cytoplasmic heredity was largely based on "vague principles". Between 1941 and 1958 the investigators of cytoplasmic inheritance "lacked the scientific techniques required to make a major change" and "only a relatively few cases of non-Mendelian inheritance were reported".

What Sapp is showing here is that the social success of an area of scientific research depends on the ability of that research to generate results and stimulate further research. Cytoplasmic inheritance failed to establish itself as a major research area not because of the ineptitude of its leaders in the social struggle (Sonneborn and Ephrussi were far from being politically inept), but because it failed to provide them with the ammunition that they needed. The Mendelian/ Morganist approach did exciting, interesting science that had tremendous explanatory power. Those interested in extracytoplasmic inheritance seemed, as Sapp says, to produce only a series of anomalies.

However, Sapp's analysis of the social relations in scientific research is, in general, correct. A brief inspection of the editorial boards of leading journals in molecular biology or of keynote speakers at international conferences shows the extent to which a relatively small group of scientists can dominate a field. This same coterie of scientists reviews grant applications and exercises job patronage, and in so doing has the power and authority that Sapp describes. I recommend strongly that research workers should read at least the introduction and the concluding chapter of *Beyond the gene*. For many of them, this view of their profession will be provocative, disturbing, and unwelcome.

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JUDITH M. HUGHES, *Reshaping the psychoanalytic domain: the work of Melanie Klein, W. R. D. Fairbairn, and D. W. Winnicott*, Berkeley, Los Angeles, and London, University of California Press, 1989, 8vo, pp. xii, 244, $30.00.