

# 9

## CODIFYING THE

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## SOCIAL SCIENTIFIC STYLE

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## THE APA PUBLICATION MANUAL

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## AS A BEHAVIORIST RHETORIC

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The intellectual, practical, and social successes of the natural sciences have made their ways of going about their business highly attractive to other communities that create knowledge. Not only have the natural sciences seemed to have found a way of producing statements of great detail and reliability, expanding our powers of prediction and control over nature, they have also been able to develop wide agreement on a large number of statements within their communities and have gained the respect and support of the broader society. Thus the natural sciences have generated wide social, political, and economic power as well as power over nature.

In particular, those communities concerned with issues of human mind, society, and culture have been moved to adopt (and adapt) what they perceive to be the methods of the physical and biological sciences. Just as natural philosophy gradually was reorganized as the natural sciences over the seventeenth and eighteenth centuries, many other parts of philosophy since the late nineteenth century have been in the process of being reorganized into what are called variously the *social sciences*, *behavioral sciences*, *cognitive sciences*, or *human sciences*.

Central to the reorganization of these knowledge-creating communities has been an imitation of the forms of argument developed within the natural sciences. The compelling force of these arguments, the consensus developed over the aggregate results of these statements, and the power over natural forces achieved through the understanding constructed from these texts, seem to remove them from the traditional realm of rhetoric—those things about which we are uncertain, as Aristotle remarks at the opening of his *Rhetoric*. By arguing without seeming to argue and compelling without apparently urging, the scientific manner

of formulating knowledge seems to offer a way out of the deep divisions of belief and imponderable conundrums that seemed to pervade psychological, social, moral and cultural questions.

However, as we have seen in the previous chapters, the literary forms of scientific contribution have developed out of active argumentative situations in particular forums. Scientific discourse emerged as a way to win arguments rather than as a way to avoid them. They remain in the realm of rhetoric because there is no certainty in science, no absolute-ness of statement. Problems of induction, reference, skepticism, and intersubjectivity haunt the lowest strata of our empirical knowledge and scientific representations. Scientific modes of communication developed as a series of solutions to the problems of persuasion. These solutions emerged within developing communities, and were embedded within emerging empirical, social, and rhetorical practices.

Scientific writing is no unitary and unchanging thing, defined by a timeless idea. Varieties of scientific writing have developed historically in response to different and evolving rhetorical situations, aiming at different rhetorical goals, and embodying different assumptions about knowledge, nature, and communication. The form of the experimental report, in particular, solves a changing rhetorical problem: given what we currently believe about science, scientists, the scientific community, the scientific literature, and nature, what kind of statement about natural events can and should we make? To treat scientific style as fixed, epistemologically neutral, and transcending social situation is rhetorically naive and historically wrong.

In attempting to mobilize the powerful forms of argument developed within the natural sciences, the human sciences neither escape rhetoric nor eliminate rhetorical choice. Though some practicing social scientists might wish to escape the uncertainties of human discourse by embracing a single, correct, and absolute way of writing science, any model of scientific writing embeds rhetorical assumptions. Recognizing and examining these assumptions reasserts our control of choices that may otherwise be determined by unconsidered tradition, stereotype, and ideology. The forging of a scientific language is a remarkable achievement; but since it is a human accomplishment, it must be constantly reevaluated and remade as the human world changes.

This reevaluation is all the more important because the assumptions of forms of scientific communication involve the fundamental practices and organization of the disciplinary community. Attempts to transplant rhetorical forms from one community to another engage basic issues of what these communities are doing and how they go about it. The form will either be changed by the soil and climate of the new disciplinary

community or it will struggle with maladaptation. This chapter and the next discuss two cases of the transplantation of the experimental report into the social sciences. In the first case, the development of experimental psychology gives a particular interpretation to the experimental report that achieves a highly codified, institutionalized form. This codification stabilizes particular intellectual beliefs, empirical practices, and social relations around assumptions of a particular kind of research program. In the second case, political science seems to have had greater difficulties in defining a consistent, stable interpretation of the experimental report despite energetic attempts to do so. The task, concerns, methods, and organization of political science seem to bring to bear many pressures on the language, which have not yet seemed to crystallize around a satisfactory form.

## A Scientific Style for the Social Sciences

To understand the scientific style that emerged in the human sciences over the last century we need to look closely at experimental psychology. Experimental psychology was the first human science to establish a specialized discourse, distinguished from traditional philosophic discourse. Experimental psychology became the model and set the standards for all the psychological specialties that aspired to the status of science. In time, it played the same role for sociology, which did not start to develop a predominant scientific style until the 1920s, and political science, which followed suit in the 1950s. Today the American Psychological Association *Publication Manual* symbolizes and instrumentally realizes the influence and power of the official style.

The official APA style emerged historically at the same time as the behaviorist program began to dominate experimental psychology. Not surprisingly, the style embodies behaviorist assumptions about authors, readers, the subjects investigated, and knowledge itself. The prescribed style grants all the participants exactly the role they should have in a behaviorist universe. To use the rhetoric is to mobilize behaviorist assumptions.

Recent versions of the *Publication Manual*, filled with detailed prescriptions, convey the impression that writing is primarily a matter of applying established rules. The third edition, published in 1983, offers approximately two hundred oversized pages of rules, ranging from such mechanics as spelling and punctuation through substantive issues of content and organization. The important section on "Content and Organization of the Manuscript" focuses almost exclusively on experi-

*Four: The Reinterpretation of Forms in the Social Sciences*

mental reports, for although it recognizes genres such as review articles and theoretical articles, it comments that "most journal articles published in psychology are reports of empirical studies."

The experimental report is to have the specified sections: title, abstract, introduction, method, results, and discussion. Each of the last three sections is to be so titled. Each section must conform to detailed instructions, at times resembling a questionnaire in specificity. In the methods section, for example, one must include separately labelled subsections (usually *subjects*, *apparatus*, and *procedure*), each reporting specified content. The instructions for describing the experimental subjects indicate the level of prescribed detail:

Subjects. The subsection on subjects answers three questions: Who participated in the study? How many participants were there? How were they selected? Give the total number of participants and the number assigned to each experimental condition. If any participant did not complete the experiment, give the number of participants and the reasons they did not continue.

When humans are the participants, report the procedures for selecting and assigning subjects and the agreements and payments made. Give major demographic characteristics such as general geographic location, type of institutional affiliation and sex and age. . . . (26)

And so on for another two and a half paragraphs.

Few could question, given the collective experience of the discipline, that such information is often important for understanding and evaluating the experimental results. But the assignment of the information to a fixed place in a fixed format lessens the likelihood that researchers will consciously consider the exact significance of such information, whether it and other possible information should be included, and exactly how this information should be placed in the structure of the whole article. The prescribed form of fixed sections with fixed titles creates disjunctions between mandatory sections: the author does not have to establish overt transitions and continuity among the parts. The method section is a totally separate entity from the introduction or results. Although problem, method, and results must correlate at some level, the author escapes the need for transitions to demonstrate the coherence of the enterprise.

The foreword of the *Publication Manual*, well removed from the substantive prescriptions, does contain several disclaimers about linguistic evolution and flexibility. It notes, for example, that

Although [the manual's] style requirements are explicit, it recognizes alternatives to traditional forms and asks authors to balance the use of rules with good judgment. . . . It is a transitional document. It looks at the literature itself to determine forms rather than employing style to contain language. (10)

Yet the introduction to the actual organizational prescriptions takes a hard line:

Consistency of presentation and format within and across journal articles is an aspect of the scientific publishing tradition that enables authors to present material systematically and enables readers to locate material easily. Finally . . . the traditional structure of the manuscript allows writers to judge the thoroughness, originality, and clarity of their work and to communicate more easily with other individuals within the same tradition. (18)

In addition to the appeal to tradition—a tradition we will find shorter and more varied than one might guess—this passage urges uniformity on three other grounds: efficiency of reference, evaluative usefulness, and ease of communication. The second reason presupposes one right way to present an experimental report and that wandering from the form is bad science, or at least keeps bad science from being evident. The other two reasons suggest an encyclopedic function for an incremental literature; the concept of incremental encyclopedism will be examined later in this chapter.

### **History of the APA *Publication Manual***

The prescriptiveness evident in the current publication manual has only gradually developed since the first "Instructions in regard to Preparation of Manuscript" appeared in the February 1929 *Psychological Bulletin*. This original stylesheet was only six and a half pages long. About a page discussed "Subdivision and Articulation of Topics," a third of which was explicitly devoted to experimental articles. Despite a "natural order" for the presentation of experiments, internal titles are discouraged: "Necessary Headings only should be inserted" (58). Advice was of a general kind; for example, to include sufficient detail to allow the reader "to reconstruct and to criticize the experimentation and to compare it with other procedures and results" (59). The committee

*Four: The Reinterpretation of Forms in the Social Sciences*

preparing this set of instructions avoided an authoritative stance, presenting these suggestions for "general guidance" only.

The 1944 stylesheet, "The Preparation of Articles for Publication in the Journals of the American Psychological Association," grew to 32 pages. Guidelines for bibliographical reference and the use of tables and graphs correspondingly increased in length, as did the explanation of the editorial policies of the APA journals. On the structure of the experimental article, however, the stylesheet says little more than the previous edition, although now conceding that the form "has now become structured into a fairly developed pattern" (350). Moreover, the stylesheet encourages use of headings to indicate "the main features of [the article's] framework" (351). The authors offer their advice for the "younger members of the profession, many of whom are writing for publication for the first time" (345). Thus pedagogy allowed prescriptions without committed prescriptiveness.

The 1952 *Publication Manual*, now a 61-page separately bound supplement to the *Psychological Bulletin*, no longer hedges its prescriptive intent: "The purpose of the publication manual is to improve the quality of the psychological literature in the interest of the entire profession" (389). The manual is the standard. And as a standard it lays out explicitly just what is demanded. The section on organization lists the familiar parts of the experimental study, but suggests that headings reflect "the particular requirements of a study," rather than the standard part titles. Nonetheless, the manual prescribes what should be included within each. For example, the method section "should describe the design of the research, the logic of relating empirical data to the theoretical propositions, the subjects, the sampling and control devices, the techniques of measurement, and any apparatus used" (397).

The 1957 and 1967 revisions, although differing in some specifics, retain the general length and detail of the 1952 manual. The 1974 edition doubles the length and detail of prescription again, devoting 12 of the total 132 pages to content and organization. The 1983 edition "clarifies" and "amplifies and refines" this second edition, but does adhere to much of its wording. Notably, to ensure that standards are met on all levels, this last edition adds a section on grammar.

Two further style changes concerning the summary and reference formats are worth noting here. In the 1927 stylesheet, the last section of a paper was defined as a summary entirely separate from the abstract to be submitted to *Psychological Abstracts*. The 1944 stylesheet clarifies that the summary should be a serially numbered list of conclusions. In 1952, the summary, no longer a list, becomes a description of the entire argument, covering "the problem, the results, and the conclusions." This

formal summary could also be used for *Psychological Abstracts*. Beginning in 1967, however, the abstract appears at the front of the published article, eliminating the final summary.

The reference format changes from traditional footnotes in 1927, to cross references, to a numbered bibliography in 1944, to the current system of author and date amplified in a reference list at the end, first prescribed in 1967. These changes help bring the references into the flow of the discussion as items for conscious attention. Both the dates and the names of authors now serve as kinds of facts in the argument.

## Early Articles in Experimental Psychology

The evolution of the published articles in experimental psychology reveals the nature of the rhetoric embedded in the *Publication Manual*, for the history of the articles shows the rhetoric in action. The characterizations that follow are based on analyses of over 100 articles and examination of several times that number from the chief journals of experimental psychology, clustered in the early period (last decades of the nineteenth century), the periods of behaviorism's rise (1916 to 1930) and dominance of behaviorism (1950 and 1965, taken as sample years), and the current period (1980 as a sample year). The selection of articles analyzed and examined is large enough to reveal the major trends, but the dates attributed to the first emergence or dominance of any particular feature are necessarily approximate. Further, any characterizations of large numbers of texts will inevitably obscure differences among texts and may not be accurate for specific features of individual texts; however, as the official behaviorist style emerges, texts become much more uniform. That movement toward prescriptive uniformity forms a central part of the story.

The founding journals of the discipline defined the acceptable range of writing for the field by the articles they published: *Philosophische Studien* (hereafter *PS*), founded by Wilhelm Wundt in 1883; the *American Journal of Psychology* (*AJP*), founded by G. Stanley Hall in 1887; and the *Psychological Review* (*PR*), founded by J. M. Cattell and J. M. Baldwin in 1894. Each began the first issue with an editorial or article discussing the emergence of a new scientific psychology based on experimental results.

Despite these rigorous programmatic statements, the early issues of these journals, particularly the two American ones, contain a wide variety of articles, only some of which could be labelled experimental. The first two volumes of the *AJP* do contain such narrowly experimental



studies as "Dermal Sensitiveness to Gradual Pressure Changes," but also contain "A Study of Dreams," "Winter Roosting Colonies of Crows," "Extracts from the Autobiography of a Paranoiac," "The Place for the Study of Language in a Curriculum of Education," "Folk-Lore of the Bahama Negroes," and "On Some Characteristics of Symbolic Logic." Many articles sought to bring empirical data to the philosophic inquiry into the mind. Indeed, the editor's manifesto in the first issue claims a broad audience for the *AJP*: teachers of psychology, anthropologists interested in primitive manifestations of psychological laws, physicians interested in mental and nervous diseases, biologists and physiologists, and anyone else interested in the advances in scientific psychology.

Early experimental work measured such quantifiables as perceptual sensitivity and reaction times, but these measurements served only as empirical entry ways into the mysteries of the mind. Although they followed the general structural pattern of experimental reports already established in the natural sciences, the early articles had more the character of philosophic essays. For example, an article in the first issue of the *AJP* by Hall and Motora begins with a Greek epigraph from Plato (72).

The two American journals did not use any internal headings in the articles; consequently, words had to bridge the parts, explaining how the whole inquiry fit together. In the first volume of *PR*, for example, Hugo Munsterberg presents a series of five "Studies from the Harvard Psychological Laboratory." These studies have no internal divisions, although they clearly follow standard experimental order. Each part grows out of the previous one. The third study, "A Psychometric Investigation of the Psycho-Physic Law," demonstrates this strikingly. The opening theoretical discussion of the psycho-physic law argues that a new kind of measurement is needed. The experimental design then provides the desired measurements. Moreover, each aspect of the experimental method is justified and explained in terms of current knowledge about the psycho-physic law. The specific parameters for measurement refer back to the theoretical problem, and the actual results follow immediately as a response to the specific parameters. Discussion of the consequences of the results for the psycho-physic law follow naturally as part of the thematic continuity of the whole essay.

Articles in the German *PS*, although they frequently use standard section headings, provide heavy continuity among the parts. Often the first paragraph or two of a labelled section considers either general thematic material or the issues raised in the previous section, so that the substance of the section is not directly discussed until it is firmly tied to the total structure of the article.



In these experimental essays, the authors reveal themselves as problem-solving reasoners, figuring out how quantitative experiments might aid understanding of philosophical issues. The discussion of methods plays a crucial role, raising and answering the problem of how one can translate the theoretical problem into concrete empirical results. For example, Munsterberg, in the series mentioned above, repeatedly proposes his methods as correcting the failure of previous methods to make proper distinctions. The effort devoted to the presentation of the methods shows clearly that they are a significant part of the intellectual achievement of the work presented in the article. Similarly, the first experimental article in the premiere issue of *PS* devotes an eight-and-a-half-page methods section to deriving the methods from the nature of the phenomena to be investigated and to evaluating alternative methods (Friedrich).

The early authors believed that psychological phenomena were internal, subjective events and that the measured data were only external indicators of what was going on inside. Trained introspection provided evidence in conjunction with more external quantitative measures. Thus the subjects of the experiments emerge as active characters in the experimental report. Individual experimental subjects, which included trained psychologists, were often identified by name. (Wundt himself was a subject in many experiments performed in his laboratory.) In the experimental report the identification of subjects shows their training and credentials for making accurate introspective judgments. The author of "Experiments in Space Perception," James Hyslop, is himself the experimental subject. Combining psychological knowledge and an unusual ability to use his eyes independently, he devised certain tricks or exercises for himself that help to elucidate principles of perception. The two-part article is imbued with first-person accounts of what he did and what he perceived.

The readers were sometimes treated as being quite knowledgeable about current work, so much so that much technical background was left understood, as, for example, in Hall and Motora's article on dermal sensitivity in the opening issue of *AJP*. Nonetheless, the audience was generally treated as concerned with broad issues of psychological understanding. The early articles almost always begin at some issue of general psychological interest and connect the specific study to that issue. In fact, that technical article by Hall and Motora is the one prefaced by the Greek quotation and appears in the same issue as Hall's editorial anticipation of broad readership for the journal.

These articles review the literature only sporadically. At most, short summaries present assorted experimental results, without establishing definitive findings that lay a stable groundwork for current studies. Fre-

quently articles begin without any specific mention of previous work. In short, the articles give the general impression of a new beginning, to be grounded thoroughly on empirical results, as opposed to the implicitly rejected nonempirical earlier work. This is consistent with a philosophic tradition that treats each new approach as a fresh attempt to ground philosophy on its true footing.

Wundt's role in his journal, which largely published the results of his own laboratory, best reveals the philosophic nature of the endeavor. Wundt, although the founder of the first regular lab and frequently called the father of experimental psychology, did not publish any experimental reports in *PS* (the experimental reports were written by his subordinates). Nonetheless, articles by Wundt appeared in the journal at least two or three times a year, and as often as eight, discussing ideas, methods, and large philosophic issues well removed from psychology. These discussions often appeared as reviews or critiques of the work of others, but always with the purpose of explicating fundamental issues. Wundt kept the empirical work of the new discipline firmly in philosophic, reasoning focus. Although his students and other followers stayed much closer to the data—and no one seemed to be granted his same right to philosophize at length in the pages of the journal—he helped maintain the philosophic thrust of the discourse.

Despite the desire to subordinate the experiments to philosophic inquiry, the experimental data proved too complex and too removed from philosophic issues to resolve the problems posed. Typically in the early articles, the continuity of rational discussion breaks down when the results section is reached. The argument bogs down in extensive tables, reporting massive amounts of data—much of it raw or subject only to simple aggregating calculations. As in an 1894 study by Jastrow in *PR*, the discussion often no more than repeats the tabular data with a few, low-order statistical generalizations. Characteristically, no conclusions relative to a substantive problem are drawn, and the ultimate meaning of the data remains murky. Authors often caution against generalizing too quickly on the basis of uncertain results in situations that remain too multifactored to analyze fully. Future, more decisive results are promised. When substantive conclusions are drawn, the intermediate analysis of the data may be missing, such as in one of Munsterberg's studies which bypasses specific explanations through phrases like, "it is evident," "of course," and "the reason lies evidently in the fact that."

The inability of this massive data to resolve philosophic issues, such as the natures of memory and perception, soon led to a divorce between philosophic and empirical work.<sup>1</sup> Articles turned to establishing low-

1. Indicative of the early divorce between philosophy and psychology is the changing

level generalizations descriptive of the results. Literature reviews grew longer as the literature grew, and there was some attempt to find common denominators or clear patterns of disagreement among the prior results and set up the current experiment as a resolution. Methods became standardized and were frequently referred to by eponyms or citations. But the results generally did not resolve substantive issues. Conclusions were often a series of numbered statements, repeating the data. Even where the numbered conclusory statements addressed the originating question, as in the 1916 article in the *Journal of Experimental Psychology* (hereafter JEP) "A Preliminary Study of Tonal Volume" by G. J. Rich, only minimal substantive discussion related results to the problem. The complex data, both psychophysical and introspective, were left largely to speak for themselves.

Since the true object of inquiry remained internal phenomena, the subject of the experiment remained an important independent actor in the story. Subjects were described to show expertise or particular qualifications for accurate observation. In Dallenbach's articles throughout the period, for example, subjects are characterized as trained in psychology and familiar with the purposes and methods of the particular experiment. Introspective accounts provide data and, importantly, possible interpretations of the measured data. As late as 1930, in a study by Ferral and Dallenbach, the introspective accounts of the subjects (which include Dallenbach) are used to guide the analysis of the other results. Another striking example, "An Experimental Study of Fear" by V. Conklin and F. Dimmick, is based entirely on introspective accounts of emotional responses to the experimental situation.

Other methods of gaining evidence about the internal processes of humans were also still acceptable. A study of the foster-child fantasy is based on a survey of adolescents rather than on an experiment (E. Conklin). Another study was an anthropological observation of "The Gesture of Affirmation Among the Arabs," to clear up some incorrect and misinterpreted facts used by Wundt (George). Studies of literary fig-

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character of the articles in the English journal *Mind*, founded in 1876 with the stated intention of being the first journal of the new psychology. The philosophic climate in England, however, did not prove conducive to the flowering of experimental psychology. Although early volumes contain glowing reports of the experimental work in Germany (for example, J. Sully, "Physiological Psychology in Germany," *Mind* 1 [1876]: 20-43), reviews of experimental work became increasingly critical (for example, G. C. Robertson, "The Physical Basis of Mind," *Mind* 3 [1878]: 23-43; and E. W. Scripture, "The Problem of Psychology," *Mind* 16 [1891]: 305-26. The general complaint against experimental work was grounded in the mind/body dichotomy; these philosophers found physical data of no value for understanding issues of mind. By the turn of the century discussion of experimental psychology ended altogether, leaving the journal as a purely philosophic one.

ures based on their works still appeared in *AJP* as late as 1920, when analyses of Charlotte Brönte and Edgar Allen Poe were published (Dooley; Pruette).

The author thus remained a problem solver, trying to gain some understanding of mental processes using empirical data, even though the discussion had now switched from a general philosophic to a more particular descriptive mode. Articles through 1920 still read as continuously reasoned arguments, with internal headings used sporadically and flexibly. Headings, when used, often reflected the specific content of the article and were not typographically prominent.

The implied audience as well remains varied—interested in the problems, but not necessarily involved in research. Through the 1920s articles still frequently start with familiar problems of everyday experience (such as fear, fantasy, and the sensation of burning heat), and they take a variety of approaches to study the problems. The articles are aimed at a wide range of people interested in the workings of the mind.

## Behaviorism Finds Its Voice

As behaviorism in its many forms came to dominate psychology between the two world wars, a rhetoric consistent with behaviorist assumptions narrowed rhetorical possibilities and became the basis for the official style reflected in the *Publication Manual*. By behaviorism and behaviorists, I mean the general turn toward behavior and away from mind as the proper subject and data for psychological investigation. Many varieties of explicit behaviorism developed, not just the classic versions of Watson and Skinner. Additionally, many other schools of experimental psychology followed behaviorist procedures, although they did not explicitly espouse behaviorism.

Toulmin and Leary associate the dominance of behaviorism and neo-behaviorism with a "cult of empiricism" fostered by an alliance with logical positivism, popular during the same period between the wars. 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.<sup>2</sup> And the behaviorist rhetoric could be identified as the only proper way to write science.

2. Lawrence D. Smith makes a similar point in "Psychology and Philosophy: Toward a Realignment, 1905-1935."

The proper way in which to write positivist, behaviorist science did not, however, appear immediately on the scene, invented in a burst of self-conscious rhetorical creativity. Instead, the style emerged over a number of years as many individuals gradually discovered the form most congenial to their ideas and work. Early works appeared in a variety of styles consistent with the patterns of the past.

John B. Watson, although often credited as the founder of behaviorism, published little behavioristic experimental work. Rather, what is taken as his seminal work, "Psychology as a Behaviorist Views It," is a polemic. It is continuous, persuasive, and aimed at a general audience; it considers a general problem and presents the author and audience as reasoners capable of making intelligent judgments. Furthermore, as editor of *Psychological Review* from 1910 to 1916 and then of the newly founded *Journal of Experimental Psychology* for another ten years, Watson presided over the kinds of articles described in the previous section.<sup>3</sup>

The famous article "Conditioned Emotional Reactions" (1920), which Watson coauthored with Rosalie Raynor, reports one of his few published experimental studies. This unusual article, although different in many respects from both articles that came before and those to come after, still bears more resemblances to the earlier rhetoric than to the later. The study, which describes the conditioning of an infant to fear rats, is told as a coherent story with no real headings or strong divisions to interrupt the flow of argument. The only marked divisions are four questions identified by Roman numerals and passages from the laboratory notes, identified chronologically. The typical structure of introduction, method, results, discussion is not even maintained. Rather the theory to be demonstrated dominates the organizational pattern, with aspects of the method and results separated and subordinated to the different questions to be answered.

Thus the authors emerge as reasoners and persuaders, constructing an argument using experimental results to persuade the readers of the truth of a general theory. The authors use the first person throughout in order to present themselves in a number of roles: as doers of the experiment, as holders of certain expectations, as investigators desiring tests of certain questions, as makers of observations, as provers of certain propositions, and as interpreters of results. Furthermore, they present the experimental results in the rather personal form of the lab notes, replete with disjointed phrases and sentence fragments. Even though the notes present the events without reference to internal processes or

3. The *Journal of Experimental Psychology* was founded as an offshoot of the *Psychological Review*, and the two journals shared editorial boards.

imputations, rhetorically they serve to show the events through the eyes of the narrator.

The authors also stand well back from the literature, which is presented largely as speculative and unfounded, even including Watson's own writing on the subject. This article is, in short, another attempt to begin inquiry into basic matters *de novo*. Here again we see the independent philosopher, impatient with earlier false starts and misguided work. The tone of the opening paragraph reviewing the state of the problem is brusque and mildly contemptuous; that of the next to last paragraph comparing the authors' conclusions with Freud's is gratuitously and gleefully nasty, reminiscent of the delightfully vitriolic exchanges of nineteenth-century German philosophers.

Thus the audience is witness to a knock-down intellectual argument and is invited to choose sides, not just between ideas, but between persons: Watson and Freud. The choice rests on the audience's response to a first-person account of a single incident: in essence, a short story. In its narrative simplicity, clarity of argument, and broadness of issue, the article clearly aims at a wide audience. Its vigor of argument assumes that readers can and will make a choice—in favor of Watson.

The subject of Watson's experiment, the infant Albert B., has an immediate presence in the drama of the piece. The detailed description shows how, by virtue of his stability and lack of fear, he is mentally fit for the test to which he will be submitted. He emerges as an individual character in an engaging narrative account of his induced phobia, very much in the tradition of the clinical accounts of the mentally ill that had until recently shared the pages of the journals with experimental reports.

However, two differences set the treatment of Albert as a subject apart from the treatment of subjects in previous articles. First, the details of his background establish that his mind is a clean slate, unaffected by special quirks, foreknowledge, or other hindering factors. The subject's identity, in other words, stands as a sign of the experimenter's control of variables, rather than as a sign of the subject's special capacity to observe his own reactions. Second, the authors exclude introspection or any other attempt to gain knowledge of the subject's internal processes or sensations. This is the obvious mark of behaviorism. Yet, despite the attempt to turn Albert into an impersonal object of study, the fullness of narrative reveals a poignancy to the story. As Albert's phobia grows, the reader sees him become a victim, moved by the manipulations of the experimenter.



## **Stabilizing an Objectified Rhetorical Universe**

In the period following the publication of this article, the objectification of the subject increases. Author, audience, and literature as well become more objectlike. All the aspects of the drama of experimental article move into a behaviorist universe. The rhetorical decisions made in the 1920s are elaborated, rigidified, and standardized in subsequent decades. The first APA stylesheet appeared in 1929; the increasing certainty and detail of prescriptions in the successive stylesheets follow and confirm the growing influence of this behaviorist style in the journals. Articles begin looking like one another, so that we can clearly identify an official style that lies behind the prescriptions of the publication manual.

Only when a community decides there is one right way, can it gain the confidence and narrowness of detailed prescriptions. In rhetoric, "one right way" implies not only a stability of text, but a stability of rhetorical situation, roles, relations, and actions, so that there is little room or motive for improvisatory argument. Within a stabilized rhetorical universe, people will want to say similar things to each other under similar conditions for similar purposes. In this context, prescribed forms allow easy and efficient communication without unduly constraining needed flexibility. The behaviorist picture of the world allows that stability and lack of free invention.

As we have seen in the article by Watson and Raynor, the behaviorist world view first made itself felt in characterizations of the experimental subject and the phenomena investigated. Not only do behaviorists categorically eliminate imputations of internal processes and introspective accounts, they no longer consider the external data as indicators of some mental process. The experimental problem switches from one of indicators to one of controls, from getting some hard data on complex individual internal processes to keeping the history of the subject and the environment sufficiently clean. The kind of narrative that Watson provides of Albert B. soon vanishes, for such a narrative grants too much personality to the subject, who is to be reported more as a type exhibiting very specific behaviors in highly controlled circumstances.

The previous tendency toward low-level conclusions that give only aggregate descriptions of the behavior observed no longer is a difficulty—it is the whole extent of the enterprise. One looks only for patterns of behavior, not underlying principles or mental operations. The increasing statistical sophistication of experimental articles serves to



exhibit and validate patterns of behavior across large numbers of subjects. The results themselves appear in increasingly calculated and patterned ways. Individual behavior disappears in a pattern, displayed in a graph or a table of secondary calculated values, rather than as a raw number. The results sections increasingly begin by describing the display tables and figures. By 1950, statistical talk, describing the statistical methods used and the limits of statistical reliability, becomes a standard part of the results section, usually immediately following the presentation of the numerical display.

Instead of a reasoner about the mind, the author is a doer of experiments, maker of calculations, and presenter of results. The author does not need to reason through an intellectual or theoretical problem to justify or design an experiment, nor in most cases does he or she need to identify and take positions on arguments in the literature. To produce new results, the author must identify behavior inadequately described and design an experiment to exhibit the behavior in question. With the methodological problem reduced to obtaining uncontaminated results, carefulness rather than good reasoning becomes the main characteristic to be displayed in the methods section. The methods section becomes less substantively interesting. Starting about 1930, the section is demoted to small print, where it remains today. Nor are methods customarily covered in summaries or abstracts.

This rhetorical diminution of methods in a science devoted to obtaining experimental results only makes sense once we see that the main rhetorical function of the methods section is not to present news or innovation, or even to help the reader conceptualize the event that produces the results. Its main function is rather to protect the researcher's results by showing that the experiment was done cleanly and correctly. In the articles from sample year 1950 that I examined, this desire to protect results by constantly demonstrating that one has done things correctly on all counts, from examining the prior literature to using proper statistical methods, becomes obtrusive and accounts for much of the length of the articles. As the conventions for demonstrating proper work become stabilized, by the growing prescriptiveness of the stylesheets and by repeated practice, this competence display is done more rapidly, so that by 1965 these preliminaries take much less space.

Because the methods section no longer serves as an intellectual transition between the problem and results, the article tends to break into disjointed parts, increasingly labelled by standard headings, as reflected in the successive stylesheets. The results become the core of the article. Discussion often merely sums up the data and is sometimes relegated to small print. Conclusions do little more than repeat confirmation of the descriptive hypotheses.

With the article primarily presenting results, constrained and formatted by prescription, the author becomes a follower of rules to gain the reward of acceptance of his results and to avoid the punishment of non-publication. Accepting this role, he subordinates himself to the group endeavor of gathering more facts toward an ultimately complete description of behavior—a project of incremental encyclopedism. As behaviorism gradually gained influence, authors began presenting results as ends in themselves, to fill out gaps in other results, rather than as potential answers to theoretical questions. In the mid-1920s, introductions rapidly changed from raising a problem to giving a codified review of literature, with each item associated with a specific contribution. The experiment to be reported in the article was then presented simply as some form of continuation of the prior work. After a brief period when close analysis of the literature was allowed in small print, disagreements over theory, results, or formulations in the previous literature tended no longer to be discussed. Articles were treated as accumulated facts; literature reviews in the articles lacked synthesis, problem-orientation, or interpretation. In 1930, Edwin Boring, then an editor of the *American Journal of Psychology*, in a note in that journal attempting to domesticate the Gestalt movement, articulated the principle: “The progress of thought is gradual, and the enunciation of a new crucial principle in science is never more than an event that follows naturally upon its antecedents and leads presently to unforeseen consequents” (309).<sup>4</sup> This communal vision—much narrower than the traditional “shoulders of giants” formulation—diminishes the role of any individual as a thinker.

Several other rhetorical consequences flow from this incrementalism. First, since the function of the article is now to add a descriptive statement to an existing body of such statements, and since the new statement would achieve this goal only if it passes certain tests, strong rhetorical pressure pushes the candidate statement (the hypothesis) near the front of the article. Only then can the reader, in reading the body of the article, judge whether the claim passes the criteria. Thus the descriptive generalization moves from a conclusion to an opening hypothesis that takes on an increasingly central role in the presentation of the experiment.<sup>5</sup> As the main unifying element in the article, the hypothesis often comes to be repeated four or more times in a single article. Similarly, as the abstract switches from a summary of results to the presenta-

4. Boring had earlier formulated this principle in “The Problem of Originality in Science,” *American Journal of Psychology* 39 (1927): 70–90.

5. The common methodological belief that the formulation of a hypothesis must precede the design of an experiment in the actual research process may in part derive from this rhetorical order.

tion of problem, results, and discussion, the "problem" comes to mean the test of the hypothesis and the "discussion" the confirmation of the hypothesis.

Second, since they are adding only bits to a larger descriptive project, articles decrease in scope and length. The single experiment replaces the series of experiments with minor variations in conditions or procedures. The confirmation of a single descriptive statement replaces the examination of a large phenomenon from a number of angles.

Articles also become shorter with the codification of format and of surrounding knowledge. With a fixed framework of knowledge and communication, one can add one's single additional bit more rapidly. In the selection of articles I examined, the low point of article size was in the mid-1960s. Articles from the same period also show significant increase in the technical vocabulary, indicating a dense specialized knowledge. Earlier most of the technical terms (except for statistical terms) were ordinary language terms, only given more precise definition; for example, *stimulus*, *condition*, *fatigue*. Even such unusual coinages as *retroactive inhibition* are not far removed from ordinary usage. But in the 1965 articles, terms, although originating in common-use vocabulary, take on such narrow concrete meanings that they diverge from normal meaning. The terms then get used in tight combination with other such terms. As well, key terms start being replaced by acronyms or abbreviations. Only those familiar with the technical background can be sure that they know exactly what is being discussed in a phrase such as, "the effects upon verbal mediation of the delay intervals interpolated between the two acquisition stages of a mediation paradigm or between the second acquisition stage and the test trial" (Peterson, 60).

Third, the *Publication Manual* adopted the new reference style, wherein the author and date of an article appear as facts or landmarks in the course of the article, visibly demonstrating the incrementalism of the literature. As anyone who has worked with this reference system can attest, it is very convenient for listing and summarizing a series of related findings, but it is awkward for extensive quotation or discussion of another text, and even more awkward for contrasting several texts in detail. The format is not designed for the close consideration of competing ideas and subtle formulations.

Finally, readers are no longer cast in the role of people trying to understand or solve some problem. Rather they are presumed to be looking for additional bits of knowledge to fit in with their previous bits. They are assumed to be looking for faults, because such faults would disqualify the experimental report as a valid increment to the descriptive encyclopedia. The author must display competence to the audience,

rather than persuade readers of the truth of an idea. If properly demonstrated by a proper experiment, the hypothesis must be accepted by the audience. In an intellectual sense, the audience has little to say about the meaning of an experiment or even about the truth of a hypothesis. Its role, rather, is to judge the propriety of the experimental proof.

Within this rhetorical world, the chaos of intellectual differences is eliminated. Individuals assimilate bits, follow rules, check each other out, and add their bits to an encyclopedia of behavior of subjects without subjectivity. There is not much room for thinking or venturing here, but much for behaving and adhering to prescriptions. Thus we get to the ever-expanding *Publication Manual*.

Over the last twenty years, a major style change in the psychological journals has again started to take place, the result of the rising influence of a cognitive psychology based on the computer model. This new approach brings with it a new epistemological and rhetorical universe. It is too soon to give a full account of this new style, nor is it clear how pervasive it will become in the face of the continuing behaviorist rhetoric. One thing is clear: this new style has not yet affected the *Publication Manual* in any significant way. The APA manual still serves basically as a codification of behaviorist rhetoric.

For those social scientists who believe that the behaviorist, positivist program creates an accurate picture of the human world and provides the surest (if not only) path to knowledge, the prescriptive rhetoric of the *Publication Manual* is precisely the right one. It offers a programmatically correct way to discuss the phenomena under study; moreover, it stabilizes the roles, relationships, goals, and activity of individuals within the research community in ways consistent with the community's beliefs about human behavior. The invention of a way to communicate consonant with beliefs constitutes a major accomplishment. Nonetheless, the realization that behaviorism has not escaped rhetoric, but has merely chosen one rhetoric and excluded alternatives, may temper adherents' certainty about their mode of communication.

For those who have received any rhetoric as a given, the recognition of the implications of an official style reopens the question of how to write. Rhetoric is always sensitive to beliefs about the world. The human sciences undergo a particularly immediate form of this rhetorical sensitivity, for these sciences create and argue for beliefs about human beings, the inevitable main actors in the drama of communication. If a social science changes our view about the nature of ourselves, we need to change our way of talking to each other consonant with our changing self-image. To neglect the implications of our rhetoric is to lose control of what we say.

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