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.

This study concerns the major clinical findings and theoretical concepts of Melanie Klein, W. R. D. Fairbairn, and D. W. Winnicott in relation to Freud's theoretical elaboration of psychical phenomena. A vague set of pronouncements early on in the book about the interelationships of these investigators are later transformed into a highly lucid and interesting account of the way in which clinical material in the analytic session reshaped the theoretical domain of psychoanalysis after Freud's death.

The more exciting elements of the book include Hughes's linkage of the personal analyses of Harry Guntrip with Fairbairn, and later with Winnicott (the former analysis is based on Guntrip's lengthy unpublished record of over 1,000 analytic sessions). A further dimension to the inner workings of the psychoanalytic scene in Britain in the post-World War II era is Hughes's discussion of the case study of 'Susan' in Marion Milner's important book *The hands of the living God* (1969)—this patient lived in the Winnicott household for seven years during Milner's treatment of her, and Winnicott's posthumously published *The piggle* provides, as Hughes puts it, "clinical material bearing on the issues with which Milner's Susan had been grappling for close to two decades".

Hughes's critical approach is refreshing, although not convincing in most places. For example, when she refers to "the downright sloppiness that plagued the work of Melanie Klein", one feels that Hughes has not prepared the ground properly. In the context of Hughes's analysis, the charge simply does not hold. But more serious problems abound in her discussions of the development of Freud's instinct and structural theories.

Hughes focuses rightly on the theoretical importance of Freud's unfinished and posthumously published 'Project for a scientific psychology' (1895). But she seems oblivious to the fact that the roots of the instinct theory can be found in Freud's 'Project' (endogenous *Q*[uantity] is not protected by a shield against stimuli), or that it evolved through three specific stages. In the first, the sexual and self-preservative (ego) instincts were given equal weight in shaping human behaviour and experience; in the second, beginning with the paper on narcissism (Freud, 1914), the self-preservative instincts were defined with reference to libido turning back onto the ego; in the third, the death and life instincts were posited as fundamental (1920) and Freud returned full circle to ideas that were embedded in the 'Project', especially with regard to the separation of two of the key psychical systems—memory and consciousness.

Hughes is certainly sensitive to the issue of the English rendering of "instinct" for the German concept *Trieb* versus the more preferred use of "drive" by the English-speaking purists. But in general she seems unacquainted with the current issues on the English translation of Freud, although several important papers of Ornston published in the last decade are in her bibliography. One significant item in the translation debates concerns Freud's structural hypothesis. Ornston, for example, supports the position that the so-called structural theory may in fact be James Strachey's invention, not Freud's. As Strachey is given more than adequate coverage as Freud's principal translator in the early chapters by Hughes, her unqualified assumption that Freud's structural theory is a fundamental "paradigm" of the Freudian system is a serious oversight.

Her strategy of analysing the differences between Freud's theoretical concepts and those of Klein, Fairbairn, and Winnicott is not entirely successful because she does not prepare the Freudian ground properly. On the other hand, the increasing interplay between clinical and theoretical material after the chapters on Freud and Klein sustains attention to the very end.

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EDWARD M. HUNDERT, *Philosophy, psychiatry and neuroscience: three approaches to the mind. A synthetic analysis of the varieties of human experience*, Oxford, Clarendon Press, 1989, 8vo, pp. xiii, 346, illus., £30.00.

More than one commentator has defended the obscurity of Hegel's philosophical writings on the grounds that the truths with which he was concerned are themselves invincibly obscure. Any book which claims to be an extension of the Hegelian programme is therefore unlikely to be an