

Chambers, John H. (1992). *Empiricist research on teaching: A philosophical and practical critique of its scientific pretensions*. (Philosophy and Education Series, Volume 4). Boston: Kluwer Academic Publishers, 282 pp., \$89.00 (hardcover).

Readers of the *Journal of Educational Thought* will find a portion of Chambers's argument in his article entitled "The Difference Between Abstract Concepts of Science and the General Concepts of Empirical Educational Research," in Volume 25, No. 1, April 1991, pp. 41-49. The book under review here is a densely footnoted and abundantly referenced elaboration of this article.

Chambers's thesis is challenging (and, in my opinion, entirely correct): "empirical research [and certainly that on teaching] is not scientific as its practitioners claim, but rather it is empiricist and a specific and limited kind of empiricist at that. It is this confusion of status which has led such researchers astray" (p. 5). Chambers argues that research on teaching has failed to produce useful and convincing conclusions of any general significance. Its findings do not extend beyond what common sense and ordinary reflection of experience can produce. In labeling educational research "empiricist," Chambers adopts Polanyi's concept of tacit knowledge: "It is bodies of such propositions [those based on the generalization of observables and personal experience and tested by trial and error], their oral expression, and the concomitant tacit knowledge ... connected with them, which I am calling, 'Empiricist Theory'" (p. 21). Empiricist concepts, he points out, are defined by observable entities, not by abstractions. This origin, it is suggested, limits the reach of propositions based in empiricist concept and renders empiricist research sterile.

I wish to have it recorded that I accept Chambers's thesis entirely and, more than that, wish that I had made it myself. But had I written this book, I might have spared the reader some dense travel through the philosophy of science — much of it of marginal relevance — by proposing the contemplation of a simple thought experiment. Imagine that you were

transported back in time 2,000 years, more or less. You are sick; and you are ignorant. Would you with confidence place your health in the hands of Hippocrates? Would you with the same or lesser confidence place your education in the hands of Socrates? It is arguable whether a modern teacher could instruct one better in the ways of thinking than Socrates might have; but few doubt that even a run-of-the-mill contemporary physician would not outperform Hippocrates, with his armamentarium of poultices and bleedings. The science of teaching has made scant progress when measured against the progress of other practical endeavors.

Had I written the text to surround Chambers's thesis, I suspect it would have turned out quite different. A great deal of space is given over to elaborating on a dozen different senses in which the word *theory* is used in the modern world; not all of them are relevant; many aren't even very interesting. He spends far more time than is necessary trying to understand the interesting but special cases of scientific theory (in physics, chemistry, and what-have-you) that have little relevance to social or behavioral theories (if any deserve the honor of being so-called "theory" in any of the dozen senses of the word). At some points, his preoccupation with hard science leads him to say nonsensical things (e.g., as when on pages 71-72 he attempts to argue that ratio scales of measurement — gram/centimeter/second — are somehow essentially linked to the scientific nature of theory). Indeed, Chambers's whole attempt to portray the nature of successful scientific theory is much too self-assured and closed. He admits to fewer doubts as to what constitutes good science than do others who seem to see deeper into this question. Take Robert Nozick as an example. Nozick argues (1965) that good theories, particularly in the social sciences, have about them the character of "invisible hand" explanations. Nozick was humble and apologetic in trying to put more precise language to this notion. The sobriquet "invisible hand" he took from Adam Smith; what impressed him in *The Wealth of Nations* was that now Smith had gone beyond the experiences available to any single individual to an abstraction that then accounted for what common sense, of the time, could not suggest. Nozick said more to me about the role of abstract theory in social and behavioral science in that brief example than Chambers does in over 100 pages — which brings me to another crotchet.

The book has a 22 page bibliography. One ought not to begrudge any author the space it takes to list those influential references that shaped the work, but 500 books on educational research and the philosophy of science? And it would be philistine to carp about a showy reference list if there were evidence that these many and disparate sources had been brought together in the work in question. But they have not. Nozick is there (the 1965 reference on page 261 and the citation on page 19 of a 1974 book, no reference given — though not all references in the bibliography are even cited in the text); but Nozick is not referenced for what he once wrote about science but for the fact that his *Philosophical Explanations* text illustrates rational normative argument.

The problem of the disembodied bibliography is not merely a stylistic quibble. Chambers is guilty, I believe, of missing the most relevant literature on his own topic. It is not missing from his list of references, one may certainly trust; little could be. It is simply missing from consideration. I refer to the work of Paul Meehl, the famous psychologist and philosopher who, in a series of brilliant papers extending across 25 years, has given the most penetrating critique of the social and behavioral sciences ever published. The reader who wishes to follow this topic may usefully start at the end point with Meehl's justifiably famous paper "Theoretical Risks and Tabular Asterisks: Sir Karl, Sir Ronald, and the Slow Progress of Soft Psychology" (1978). In Meehl's 1978 exposition on the shortcomings of theory in the "soft sciences," he interjected the opinion that soft psychology has one theory worthy of the name: psychoanalysis. Psychoanalysis is not the embodiment of the gram/centimeter/seconds system that Chambers would anoint with the title "scientific theory," and yet it appeals to Meehl (and to me, I might add) because it is rich with abstract concepts and it transcends — indeed at times virtually ravages — common sense. Chambers has not a single reference to psychoanalysis, not any mention of Freud that I can find, save a passing reference to A.S. Neill as being a "quasi-Freudian." In short, less attention to hard science theory and more concern with the role of theory in the social and behavioral sciences would have cast more light on Chambers's thesis.

Nonetheless, taken as a whole and without pedant concern for details, Chambers has laid bare the pretensions and the shortcomings of research on teaching. Has he pointed a direction out of the confusion? Yes, more than many others. He exposes quite adroitly the hypocrisy in the claim of some researchers that they have left behind simplistic views of teaching and learning and replaced them with multivariate and contextualized accounts of these phenomena. For the most part they haven't, so when he calls for research embedded in pedagogical theory, the call has a convincing ring: Adequate theory and research "would also need to include a clear conception of what it is to teach in general and also what it is to teach particular subject-matter in particular contexts to particular kinds of learners" (p. 234). Too often that same cry has been sounded to rally interest in the latest three-factor ANOVA design. Chambers will not be deceived by such artifice. Nor will the reader of *Empiricist Research on Teaching* who reflects sincerely on its critique of the scientific basis of the art of teaching.

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REFERENCES

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