The Demise of Logical Positivism: Implications of the Duhem-Quine Thesis for Psychology

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*I intend to focus here on the collapse of logical positivism or more generally, logical empiricism, by focusing on the Duhem-Quine thesis. My reason for doing this is primarily because of the absence of discussions of this thesis in methodology in the mainstream psychological literature. I do not attempt a historical overview but focus instead on the logical problems inherited by psychology in its adoption of positivist tenets. Whenever psychologists discuss the demise of positivism, they quickly point to the work of Kuhn, Feyerabend, or even Popper as the leading causes of the demise. Depending on the purposes for which they invoke these names, this triumvirate is either promptly dismissed as unrealistic about science or gratefully embraced as representing the new beginning.

The Duhem-Quine thesis, however, presents psychologists with greater difficulties, to which I shortly return. First, however, I want to say something about positivism in psychology. In order to be clear about what the demise of logical positivism could mean for psychology, it is important to point to precisely where and when psychologists rely on what we now take to be the mistaken assumptions of logical positivism. What I had hoped to present were a series of clear-cut cases. Unfortunately, there appears to be no such thing, at least not in our mainstream journals. To illustrate this point, I took one issue of *Psychological Review* and examined each article in detail. To be sure, in the seven articles in the July 1989 issue there were no references to logical positivism, or to any philosophy of science for that matter. However, like many *Psychological Review* articles of the past 20 or 30 years, four of the seven actually contained experiments that were deemed crucial to the theories being proposed. In effect, the difference between the *Psychological Review* and the *Journal of Experimental Psychology* is only one of degree: The *Psychological Review* papers merely have longer introductions and discussions. This is in no way to be critical of *Psychological Review*. It has been,
after all, the editorial policy of that journal to include empirical work, as recently reiterated by the new editors in 1989, who stated quite plainly that

An article represents an important theoretical advance if it has the ability to explain a set of existing research findings and to identify empirical work that might be fruitfully carried out in the future . . . . We expect that most articles we publish will contain some empirical work in support of the theoretical positions advanced. (Kintsch & Judd, 1989, p. 3)

In the July, 1989, issue the theoretical viewpoints in six of the seven papers were of the sort we are familiar with in psychology: empirical generalizations, not fully fledged (or even partial) theories. For example, in an article on the vascular theory of emotional efference, Zajonc, Murphy, and Inglehart (1989) argue that facial muscular movement restricts venous blood flow and thereby cools the arterial blood supply to the brain. Cooling is more pleasurable than warming and hence there is a link between emotion (as defined strictly along pleasant versus unpleasant dimensions) and the temperature of the brain. A series of studies are then presented which I do not describe, but which putatively demonstrate this principle. In each of the five studies one or another alternative hypothesis is eliminated. The conclusion: "facial movement alone is capable of inducing changes, albeit small, in the subjective feeling of the individual" (Zajonc et al., p. 409). To call this a theory of emotion or of anything else psychological is, in my view, farfetched. Why farfetched, and where is the connection to positivism? Like much of mainstream psychology, the effects of positivism are insidious. Perhaps a more kindly description is they serve as an unspoken grammar. We have taken in the residues of positivism (both logical and prelogical) with our education and we no longer acknowledge or recognize the roots of our methodologies. This would be difficult to do in any case. Prelogical positivism is intimately tied up with the history of behaviorism and functionalism in psychology; it influenced our development of experimental methods, and it fueled the development of our statistical techniques (see Danziger, 1990). So, to go back to Zajonc's paper, positivism is not there to be taken out, to be excised in some orderly fashion; it is part of the very fabric that allowed that paper to be written, refereed, and published in the first instance. Perhaps the only obvious place where we might point to the influence of positivism is in the outcome of such research endeavors. In various forms, we have the traditional version of the logical positivist process of hypotheticodeductive justification. Hypotheses were tested by deducing observational consequences from them, along with a set of initial conditions. If the observational consequences passed the test, the hypotheses were confirmed. Theoretical statements then become a collection