Deconstructing the Subject: Banishing the Ghost of Boring

A Review of

Constructing the Subject: Historical Origins of Psychological Research
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Reviewed by
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"A picture held us captive. And we could not get outside it, for it lay in our language and language seemed to repeat it to us inexorably" (Wittgenstein, 1958, sect. 115). This idea most nearly describes psychologists' understanding of their own history. The picture that held us captive is repeated in many historical anecdotes that make up the early pages of our textbooks, sadly, in our history books themselves, and most often, in the very investigative practices that endure to the present. To understand this picture we must explore it, "but the picture seems to spare us this work: it already points to a particular use. This is how it takes us in" (Wittgenstein, 1958, p. 184). Taken us in it has. The picture is familiar enough. Consider the statements drawn from a standard history of psychology text (Schultz & Schultz, 1987) that follow. "Before Galton's efforts, the phenomenon of individual differences had not been considered a subject for serious study in psychology; this was a serious omission" (p. 115). "More than 30 years of research was conducted in Tolman's laboratory, and it is important to consider the nature of the research that supports his theory of learning" (p. 233). Consider also Hilgard's (1987) claim that

the issue between the nomothetic and the idiographic is in some respects parallel to another distinction that arose in Germany between Naturwissenschaft and Geisteswissenschaft, which lay behind Wundt's distinction between problems that were suitable for experimental investigation and those which had to be approached historically. (p. 786)

This is the standard disciplinary historical rhetoric: Great people had great ideas that they put into practice in the form of great research, or sometimes they participated in new intellectual movements, or sometimes both are true. Ideas are simplified; Galton discovered individual differences, and Tolman produced research to support his theory of learning. There is a clear distinction between nomothetic and idiographic that eclipses previous distinctions. Let me hasten to add that not all textbooks are this acontextual. However, the patterns are familiar enough. It is the ghost of Boring that still hovers over the history of the discipline.
Danziger aims to free us from this picture in a most extraordinary way. Suppose we now ask not about the emergence of the study of individual differences, the empirical support for a particular theory, or the trivial pursuit of intellectual forerunners but instead follow Danziger's analysis and ask how Galton came to assert that individual performances could be compared with each other in the first place. How did the estimation of statistical significance become the only criterion on which a theory was supported? How did the nomothetic and idiographic distinction come to serve as a rationalization for the pursuit of universal "laws" through the interpretation of the idiographic as a strictly individualist notion? Such questions lead us away from standard accounts of disciplinary history that emphasize the accumulated contributions of individual investigators. We turn instead to what Danziger has called the investigation of the "constructive schemes that psychologists have used in the production of those objects that form the accepted content of their discipline" (p. 3). Constructive schemes are the interpretations of the entire research enterprise as well as the
rules required to produce the research in the first place. By examining the investigative practices of psychologists (rather than their self-proclaimed methodologies), Danziger has broadened the problem of methodology to include a consideration of who was intended to be the eventual consumer of the research as well as what were the normative practices and social contexts of the research. By unearthing the codification of research practices in the first half-century of psychology's existence, when these practices were still in flux, Danziger urges us to come to a different appreciation of the relative rigidity and uniformity that set into the discipline following World War II.

Central to Danziger's analysis is the understanding that psychology's subject is an elaborate construction. By showing how three models of research eventually developed into the professional investigative model that came to be accepted (along with its statistical accoutrements), Danziger also (and crucially) undermines our confidence in the naturalness, or necessarily scientific, nature of the model itself. Rather than a single scientific psychology developing in the early 20th century, three models (i.e., the Galtonian, the Wundtian, and the clinical experiment, which originated with French investigators of hypnosis) strove to define the nature of psychological investigations. Each was driven by different professional interests, and each competed for different audiences. How these models eventually came to be dominated by a model that made claims about aggregated individuals and came to rely on a statistical system that was "constituted by the attributes of a constructed collective subject" (p. 78) is surely one of the most fascinating stories of 20th-century psychology. Says Danziger, "the locus of operation of abstract psychological laws was an abstract individual who was the product neither of nature nor of society but of statistical construction" (p. 129). Although it may be an esoteric form of knowledge, quantitative psychological knowledge was viewed by those psychologists who devised it to have profound social implications. "The keepers of that knowledge were to constitute a new kind of priesthood, which was to replace the traditional philosopher or theologian" (p. 147), a process Danziger traces out in fine detail. This is no trivial or simple story. To make his case, Danziger recounts the influence of nation-states, world wars, academic practices in general, educational administrators, and a host of other characters and events seemingly unrelated to the practices of a pure psychological science. That they were indeed relevant to those practices and that Danziger clearly shows us how this is the case is the overriding strength of this book. His work here is a clear and sharp signal that the historiography of psychology has come of age and, I hope, finally left its heroworshipping and presentist naiveté behind.

Sociology of psychological knowledge

To ask questions about investigative processes and investigative practices requires a broader lens than that provided by disciplinary history, and Danziger bases his analysis in the sociology of science or, more broadly, in the sociology of knowledge traditions. Knowledge claims are just that, claims that are transformed into accepted knowledge through a process of acceptance that involves editors, reviewers, textbook writers, instructors, and others. Knowing that one's knowledge claims will be subjected to this process affects those claims from the outset. The production of psychological knowledge involves the production of a highly particular kind of knowledge that is constrained by the rules of admissibility and the desirability of the end product (i.e., quantitative results, practical results, etc.). However, the rules of admissibility are themselves negotiated between the knowledge producers and those groups that wield institutional power over the knowledge producers. In order to survive and expand, the knowledge producers must form effective alliances with established groups that can ensure their support. In addition, each new knowledge product must have a significant social and market value of its own by its appeal to powerful interest groups that might have some use for this knowledge. Thus, says Danziger, "American psychologists scored some real successes in this direction by providing knowledge products that mobilized the interests of educational and military administrators as well as the administrators of private foundations" (p. 181). The successful establishment of psychology depended on a political process that required the formation of alliances, the defeat of competitors, the formation of power bases, and so forth. Having established themselves, psychologists focused more extensively on developing internal norms of conduct consistent with the external political demands that it satisfied. Individual researchers simply adopted these norms as an unconscious background to their work, and a kind of social amnesia set in for the original historical development of the discipline.

In traditional historical texts we do not find psychologists positioning themselves in a social context; psychology just happened. It is this view that Danziger wishes most obviously to dispel. He states that
Thus, knowledge is to some degree affected by the political forms imposed on the knowledge-generating situations. It is the "to some degree" claim that is crucial, as I will show momentarily. One additional point that requires comment is the notion that psychology became successful only to the degree that it could claim to have expert knowledge or knowledge different from everyday or common knowledge. This was achieved by psychologists distancing from the mundane and ordinary and by them drawing on what Danziger has called the "mystique of the laboratory" and the "mystique of numbers," neither of which was invented by psychologists (p. 185). The artificial laboratory and the imposition of a numerical form on the responses obtained there gave expert psychological knowledge a significance far beyond mere laypeople's knowledge. In addition, certain investigative practices took on the form of the social situation for which they were gathering data, such as the mimicking of the school examination in mental testing. This apparent homology between the investigative and applied settings further legitimized psychological research. This convergence of contexts of investigation and application (the analysis of which I have only very briefly and superficially described) has profound consequences for Danziger. "In the investigative contexts that became increasingly popular during the first half of the twentieth century, the individuals under investigation became the objects for the exercise of a certain kind of social power" (p. 190), which Foucault (1978) termed the power of a discipline. Thus, the kind of knowledge produced by psychologists was fed back into disciplinary institutions to enhance their efficiency. It was administratively useful knowledge, says Danziger, "required to rationalize techniques of social control in certain institutional contexts. Insofar as it had become devoted to the production of such knowledge, mid-twentieth-century psychology had been transformed into an administrative science" (p. 190).

**Psychological reality**

Danziger asks us to consider a radical revisioning of our disciplinary history. Where does this leave psychology? The technical and ideological validities of psychological knowledge are, for Danziger, no more than local. "Technically, its relevance depended on the existence of a certain institutional framework, and ideologically its plausibility extended only as far as the cultural forms in which the shared faith was expressed" (p. 191). Does this make all claims of psychological knowledge constructions of particular investigative practices? Are there psychological realities not accounted for by the limits of the investigative contexts? Regularities produced within investigative contexts are, obviously for Danziger, socially constructed realities. Here is the rub: If the observed regularities never represent or, at the very least, never allow us to infer the independent existence of irreducibly psychological, nonphysical, nonsocial phenomena, then the very idea of an independent discipline of psychology is cast in doubt. Danziger is quite correct, in my view, to name the pernicious effects of positivist philosophies as critical in bowdlerizing all metaphysics from the discipline and, hence, with it any considerations of what psychology really ought to be about in the first place. The atheoretical or at best functionalist discipline that survived in the 20th century came to depend more and more on its investigative practices for its self-definition and thus became especially vulnerable to what Danziger has called the "corrosive effect of social constructivism" (p. 194).

However, even those attempts at specifying a level of psychological reality beyond the investigative context are theoretical productions and no less productions because they do not obviously rise out of investigative contexts. For example, the work of S. Freud, Wundt, Piaget, and others clearly pointed to mechanisms of psychic causality that were not directly dependent on investigative contexts. However, Danziger notes that these were nonetheless limited in their relation to the level of empirical observations. For Danziger, the theoretical domain and the practical domain should confront and reciprocally criticize each other, which makes possible the "escape from the closed world of unreflected investigative practices" (p. 194).

In his final chapter, Danziger confronts these issues head-on. However, because the focus of the book is historical, he
does not have enough space to lay the foundations of a debate and develop the arguments. Thus, on the one hand, my considerations are necessarily based on limited material. On the other hand, in the sociology of knowledge tradition, investigators of scientific practices frequently "bracket" questions of truth, objectivity, or universality, and it is to Danziger's credit that he is willing to consider the implications of his analysis for such questions. On a sociology of knowledge view, these claims are themselves objects of scrutiny as sociohistorical constructions. However, sociologists of knowledge readily acknowledge that just because science is socially constructed (like all human practices) does not mean it is also not productive or true by some understanding of the negotiated nature of truth. Indeed, some have argued that scientific truth claims are possible only because of the fundamentally social activities of their producers and consumers (Coulter, 1989).

Psychology, however, poses more difficult problems. The truth of psychological experiments is not readily bracketed. Danziger relies on recent realist philosophies of science, particularly those influenced by Bhaskar, to defend some extrapsychological reality. What I think Danziger finds appealing in this account is the explicit need expressed in Bhaskar's work for a division and clarification of both an ontological and epistemological dimension in any adequate account of science and the social sciences. How can we change the world, asks Bhaskar (1989), if we do not understand its underlying structures? Because psychology has no ontology, it presupposes one that is ultimately false. In the final pages of Constructing the Subject, there is a loose confrontation between a critical realist philosophy of science and what has been to this point a deeply committed, sociohistorical account of the history of psychology. Danziger now wants to hold fast to something of the old order after, perhaps, having shattered that order decisively. Danziger argues that "to say that psychological knowledge bears the mark of the social conditions under which it was produced is not the same as saying that it is nothing but a reflection of these conditions" (p. 195). This of course is the relativist worry that so frequently halts all intelligible discussion of constructionist epistemologies. So what are the criteria we should use to evaluate theory? For Danziger, they include the attribute of depth and the assessment of the social and moral consequences of different kinds of knowledge claims. However, this is not Bhaskar's realism, it is a version of a constructionism. Danziger then states that

of course, in the day-to-day business of technical research such questions do not need to be raised. On this level specific knowledge claims can be compared with each other as long as one takes for granted the vast assembly of hidden practical and theoretical assumptions they share. (p. 196)

He adds that "if we want to raise the question of whether such claims have any validity (or any real meaning) outside this framework, we have to extend our query to the framework itself" (p. 196). Thus, on the realist account, such questions are still worth pursuing. However, on the very next page, Danziger urges us to extend our disciplinary alliances "to groups of people who are more interested in psychological knowledge as a possible factor in their own emancipation than as a factor in their management and control of others" (p. 197).

What these passages indicate for me is that the realist project gives us deep difficulties once we concede the constructionist analysis (e.g., Shotter, 1990). Once we see the fundamentally moral project embedded in psychology, as I think Danziger has done, it makes no sense to appeal to entities that exist independently of our knowledge or our experience of them. Whether we are critical realists (e.g., Bhaskar) or pragmatic realists (e.g., Harré), the door is closed. Perhaps this is an inevitable schism between the human and established sciences, but in any case, to take the world as socially constructed is to be forever enmeshed in a world of rights, duties, obligations, privileges, identities, roles, personalities, selves, multiple forms of human association, and so forth (Shotter, 1990). And there are very definite standards in this world, although not realist anchor points. These are the conversational realities in which we not only partake but that constitute an intralinguistic reality that provides a set of locations structured by moral and political considerations. This then is the ontology of social constructionism, which is not a reality of things and structures but of "persons in conversation" (Harré, 1983, p. 20).

In this case, does one need to rely on some realist formulation to conduct psychology in the ways Danziger seems to want us to proceed? I think not. According to Harré, we inhabit an umwelt, which is just that world that is available to us. If this world is socially constructed, then we do not have to look to impersonal standards against which to judge knowledge claims. One cannot help but observe certain standards in one's conduct. A failure to do so would risk the ability to legitimate one's actions or to make those actions intelligible to others. As Shotter (1990) claims,
if our forms of rationality are socially constructed, and if one knows this to be the case, then one cannot rationally deny in one's talk the (moral) grounds upon which one's rationality depends while still claiming, at the same time, that one's denials are rational. (p. 221)

The social constructionist program opens up a whole new series of practical and theoretical problems that might prove intractable. However, this is not the place to theorize on those problems.

I suspect that my analysis of this part of Danziger's book will not be popular—it is perhaps easier to retreat to some safe haven of realism. To grant Danziger his key insights into the emergence of psychological investigative practices, however, and then to deny their relevance to our current practices is to denigrate history as trivial and meaningless. This is precisely the stance of many of our colleagues—for them history is irrelevant, and our teaching of history to undergraduates is at best a form of light, intellectual exercise and at worst a waste of time. We look to our historians for narratives that help us make sense of our current conundrums, whether we mean to or not! We cannot now let Danziger off the hook for what his historical analysis tells us about the discipline we have inherited.

Danziger is to be commended for his incisive and compelling archeology of investigative practices. Without a doubt, this is the most important book on the history of psychology to come along in years. It is my sincere hope that at some future time the understanding and the very teaching of the history of our discipline will have been profoundly altered by Danziger's analysis. Indeed, if that is the case, then that will stand as a testament to the intellectual integrity of his work.

References


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