

## CHAPTER 3

# CONTROLLING THE METALANGUAGE

## AUTHORITY AND ACQUIESCENCE IN THE HISTORY OF METHOD

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In *Naming the Mind*, Danziger (1997) analyzed the emergence of the fundamental categories of psychological inquiry. Challenging the implicit assumption that behavior, personality, intelligence, learning, and motivation can be understood as natural kinds, he carefully explicated their origins and dynamics as negotiated products of scientific communities. In Chapter 9, he focused on the introduction of the concept of *variables* in psychological discourse and how this change profoundly altered the shape of psychological inquiry during the 20th century. Danziger employed the term “metalanguage” to describe the shared discursive practices regarding method that came to be nearly universal in mainstream psychology, and indeed helped to define the mainstream.

This aspect of Danziger’s work has overlapped with my own. Starting in 1988, I began to ask how the terms *independent variable* and *dependent variable* became universal in introductory psychology textbooks, and how these terms were used to define a relationship between causality and experimentation. By the 1970s, all North American psychology students were taught that an experiment consists of manipulation of an independent variable while holding all other variables constant and observing the effect on a dependent variable, and that this is the best or sole method for the discovery of causes. My interest in the source of this methodological dictum led to a number of investigations of textbook

conceptualizations of experimentation, ideas of “cause” in modern psychology, the influence of Ernst Mach’s philosophy of science, and the place of experimentation in social psychology (Winston, 1988, 1990, 2001; Winston & Blais, 1996; MacMartin & Winston 2000). I argued that the definition of experiment and its relation to causality was, for the most part, “home-grown” in psychology rather than imported into the discipline, though related to Machian philosophy of science in important ways. Further, I argued that this conceptualization profoundly shaped the way psychological questions were asked and answered.

In this chapter, I have three aims. First, I will augment and amend aspects of my earlier work. Second, I will highlight some commonalities and differences of emphasis in Danziger’s and my analysis of change in the metalanguage. In this regard, I am grateful for the discussions that he and I have had over a number of years. Third, I examine the process of change in metalanguage by considering a case in which such a change was blocked by powerful authorities, during the same period that the contemporary definition of experiment was introduced.

### EARLY USE OF THE CONCEPT OF *INDEPENDENT VARIABLE*

Between 1932 and 1934, a number of leading psychologists began to employ a new way of speaking about the “causes of behavior.” R. S. Woodworth, E. G. Boring, and E. C. Tolman, substituted the concept of the *independent variable* for the concept of *cause*, a term with a long and problematic history, accompanied by much metaphysical baggage. Each of these authors used this term in a slightly different way. The introduction of the concepts of *independent variable* and *dependent variable* into psychological discourse was an important event in terms of providing a common language for the discussion of investigations based on highly divergent theoretical systems. In this analysis, I was interested in how these terms came to define what an experiment was, and how this conceptualization then encouraged certain kinds of inquiry.

In my previous work, I suggested that the terms *independent variable* and *dependent variable* did not appear in psychology until the 1930s (e.g., Winston & Blais, 1996). A more recent search of the expanded PsycINFO database indicated that some papers of the 1920s may already have used these phrases. For example, “independent variables” appears in the abstracts of both Culler (1927) and Heidbreder (1927). However, their use is not the same as in the 1930s: the term is used to mean “variables acting independently of each other,” rather than experimentally manipulated factors.<sup>1</sup> Danziger (1997) noted that William James (1890/1950) had used the term “independent variable” in the *Principles* (I, p. 59), but here again, the meaning in context appears to be “independent of each other,” not the experimental factor which is manipulated by the experimenter.<sup>2</sup> These early

uses of “independent variable” are not clearly derived from the original mathematical meaning, described below. In this case, the use of the same term is misleading for the contemporary reader. But in other cases, a slightly different term is used to mean something close to the 1930s meaning of *independent variable*.

One example of the use of a related term is from William Stanley Jevons (1874). In his influential *Principles of Science*,<sup>3</sup> he described the nature of experiment in terms of active manipulation and identified the terms to be used:

Almost every series of quantitative experiments is directed to obtain the relation between the different values of one quantity which is varied at will, and another quantity which is thereby caused to vary. We may conveniently distinguish these as respectively the *variable* and the *variant*. (p. 440)

Both the terms “variant” and “variate” were commonly used for variable at the turn of the 19th century and in the early 20th century. In his highly influential *Statistical Methods for Research Workers*, R. A. Fisher (1925) used the terms “independent variate” and “dependent variate,” which he introduced to explain regression functions and their graphic representation. But he did not present this idea as a definition of experiment nor as the factor explicitly manipulated by the experimenter.

The original meaning of *independent variable* and *dependent variable* had nothing to do with experimentation. The concepts of function and variable were clearly present in the work of Leibniz, although his concept of a function was certainly not identical to the modern one. The specific terms *independent variable* and *dependent variable* were introduced by John Radford Young in the *Elements of the Differential Calculus* (1833): “on account of this dependence of the value of the function upon that of the variable, the former, that is  $y$ , is called the *dependent variable*, and the latter,  $x$ , the *independent variable*” (p. 2). Johann Gustave Lejeune Dirichlet provided the modern definition of a function in 1837, which O’Connor and Robinson (2002) translated as follows:

If a variable  $y$  is so related to a variable  $x$  that whenever a numerical value is assigned to  $x$ , there is a rule according to which a unique value of  $y$  is determined, then  $y$  is said to be a function of the independent variable  $x$ .

The important feature here is that the independent and dependent variable are interchangeable: there is no implication that  $x$  is the cause of  $y$ , and the relationship can be expressed as  $y = f(x)$  or  $x = f(y)$ . There is no requirement that the independent variable be manipulated, or that they have the asymmetrical status noted by Danziger (1987). As I have described elsewhere (Winston, 2001), it is in the writings of Ernst Mach that the connection between functions, experiment and explanation is outlined. By the 1880s, Mach was clear that the concept of a function, expressed mathematically, was to replace the metaphysically tainted concepts of cause and effect. Functions were purely descriptive and their economical

descriptive power was for Mach the only proper and useful form of scientific explanation. Once Mach and others began to speak of mathematical functions as the replacement for causal statements, it was natural to substitute the terms proper to functions, independent and dependent variables, for statements of cause and effect.

However, it is possible to overstate the role of Mach in introducing the more general talk of variables in Psychology. As Danziger (1997) noted, texts and articles on statistics already made the concept of variables familiar to psychologists. With the increasing introduction of statistics into research and teaching during the first three decades of the 20th century, variables, variates, or variants would figure prominently. The terms *independent variable* and *dependent variable* were perfectly appropriate for any discussion involving a regression line. The literature of Applied Psychology, especially discussions of prediction, used *independent variable* in its original sense from the mathematics of functions during and after the 1930s (e.g., Wilson & Hodges, 1932). The use of these terms for prediction of a criterion measure from a test eventually caused some friction between experimentalists and multivariate researchers as experimentalists came to use *independent variable* to refer only to manipulated conditions (see Winston, 1990).

### INDEPENDENT AND DEPENDENT VARIABLES APPEAR IN PSYCHOLOGY TEXTBOOKS

In introductory textbooks, the earliest use of the terms *independent variable* and *dependent variable* to define experimentation was in Woodworth's (1934) *Psychology*. In the third edition of his widely used textbook, he altered and narrowed the definition of "experiment" to include only studies in which a variable was explicitly manipulated:

An experiment is one means of obtaining observations bearing on a definite question . . . the experimenter "controls the conditions." He does not let things happen at random; in the ideal experiment he has all the factors under his control that have any influence on the process to be observed. The result shows him what happens under certain known conditions. He varies one factor and notes the difference that makes in the result. The rule for an ideal experiment is to control all the factors or conditions, to keep all of them constant except a single one—which is then the *independent variable*—and to vary this one systematically and observe the results. The results are changes in the *dependent variable*. When he finishes his series of experiments, he knows the changes in the *dependent variable* which are produced by changes in the *independent variable*. (1934, p. 18)

Although Woodworth was the first to present the now universal textbook definition, E. G. Boring (1933e) had defined experiment as the manipulation of an independent variable in the previous year, in his *Physical Dimensions of Consciousness*, a book Boring designed as a tribute to E. B. Titchener:

The experimental method, upon which all science rests, is, logically considered, a method of the induction of a generalized correlation by means of controlled concomitant variations. In the simplest experiment there are always at least two terms, an independent variable and a dependent variable. The experimenter varies *a* and notes how *b* changes, or he removes *a* and sees if *b* disappears. He repeats until he is satisfied that he has the generalization that *b* depends upon *a*. The independent variable, *a*, can now properly be spoken of as a *cause* of the dependent variable, *b*. (pp. 8–9)

Boring gave no source for this formulation. A search of correspondence between Boring and Woodworth at both the Harvard and Columbia University archives failed to yield any discussion of this issue, although there is correspondence regarding many other organizational and professional issues. It is not surprising that two leading figures would make a nearly simultaneous change in language. However, they were not the only important actors here. Danziger (1997; Danziger & Dzinis, 1998) described how Edward C. Tolman also introduced the concept of independent variables.

In his *Purposive Behavior in Animals and Men*, Tolman (1932) emphasized the language of “variables” when summarizing different systems of psychology in Chapter XXIV, “The Final Variables of Purposive Behaviorism.” What is important here is that the term *variables* allowed Tolman to contrast individual difference psychology, structuralism, Gestalt psychology, and behaviorism using a common language. I have argued previously that Tolman did not quite use the Boring/Woodworth formulation in 1932, and that he used an idea of “independent causes” which were clearly variables, without saying independent variables. This assertion was not correct: on page 405, Tolman referred to heredity and previous training as “two independent variables.” On page 406–407 it is clear that Tolman uses the terms “independent causes” and “independent variables” interchangeably. As Danziger (1997) has argued, this is the most important feature of the introduction of variables: the term is used to describe not just a methodology but the theoretical entities of interest. Moreover, method and theory subsequently take on an isomorphism uncharacteristic of discourse in the natural sciences.

In 1935, Tolman (1932/1967) wrote in “Psychology and Immediate Experience” that both molecular (physiological) and molar behaviorists shared a common program of identifying the “independent or causal variables” (p. 102). He summed up his integrative position:

A behaviorism seeks to write the form of the function  $f_1$  which connects the dependent variable—the behavior, B—to the independent variables—stimulus, heredity, training, and physiological disequilibriums, S, H, T, and P. (p. 113)

Thus Tolman emphasized the concepts of independent variables and dependent variables as a way of defining the project of psychology, and as the proper way to conceptualize the theoretical terms of interest. In addition, these terms aided in the explication of his “intervening variables,” which was a crucial move in the

synthesis he attempted through Purposive Behaviorism. Unlike Woodworth, he did not present these terms as a way of defining what an experiment is, but as a way of defining what an explanation of behavior would look like.

There can be little doubt that Tolman's continued use of these terms helped in their popularization. His APA presidential address of 1937, published as "Determiners of Behavior at a Choice Point" in the *Psychological Review* the next year (Tolman, 1938), the terms *independent* and *dependent variable* were featured very prominently (in figures, 2, 3, 6, 7, 8, 12, & 15) and the function relating them was said to provide the "cause." It is interesting that by this time, the term *cause* had been allowed back into discussions of function, despite Ernst Mach's attempts to eliminate this idea as freighted with metaphysical baggage (Winston, 2001). Insofar as Tolman's position provided a unified vision for psychological inquiry, providing an analysis of the problems of behaviorism, Gestalt psychology, individual psychology and purposive psychology, Tolman's language of independent variables, intervening variables, and dependent variables allowed psychologists of varied commitments to speak a common scientific language.

Tolman was teaching at Harvard during the summer of 1932, and Skinner (1998) reported that he "saw a great deal of him" (p. 290) although Tolman (1952) did not mention this absence from Berkeley in his autobiography. Skinner suggested that he and Tolman were speaking about variables in a similar way at this time. Skinner's statement of behavior as a function of a set of variables was, he thought, related to that of Tolman. However, Skinner rejected intervening variables in favor of "third variables," such as deprivation, which altered the functional relationship between stimulus conditions and responding.

Skinner's (1998) retrospective account suggests that he had priority in the modern use of "variables." In Skinner (1931) he argued that the experimental results of the study of a reflex can be expressed as the function:  $R = f(S)$ , or considering third variables,  $R = f(S, A)$ . He treated this relationship as experimentally derived by manipulation, and not merely as the statistical relationship between two sets of numbers (pp. 451–452). He did not use the terms *independent variable* and *dependent variable*, although they are clearly implied by his formulation.<sup>4</sup> Moreover, Skinner's philosophy of science, so heavily influenced by Mach (see Winston, 2001), emphasized the determination of functional relationships as the aim of his science of behavior.

Priority claims are hardly the important issue here. What is significant is the social matrix in which such changes in language occurred and the social process by which they became codified and enshrined. Woodworth, Boring, and Tolman occupied positions of influence in the APA, by then a rapidly growing organization. Skinner was a new PhD, but outspoken and confident in his views. Although Boring found Skinner difficult and had severe criticisms of Skinner's dissertation (see Bjork, 1993), he respected his intellectual talents and promoted Skinner's career in a variety of ways. Skinner was able to publish his new conceptualization

of the reflex with little delay, with the help of William Crozier, who was an editor of the *Journal of General Psychology*. Boring and Tolman were also in frequent contact, as Boring was attempting to recruit Tolman to return to Harvard, where he had received the PhD in 1915 (Innis, 1992). There were ample opportunities for discussion of general theoretical issues amongst these four important individuals. They in turn had numerous opportunities to encourage the formulation of all psychological problems in terms of *independent* and *dependent variables*. I do not mean to imply that a cabal was formed with any explicit plan to alter the language of psychologists, only that in conversation these individuals may have discovered collective as well as individual reasons for adopting and promoting a new discourse.

### FAILURE TO CHANGE METALANGUAGE: SAUL ROSENZWEIG AND *Ee*

In order to understand the process by which a new linguistic practice is introduced and taken up, it may help to consider cases in which attempts to change scientific language *fail*. One clear attempt occurred in 1933, and provides an instructive example in the microhistory of the regulation of language. In contrast to the introduction of *independent variable* and *dependent variable* at the same time, where no archival record of the relevant correspondence has been found, the discussions regarding this attempt to change the metalanguage are available, and can clarify the role of power and status in bringing about or inhibiting such change.

Saul Rosenzweig (1907– ) became a graduate student at Harvard in 1929 and received the PhD in 1932. He had some difficulty obtaining an academic position, despite an excellent record, due in part to the Depression and possibly to antisemitism (see Winston, 1998). He continued his work at the Harvard Clinic with Henry Murray. Boring (1932) described him as “top-notch intellectually” and “our best graduate this year.” In 1933, Rosenzweig published an important and often neglected paper: “The Experimental Situation as a Psychological Problem.” Some forty years before the topic became fashionable, he introduced the idea that the experiment must be considered a *social situation* in which the elements were quite different from an experiment in chemistry. As a conscious being, the person serving as “subject” may “regulate their own reactions: they have minds of their own and are self critical. From this results the difficulty that uncontrolled experimental materials, *viz.*, motives, may be brought into the experiment” (1933a, p. 353). Rosenzweig suggested that the subjects might engage in playing the part or role they thought was expected of them, might attempt to appear smart or compliant, would develop hypotheses about what the experimenter was after, and would generally behave in an active rather than passive fashion. He was concerned that neither the term *subject*, which had only recently become standard, nor the older

term *observer*,<sup>5</sup> captured this aspect of the person participating in an experiment. Rosenzweig therefore introduced a new term, *experimentee* or *Ee*, to be used along with *Er*, rather than *E* for the experimenter. At the beginning of the article he noted:

The word > 'experimentee' is the exact equivalent of the German > 'Versuchsperson.' It is a general designation that may be used to refer to the person, whatever his function (that of observer or that of subject) working in coöperation with, but in a complementary relation, to the experimenter. It is an authorized English word and it is suggested that it be more commonly employed. (p. 338, n. 1)

The same year, Rosenzweig used the term *experimentee* in two additional publications. In the *Journal of Genetic Psychology*, Rosenzweig (1933d) examined children's preferences for repeating successful and unsuccessful activities. In the first footnote, he argued for the equivalence of *Ee* and *Versuchsperson*, noting that "our Ees were not strictly 'subjects,' in the sense of behavioral animals, nor were they 'observers' . . . They were in part both and the term 'experimentee' covers this general function" (p. 423). Rosenzweig and Koht (1933) used the same term in an article on time estimation, with a briefer footnote on *Versuchsperson*.

E. G. Boring, head of the Laboratory and effective head of the not-yet-independent Department, did not like this change. Rosenzweig, who distinctly recalls the interchange with Boring in 1933, reported that Fernberger asked Boring whether he had approved of the use of *experimentee*, which led Boring to intervene (personal communication, July, 2002). Boring and Rosenzweig began a discussion of the matter. Boring (1933a) wrote:

I don't like *Er* and *Ee* at all. They're just your private substitutes for *E* and *S*, which have after some difficulty gotten themselves accepted quite generally in the rapidly expanding American Literature. For you now to attempt substitutes is unfortunate, from your point of view because you have to overthrow tradition and are likely therefore to be unsuccessful after much effort, from the public's point of view because any degree of incomplete success by you leads to confusion, ambiguity, and distraction of attention from the things that really matter.

Boring went on to give a history of the terms *E*, *S*, and *O*, and argued that *S* was now the general term that encompassed both introspecting and non-introspecting organisms. He could see no reason at all to change *E* to *Er*. Boring emphasized the issues of clarity and confusion, and chastized Rosenzweig:<sup>6</sup>

We had enough trouble getting these things established fifteen years ago, more or less; but now you want to buck tradition. (Or don't you?). Moreover, the public was represented in 1920 by 400 members of the APA, whereas now it is represented by 1600 members. If we felt the inertia then, think how much more you will feel the inertia now.

He suggested that journal editors ought to change *Ee* and *Er* back to *S* and *E*, to avoid confusion. This emphasis on harmony is, I believe, central to understanding the way things looked to Boring.



Rosenzweig (1933b) wrote back a few days later, mentioning his recent meeting with Boring on the matter and enclosing a six page defense of *Er* and *Ee*. Here he outlined the ambiguities of the term *subject* and the complex functions served by the person “experimented on.” He attempted to answer a number of Boring’s objections, including the awkwardness of *Ee*, and compromised by suggesting the *Er* could stay as just *E*, but that a designation for *experimentee* was still necessary. Three days later, Boring (1933b) replied that he remained unconvinced. With his characteristic thoroughness, perhaps to the point of obsessiveness, Boring endeavored to show Rosenzweig that the term *subject* was the term in general use no matter what precise role the person assumed. He surveyed 49 articles to demonstrate this assertion empirically. Inviting Rosenzweig to reply, perhaps with an expanded sample, he proposed to circulate a memo to Howard Warren, Samuel Fernberger, Henry Murray, and the *American Journal of Psychology* editorial board explaining that *Ee* and *Er* were “unnecessary redundant terms” and should not be used.

Another note to Rosenzweig followed three days later on December 22. Now Boring (1933c) reported that he had already discussed the *Ee* and *Er* issue with Henry Murray (who was Rosenzweig’s ostensible “boss”) over lunch, and “you can ask Dr. Murray what he thinks the result was.” Apparently, Murray thought that Boring was pressuring Rosenzweig to retract his use of *Ee* and *Er*, and Boring felt obliged to tell Rosenzweig that this was not his intent. Boring, who imposed extremely strict rules on himself, would not let it be thought that he would bully anyone. Rosenzweig (1933c) replied on the 26th. He politely thanked Boring for his careful analysis, but he refused to accept Boring’s position “in an unqualified way.” That is, he agreed that *subject* was used as a general term, but argued now that use of *experimentee* would avoid the behavioristic connotations that *subject* had acquired. While defending his proposal, he acknowledged that using *experimentee* might “arouse needless antagonism on the part of the reader, cause confusion, and distract attention from more important aspects of any publication containing this usage.” Characterizing the action as his own decision, he reported that he would now use *subject* rather than *experimentee*, and had already made the change in his next publication in the *British Journal of Psychology* (Rosenzweig & Mason, 1935). He thanked Boring for showing him that “established usage (even if inconsistent) cannot be ignored with impunity,” and that antagonism or confusion of the reader was not worth the possible gain.

Boring (1933d), who never took a holiday from his correspondence, wrote back on December 29:

You have yielded handsomely to my judgment and have simplified my editorial problem greatly. I am circulating this memo to you, Murray, Warren, Fernberger and my three colleagues on the AJP. I discussed the matter with Warren and Washburn Wednesday evening. Of course I put a prejudiced case, but so far I have not found anybody who sees so much advantage in *Ee* and *Er* as to be willing to affirm that belief to me. Enough.

In politely but very firmly bringing this disagreement to an end, Boring exercised considerable authority. It was important for him to bring Fernberger and Murray on side, as Fernberger had somehow let Rosenzweig's use of *Ee* stand in the *Journal of Experimental Psychology*, and Murray had recommended publication of Rosenzweig (1933d) in the *Journal of Genetic Psychology*. Rosenzweig did not forget this blocking of his aim to bring the social relationship between experimenter and subject to the attention of psychologists. In his obituary for Boring, Rosenzweig (1970) devoted considerable attention to Boring's use of *Zeitgeist*, and described the events of 1933:

... I reluctantly recall a personal encounter with the *Zeitgeist* as represented by Boring himself... I had introduced the term *experimentee* as inclusive of both *subject* and *observer* (a term still used in those days) but found myself thwarted by a protracted campaign of opposition from him, by letter, interview, and memorandum, that eventuated in my diplomatic acceptance of the Department Chairman's judgment. (1970, p. 68)

Rosenzweig felt in 1933, 1970, and 2002 (personal communication) that the issue was not just the proper use of a novel term, but that the term signalled a different understanding of the nature of psychological experimentation from that which prevailed in 1933. The idea of Boring himself as the embodiment of the *Zeitgeist* remains intriguing. But it was Boring who took the necessary action. As a result, Rosenzweig acquiesced and never used the terms again.

The case of *experimentee* illustrates the way in which the regulation of language in this academic context was carried on in explicit, active, fashion, rather than through implicit understanding. Had Boring endorsed Rosenzweig's proposal, additional communications to his network of editors and leading figures in Psychology would have undoubtedly gone out. Although formal instructions to authors on the preparation of manuscripts in psychology first appeared in 1929 (see Vandenbos, 1992), the power of those such as Boring to persuade or even command was formidable. He was able to immediately forestall further use of Rosenzweig's terms in the *Psychological Review*, *Psychological Bulletin*, *Journal of Experimental Psychology*, the *American Journal of Psychology*, and other outlets. As opposed to the formal gatekeeping of journal editors (see Lubek & Apfelbaum, 1987), Boring's power was more widespread. It is tempting to think of Boring's network in terms of the "actor-network-theory" relating scientists, engineers, politicians, texts, and devices, as described by Latour and others (e.g., Latour, 1987, 1999).<sup>7</sup> Although the *social* network surrounding Boring is much more limited in scope and different in character than the *asocial* system of technoscience conceived by Latour, the process by which colleagues and texts are enlisted in disputes according to actor-network-theory seems an apt description of the Rosenzweig case. Specifically, Boring's report of his discussions with Henry Murray and Howard Warren, and his employment of an informal survey regarding

the use of “subjects” illustrates the social production of authority through texts as outlined by Latour.

Boring’s choice to promote the definition of experiment in terms of *independent* and *dependent variables* but reject the term *experimentee* was hardly arbitrary. In the former case, the language was a means to speak of diverse practices and conceptions, from Titchenerian structuralism to Watsonian behaviorism, in a common voice. In the case of *experimentee*, Boring foresaw confusion and division, rather than the Peaceable Kingdom of Psychology that both he and Woodworth hoped for.<sup>8</sup> Moreover, Rosenzweig’s attempt to alert psychologists to the uniquely social nature of experimentation with humans was potentially problematic for a unified, natural science view of human and animal psychology.

### SPREADING THE LANGUAGE OF INDEPENDENT VARIABLES

The appearance of the standard conception of independent variables, experiment, and causality in the discourse of Boring, Woodworth, and Tolman did not ensure that this formulation would become universal, nor was the adoption of their language an immediate consequence. Uniformity of metalanguage was not achieved until the 1960s or 1970s.

Between the 1930s and the 1970s, the concepts of independent and dependent variables appeared with increasing frequency in psychology textbooks and the psychology literature. As documented by Winston and Blais (1996) in a sample of texts from Psychology, Sociology, Biology, and Physics, this development was initially unique to Psychology, although Sociology texts began to use these terms in the 1970s. The change in Psychology was accompanied by a narrowing of the definition of experiment to include only studies in which the independent variable was explicitly manipulated, and the frequent assertion that only such studies would reveal causal relationships. This formulation appeared to serve important pedagogical aims: all psychological research could be formulated with a common framework, easily understandable even for introductory level students. This smoothing over of disagreement by means of a common emphasis on method rather than theory meant that textbooks could present the appearance of a unified and coherent discipline. Such coherence would make the introduction of psychology to a mass audience an easier task.

Introductory textbooks are now of interest in their own right, as important records of the changing relationship between scientific authors and nonscientific audiences (see Morawski, 1992, 1996; Weiten & Wight, 1992). However, it is unlikely that the introductory textbook played a major role in guiding how psychological research was formulated after the 1930s. Texts for Experimental Psychology courses are likely to be more important than introductory texts in socializing new recruits to the discipline; as students begin the active participation in “laboratory

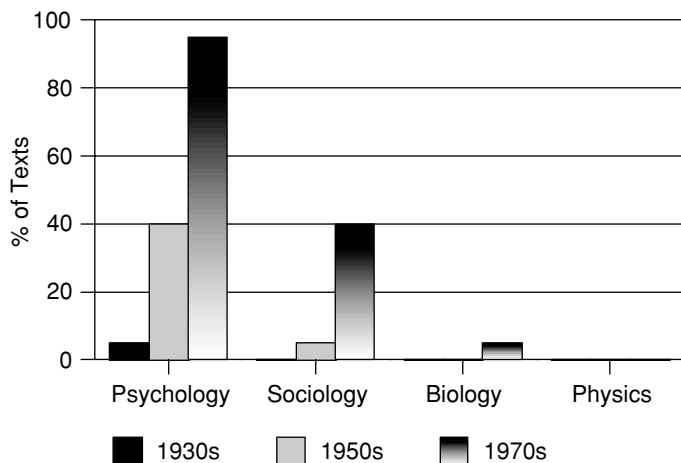


FIGURE 1. % of texts using the terms “independent variable” and “dependent variable” by discipline. Adapted from Winston & Blais, 1996.

life,” the sense of a new identify can be formed. Although not a laboratory manual, Robert S. Woodworth’s *Experimental Psychology*, known as the “Columbia Bible” played a crucial role (Winston, 1990). In the widely used “Bible,” Woodworth clearly demarcated experimental from correlational research in terms of whether or not variables were actively manipulated, and assigned the discovery of cause and effect to experiments only. This discursive move gave experimentation a distinct epistemological advantage, and thereby elevated experiments to the premier form of knowledge generation.

With the exception of Woodworth (1938), experimental psychology textbooks from the 1930s gave no definition or only a vague definition of experimentation, and did not use the terms *independent* and *dependent* variables. Most frequently, experiment was defined as introspection via controlled self-observation or simply controlled observation, and the requirement of manipulation was generally not mentioned. The shift in definition is clearly evident but still not universal by the 1950s. One text in particular stands out as transitional: Underwood’s (1949) highly influential *Experimental Psychology*, which explicitly used *independent* and *dependent variables* and defined true experiments as requiring manipulation.<sup>9</sup> Underwood’s text may have been the first to devote a full chapter to explaining just what an experiment is supposed to be. Moreover, this text is very different from all previous ones: in older texts, “experimental psychology” is used to designate a collection of topics as well as a method; the focus is on the content of the discipline. Underwood’s focus was more on a universally applicable method and less on content.

Postman and Egan (1949) published a similar text in the same year, and they also followed the Boring/Woodworth definition of experiment. But not all did.

In *Beginning Experimental Psychology*, Bartley (1950) noted that “The concept of what experimentation is and what constitutes an experiment varies considerably from person to person” (p. 30), cited Bentley’s (1937) discussion of eight different definitions of experiment (see Winston 1990), and offered no resolution. The diversity on such a fundamental issue suggests that the textbook uniformity was by no means a certain outcome. Of 15 experimental psychology texts from the 1970s that I examined, all but one used the contemporary definition in terms of manipulation of an independent variable. In the 1950s, there is little mention of non-experimental methods that psychologists might use. But in the 1970s, experimentation is compared and contrasted with other approaches, particularly “correlational” strategies. Observation, correlation, and experimentation are presented in that order, with hints that experimentation is historically the most recent method and epistemologically the most highly developed. Experiments are described as the final step in systematic inquiry, whereas observation and correlation are preliminary steps. A similar shift can be seen in Social Psychology textbooks and handbooks (MacMartin & Winston, 2000). In some cases, the textbooks or handbooks in Social Psychology made explicit reference to Underwood (1949) or Woodworth (1938) as authorities on the precise definition of experiment. In contrast to the direct exercise of power described in the case of Rosenzweig and Ee, the influence here is likely to be informal and indirect. Woodworth, Boring, and Tolman had substantial prestige within the large network of Columbia and Harvard PhD’s, and their discursive practices were both respected and imitated.

These textbook changes were accompanied by changes in the rhetoric in journal articles. Danziger and Dzinis (1997) examined the use of the terms independent and dependent variables in the *Journal of Experimental Psychology*, *American Journal of Psychology*, *Journal of Personality* and the *Journal of Abnormal and Social Psychology*. The percentage of articles using these terms rose from 0% in 1938 to 2% in 1948 to 14.6% in 1958. Surprisingly, the *Journal of Abnormal and Social Psychology* showed the most frequent use, i.e., 27% of articles in 1958. Danziger and Dzinis suggested that in those areas where experimentation was not as well established, there might exist a stronger inclination to affirm this new methodological commitment with explicit terminology. The period from the late 1940s to the early 1960s saw the rapid transformation of American Social Psychology from a methodologically diverse to methodologically homogeneous enterprise.<sup>10</sup> The proportion of articles in the *Journal of Abnormal and Social Psychology* (later the *Journal of Personality and Social Psychology*) with explicit manipulation of variables rose from less than 30% in 1949 to over 80% by 1959 (Christie, 1965; Higbee & Wells, 1972). In contrast to traditional experimental areas such as perception where the use of experimentation was never an issue, this transformation in Social Psychology required explicit demarcation of experiment and its significance.

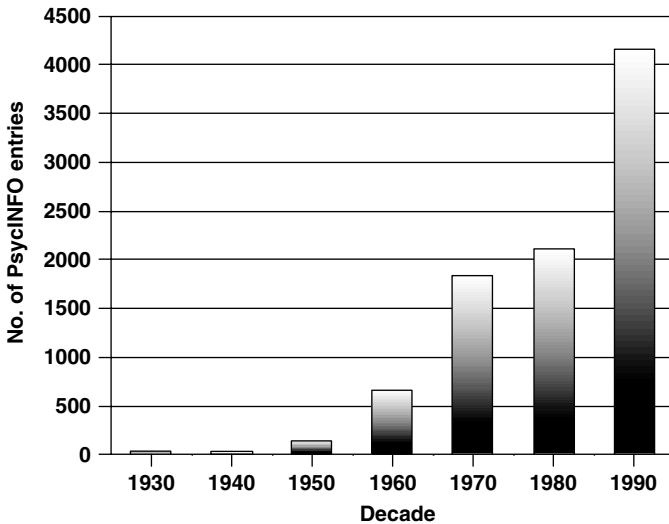


FIGURE 2. Number of PsycINFO entries using the terms “independent variable” or “dependent variable” by decade.

The more general spread of the terms *independent* and *dependent variable* can also be seen in the PsycINFO database. The number of abstracts using either term increases rapidly after the 1960s, rising to over 4100 in the 1990s, as shown in Figure 2.<sup>11</sup> However, these data must be viewed in context: 4000 abstracts represents less than 1% of the 1990s database. In most cases, it would be unnecessary for the authors of journal articles to identify the manipulated variable as the independent variable. For Woodworth, these terms were primarily a pedagogical device for the uninitiated. Their use helped to achieve the clarity that Woodworth was known for in his textbooks. For the undergraduate psychology major, hoping to enter graduate school, adherence to and demonstration of the appropriate metalanguage of method would be demanded in all laboratory reports and examinations. Having been taught to formulate psychological investigations in these terms beginning with introductory psychology, the newly minted researcher would need only to state that the problem under study was “the effect of x on y”; the journal audience, all trained in experimental psychology, knew which was which.

The nearly universal promotion of the view that causality could only be discovered by manipulation of the independent variable had important consequences for the investigative practices of psychology. Such a view helped delegitimize nonexperimental models for scientific psychological inquiry, such as the natural history approach proposed by Gardner and Beatrice Murphy in the 1930s (Pandora, 1997). At the very least, naturalistic observation would be relegated to inferior epistemological status, as would correlational studies. Second, promotion

of psychology as the only social science to use experimentation helped establish a distinct disciplinary identity resting on the claim that psychology could identify the causes of human action while other, nonexperimental human sciences could not. Third, the requirement of manipulation and control for causal inference helped to justify the choice of laboratory animals as subjects in that animals would permit the degree of manipulation and control necessary for such inference. These requirements would also encourage the use of other “captive” populations, including undergraduates required to fulfill a requirement in introductory courses. Finally, the reduction of human experience and social interchanges to a set of manipulated variables provided a justificatory rhetoric for the laboratory study of attitude change, aggression, competition, moral development, and other socially embedded phenomena in the ahistorical, acultural, and decontextualized approach common from the 1950s through 1980s.

Danziger (1997) identified another critical aspect of the shift in metalanguage. What changed, he argued, was not simply the terminology or definition of experiment. The *independent variable* changed from being a statistical or procedural concept to standing for the theoretical entity of interest. That is, a variable in an experiment came to be treated as “an objective natural force with causal efficacy” (Danziger, 1997, p. 172). This shift in ontological status describes how “variables” in general came to be used in psychology, not just in experimentation. Thus “anxiety” was transformed into much more than a statistical aggregate of responses to questionnaire items, and was assumed to be a variable operating in the world, discovered rather than created. Moreover, the assumption that psychological reality was pre-structured as variables generally went unexamined, becoming a part of the taken-for-granted in psychological research. For Danziger, this treatment of variables shaped the way in which theoretical as well as empirical statements were constructed.

The general acceptance of the view that manipulation of the independent variable was the sole path to understanding the causes of behavior was enabled and supported by the intertwined discourses of positivism, neo-behaviorism, and operationism that existed in many variations in the 1930s (see, e.g., Green, 1992, Smith, 1986). Moreover, the spread of the new language was embedded in the development of other methodological and technical concepts and tools, such as the idea of randomization and experimental control (Dehue, 2000, 2001) and the spread of analysis of variance (Danziger, 1997; Rucci & Tweney, 1980). Both Danziger and I have emphasized the relationship of changes in investigatory practices to the demands of applied psychological work and the adoption of a technological view of science. Van Hoorn (1983) argued that the triumph of a technological approach to psychology and other social sciences was greatly accelerated after World War II, and was tied to aims of social control. The history of these complex interrelationships is beyond the scope of this chapter. However, consensus regarding experiment, functions, and variables should be seen as part

of the longer history, beginning at least with the fundamental epistemological split described by Danziger (1979) in "The positivist repudiation of Wundt." In one of his earliest historiographic papers, Danziger illuminated the critical turning point when Külpe, Ebbinghaus, and Titchener all adopted a Machian philosophy of science, in which functions and variables would come to play a crucial role.

## CONCLUSIONS

The success of the Boring/Woodworth definition of experiment, and the failure of Rosenzweig's social conception of *Experimentee* during the 1930s both illustrate the regulation of scientific language through power and prestige. Both of these innovations in the metalanguage of Psychology were more than a change in terminology, and offered accompanying versions of the meaning of an experiment. The language of *independent* and *dependent variables* promised to reveal causal relations and unify the discipline under a methodological rather than theoretical hegemony. The aim of using *Experimentee* was to bring recognition of the experiment as a fundamentally social encounter and to change the conception of the subject from a passive recipient of stimuli to an active being, an agent capable of reformulating or even defeating the experiment.<sup>12</sup> The two proposals had an important asymmetry: one offered harmony by smoothing over epistemological debate, the other pointed toward confusion over just what was happening in psychological investigations. The steps necessary to promote one and prohibit the other were also asymmetrical: Rosenzweig's proposal was "terminated" within a matter of weeks via direct action, while the new definition of experiment spread gradually and "naturally" over several decades.

E. G. Boring accomplished the swift death of Rosenzweig's proposal through the exercise of institutional power, professional status, and personal contacts. His network of relationships was based on long personal histories of interaction and professional interchange. Moreover, Boring's vast correspondence files suggest that he functioned as a very central "node" in this network, through which news, policies, job postings, recommendations, and other discursive materials flowed. Careful consideration of such networks is a useful corrective to a historiographically oversimplified narrative, in which Boring's individual will triumphs over that of Rosenzweig.

In contrast to the Rosenzweig case, the gradual spread of the standard conception of experiment, variables, and causality was accomplished without the dramatic intervention of powerful individuals, although powerful individuals introduced the change. Boring, Woodworth, and Tolman were interconnected by their commitment to laboratory experimentation, to a psychology of basic principles rooted in their vision of natural science, and to the APA and the Society for



Experimental Psychologists. The textbook authors who promoted these conceptions were also interconnected. In the 1920s and 1930s, graduates of Columbia University were strategically placed in departments around the country, and former students typically remained in close contact with their mentors. As younger psychologists produced both introductory and experimental psychology textbooks, they would naturally draw on the work of Woodworth, particularly the “Bible.” The resulting intertextuality (see Culler, 1981) should not be surprising. There is no need to search for missing documents to explain the uniform presentation of method.

It is customary, as I have done here, to speak of such changes in language in terms of “spread,” as in the spread of weeds, fire, or disease, all highly unsatisfying metaphors. However, the vagueness of “spread” may be remedied in two ways. First, in *Naming the Mind*, Danziger outlined the functions served by the new talk of variables. He provided a cogent account of what new activities and positions were enabled by this move, and used the example of the Harvard Psychological Clinic to demonstrate how the language of variables provided unity in a climate of faction and feud. Second, change in the discursive practices of psychologists can be situated within a more general cultural history. In this chapter, I have neglected the dramatic post—WWII expansion of Psychology that accompanied the “spread” of the language of independent variables. The increased federal funding, societal interest, political and social applications, and pressure for scientific legitimization provide an important context in which to view micro-level changes in the language of psychology (e.g., see Capshew, 1999; Herman, 1995).

Although the postwar environment is crucial for understanding stability and change in these discursive practices, the context of 1930s psychology must also be considered. The unresolved tensions within the discipline were considerable: conflicts between psychologists in the laboratory and growing numbers in applied work, between the activist founders of SPSSI and those who resisted such a role for psychology, between the un- or underemployed and the well established.<sup>13</sup> Despite the ravages of the Great Depression, the APA continued to grow rapidly during the 1930s, changing from an exclusive club of the like-minded toward a mass organization of competing factions. In this context of intradisciplinary discord, tight control over the language of experimentation was essential for creating the dominance of neo-behaviorism and the laboratory study of animal learning in the subsequent decades.

## NOTES

<sup>1</sup> Although the abstract for Meakin (1904) contains “independent variables,” the phrase does not appear in the original article. The PsycINFO abstracts for this period were created recently (see PsycINFO News, 1996), and Meakin (1904) does not constitute an early use of “independent variable” in the psychological literature.

- <sup>2</sup> In James, the relevant passage is a discussion of whether cortical ablation in dogs produces motor disorders as a result of sensory anesthesia, or “independent” of the sensory deficit. James concluded from studies showing motor effects without sensory effects that “The motor and sensory symptoms seem, therefore, to be independent variables” (p. 59).
- <sup>3</sup> For a negative view of Jevons’ philosophy of science, see Pearson’s (1892) *Grammar of Science*.
- <sup>4</sup> Skinner used the term “dependent variable” in *Behavior of Organisms* (Skinner, 1938), but did not emphasize the formulation of his behaviorism in these terms until *Science and Human Behavior* (Skinner, 1953).
- <sup>5</sup> The use and decline of the term “observer” is discussed in Danziger (1990).
- <sup>6</sup> Chastisement by Boring for one infraction or another was a fairly common occurrence for Harvard graduate students. See, Winston (1998) for examples.
- <sup>7</sup> This approach is sometimes known as “actant-network” theory, with the term “actant” instead of “actor” emphasizing the active role of texts, devices and other objects as “actors.” The founders of ANT have declared the movement “over” or “dead” (see Law & Hassard, 1999) although this claim can be taken as ironic.
- <sup>8</sup> Quaker minister Edward Hicks (1780–1849) painted a large number of works depicting the “Peaceable Kingdom” of Isaiah. Boring was raised in a part Hicksite (founded by Edward Hicks’ cousin Elias Hicks) and part Orthodox Quaker family, and Quaker values had a substantial influence on him (Winston, 1998). This did not mean that Boring was accepting of innovation.
- <sup>9</sup> For information on Underwood and his influence see Freund (1998).
- <sup>10</sup> See Lubek (2000) for detailed discussions of the transformation of Social Psychology.
- <sup>11</sup> All documents in PsycINFO, not just journal articles, were included in this survey.
- <sup>12</sup> To an extent, Rosenzweig’s conception was incorporated into the Fourth Edition of the *APA Publication Manual* 61 years later (APA, 1994). Recommending that in most cases, *participant* or other terms be substituted for *subject*, the *Manual* also mandated an end to passive language for discussing participants, and required terms that described them as actors rather than acted upon.
- <sup>13</sup> See, for example, Finison (1976), Harris, Unger, & Stagner (1986), O’Donnell (1979), and Samelson (1992).

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