The Legacy of Positivism in Empirical Composition Research

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Most researchers would agree that, to know what you’re doing, you need to know how your model of knowing affects what you are doing. Your model, however, may not be the same as your colleague’s.

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David Foster’s recent article in volume 8 of the Journal of Advanced Composition, “What Are We Talking About When We Talk About Composition?” makes me once again aware that some folks in composition studies perceive an inchoate hegemony of empirically minded researchers who, corseted by their zeal to be accountable to the norms of science, resist the view that “much important knowledge in composition is not and cannot be scientific” (34). I’m not sure that I know what Foster means by “scientific,” but his use of the term conjures up images of white-coated researchers running subjects through mazes in bare, square rooms with two-way mirrors.

This is a disturbing thought; I’d like to think that, as a profession, we had moved beyond these xenophobic images, since the days when Janet Emig described the important work of Elliot Mishler (“Inquiry”). It was Mishler who called attention to the lack of ecological validity of studies using context-stripping methods to study human behavior and cognition. And I’d also like to think that since Shirley Heath published her ten-year study of the difficulties non-mainstream children face when they enter public school, those of us in the education game have become acutely aware of the need to locate our research efforts in studies of literacy and other issues that will actually help teachers better serve children, adolescents and other language users.

Nevertheless, Foster’s essay and John Schilb’s “Ideology and Composition Scholarship”(which also appears in volume 8) suggest to me that hermeneutically oriented observers of the composition scene may perceive the devils of positivism to be alive and well in the empirical research community. Although it may be déclassé to speak of positivism in a post-Kuhnian universe (“post-positivist” is the term that philosophers of science and others use), it appears that to Foster and Schilb positivism’s legacy—in the form of “scientistic” assumptions (Foster) and the obfuscating rhetoric of “empiricism” (Schilb)—is still very much with us.
I’m not sure that I entirely disagree. And thus I’d like to focus in this essay on what I see as the legacy of positivism that many empirical researchers have inherited. I’d like first to distinguish between different kinds of positivism—some of which withered during the first half of this century, and others which appear to be more intransigent. I will discuss the sources of this intransigency by describing three logical positivist doctrines—physicalism, operationalism, and the unity of science—which have become interwoven into the behaviorist rhetoric of the 1940s and 1950s. Then, drawing from research conducted by Charles Bazerman, Lucille McCarthy, John Swales, and Thomas Huckin, I will suggest that behaviorist/positivist rhetoric has been codified in “charter texts” such as the *Publication Manual of the American Psychological Association* and Donald T. Campbell and Julian C. Stanley’s *Experimental and Quasi-Experimental Designs for Research*. Yet, despite the historical influence of these texts, the emergence of alternative research paradigms in educational research (as well as in the other social sciences) suggests that other epistemologies, especially constructivist and phenomenologist, are flourishing.

Disagreements and rival camps in composition are virtually inevitable because the journals and conference papers that a composition specialist reads and hears reflect a diversity of backgrounds, training, and methodological preferences. Strong criticism from colleagues with rival theories keeps us honest; too much discord, however, may be counterproductive, especially when it is based on a lack of knowledge regarding the differences between our model of knowing and our colleagues’. When we articulate our models of knowing and discuss our differences in good faith, it becomes much easier to stop hurling epithets (such as “number-crunchers” and “storytellers”) and engage in the kind of “multimodal approaches” that Janice Lauer and others have advocated (Lauer; Lauer and Asher).

**Positivist Thought in the Emerging Social Sciences**

Within the social sciences, specifically educational research, a number of scholars have attempted to trace the history of the paradigmatic differences between quantitative and qualitative methodologies. Much of this inquiry has focused on the different epistemological assumptions that underlie each methodological approach. In the process of examining the “model of knowing” that informs experimental research, scholars have described the historical roots of positivism in a fair amount of detail. For example, John K. Smith (a sociologist of education) and Denis Phillips (a philosopher of science) see positivism rooted in the writings of Auguste Comte (who coined the term), Emile Durkheim, and John Stuart Mill. Comte, Durkheim, and Mill believed that social and cultural phenomena were amenable to scientific investigation. For Comte, “sociology,” as he called it, was simply the most recent application of scientific methodology. As he saw it, scientific methodology was a systematic form of inquiry that had emerged in mathe-
matics and astronomy and later had expanded its domain with the birth of modern chemistry and the other physical and biological sciences. Durkheim, while differing from Comte and Mill on a number of points, also held that objects of study in the social sciences could be investigated using the methodology of the "hard" sciences. We are fairly familiar with the assumptions in present day social science that derive from Comte's, Mill's, and Durkheim's views:

- social scientists, like physical scientists, are detached from their objects of study
- investigations of social phenomena can therefore be conducted in a value-neutral fashion, the researcher eliminating all personal bias and preconceptions and employing language that expresses objectivity
- social science, like the physical sciences, is nomothetic—that is, it is possible to extrapolate from social scientists' data social laws that apply across numerous contexts

John K. Smith points out that the followers of this school of thought believed that social laws, like physical laws, would

state the necessary and invariant relationships that existed between and among social objects. Furthermore, these laws, as formulated with the use of Mill's canons of inquiry, such as concomitant variation and differences, would allow not only the explanation of social phenomena, but also for the ability to discover causes and to make predictions. These related possibilities were seen as essential for any active intervention to improve society. (7)

The Idealists

There were, however, challenges to the positivist school of thought. In Germany, a countermovement headed by Wilhelm Dilthey and Max Weber was gaining momentum. Unlike Comte, Mill, and Durkheim who were operating within the empiricist tradition of Locke and Newton, Dilthey's and Weber's views can be seen as related to an idealist tradition closely related to Kantian phenomenology. Central to Dilthey's conception of "cultural studies" was the view that social "facts" and human values were not dichotomous entities but were inextricably bound up with each other. This meant that (1) the subject of cultural studies was the product of the observer's mind and therefore could not be regarded as being separate; (2) the values of the observer entered into decisions about what was to be studied and what kinds of understanding were significant; and (3) the observer was both subject and object of inquiry—existing in a subject/subject relationship to so-called objects of study, rather than the observer/object of study dichotomy that the positivists claimed.

Max Weber, although differing in many respects from Dilthey, also saw social science as a self-reflexive activity which would not be served by
importing methodology from the physical sciences. He agreed with Dilthey
that the cultural/social sciences were descriptive and ideographic rather than
explanatory or predictive, and he believed that searching for overarching laws
or regulative principles was feckless. To Weber and Dilthey, the goal of the
cultural sciences was understanding, or *verstehen*. The concept of *verstehen*
can be inferred from Weber’s maxim, “Man is an animal suspended in webs
of significance he himself has spun.” John K. Smith suggests,

A very basic definition [of *verstehen*] centers on the attempt to achieve a sense
of the meaning that others give to their own situations through an interpretive
understanding of their language, art, gestures and politics. . . . At its core the
essence of understanding is to put oneself in the place of the other—something
which is possible if one possesses a degree of empathy with the other or has
the disposition to recreate the experiences. (12)

We can observe a contemporary formulation of *verstehen* in Elliot W.
proaches to Qualitative Research.” Eisner writes,

Just how does one focus on experience and meaning? How does one make
sense of what is nonobservable? . . . One way is to make inferences from
observables to what is not observable. Manifest behavior is treated primarily
as a cue, a springboard to get someplace else. The other way is to “indwell,”
to empathize; that is, to imaginatively participate in the experience of another
. . . . The difference between the two is subtle but important. In the former,
observables are used in a kind of statistical fashion; one . . . estimates the
probability that *this* behavior means one particular thing or another. There is
no real need for empathy. The latter banks on the observer’s ability to
imaginatively project himself into the life of another in order to know what
that person is experiencing. (6)

Understanding these differences between nineteenth century positivism
and idealism as they related to the emerging fields of the social and human
sciences is important if we are to place our own debates over knowledge in
composition studies into a cultural and historical context. Arguments over
what constitutes *evidence* and what will pass muster as a *claim* have had many
antecedents in other disciplines: psychology, anthropology, history, sociol­
ogy and education, to name a few. Suffice it to say, the influence of scientific
inquiry and especially the warrants of scientific methodology grounded in
probability theory has surfaced many times in the twentieth century, despite
philosophical critiques of traditional models of science and the assertions of
numerous commentators (for example, Adorno, Habermas, Popper) that the
model of scientific inquiry as applied to the social sciences is inappropriate.
In particular, two schools of thought—logical positivism and behaviorism—have
been closely linked.
The Emergence of Logical Positivism in Europe

As a movement, logical positivism originated in Europe in the 1920s with a group known as the Vienna Circle. Members of this coterie represented a number of different scientific backgrounds. Moritz Schlick, Rudolf Carnap, Philipp Frank, and Schlick's student, Herbert Feigl, were trained in physics. Kurt Gödel, Hans Hahn, and Gustav Bergmann were mathematicians. Other members of the Circle included an economist-sociologist, Otto Neurath, a historian, Victor Kraft, and a lawyer, Hans Kelsen. Members of the Vienna Circle, although differing in many of their views, can be said to have shared a few goals. One goal was to expunge from science (and, by implication, the social sciences) any trace of what they considered "metaphysics." By metaphysics, they meant speculative propositions and constructs that could not be confirmed or falsified through empirical or logical methods. For example, by this criterion Freud's "theory" of the unconscious would be considered metaphysics. Related to this goal was the positivists' development of the verifiability principle of meaning, which has been paraphrased as, "if it can't be seen or measured (or is not coherent according to the formal axioms of symbolic logic), it is not meaningful to talk about" (Phillips 5).

According to the verifiability principle, utterances are meaningful only if they are capable of being verified empirically (that is, through sense experience) or if they are (formally) logically coherent. To the positivists, all other utterances were "metaphysics." The implication of this position was that no (philosophically) meaningful discourse could occur outside the realms of logic and science. The members of the Vienna Circle had great antipathy toward German speculative philosophy and toward sweeping metaphysical theories that had flourished on the continent during the previous century. Thus, despite their diverse backgrounds, the members of the Vienna Circle comprised a "community" in the sense that they agreed that all knowledge was amenable to the methods and doctrines of science—without resort to metaphysics. And because many members of the Circle were trained in logic, a corollary aim was to demonstrate the role of mathematics and logic in science and to "depict knowledge in a way that did full justice to its empirical and logical components" (Laurence D. Smith 27).

Despite their rather limited (in our eyes) criteria for evaluating the meaning of propositions, the logical positivists were not realists with respect to the status of theoretical entities. Theoretical entities, and also laws, could be thought of, as A.J. Ayer (one of the American positivists) put it, "simply as the conceptual tools which served for the arrangement of the primary facts" (qtd. in Phillips 6). Although the verifiability principle was discredited eventually among philosophers of science, vestiges of it remain in the lines of reasoning that underlie experimental research methodology. These vestiges can also be seen in the powerful warrants that experimental researchers invoke when they describe methods that will ensure "uncontaminated
data" and report results obtained through using statistical procedures that provide external validity for their studies.

**Logical Positivism in American Behaviorism**

Three doctrines that the logical positivists espoused are relevant for this discussion: operationalism, physicalism, and the unity of science. Agreement regarding these concepts between the logical positivists in Europe and the American behaviorists (Watson) and neobehaviorists (Tolman, Hull, and Skinner) has caused a number of writers to hypothesize an alliance between the two groups. The first of these doctrines, *operationalism*, refers to the idea that the meaning of a scientific term, concept, or proposition consists of the operations performed in defining or demonstrating it. Related to the operationalist perspective was the doctrine of *physicalism*, whose adherents held that every meaningful statement (other than tautologies) must refer directly to observable properties or to the spatial-temporal conditions of an activity or event. Meaningful statements did not, however, admit of the existence of a perceiving observer. Accordingly, the statement, "there is now a red triangle in my visual field," should be replaced by, "this thing is triangular and red." According to Laurence D. Smith, Otto Neurath even went so far as to argue that observation statements should be formulated in the third person. Thus, an observation reported by Neurath himself should be formulated as, "Otto now sees a red circle" or "Otto now joy" (58). By insisting on physicalist, data-focused statements, Neurath and his colleagues believed they could ensure that "psychological concerns" (concerns that relate to the nature of thought) were outside the realm of philosophically tenable propositions.

If physicalism were viable as a linguistic practice among social scientists, then the goal of the *unity of science* might be plausible. This notion had been around since Comte's time; however, the appearance of behaviorism in psychology suggested to some positivists that such a goal might be realized. Believers in the unity of science doctrine envisioned three types of unity: a unity of scientific concepts, of scientific laws, and of scientific method in all fields. The teleology of a *unity of scientific concepts* entails a reduction by one means or another of all scientific concepts to the language of physicalist observation. Such a reduction would bring about, according to Rudolph Carnap, "a unity of language in science, viz., a common reduction basis for the terms of all branches of science" (qtd. in Laurence D. Smith 62).

The doctrine of a *unity of scientific laws* was argued for by those positivists who believed that it would be possible to extrapolate from the sciences a hierarchy of laws. Within this hierarchy, the laws of social science could be seen to be derived from the laws of biology, which in turn could be seen to be derived from the laws of physics and chemistry. The third type of unity, a *unity of method*, was, in effect, methodological monism; that is, all sciences would employ the hypothetico-deductive method. Carl Hempel characterizes this type of unity like this:
Notwithstanding many differences in their techniques of investigation, all branches of empirical science test and support their statements in basically the same manner, namely by deriving from them implications that can be checked intersubjectively and by performing . . . the appropriate experimental or observational tests. This, the unity of method thesis holds, is true also of psychology and the social and historical disciplines.

(qtd. in Laurence D. Smith 63)

Given the reductive characteristics of the doctrines of physicalism, operationalism, and the unity of science, it is not surprising that a number of writers have hypothesized a meeting of the minds (if not an actual alliance) between the logical positivists and the behaviorists. For example, it appears that members of the Vienna Circle became familiar with John C. Watson’s experiments after reading Bertrand Russell’s *Analysis of Mind*, published in 1921. References to Watson’s works appeared in the writings of some members of the Vienna Circle in the early 1920s and Watson’s and Pavlov’s names were invoked in support of the physicalist perspective (Laurence D. Smith 60).

Although a so-called alliance between the later or “neobehaviorists” and the logical positivists has not actually been documented, Charles Bazerman appears to assume such an alliance. Bazerman argues that the development of the experimental article in psychology from 1929 (when the first *APA Publication Manual* appeared) through the late 1960s reflects a positivist-behaviorist world view with an appropriate rhetoric. He writes,

The positivist principles of “physicalism” and “operationalism” legitimated the behaviorist limitations of allowable questions, method, and data. The behaviorist method then could be considered identical to scientific method, excluding other forms of psychological investigation as unscientific. And the behaviorist rhetoric could be identified as the only proper way to write science. (“Codifying” 268; emphasis added)

The Positivist/Behaviorist World-View and the Experimental Article

In “Codifying the Social Scientific Style: the *APA Publication Manual* as a Behaviorist Rhetoric,” Bazerman traces the influence of successively more prescriptive *APA Stylesheets* on the conventions of the experimental article in psychology. His point is that experimental articles published between the 1930s and the present demonstrate both the influence of the increasingly more sophisticated statistical technology and a world view that stresses a rule-governed approach to research design. It’s possible to see how, under the historical conditions that Bazerman describes, “social accounting” for one’s results to the members of the research community could become the *sine qua non* for reporting one’s research (Yearley). The process of social accounting is quite subtle and involves the writer’s using the conventions of the four-part structure of the experimental article as rhetorical resources. First, in the
article introduction the researcher formulates the problem statement within the context of the literature review. By doing this, the researcher can demonstrate that the question or problem that his or her study addresses is relevant to established knowledge in the field. At the same time, the researcher must lead readers to see the gap or lack in the literature that has led to the present study. John Swales has described this strategy as "creating a research space" (79). Secondly, in the methods section the researcher demonstrates that his or her methods are aimed at producing uncontaminated results. Bazerman suggests that the main function of the methods section is "to protect the researcher's results by showing that the experiment was done cleanly and correctly" ("Codifying" 272). Through this process of social accounting to the research community in the introduction and methods sections, the writer is positioned to draw the community's attention to "the news," that is, the results section. That is, having located the study in the community's established knowledge, and having dutifully noted the correctness of his or her procedures, the researcher is positioned to introduce original claims in the results section. The discussion section follows, confirming the descriptive hypothesis, and also, according to Thomas Huckin, placing the results (the news) once more into the context of accumulated knowledge in the field. Thus, what is original (and potentially threatening) in the study is neutralized through its being contextualized into the community's existing knowledge.

The audience considerations that I've described above are based on recent rhetorical studies of the conventions of the experimental article in the social and physical sciences. In particular, Bazerman's study of the experimental article as an evolving literary form is relevant to this analysis. Central to Bazerman's historical account is his view that a positivist/behaviorist view of scientific accountability became codified into the various conventions of the experimental article. John Swales' and Thomas Huckin's research studies have added further confirmation of the "stabilized behaviorist rhetorical universe" that Bazerman describes.

A Charter Document in Education Research

Another way to consider the significance of the APA Publication Manual to a discourse community is to consider it as a "charter document." Lucille McCarthy draws a parallel between the charter documents of political, religious, and social groups and those of discourse communities in the sciences and social sciences. McCarthy suggests that a charter document provides

an organizing framework that specifies what is significant and draws people's attention to certain . . . relationships . . . The charter defines as authoritative certain ways of seeing and deflects attention from other ways. It thus stabilizes a particular reality and sets the terms for future discussion. (N. pag.)
The concept of the charter document is, I believe, useful for showing the legacy of positivist thinking on the conventions that researchers use when they write. McCarthy applied this concept operationally to investigate the effects of the American Psychological Association's *Diagnostic and Statistical Manual of Mental Disorders* (third edition) on a child psychiatrist's use of conventions in interviewing and writing a diagnostic evaluation of a head trauma patient. It also seems appropriate to think of the *APA Publication Manual*, as described by Bazerman, as another kind of charter document, especially with regard to its function in codifying the conventions of the experimental article in psychology. Donald T. Campbell and Julian C. Stanley's monograph, *Experimental and Quasi-Experimental Designs for Research*, seems also to have had the influence of a charter document in the field of educational research. Writers on methodology in educational research such as Elliot Eisner and Elliot Mishler have described the considerable influence of Campbell and Stanley's monograph on research in educational psychology. For the purposes of this essay, I will simply note that the graduate students that I observed in the rhetoric program at Carnegie Mellon University read *Experimental and Quasi-Experimental Designs for Research* in their introductory research course. I think it fair to say that since the monograph was presented to them as part of their formal instruction, that there would have been little reason for them to question the underlying epistemological assumptions of the text, at least at that point in their training.

At the beginning of their monograph, Campbell and Stanley describe their goal as an attempt to "achieve an up-to-date representation of the interests and considerations" of an earlier text on experimental methodology, W.A. McCall's (1923) *How to Experiment in Education* (1). Statistics technology had become increasingly more sophisticated in the 1940s and 1950s, they point out, and it was time to reconsider the parameters of experimental design in the light of the new technology. In addition to bringing educational researchers up to date, Campbell and Stanley appear to have had a rhetorical purpose in writing. They exhort their readers not to be discouraged by the negligible progress in experimental research in education, claiming that many behaviorist researchers defected from the experimentalist camp, abandoning the research reports for "essay writing, often accompanied by a conversion from . . . behaviorism to Gestalt psychology or psychoanalysis" (2). They then "explain" the failure of second and third generation researchers to continue in the work of experimentalists:

For the experimenters, a personal avoidance-conditioning to experimentation can be noted. For the usual highly motivated researcher the nonconfirmation of a cherished hypothesis is actively painful. *As a biological and psychological animal*, the experimenter is subject to *laws of learning* which lead him inevitably to *associate this pain with the contiguous stimuli* and events. These *stimuli are apt to be the experimental process itself*, more vividly and directly
than the "true" source of frustration, i.e., the inadequate theory. This can lead, perhaps unconsciously, to the avoidance or rejection of the experimental process. (3; emphasis added)

This story, as Campbell and Stanley tell it, is laden with behaviorist terminology and assumptions. Their explanation of historical events suggests a behaviorist gloss on a waning of interest in using experiments to investigate educational issues. Behaviorist and positivist tropes appear to merge, as they conclude their "analysis" by exhorting their colleagues to

somewhat inoculate young experimenters against this effect, and in general must justify experimentation . . . as the only available route to cumulative progress . . . We must instill in our students the expectation of tedium of disappointment and the duty of thorough persistence, by now so well achieved in the biological and physical sciences. (3)

In the passages quoted above, Campbell and Stanley's lexical choices and interpretive strategies seem designed to help them argue that science is still the royal road to knowledge, an argument that they support by noting that new technologies have made educational experimentation more precise than in the past.

The remainder of the Campbell/Stanley monograph is a highly detailed discussion (including an extensive literature review) of the new knowledge generated over forty years that should influence the construction of robust research designs. It's important to remember that Experimental and Quasi-Experimental Designs for Research was published in 1966—at a time when qualitative research on writing and reading was looked on with a fair amount of suspicion by quantitatively trained researchers. Therefore, it is not surprising to discover case-study research dismissed in the following perfunctory fashion:

Such studies have such a total absence of control as to be of almost no scientific value. The design is introduced here as a minimum reference point. Yet because of the continued investment in such studies and the drawing of causal inferences from them, some comment is required. Basic to scientific evidence . . . is the process of comparison, of recording differences, or of contrast. Any appearance of absolute knowledge, or intrinsic knowledge about singular isolated objects, is found to be illusory upon analysis. Securing scientific evidence involves making at least one comparison. . . . It seems well-nigh unethical at the present time to allow, as theses or dissertations in education, case studies of this nature. (6-7)

Thus, Campbell and Stanley take great pains to warn their readers that not only is non-experimental research non-scientific, but that dissertation advisors are acting unethically when they sanction doctoral students' attempts to
use research designs that do not adhere to the experimental paradigm. I would suggest that as charter documents in the educational research community, both *Experimental and Quasi-Experimental Designs for Research* and the *APA Publication Manual* need to be seen as exerting a powerful influence on graduate education departments (as well as in professional forums) at the end of the 1960s. This was the period during which Janet Emig was writing her case study dissertation at Harvard, and Donald Graves was entering the doctoral program in education at the State University of New York at Buffalo, where he conducted his case study of seven-year-old writers a few years later. It's a bit chilling to imagine the difficulties that these two researchers must have confronted getting their dissertation chapters accepted by committee members living in the Campbell/Stanley universe.

Having read Emig's dissertation the year I spent at Carnegie Mellon, I became well aware of the kinds of rhetorical problems she faced in being accountable to the academic gatekeepers for whom she wrote. She took great pains to write along the grain of convention, and her literature review carefully warrants the unorthodox methodology that she used. Emig, of course, did get the last word, at least among composition specialists. Many of her articles are now canonical, including her 1982 attack against the positivist institutionalized norms of experimental research in education ("Inquiry"). That invective was one of many in the early 1980s, as the tide began to turn against experimentalism in the educational research community in the United States and England.5

Those of us who entered composition studies in the late 1970s entered a young, hybrid field at the time when attitudes about what constitutes evidence and what constitutes knowledge were changing radically. There are obviously many intellectual as well as economic, and socio-political reasons for these changes—reasons too complex for me to attempt to discuss here. Let it suffice to say that as I sit typing this essay in 1989, I am looking at a sheaf of essays for the second CCCC "Research Network" preconvention workshop. This informally constituted group convened during the 1987 CCCC meeting with the purpose of providing a forum for composition scholars, theorists, and researchers to enter into a productive dialogue regarding differences among their seemingly incompatible assumptions and methodologies. At last year's meeting Patricia Bizzell sat on the same panel with George Hillocks, and this year, Beth Flynn, who has interpreted student texts from a feminist perspective, will be acknowledging the sources of her assumptions, as will Linda Flower.

**Epistemological Ecumenicalism**

In short, I suspect that the fear of a positivist-minded hegemony that I see hermeneutically trained colleagues ritually professing is the product of a sort of epistemological ethnocentricity. To the extent that we do not understand each other's models of knowing, rivalry and hostility are, I think, inevitable.
However, one way to deal with this problem is to learn more about one another's models of knowing while articulating our own assumptions and underlying models. Difficult though that process may be, it would be an important step toward developing a climate of epistemological ecumenicalism among the diverse groups that constitute our field.

To Foster and Schilb, the authors whose essays provoked the intellectual "autobiography" above, I would like to say this: yes, David and John, it's hard to avoid the legacy of positivism, when I and others write along the grain of convention in our major research forums. However, as an empirically oriented researcher, it's important to me to be accountable to what I call "the data." As this essay suggests, that very phrase, "the data," implies a model of knowing that's different from yours. All that I can hope to do is to make that model as explicit as possible in the good faith that you will do the same.

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Notes

1Laurence D. Smith argues that semi-historical accounts by Sigmund Koch and Brian D. Mackenzie are based on "strong historical and substantive parallels between the two movements" rather than a close historical analysis.

2In fact, Smith documents otherwise in his historical study, using letters, notes and other archival materials belonging to the estates of Tolman, Hull, and Skinner.

3Bazerman's argument is supported by John K. Smith, who notes that during the 1940s and 1950s, American psychology "as a whole came to be dominated by a view of science that coincided in broad outline with the shared view of the behaviorists and logical positivists" (6).

4Bazerman discovered that the practicing scientists he studied read for "the news"; that is, they went to the results section of an article first, after having skimmed the abstract. See "Physicists Reading."

5See, for example, Britton, et al. for an alternative descriptive and developmental study of adolescents' writing abilities. In the United States, Elliot Eisner, Elliot Mishler, Henry Giroux, Egnon Guba, and Yvonna Lincoln were among the vanguard of scholars attacking the positivist assumptions underlying experimental methodology in educational research.

Works Cited


—. "Inquiry Paradigms and Writing." College Composition and Communication 33 (1982): 64-75.


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