

CHAPTER 1

RECONSTRUCTING THE SUBJECT

KURT DANZIGER AND THE REVISIONIST PROJECT IN HISTORIOGRAPHIES OF PSYCHOLOGY¹

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The greatest obstacles to good scholarship are to be found in the ‘god tricks’ that serve to hide and obscure the necessary partiality involved in knowledge production.

Kurt Danziger (1998)

INTRODUCTION

It is both a pleasure and a privilege to contribute to this volume of scholars honoring the work of Professor Kurt Danziger. His influence is deeply felt by all of us who have attempted to understand the history of the discipline of psychology as more than a mere accumulation of ideas and empirical results and he has inspired the critical work of those who have attempted to change the mainstream of that discipline by showing us that investigative practices are deeply carved out of taken for granted worlds.

I would like here especially to consider the relationship between Danziger’s work and questions of theory. This is a tall order indeed so I intend to engage only a modest aspect of that topic here, namely the epistemological implications for psychological theory embedded in the historical work conducted by Danziger. The continuous interplay between our theoretical discourse and investigative practices and the embeddedness of those practices in social contexts not of our own making makes psychological theory the outcome of more than the ideas of individual

scientists. Addressing this topic should be relatively effortless given that Danziger has himself addressed the implications of his work in both of his major historical books, *Constructing the Subject* (1990a) and *Naming the Mind* (1997a) as well as in numerous other papers (e.g., Danziger, 1990b, 1993a, 1994). On the other hand, having addressed the issue he has also shown that it is not a simple matter of drawing out a few implications from his work but that on his own account history and theory are clearly not independent activities. In this sense Danziger can be counted among those who, like others such as Michel Foucault, have challenged the prevailing ethos of the human sciences and argued that from the vantage of history there is so much more at stake in these sciences than was at first supposed.

By way of introducing the narrative, the point of my paper is perhaps best captured by the following anecdote: While attending a conference in 1989 at which Danziger gave a talk in which he outlined some of his key notions in the history of psychology prior to the publication of *Constructing the Subject* (Danziger, 1990a), I was seated beside a senior psychologist. During the ensuing discussion, which consisted mainly of a rather predictable debate on the distinction between intellectual and contextual history, Danziger held his ground without allowing himself to be drawn into the more exaggerated and heated aspects of the contest. My senior colleague turned to me and pronounced that he wished Danziger would take a stronger stand because “we need our historians to provide us with a vision.” Presumably my colleague meant a vision of what the discipline *could* be in the light of the kind of critical history Danziger has written. In retrospect however I do believe that there is a vision in the work of Kurt Danziger, and it is that vision that I would like to place on the table. For in elaborating this particular aspect of Danziger’s work we will come closer to addressing the question of history and its relationship to theory.

THEORY

Theory, from the Greek *θεωρία* and Latin *theōria* meant, among other things, contemplation or observation. This meaning has lingered in the modern English usage of the term; as late as 1710 John Norris could say that “speculative knowledge contemplates truth for itself, and accordingly stops and rests in the contemplation of it, which is what we commonly call theory” (as cited in the *OED*). At the same time of course we see the gradual adoption of the term within the sciences and its strict application by Newton to mean “invariant relations among terms designating manifest qualities” (Losee, 1980, p. 91). He divided this meaning strictly from his views on ‘hypotheses’ which are statements about terms for which no measuring procedures are known (hence Newton’s famous dictum, “Hypotheses non fingo”). This view was modified over a period of 300 years up to the logical empiricists of the twentieth century who claimed that theories must be deductive systems in

which laws are theorems. The spectacular demise of the logical empiricist system in the space of forty years has been widely described and analyzed and I will not pursue this here. In short, it is the failure (or impossibility) of maintaining the key distinction between a theoretical language and observational terms that created such difficulties both for logical empiricists and for those who would formalize theory in science more generally.

Theory in the human sciences, and in psychology in particular, never approximated the grand schemes articulated by luminaries of the logical empiricist movement such as Hempel and Carnap. Nonetheless, the latter provided a kind of framework outside the discipline that could be called on at auspicious moments for defense. Logical empiricism worked as a kind of Non-proliferation Treaty for theory, where theory could be contained so long as it was held to be, in principle, a species of deductive system. However the notion that observations were dependent on, continually infected by, or otherwise structured by theoretical considerations (e.g., Hanson, 1958; Kuhn, 1970) opened up the question of theory in the philosophy of science more generally and eventually did so in psychology (e.g., Stam, 1996). The obverse is frequently left unsaid, namely that theory is deeply dependent on some presumed observational regularities of life itself, even in its most post-positivist moments. That is, theorizing and observing are not different kinds of activities so much as they are different forms of a similar activity of sense-making that varies in its systematicity, practical arrangements and consequences (Stam, 2000). In this the writing of history is not different in so far as it requires the adjudication of evidence always within a framework of prejudices and preconceptions, theoretical predilections and considerations of what audience one expects to address.

HISTORY OF PSYCHOLOGY

How do these considerations inform a discussion of the historical work of Kurt Danziger? Historiography is obviously deeply dependent on some presuppositions and pretexts that allow the enterprise to establish its legitimacy. The difficulties of the philosophy of history notwithstanding, the peculiar nature of disciplinary history only compounds such difficulties. For not only are we confronted with the question of what constitutes proper historical inquiry or 'explanation' but also with what constitutes the discipline in question, in Danziger's case, psychology. These are not trivial problems, for while we may have agreement on how to proceed in writing a history of psychology we may not agree on what ought to constitute psychology, or vice versa. Fortunately, it turns out that these questions are related so that the answer to one is at least affected if not inspired by one's answer to the second. This relationship exists on several levels: First, any historical undertaking is concerned with the activities, artifacts, expressions and desires of human beings

and human collectives. Such conceptions of human nature that the historian has, and that are predominant in the discipline and culture of the historian, must obviously influence the work at hand, or on some accounts of history, make the work possible in the first instance. Second, the disciplinary history of psychology is also, in part, a history of the activities and artifacts of human beings, namely psychologists. Hence, the historian of psychology is first of all a historian, piecing together a narrative or account of places, persons, desires, contexts and ideas.

Nevertheless, if our conception of human persons is like that of the mainstream of the discipline, that is, largely scientific, individualistic and functional, then our history will focus on the development of disciplinary achievements and not on the institutional, political, social or even depth-psychological forces involved in creating such a model of human being in the first place. Or, if it is our primary goal to tell a story of the rise and development of aspects of the discipline proper, it is likely that we will remain confined to a disciplinary trajectory.

To illustrate, I want briefly to examine critical history's perennial foil, Edwin Boring.² The first sentences of the 1929 edition of his text are,

The history of psychology is inextricably bound up with the history of philosophy, whereas the rise and development of experimental psychology is explicitly a phase of the history of scientific endeavor. (p. 3)

As Boring himself noted later in his memoirs, he wrote his history of psychology out of a conception of history as progress. He noted that "... History is an ever-flowing stream through the centuries, a stream of events that occur in the nervous systems of persons situated so that their thoughts and acts become links in the course of progress" (Boring, 1961, p. 49). Such presentist history has long been criticized and the obvious variants still available today in the form of some undergraduate text books only speak to the lasting importance of such historiography to a discipline without a center, still uncertain about its scientific and institutional status.

As soon as notions such as "progress" and the very history of science itself become contested, however, then the writing of such a disciplinary history becomes a matter for revision. And it is here that I would place Professor Danziger among the foremost practitioners of this revisionist history in psychology. Inspired by new histories of science and the social studies of science, it was possible to confront the seemingly ironclad notion of the division between an internal reconstruction of science and its external reconstruction. The theories of scientists are generated in the *activities* of scientists that are conducted in social institutions that have at least some of the characteristics of other, non-scientific social institutions. Knowing the historical location and specificity of our activities ought to be, on this account, a normal part of the understanding of generating theory and research.

Indeed, both the understanding of the history of a problem and the construction of its theory are, on this account, not radically different activities. Among

other things, history makes it possible to ask what the relationship is between one's interest as a scientist, one's membership in a particular local community and one's accounts of one's scientific activities. For example, the relationship between ideas of intelligence and intellect, the institutionalization and universalization of education, and the grading of human abilities all played a role in the development of the intelligence test in a manner that complicates any story of heroic pioneers who developed such tests (e.g., Danziger, 1997a). The post-war institutionalization of North American experimental social psychology can hardly be conceived along the lines of brilliant individuals applying a new technology to a whole new field of human experience using selected insights garnered from Kurt Lewin. Instead, a complex relationship exists between ambitious post-war psychologists (who largely came from working-class urban environments), the need to demarcate social psychology from other fields of endeavor inside and outside psychology, and the recognition that only a psychology based on individuals would ever survive as social psychology inside the discipline. Along with more idiosyncratic contributions of the individuals involved, this provides a more coherent and context-sensitive account of the development (and failures) of contemporary social psychology (e.g., Stam, Lubek & Radtke, 2000). The point is not that the history of intellectual endeavors is complex but that the history of such endeavors are always unfolding and open to further contextualization and elaboration and can never *just* be accounts of intellectual achievements.

As an aside, note that the focus on the formative activities of scientists in laboratories and their relation to theory are also a consequence of the under-determination of theories by data. That is, in the absence of genuine epistemological authority, most notably within the human sciences (see e.g., Weimer, 1979), scientists must retreat to sophistication and commitment as well as traditions of investigation. But such a retreat is negligible if the practical activities of one's scientific activities are determined to be progressive by the community of science or one is able to demonstrate technological achievements to the world at large. In the absence of both, the activities of psychologists are important not only for what they reveal of the construction of a discipline but for what they hide. That is, psychological theories both permit and premit, precisely what a historical recounting of the activities of psychologists ought to open up to view. History makes visible not just the obvious but the hidden interrelated processes of constructing and then separating data and theory out of a world of artifacts.

In *Constructing the Subject* (1990a) Danziger demonstrates how a treatment of the investigative practices of the discipline radically shifts the emphasis of disciplinary history. Rather than tracking a unitary conception of the discipline, the question of investigative practices makes clear how psychology became a hybrid of various technologies of investigation. Together with a division of labor in the laboratory, the careful development of markets for its knowledge and the incorporation of statistical devices into its methods and manner of theorizing in the

aggregate, psychology's history no longer resembled the kind of linear, incremental enterprise we had come to expect from histories of psychology. In a later paper Danziger (1993a) extends this analysis to the "historicity of psychological objects" or the very things to which our theories refer. Here he reminds us that our objects of investigation are constructed, that is, they are the product of human activities, they have definitive uses, and they have a reference that itself needs to be explicated. This paper on the historicity of psychological objects seems to me a transitional one, pointing to a need for further analysis that was left open by the use of the term "objects." The latter have the status of kinds of 'hybrid' entities (or quasi-objects, cf. Latour, 1993) that are at once natural and social, material and discursive. Latour's claim is that distinctions between 'constructed' and 'material' accounts are misguided, all of our research objects have the character of both material and discursive, socially mediated properties. The construction and proliferation of such hybrids is not just the outcome of an investigative practice but includes the reformulation of powerful linguistic resources as well.

It seems that this problem is addressed in *Naming the Mind* (1997a). In this volume the project is extended to the level of concept and terminology (rather than investigative practices or their objects). Indeed, by not just focusing on theory or strictly formal expositions, Danziger is able to keep from lapsing into old debates on the nature of psychological categories. Instead he argues that the very act of categorization in psychology displays a naive naturalism whereby natural kinds are presumed to exist in the categories that make up the theories of psychology. Yet by the time these theories are articulated in a formal sense, the act of naming and pointing to the appropriate object of investigation has already smuggled in a host of presuppositions and assumptions. Terms such as intelligence, emotion, motivation and the like are neither neutral nor natural but carry histories of conceptualization and use that deeply influence the possibilities open to the psychological theory that uses the concept. As Danziger notes, some of our most important terms are scientized and institutionalized variants of an eighteenth-century moral language.

Naming the Mind completes the earlier study of the investigative practices of psychologists in *Constructing the Subject* by combining this work with a categorical and discursive study. Danziger's argument shifts from the crucial role played by investigative practices to the language guiding and in turn produced by those practices. As I will discuss below, this shift is important for the way Danziger has come to see the shaping of the discipline and the importance that a psychological discourse has above and beyond the research practices of its members. In short, Danziger argues that psychology established itself institutionally through astutely combining universal biological meanings with local social meanings that were mediated by the development of specific technologies. Certain investigative practices, certain methods of psychological research and assessment—intelligence and personality testing, techniques for measuring the strength of attitudes and motives, standard learning situations, and so on—provided the basis for constituting classes

of scientifically validated phenomena that could be produced in a variety of practical settings. In the course of time, the role of such technologies in establishing the meaning of psychological categories became ever more decisive.

At this point we have come full circle, for the investigative practices are implicated in, and part of the discursive structure of the discipline. To return to my earlier formulation, it is here that theorizing and observing are indeed activities that are not separate but in their mutual organization and maintenance come to constitute disciplinary practices and findings. To stay with the visual trope, both observing and theorizing are attempts to make visible that which is conceived of as invisible and to render invisible or subsidiary other, competing accounts of the subject matter. The creation of objects of investigation and the findings related to those objects are rhetorical accomplishments as well as moments of invention. It is not only the language of psychology that is changed and shaped by the constitution of these objects but it is practices that are made possible by the objects in their creation.

Narrating history in this manner leaves open a question that Danziger himself has worried about in his work across the span of two decades, namely, what are the implications for the current enterprise of psychology, or as he asked in *Constructing the Subject*, “when allowance is made for the factors that led to a relativizing of psychological knowledge, is there no remainder?” (p. 192). I will return here to an earlier worry that I noted in my review of *Constructing the Subject* (Stam, 1992), but with an intervening decade to consider the problem I would like to take a slightly different approach to this question. I was originally concerned that in that book, Danziger had backed out of the implications of his own analysis by noting that psychological realities could not be entirely accounted for by the limits of their investigative contexts and remained hidden under a veil of socially constituted practices. Like some of Danziger’s critics (e.g., Ash, 1993; Mills, 1993) there is a continuing worry that something is being glossed or overlooked and that that *something* in fact consists of the core phenomena of the discipline.

Danziger’s answer to this was initially to call on a form of critical realism as a solution (Bhaskar, 1978). The domain of the real was distinguished from the domain of the actual, on Bhaskar’s account, and the possibility was held up that there are determinant psychic mechanisms responsible for, or underlying, the observed regularities constituted through the investigative practices of psychologists. However, Danziger himself has moderated these claims on the realist-relativist question in his further work. For example, in his 1993 paper Danziger argues that psychology’s objects are not natural kinds and that methods are not theoretically and ethically neutral. Instead, argues Danziger, theories ought to be evaluated on criteria of practical consequences and reflexivity. By the time of the publication of *Naming the Mind*, this has retreated even further to the background. Here Danziger refers not to ‘objects’ but to the problematic relationship between discursive categories and the phenomena themselves. Danziger clearly notes that the relationship

here is *constitutional*, not representational, by which he means that a psychological object depends on “its human creator and the relationship between the object’s existence and its representation has become quite intimate” (p. 187). Relying on Ian Hacking’s notion of a ‘human kind’ as opposed to a ‘natural kind,’ Danziger notes that psychological objects aren’t just legends either. They have a circulation (in Hacking’s words [1994] they are subject to “looping effects”) in a cultural and human context and their circulation amends as well as reifies the phenomena in question.

WHAT CAN HISTORY BE?

Although his critics have accused him of, among other things, being a sociological reductionist (Mills, 1993) or of denigrating the possibilities of writing a history from the ‘inside’ of the discipline (Rappard, 1997)³, I think these critiques are off the mark (and Danziger has spoken eloquently for himself in reply, e.g., Danziger, 1993b, 1997b, 1998). I would like to place my comments in the context of broader debates in the philosophy of history. This is because the critiques of the work of historians such as Danziger are often couched in terms of the pernicious effects of relativism and explicitly or implicitly are aimed at propping up some conception of realism (e.g., Fox-Genovese & Lasch-Quinn, 1999). In his mature writings, it was R. G. Collingwood who recognized the mistake in this for the enterprise of history. Often accused of skepticism himself by reviewers of *The Idea of History* (1946), Collingwood was careful not to become mired in this debate. For after all, in his earlier works such as *Speculum Mentis* (1924) he endorsed a realist program for history, if only implicitly, and by the time the *Idea of History* was published he had worked out precisely why he was not a realist. Skepticism, he argues, is a consequence of realism, “the discovery that the past as such is unknowable is the skepticism which is the permanent and necessary counterpart of the plain man’s realism” (1965, p. 100). It is the search for a factual past that is an illusion because the past as such can never be known again. Instead it led Collingwood away from “an unknowable past-in-itself” to the activities of historians themselves (Goldstein, 1970). Here Collingwood is often seen as relegating history to an act of imagination but this is too quick: In a paper on the historical imagination appended to the *Idea of History* he argues,

... neither the raw material of historical knowledge, the detail of the here-and-now as given him in perception, nor the various endowments that serve him as aids to interpreting this evidence, can give the historian his criterion of historical truth. That criterion is the idea of history itself: the idea of an imaginary picture of the past. That idea is, in Cartesian language, innate; in Kantian language, *a priori*. It is not a chance product of psychological causes; it is an idea which every man possesses as part of the furniture of his mind, and discovers himself to possess in so far as he becomes conscious of what it is to have a mind. (1946, p. 248)

What keeps the “self-dependent, self-determining, and self-justifying” (p. 249) historical imagination from falling into skepticism is the discipline of history itself. Although Collingwood was not entirely clear about this, it is the structure of the discipline and what this discipline considers as good research practices, reliable evidence and the like that prevents the individual knower/historian from sliding off into the *mere* play of imagination. And Collingwood defended the notion of the autonomy of history precisely to preserve its status as a communal enterprise (Goldstein, 1970).⁴

Collingwood saves history from the endless spiral of skepticism by an explicit turn to the imagination or the psychology of the individual historian. By extension, the community of historians makes history possible outside of any other authority. In this manner, Collingwood sees clearly that it is in its communal activities that historians decide history. This formulation predates the work of others who take up the problem of narrative, plot, and understanding, in particular Paul Ricoeur and Hayden White. The latter become preoccupied with a question of how language in its myriad forms makes the structure of story possible but when Ricoeur argues (against positivist textual objectivity) for a dialectic of understanding and explanation he means, in a manner reminiscent of Collingwood, that understanding is the ability to take up again, within the self, the work of structuring that is performed by the text. Explanation is always secondary to this understanding in that it consists in bringing to light the codes underlying the work of structuring. It is clear that understanding for Ricoeur is an imaginal act and explanation is made possible by the discourse available to us from our cultural understanding and presuppositions. History must be configured and brought into meaningful relation with other events in time, that is, made subject to emplotment.

In the first volume of *Time and Narrative* Ricoeur (1984) attends to the necessity of narrative (through configuration and emplotment) for a historical understanding. Indeed, history must be configured, it must be brought into a meaningful relation with other events in time. Like Hayden White, Ricoeur argues that history is combined of the found and made-up, of the documented and the narrated. Without configuration and emplotment there can be no history. Once narrated history is appropriated it is not only meaningful but it in turn becomes the ground for further configuration. Narratives extend the past into the present and make it possible to imagine a future (Ricoeur, 1988).

Like Ricoeur, a range of historians of the twentieth-century has taken up the problem of the relation between the found and the made-up or the documented and the narrated. Hayden White (1973, 1978) too has argued that to produce a history, the chronicle must be converted to a meaningful narrative and hence must be emplotted. But the past has no plot and hence the historian provides an account, a narrative that emplots or encodes the traces or evidence. White is more formalist than Ricoeur (and other narrative historians), however, in so far as he argues that modes of emplotment are fundamentally dependent on tropes since there is no other entry into the rhetorical structure of language. Indeed, figurative characterizations

are presupposed by the events to be represented and hence White's claim that language operates tropologically to prefigure a field of perception. The boundary between the language that makes history and the content of that history remains always opaque.

Despite White's move into the formalism, so characteristic of later 20th century theorizing, he effectively supports Collingwood's contention that history is an independent enterprise (even as he does not support Collingwood's notion that history requires the reenactment of historical agents). All such theories of history are meant to prevent the encroachment of positivism and scientism on a historical consciousness. They embody the insights characteristic of the *Geisteswissenschaften* debates of the late 19th century where Dilthey already formulated the notion that lived experience is mediated through the imagination as well as the socio-cultural practices of the historical world (Makkreel, 1992; Mos, 1996).

It was Ankersmit (2002) who recently argued that historical representation is a matter of the organization of the truth rather than the truth itself. Our representations may be "sensible, fruitful, helpful, thought-provoking (or not), but, while the data deployed may be true or false, the proposal deploying them cannot be" (p. 38). Hence the criteria are broadly aesthetic; there is no direct line back to the agents of history except through another point of view. But this point of view is not in the past but is embedded in the aesthetic language of the historian (Ankersmit, 1996). On this account history takes its force precisely from the need to represent the past in the absence of a fixed algorithmic manner of moving from the past to writing about that past (see also Stam, 2003).

Attempts to retain for history its privileged capacity to judge the past are rare and today they exercise mostly those who see themselves as defenders of some version of 'objectivity' in the self-styled culture wars whose battles seem to be largely confined to university campuses in the United States (see Fox-Genovese & Lasch-Quinn, 1999). To return to Danziger's work on psychology then, in the context of the larger debates in the philosophy of history Danziger's work is not nearly as controversial or threatening as it appears to psychologists. But precisely because his audience has consisted largely of psychologists (and it would be a mistake not to write for psychologists), Danziger finds himself on the defensive for reasons that might seem odd to professional historians. Part of this is due to the role that histories of psychology have traditionally played in the discipline.

Historical studies of psychology are first and foremost histories. Nonetheless, as Danziger has already pointed out on several occasions, their traditional function was to serve a pedagogical role within the discipline (e.g., Danziger, 1994). This function precludes historical studies from contributing to psychology as a disciplinary project and constitutes a mobilization of the tradition for the purposes of celebrating the accomplishments of the past and justifying the present. Such representations of the past are premised on the continuity of the present. On an aesthetic reading such histories are the least interesting and most conventional. They reflect

the discipline as we have come to understand it, without in any way illuminating the subject matter of the discipline, namely the nature of human psychology itself.

What Danziger has demonstrated with his histories of psychology is a way of proceeding that allows us to turn history on the subject matter of psychology itself. In this respect psychological studies are always historical; they reflect the formalization of language and the development of techniques that emerge out of our shared cultural goods. In that sense they do not entirely escape their origins in particular life-worlds. For even when we apply such routine tools as statistics to our psychological topics we do not escape a concern with number, efficiency, normativity and so on that are entailed in such devices.⁵

CONCLUSIONS

Disciplinary histories are specialized forms of history but history nonetheless. What Danziger's work makes so clear with respect to Collingwood's claim that it is the community of historians that ultimately regulates the work of the historian, is that likewise, it is the community of psychologists that regulates the work of the psychologist. What we do not know is, which discipline is to be regulative for the *history* of psychology. It is here that we can see the argument most clearly, for it is those who are wedded to a progressivist or positivist notion of history that see a limited role for that history and wish that history to be on bended knee before the *scientific* authority of psychology.⁶ But the respect and authority of science can never be granted to a historical account of it, even if that history is merely 'celebratory' or presentist. For history cannot be science, in the same way that history is never *just* literature. It is here that psychology and history come together, for in order to know what the institution of psychology *is* we must have a history of it as it has been practiced. Yet the *history* of psychology already presupposes that we know what psychology in fact is. Hence the inseparability of the enterprise of determining the subject matter of psychology from its history. The story we tell about psychology is always both a historical and an implicitly teleological one.

Critical historians of psychology have shifted their allegiance and they are no longer beholden to the scientific claims of the discipline. After all, these are exactly what need to be understood again from a historical perspective. Their regulative community exists elsewhere, in the history of science, within the community of critical psychologists, and so on. Hence their histories contribute theory to different communities with different sensibilities and criteria for knowledge. It is not that these communities necessarily speak incommensurate languages, but there are recognizable differences. It is Professor Danziger who is among the very best of those who have shown us that the picture of paradise created by traditional psychological histories was illusory and having tasted the forbidden fruit of critical

historical knowledge there is no return from the exile in which we find ourselves. The vision in Danziger's work then consists of a discipline that is no longer fettered to the chains of an epistemology that constricts our theoretical claims at every turn. In his own words,

... changes in psychological categories will continue to be heavily dependent on changes in the societies within which these categories have a role. Their meaning will continue to be negotiated and contested among the groups to whom they matter. (Danziger, 1997a, p. 193)

To end where I began, I would like to close this chapter with another anecdote: several years ago I attended a conference in Canada and was engaged in conversation by a retired colleague from a western Canadian university. He asked me to recommend some historical works on a particular topic and, as luck would have it, he just happened upon a topic that allowed me to rattle off a series of book titles. Impressed, he inquired, "Weren't you a graduate student of Kurt Danziger's?" I had to disappoint him and told him that no, I had studied with the late Nick Spanos, who although having had historical interests, was better known for his critical work on hypnosis and multiple personality. My colleague seemed disappointed but I took it as a compliment. I can only hope that Professor Danziger takes it the same way.

NOTES

- ¹ Department of Psychology, University of Calgary, Calgary, Alberta, Canada T2N 1N4. An earlier version of this paper was presented at a symposium in honor of Kurt Danziger at the European Society for the History of the Human Sciences meetings, Berlin, Germany, August 2000. I thank Adrian Brock and the organizers of that symposium for inviting me to participate and I am grateful to Kurt Danziger and the Editors for their generous comments on earlier drafts.
- ² I should add here that I do not wish to denigrate Boring's contributions to the institutional development of the history of psychology, especially with regard to the important role he played in legitimating historical studies as a pursuit *within* psychology. Boring could also be ambivalent in his presentism: "a psychological sophistication that contains no component of historical orientation seems to me to be no sophistication at all" (1929, p. vii).
- ³ Or even of criticizing "celebratory" accounts in favor of "condemnatory" accounts (Dehue, 1998).
- ⁴ Connelly and Costal (2000) have recently argued that Collingwood's ideas on history also contained a version of a historical psychology that remains largely unelaborated.
- ⁵ One reviewer of this chapter noted that this and other descriptions makes it appear that Danziger's work has something in common with the French *Annales* school which formed around Fernand Braudel in the 1950s and 60s. Known for its 'total' approach to history, there were no details of daily life too large or too small to contribute to historical accounts (often called 'social history'). Braudel was famous for wishing to break down the boundaries of the social sciences in the name of an 'interscience.' Nonetheless, Danziger does not share the school's penchant for economic explanations and the need for structural accounts, however sophisticated. Furthermore, intellectual work must always be more than the product of economic and social history since it is constituted in an international discourse that is continually contested across large geographical, social and economic domains.
- ⁶ Kendler's (1987) textbook is perhaps one of the clearest examples of this.

REFERENCES

- Ankersmit, F. R. (1996). *Aesthetic politics: Political philosophy beyond fact and value*. Stanford: Stanford University Press.
- Ankersmit, F. R. (2002). Representational democracy: An aesthetic approach to conflict and compromise. *Common Knowledge*, 8(1), 24–46.
- Ash, M. G. (1993). Rhetoric, society, and the historiography of psychology. In H. V. Rappard, P. van Strien, L. P. Mos & W. J. Baker (Eds.), *Annals of Theoretical Psychology, Vol. 8* (pp. 49–57). New York: Plenum.
- Bhaskar, R. (1978). *A realist theory of science*. Atlantic Highlands, NJ: Humanities Press.
- Boring, E. G. (1929). *A history of experimental psychology*. New York: Century Co.
- Boring, E. G. (1961). *Psychologist at large*. New York: Basic Books.
- Collingwood, R. G. (1924). *Speculum mentis*. Oxford: Clarendon.
- Collingwood, R. G. (1946). *The idea of history*. Oxford: Clarendon.
- Collingwood, R. G. (1965). *Essays in the philosophy of history*, edited with an introduction by W. Debbins. Austin, Tex.: University of Texas Press.
- Connelly, J. & Costal, A. (2000). R. G. Collingwood and the idea of a historical psychology. *Theory & Psychology*, 10, 147–170.
- Danziger, K. (1990a). *Constructing the subject: Historical origins of psychological research*. Cambridge: Cambridge University Press.
- Danziger, K. (1990b). The social context of research practice and the history of psychology. In W. J. Baker, M. E. Hyland, R. van Hezewijk & S. Terwee (Eds.), *Recent trends in theoretical psychology, Vol. II* (pp. 297–303). New York: Springer-Verlag.
- Danziger, K. (1993a). Psychological objects, practice, and history. In H. V. Rappard, P. van Strien, L. P. Mos & W. J. Baker (Eds.), *Annals of Theoretical Psychology, Vol. 8* (pp. 15–47). New York: Plenum.
- Danziger, K. (1993b). History, practice, and psychological objects: Reply to commentators. In H. V. Rappard, P. van Strien, L. P. Mos & W. J. Baker (Eds.), *Annals of Theoretical Psychology, Vol. 8* (pp. 71–84). New York: Plenum.
- Danziger, K. (1994). Does the history of psychology have a future? *Theory & Psychology*, 4, 467–484.
- Danziger, K. (1997a). *Naming the mind: How psychology found its language*. London: Sage Publications.
- Danziger, K. (1997b). The future of psychology's history is not its past: A reply to Rappard. *Theory & Psychology*, 7, 107–111.
- Danziger, K. (1998). On historical scholarship: A reply to Dehue. *Theory & Psychology*, 8, 669–671.
- Dehue, T. (1998). Community historians and the dilemma of rigor vs relevance: A comment on Danziger and Van Rappard. *Theory & Psychology*, 8, 653–661.
- Fox-Genovese, E. & Lasch-Quinn, E. (Eds.) (1999). *Reconstructing history*. New York: Routledge.
- Goldstein, L. J. (1970). Collingwood's theory of historical knowing. *History and Theory*, 9, 3–36.
- Hacking, I. (1994). The looping effects of human kinds. In D. Sperber, D. Premack & A. J. Premack (Eds.), *Causal cognition: A multi-disciplinary approach* (pp. 351–383). Oxford: Clarendon Press.
- Hanson, N. R. (1958). *Patterns of discovery*. Cambridge: Cambridge University Press.
- Kendler, H. H. (1987). *Historical foundations of modern psychology*. Pacific Grove, CA: Brooks/Cole.
- Kuhn, T. S. (1970). *The structure of scientific revolutions*, 2nd ed. Chicago: University of Chicago Press.
- Latour, B. (1993). *We have never been modern*. Cambridge, Mass.: Harvard University Press.
- Losee, J. (1980). *A historical introduction to the philosophy of science*, 2nd ed. Oxford: Oxford University Press.
- Makkreel, R. A. (1992). *Dilthey: Philosopher of the human studies*. Princeton, N.J. Princeton University Press.

- Mills, J. (1993). Contextualizing Danziger within sociological theory. In H. V. Rappard, P. van Strien, L. P. Mos & W. J. Baker (Eds.), *Annals of Theoretical Psychology*, Vol. 8 (pp. 65–70). New York: Plenum.
- Mos, L. P. (1996). Immanent critique of experience: Dilthey's hermeneutics. In C. Tolman, F. Cherry, R. van Hezewijk, & I. Lubek (Eds.), *Problems of theoretical psychology* (pp. 368–377). Toronto: Captus.
- Rappard, J. F. H. van (1997). History of psychology turned inside(r) out: A comment on Danziger. *Theory & Psychology*, 7, 101–105.
- Ricoeur, P. (1984). *Time and narrative*, Vol. 1. (K. McLaughlin & D. Pellauer, trans.). Chicago: University of Chicago Press.
- Ricoeur, P. (1988). *Time and narrative*, Vol. 3. (K. Blamey & D. Pellauer, trans.). Chicago: University of Chicago Press.
- Stam, H. J. (1992). Deconstructing the subject: Banishing the Ghost of Boring. [Review of *Constructing the subject: Historical origins of psychological research*]. *Contemporary Psychology*, 37, 629–632.
- Stam, H. J. (1996). Theory & practice. In C. Tolman, F. Cherry, R. v. Hezewijk, & I. Lubek (Eds.), *Problems of Theoretical Psychology* (pp. 24–32). Toronto: Captus Press.
- Stam, H. J. (2000). Theoretical Psychology. In K. Pawlik & M. R. Rosenzweig (Eds.), *International Handbook of Psychology* (pp. 551–569). London: Sage.
- Stam, H. J. (2003). Retrieving the past for the future: Boundary maintenance in historical and theoretical psychology. In D. B. Hill and M. J. Krall (Eds.), *About psychology: Essays at the crossroads of history, theory, and philosophy* (pp. 147–163). New York: SUNY Press.
- Stam, H. J., Lubek, I. & Radtke, H. L. (2000). Strains in experimental social psychology: A textual analysis of the development of experimentation in social psychology. *Journal of the History of the Behavioral Sciences*, 36, 365–382.
- Weimer, W. B. (1979). *Notes on the methodology of scientific research*. Hillsdale, N.J.: Erlbaum.
- White, H. (1973). *Metahistory*. Baltimore: John Hopkins University Press.
- White, H. (1978). *Tropics of discourse*. Baltimore: John Hopkins University Press.