

## The Future of Psychology's History Is Not its Past: A Reply to Rappard

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**ABSTRACT.** Contrary to Rappard's (1997) reading of my paper (Danziger, 1994), I did not advocate that the history of psychology be left to historians. However, that does not mean we can afford to ignore historians' criticisms of insider history. Underlying our disagreement there are different conceptions of the role of history in the natural and social sciences and of the position of psychology among these.

**KEY WORDS:** historiography, history, psychology, science

There are three parts to this debate. First, there is disagreement about the existence of a problem. I think that the historiography of psychology faces a fundamental problem; Rappard (1997) does not believe that there is a problem. Not seeing the problem, he naturally has difficulty following my suggestion for a solution. That leads to the second part of the debate, which, I believe, is based on misunderstanding rather than disagreement. In the final section of this reply I explore a deeper reason for our disagreement and suggest that different views about the relationship of science and history and about the status of psychology among the sciences are involved.

### Is There a Problem?

My article 'Does the History of Psychology Have a Future?' (Danziger, 1994) was based on the diagnosis of a fundamental problem that had beset the historiography of psychology and that would have to be confronted if the field was to make a significant contribution to the discipline. What needed to be confronted was the question of the role that history, and therefore the historian, played in the life of the discipline. It is impossible to overlook that there are different conceptions of that role and that this entails different kinds of history. The historian Paul Forman (1991) has formulated this issue with great sharpness and clarity for the natural sciences, distinguishing between a kind of history that essentially 'celebrates' scientific progress and another kind that adopts a more critical perspective. More specifically, he sees the difference between these approaches as based on the historian's acceptance or rejection of the moral authority of the scientific in-group. In my opinion, Forman's sharp formulation is useful in diagnosing and confronting a problem that does exist in the historiography of psychology. What role shall history play? Shall it have the role of celebrating the past in terms of its 'achievements', or shall it explore the historicity of current criteria of achievement?

Rappard (1997) believes that '[t]he early insider-historians . . . seemed not particularly inspired by celebratory feelings' (p. 104). Moreover, Forman's analysis, being directed at the natural sciences, is not applicable to psychology because it is not a natural science. Here lies the crux of our disagreement. But first I have to say that the issue is not one of the feelings of historians, celebratory or otherwise, but of the meaning and the message conveyed by certain historical texts. And there the similarity between much of the earlier historiography of psychology and the earlier historiography of natural science is striking. Indeed it would be strange if it were otherwise. For many years the dominant trend in the historiography of the field was based on the presentation of modern psychology as a natural science. That was the message summed up in Ebbinghaus's endlessly repeated slogan about psychology's long past and short history. Boring's *History of Experimental Psychology* (1929), the unquestioned authority from which so many derivative texts drew their inspiration, begins with this slogan and, in case anyone did not get the message, adds that psychology's short *scientific* history was meant. If psychology is a social science, it is one that has harboured a strong tendency to deny that status and to proclaim affinity with the natural sciences.

That would-be identity has provided the framework for much of the literature in the historiography of psychology, a literature marked by abuses often noted by professional historians (see, e.g. Ash, 1983; Smith, 1988; Young, 1966). To mention only a few, we find histories that are no more than literature reviews extended backward in time, we find story telling substituting for history, we find great man hagiography, we find the cult of 'anticipations' and the awarding of good and bad marks on the basis of some current scientific orthodoxy, we find gross insensitivity to historical context, we find the formulation of 'timeless' problems in the language of the present, we find the construction of spurious lines of ancestry, we find the mythology of progress. What historian of psychology could feel smug in the face of such shortcomings?

Yet, among psychologists who dabble in the history of their field, the critical allegations of historians have not resulted in any rapid or widespread reform. Why is this? The brief superficial answer is: ignorance. But this is an ignorance that has been maintained for decades. Would physiological psychologists have gotten away with turning their backs on relevant physiology for a generation or more? I doubt it. Physiological amateurism would not be tolerated as complacently as historical amateurism. We have to ask ourselves: what maintains this extraordinary tolerance of historical amateurism within the discipline of psychology?

One reason probably lies in the relative esteem that history is accorded by many psychologists. If history were a natural science it would have to be taken seriously, especially if its findings were felt to be psychologically relevant. But history is not a science, and so its scholarship can be safely ignored. Besides, for many psychologists history is not considered psychologically relevant because they believe their discipline to be concerned with historically invariant aspects of human nature. Such attitudes are grounded in a subjective affiliation with the natural sciences.

Where Forman's analysis is useful, I believe, is in its sensitivity to the 'moral' aspects of this situation. Extending his analysis to the situation under discussion leads to the suggestion that the fate of history-in-psychology should be seen not just in terms of purely intellectual attitudes or deficiencies, but in terms of implicit moral commitments. Within psychology one can continue to present history in terms of

scientific ‘anticipations’, engage in great man hagiography, write history from the point of view of current orthodoxy, and so on, because all this is based on sharing the values, prejudices, moral commitments, of the scientific community to which one is addressing oneself. Professional historians may regard this as bad history, but it is not bad history for its intended audience. That is because of the role played by such history within a disciplinary context, a role summed up in the term ‘celebration’. What is being implicitly celebrated here is the moral, and also the epistemic, authority of some version of scientific or disciplinary orthodoxy. What is rendered invisible is the essential historicity of that authority.

### **What is the Solution?**

My application of Forman’s diagnosis did not lead me to draw the same conclusions as he did. In this respect Rappard has quite misunderstood the thrust of my article. I did not propose that the history of psychology be left entirely to professional historians; quite the contrary. Over half the text of my paper was devoted to a discussion of the conditions favouring a new lease of life for historical studies by psychologists and to some speculation about the positive consequences of such a development for the discipline. Rappard is right when he suggests that the stark alternative of celebratory insider history vs critical outsider history would condemn the field to perpetual irrelevance with regard to the central tasks of psychology. My article was directed at precisely the question of whether there might be ways out of this dilemma. I devoted the longest section of the paper to describing the factors associated with the emergence of a more critical kind of insider history. It should be obvious that I consider the stark contrast between celebratory insider history and critical outsider history to be itself a product of certain historical conditions and therefore a temporary state of affairs.

Rappard’s misreading of the later sections of my article is surely related to the disagreement I discussed in the previous section. He does not believe that the historiography of psychology has a problem, certainly not the problem Forman identifies. Not seeing the problem, the solutions remain opaque. I consider this non-recognition of what we might call the Forman problem to be unfortunate. The way forward for historical studies in psychology seems to me to involve a frank confrontation of this problem. Pretending it does not exist is likely to result in a perpetuation of the errors of the past and in the trivialization or exclusion of historical studies.

In terms of the analysis I presented in my paper, the issue may be less acute in Europe than in America. For, as I indicated, the unreserved identification of psychology with the natural sciences, and the corresponding role of historiography, was associated with American dominance of the field. Nevertheless, the legacy of that period has left its mark on European psychology and is not without representation in its historiography. Denial will not make it go away.

### **Science and History**

There is a more profound level of disagreement which underlies this debate. Rappard assumes that, like him, I regard history as being absent in current natural science whereas it is present in current social science. However, that is not how I presented the difference in my article. Going beyond Forman’s analysis of the kind of

historical consciousness found in the natural sciences I pointed out that 'some kind of historical tradition is in fact recognized in every research paper' (p. 471). The absence of a reflective historical consciousness does not mean the absence of history, I argued. There is always history *in* science, whether it be natural or social science. The difference lies in the way history is mobilized in the present: the consensus building shallow history of the research paper in the one case and the reflective historical study in the other. As Gyorgy Markus (1987) has suggested, this difference should be linked to the existence of variant forms of organizing scientific work in the two cases. That results in history having different functions and therefore different manifestations.

The relationship between science and history involves much more than explicit science and explicit history. It is not in its explicit theoretical and empirical formulations that the historicity of science manifests itself, but in its basic categories, its technology, its work organization, its values, its place in society, its professional socialization, its implicit metaphysical commitments. Indeed, the apparently ahistorical mobilization of tradition that is so characteristic of natural science is itself the product of a history that is quite open to study (e.g. Shapin & Schaffer, 1985). History is certainly in natural science as well as in social science; it is the manner of this presence and the response of those affected by it that constitutes the difference.

If the historicity of natural science remains so much less visible than that of the social sciences this is in no small measure due to the way in which tradition is mobilized within the former. The maintenance of a consensual framework, enabling the cumulative growth of empirical knowledge, goes hand in hand with the maintenance of ignorance regarding the historicity of the framework itself as well as of that huge part of history which does not fit into it. I certainly do not wish to devalue the efficiency of this social arrangement for extending our collective knowledge of nature, but when we study science we have to recognize that not everything that is going on is necessarily present in the consciousness of its practitioners.

The belief that history is not to be found within natural science is part of the conventional self-understanding of scientists. It is a practically useful belief that fosters collective problem solving in a taken for granted framework. But the historian who shares this belief is cut off from investigating the largest part of the historicity of science. Shedding this belief, however, means adopting a standpoint outside the scientific community. In the case of the natural sciences that leads to a parting of the ways of scientists and historians.

But history remains irrelevant to science only as long as no better way of doing science than the historically received way appears possible. In psychology that condition does not obtain. That is not because psychology can be unilaterally declared to be a social science but because of the 'perennial ambiguities surrounding the status of psychology as a natural or a human science', as I put it in my paper (p. 473). It is this ambiguity, this continued openness, that defines an arena in which historical studies can make a presently relevant contribution.

Unlike Rappard, I do not believe that the potential value of historical studies for psychology can be established simply by proclaiming it to be a social rather than a natural science. Given the course which the discipline has followed, historical studies can play a critical role within it only by recognizing the powerful reality of

the project of psychology as a natural science while also revealing the profound historicity of that project, both in conception and in implementation. But, as I have indicated, such a development depends on particular historical circumstances.

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