History from the Top Down

Franz Samelson

In recent years, interest in the history of psychology seems to have grown exponentially, which may or may not be a 'good thing'. At least, judged by the number of new titles, including texts, monographs, and essay collections on historical topics, publishers apparently expect the field to provide them with a sizeable market even if not turning into 'big business'. Although the upcoming centennial of the founding of the American Psychological Association and allied ceremonial events may account for part of this expansion, several other developments have contributed to it. To name two among them, there was first the post-Kuhnian redefinition of the task set for a disciplinary history. Secondly, psychologist-historians discovered a new cache of data in the form of unpublished archival source material — although even today the official APA publication manual still does not allow, in its meticulous prescriptions for proper references, for the legitimate existence of such unpublished material, thus providing us with a contemporary example of what Danziger discusses as the object-constituting activities deriving from supposedly neutral but in fact quite imperialistic methodologies.

If we ignore the purely antiquarian efforts and the unreconstructed textbooks among the new crop, we find that the more up-to-date historical writings may invoke such categories as paradigms, contextual vs. presentist perspectives, and critical vs. Whiggish approaches. But beyond this point they usually add only limited ad hoc analyses to their predominantly descriptive contents. One exception is David E. Leary's (1987) organizing metaphor for his marvelous thumbnail sketch of the emergence of American

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psychology entitled *Telling likely stories*. Another one is the historical work of Kurt Danziger, one of the few psychologists who have attempted to conceptualize the epistemological issues underlying our historiographic efforts. His early essay on the social origins of modern psychology, appearing in the seminal but relatively unknown *Psychology in Social Context* (Buss, 1979), made an effort to formulate the tasks for a ‘new’ history of American psychology. One of its contributions was the introduction of his concept of “intellectual interests”, conceived as the merging of social (group) interests and of cognitive constructions. It was intended to lay to rest the vociferous argument about externalist and internalist historical explanations by showing that intellectual interests focus in both directions, inward as well as outward. Unfortunately, neither the epistemological issues laid out in this essay nor the key concept addressing them were picked up by other historians of psychology, apparently more interested in storytelling than in theoretical problems. As for the historical question of the origins of American psychology, the article stressed, and in my view over-stressed, the practical-administrative orientation of the emerging field in contrast to the aims of German psychology. I shall return to this issue below.

Danziger’s present essay, entitled *Psychological Objects, Practice, and History*, is similar in character to the earlier *Social Origins* and its sequel on *Critical History*, presented at a Cheiron symposium a few years later (Danziger, June 1981). But it pushes the analysis several steps further. Progressing beyond the “intellectual interests” of the discipline’s protagonists and the goals they represent, the discussion takes aim at the conceptual construction of “psychological objects” and the effects contributed, in a significant way, to the constitution of such objects by the psychologists’ “investigative activities”. The treatment goes beyond any simple formula, though, and is written quite densely, in the sense of touching in quick succession upon a number of related issues or aspects: the embeddedness of theory in collective activity, the absence of a “hidden hand” providing an overarching rationality, the problems of objectivity and reflexivity, to cite a few. Though intrigued by several of these ideas, I have for that very reason some difficulty deciding what the central argument of the essay is. Perhaps it is the *Transzendentale* claim that “psychological objects” (experimental subjects as well as general psychological phenomena-categories) produced for and by the psychologists’ interventions including, but not restricted to, their formal methods, are eo ipso and unavoidably different from “natural objects”, even though a naturalist empiricism fails to notice this transformation. As a consequence the psychologists’ methods, being

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**2 • History from the Top**

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2 • History from the Top Down

part of the problem instead of part of the solution, are in principle incapable of investigating the size and depth of this gulf. Only a meta-psychological, historical approach can come to grips with this problem of indeterminacy.

Altogether, the essay consists of three intertwined parts. The first one presents a forceful criticism of the received ahistorical and "positivist"-empiricist view of psychology as (natural) science and the associated presentist-justificationist approach to the history of psychology — a set of arguments I am very much in sympathy with.

A second, complementary part tries to lay out an alternative epistemology, constructivist in essence, with overtones from, among other, Kant, Marx, and Mannheim. The upshot of its intricate arguments remains a bit abstract for my taste: all the major sections seem to culminate in, and end with, a call to supplant the traditional ahistorical constructions with a historical perspective and analysis as the royal road to the solutions for our fundamental problems. But this call, in principle quite appealing, too, fails to spell out clearly how this historical approach would in fact proceed. And it never acknowledges that even a history calling itself “critical” may operate in all manner of ways and may arrive at all manner of conclusions, including possibly wrongheaded ones. Where should we look for the freischwebende historian-intellectual fit for the task?

Thus the third part, consisting of illustrative instances of historical analysis, is saddled with the burden of demonstrating the feasibility and power of the recommended approach and thus justifying the trust Danziger asks us to put in it. Unfortunately I find that the examples given fail to convince me. The fact that they consist of rather streamlined treatments and “hints” of historical developments, supplemented by references to other publications presumably spelling out the details, may in large measure account for this reaction. For one reason or another then, this part does not seem to be able to carry the load placed on it: to convince a skeptical reader of the fruitfulness of Danziger’s approach.

This is too bad. After all, if his analysis makes sense, and I believe it does, it should not be difficult to come up with detailed historical instances of the postulated processes involving the construction of psychological objects, the contribution of investigative practice, the role of shifts in professional goals, and alliances with centers of social power. If I understand what Danziger is arguing in fairly abstract terms, I can think of some candidates to serve as such examples: C.C. Brigham’s and E.G. Boring’s rather explicit creation of a psychological object called “intelligence” as differing from its natural form, because the techniques used in producing it...
did not allow them to make statements about the latter, or “wider” concept as Boring described it (Samelson, 1979); and the subsequent elaboration of this practice into the powerful magic trick called “operational definition” which allowed psychologists to make the distinction between constructed objects and natural objects appear and disappear at will. Or the role of John B. Watson’s “investigative practices” in producing a paradigm-creating “phobia” in Little Albert, practices visible in blurred form in Watson’s motion picture of this episode, and hinted at in his written account (but omitted from later re-tellings) which acknowledges that it was necessary to pull Albert’s thumb from his mouth “again and again” in order to break down his natural defenses and to produce the desired “conditioned fear reaction” (Samelson, 1980). Such examples, and others I would invoke, may or may not be acceptable or useful to Danziger; but this uncertainty about the specifics leads me to suspect that his argument was developed essentially “from the top down”, a procedure which does not quite fit my taste.

More serious, however, is my problem with one of the substantive issues presented: the emphasis which, carried over from the 1979 essay, is put on the dominance of practical goals in the development of American psychology. Of course, there were strong pressures in this direction in a society whose business, as we all know, was business. But again, it may be problematical to proceed from the top down instead of trying to determine how the impact of such forces worked itself out in particular historical episodes. After all, there were pressures from countervailing forces, too, I believe. The fact that even William James already held forth on the potential utility of the new psychology to practical men, as did many others after him, may deserve another critical look.

As Danziger himself emphasizes repeatedly, practice contributes in important ways to the constitution of the object. In what ways then did James’ practice contribute to making psychology into a practical-administrative science? I confess that I don’t see an obvious answer. Even in the case of the principal spokesman for practicality and control, John B. Watson, we need to distinguish between what he preached and what he did. As I tried to document recently (Samelson, August 1988), it is hard to find in Watson’s research practice, even or especially after 1913, much evidence that his research was directed at creating a psychology of social control. And although after his expulsion from academia he defiantly out-earned his former colleagues by manipulating and controlling people through advertising (or at least trying to), some of his writings even then expressed the...
aim of liberating the individual from the control by established powers rather than assisting these powers in his/her subjugation.

One reason for doubting that utility, practicality, and social control provided the mainstay of the developing discipline is that, when the time came to "put up or shut up", the practical successes of psychology turned out to be pitifully small over a long stretch. After all, even after one hundred years the ability of psychological science to provide the means of social control is — heaven be praised — rather limited. Thus psychology’s promises of future practical utility to its sponsors could not be repeated forever without growing stale, and different sources of legitimation had to be pursued.

In fact, when we search for the solid core of knowledge the new American psychology was selling, as reflected in the texts and journals of its first thirty years and beyond, it is hard to fathom just what enabled psychology courses to make such massive inroads into college curricula. Certainly, the rapidly expanding institutions of higher learning provided a sellers’ market for new products; but why psychology was able to gain a favored position in undergraduate education, side by side with the traditional core curriculum, for the next hundred years has remained a mystery to me. There was hardly any practical knowledge usable for the control of others; there was not even much scientific knowledge beyond some small islands of mostly esoteric information on the senses. In its stead it was “scientific thinking”, training in the scientific method, coupled with the exorcism of old superstitions, mysticisms, and old wives’ tales, that provided presidents, scientist-colleagues, and trustees with the warrant for admission of the new psychology. Though this scientific version of the Enlightenment may have involved the spreading of an ideology beneﬁting certain social groups, this process should be distinguished from the provision of technical means of social control to particular social agents and power holders.

True to the tradition of the American Psychological Association, which for its ﬁrst ﬁfty years insisted that its object was the promotion of psychology exclusively as a science, the practical parts of psychology, useful to administrators and people changers, were shunted off from the main trunk of academic psychology onto the educational and applied sidetracks of the "second" psychology, as Cronbach (1957) called it plaintively, to be staffed largely by women, non-Anglos, and marginal students. Academic psychology’s proper pecking order was displayed and at the same time rationalized
in Kurt Lewin's oft quoted oracular pronouncement: "There is nothing as practical as a good theory."

It is impossible to develop the argument in all detail here. Suffice it to say that more detailed investigation of the relevant historical material than what Danziger's essay provides might be desirable. And it seems supremely ironic that the major "factual" support for his argument derives from a set of numbers (which are actually presented elsewhere, in Danziger, 1987, 1990); numbers produced by coding the statistical designs of empirical studies, sampled from the journals of the past eighty years, into a set of abstract methodological categories — the kind of "scientific" analysis I would expect from a thoroughly presentist dévoté of a psychological historiometry, not from an advocate of historical contextualism. Reflexivity is a hard task master.

But to end my comments on such a critical note is not an attempt to belittle the merits of Danziger's arguments. Rather, its aim is to indicate that his ideas should be taken seriously by engaging them in debate.

References