10 STRAINS AND STRATEGIES IN WRITING A SCIENCE OF POLITICS

THE UNSETTLED RHETORIC OF THE *AMERICAN POLITICAL SCIENCE REVIEW*, 1979

Psychology, by treating the individual as a separate biological behavioral unit can create a disengaged, objectified discourse that seems to separate both the experimental object and the experimenter from the historically evolved forms of culture in which humans act. Indeed, as we have seen, one of the important themes in the rhetoric of experimental psychology is to represent one's experimental subjects as sufficiently clean *tabulae rasae* and the conditions of one's experiments far enough removed from daily life so as not to be contaminated by the uncontrolled complexities that move our lives. But other social sciences, such as economics, sociology, anthropology, and political science, must deal more immediately with the complexes of human-made culture, for these human-made complexes are exactly their subject. As a result, when they come to try to represent any particular case, they must contend with many forces that cannot be contained within the laboratory walls. Culturally embedded studies must overcome many obstacles in arguing from the particular to the general, for the complex of details and local variables of each case can generate unending alternative descriptions and generalized accounts of the processes involved. To move from plausible conjecture to forceful persuasion to compelling argument, the researcher of human sciences must develop a rhetorical tool kit, different and perhaps more subtle than that developed in the natural sciences. And that rhetorical tool kit will also have likely consequences for the interaction with the object of study, the structure of communication, the social system of the discipline, and the discipline's goals and activities.

Despite the rhetorical problems posed by the social sciences, many social scientists have attempted rather direct importation of what they perceive to be the methods and communication styles of the natural sciences. As in the case of experimental psychology, the model of scientific communication adopted is likely to be a simplified abstraction (often supported by a prescriptive philosophic position), that ignores the complex rhetorical dynamics and historical fluidity of actual communication in the social sciences. In some respects the models of scientific communication transplanted wholesale into the social sciences more resemble that of high school laboratory courses. The high school science laboratory is an orderly and predictable place filled with well-defined objects, well-established formulations from textbooks, fixed expert-amateur social relationships, and predetermined discoveries. The social, intellectual, natural, and creative worlds are held constant so that students can rehearse set operations to be reported in set formulations. In such stabilized conditions, language can appear an unproblematic representation of a stable reality.

There are gains and costs in such stabilizing simplifications. In experimental psychology, both the gain and cost have been a thoroughgoing commitment to a particular kind of research program that has seemed appropriate to large and influential parts of the research community. In economics, as Donald McCloskey argues persuasively in *The Rhetoric of Economics,* the gain has been clarity about the mathematical realization and relationship of economic forces, but the cost has been a kind of hypocrisy of the discourse that leads important issues and forms of argument to appear in only covert ways. The official style of contemporary economics seems to exclude a wide range of nonmathematical disciplinary reasoning, individual and cultural dynamics in economic participation, and traditional moral, social, and policy questions about economic choices. However, as McCloskey argues, these excluded forms of discourse have not vanished; they have just become hidden, making their discussion fragmentary and insufficient. He believes explicit recognition and acceptance of these topics will lead to a more satisfactory and productive discussion among economists without losing the clarity gained by the current official style.

A similar debate has been going on in anthropology concerning the status of ethnographies. Under the banner of scientific objectivity, ethnographies had been represented as impartial, disengaged observations of stable social realities, recorded in a socially inert, acontextual manner. Recently, however, issues of the social and literary participation of both ethnographers and informants have been raised. The ethnographic text reflects the interactions of ethnographer and informant

with each other, with the tribal community, with western society, and with the professional community of anthropologists. The text also serves as a form of social action within all these collectivities. Through critique and practice, anthropologists such as Clifford, Fabian, Geertz, Marcus and Cushman, Rosaldo, and Tyler have been attempting to reformulate ethnographic writing to consciously address the rhetorical complexity of the documents.

Political science, as well, in adopting what it considers a scientific style of communiction has relied on simplifications both of scientific discourse and of the rhetorical problem the discipline faces. Unlike anthropology, however, political science has not developed a significant reflexive literature to consider the true complexity of its rhetorical task. The discourse of political science suffers from a number of unrecognized strains, which I hope to begin to uncover in this preliminary study.

Political Science's Version of Scientific Writing

Since the middle of this century, the study of politics has been developing a form of scientific presentation relying heavily on mathematics for both evidence and argument. In this presentational style, most often the numerical data are gathered and analyzed statistically, but sometimes the argument takes the form of abstract mathematical reasoning, as when game theory is employed. Articles assuming this mode of discourse may be more fully characterized as opening with a problem expressed through a review of literature that orders the existing knowledge in a coherent system of findings and issues. The article then proposes a hypothesis or solution to the problem, presents (and perhaps justifies) a methodology, then tests the hypothesis through mathematical data and argument. At the end only narrow conclusions are formed, limited to what can be documented by the mathematical argument. In 1979 over 70 percent (30 out of 42) of the articles in the *American Political Science Review* could be so characterized. The remaining articles, other than a presidential address statistically examining one aspect of quantitative political studies, are devoted to historical narratives about political movements and philosophical discussions of new and classical political theory. Moreover, the articles in the natural scientific mode averaged 1.63 authors per article compared to an average of 1.08 authors per article for the historical and philosophical texts. The multiple authorship implies a research team practice resembling

that of the natural sciences, resulting from the complexities of data collection and analysis (e.g., Physics Survey Committee, p. 1368).

Such a textual organization is a direct correlate of the model of scientific activity presented in scope and methods books that explain to students of political science how to go about their intended profession (e.g., Greenstein and Polsby; Hayes and Hedlund; and Isaak). These books emphasize hypothesis testing and data collection as the core of science. Isaak, for example, discusses induction in the following terms:

> We test a hypothesis by seeing if it fits the world of observation. Suppose we want to test the hypothesis, "Businessmen tend to be conservative." A sample of businessmen would be questioned . . . to determine their ideological orientations. On the basis of this sample—and the confidence we place in our conclusions depends upon its size and randomness—we accept or reject the hypothesis. (91–92)

The task of the political scientist is to compare claims to empirical reality; the function of political science writing is to communicate the findings of these comparisons.

A closer examination of political science articles, however, reveals difficulties and complexities in this straightforward aspiration to a scientific ideal. Arguments do not fit together as crisply as the ideal would have it, and the political scientist as author inevitably finds himself in explanatory, justificatory, reconciliatory, and persuasive tasks that are not part of the idealized version of the scientific report.

Analysis of Political Science Texts

The following analysis is based on examination of all articles in the *American Political Science Review (APSR)* of 1979 (volume 73). Three articles, selected for their range of topics and styles, are analyzed in detail: Edward T. Jennings, Jr., "Competition, Constitutencies, and Welfare Policies in American States," 414–29; Diane L. Fowlkes, Jerry Perkins, and Sue Tolleson Rinehart, "Gender Roles and Party Roles," 772–80; and Benjamin I. Page and Calvin C. Jones, "Reciprocal Effects of Policy Preferences, Party Loyalties and the Vote," 1071–89.

The most obvious characteristic of the papers in *APSR* is their length. The mathematically developed articles in *APSR* in 1979 run from seven thousand to fifteen thousand words in length, with a mean of about twelve thousand words. The articles each occupy from nine to twenty pages of closely packed, double-column pages. In comparison, Watson

and Crick's famous paper (examined in the second chapter of this book) is under one thousand words; most of Compton's papers on x-radiation (discussed in chapter 7) are between one and two thousand words.

The only groups of papers averaging a comparable length that I found in the course of my researches were late eighteenth-century articles in the *Philosophical Transactions* (see chapter 3) and recent articles in physics (see chapter 6). The late eighteenth-century articles gained length through the long series of experiments (as many as ninety-five) reported in a single article. The recent articles in *Physical Review* have reached an average length of about ten thousand words through the embedding of arguments within complex theoretical contexts. However, neither of these reasons accounts for the length of the *APSR* articles. As the following analysis suggests, the reasons are rather to be found in the kinds of rhetorical work that must be accomplished within the political science article. The amount of that rhetorical work appears comparable to that required in the twelve nonmathematical essays appearing in volume 73 of *APSR*. Although developing arguments through political theory or historical accounts, and although adopting overtly different styles, the nonmathematical essays run about the same length as the mathematical ones.

Establishing the Literature

One of the kinds of work taking substantial space in the political science articles is discussing the prior literature. The typical bibliography of an *APSR* article in 1979 has from twenty to forty items, whether the article is mathematical, historical, or theoretical. The citation method obscures the actual number of textual references; the three articles examined closely each had from thirty to fifty mentions, discussions, or characterizations of other sources. In comparison, Watson and Crick had six footnotes, Compton typically referred to less than ten sources, and through 1960 articles in *Physical Review* averaged under a dozen references per article. Only in the most recent theoretically embedded articles in *Physical Review* has the average number of references grown to around twenty-five.

Unlike the references in recent physics articles, however, the references in *APSR* do not reflect embedding in a highly codified literature. Rather than infusing all parts of the argument, the references are concentrated in extensive opening reviews of the literature (in one case comprising half the article) and in the last few pages of conclusions. These reviews of literature, rather than discussing selected recent arti-

cles with direct bearing on the subject at hand, instead assemble and discuss all the literature in the problem area. Unlike articles in codified sciences where older texts have developed stabilized meanings and have been incorporated into the tacit assumptions of shared knowledge (Cozzens, "Taking"; Messeri) so that only recent articles tend to be explicitly mentioned and discussed (Price, *Little Science*), the political science articles reassemble, reinterpret, and discuss anew wide ranges of the literature, dating back into the discipline's history.

The article by Jennings begins with V. O. Key's seminal comment on welfare policies (1949) and then discusses every major test of Key's hypothesis (1959, 1963, 1969, 1970, 1976). The discussion then reinterprets Key's original comments. Jennings obviously cannot rely on the audience identifying and understanding the background literature in the same way he does; in his extensive discussion of the literature he establishes his vision of the prior work. Similarly, Page and Jones review thirty years of voter studies in nine pages and over seven thousand words; Fowlkes, Perkins, and Rinehart mention all work they consider important on women in party organizations and on differentiation of party membership—much of this work between ten and twenty-five years old. Whereas Price calculates that 72 percent of the references in recent volumes of *Physical Review* are to papers published in the preceding five years, a similar calculation for these three political science articles reveals that only 30 percent of the cited sources are from the past five years.

This extensive reinterpretation and reconstruction of the literature requires a broad-stroke treatment of a large number of sources. Works are frequently categorized as part of a group, with only representative articles discussed in detail. In their seemingly detailed discussion of prior voting studies, as an extreme example, Page and Jones actually discuss their own versions of typical arguments and then list sources which they claim take these approaches. Brief general characterizations, group characterizations, and simple lists of sources are common in all three articles.

Such patterns of generalization rely on the audience's faith in the author's judgment for their persuasiveness. Little compelling evidence can be given to justify interpretations and evaluations or eliminate alternative judgments. Selected detailed discussions of some sources do provide details of some interpretations, but even here justification for the readings is rarely provided. For example, Jennings summarizes two studies which he claims "can be interpreted to support [the preceding] analysis" (416). Each summary is about a hundred and fifty words long; however, Jennings never explicitly identifies the issues open to interpretation or the justification for his interpretation.

Establishing One's Contribution

The lack of codification of the literature offers the political scientist large opportunities for putting his or her current work in the most advantageous light. With prior work regularly open to reinterpretation and criticism, each new contribution can be represented as a radical new departure or a fundamental solution to ancient gordian knots. All three articles, in fact, claim all prior work misses the boat; the reviews of literature, consequently, are critiques of the fields in question.

Fowlkes, Perkins, and Rinehart suggest that gender roles in politics have been incorrectly conceptualized. Jennings argues that "the logic underlying standard formulations of the interparty competition (IPC) hypothesis" (415) is faulted and needs reformulation. Most totally rejecting the literature, Page and Jones suggest "that virtually all past voting studies have erred by ignoring the possibility of reciprocal causal effects among the central variables of the electoral process" (1071).

The problem of each paper is simply to rectify the earlier mistakes. Without strong codification of the literature, more precise forms of contribution (such as the solution of recognized problems, the reconciliation of anomalies, proposing a new account of previously identified phenomena, or extending previous work to new domains) are difficult to identify. Moreover, the consequences of the current contribution for related work are also difficult to pinpoint. Within the loosely connected, personally interpreted and evaluated political science literature, any particular new finding, though interesting or striking, may not suggest immediate follow-up work.

Emphasizing the methodological innovations of a study is a way of increasing its consequentiality and importance. A new way of seeing creates a clear imperative for future studies, even though facts and hypotheses may not reverberate strongly with the work of others. Two of the three articles analyzed emphasize methodological innovations. Page and Jones offer the most pronounced case. They open with one-page review of the literature, followed by an eight-page methodological critique of the literature and a three-page description of the authors' methodological innovations. Less than five pages are devoted to the actual presentation of data and discussion of findings. Of the ten paragraphs of conclusion, nine are devoted to methodological issues and only one to empirical discoveries.

This is a distinctly different function for method discussions than we have seen elsewhere. In the early *Philosophical Transactions* the growth of methods sections served to identify the conditions of the experiment,

establish verisimilitude, and argue for the results. In experimental psychology, we saw the importance of methods sections in protecting the acceptability of results. Compton described methodological innovations (such as the cloud chamber) as a means of obtaining new data. In most cases methodological innovations were not seen as invalidating previous results unless they revealed a serious flaw in prior work, as when Duane challenged Compton over the geometry of the box surrounding the target. More usually the results of prior methods are preserved as valid, although perhaps limited or crude.

Technical Studies and Real World Meanings

In political science, uncertainties over the consequences of findings and methodological propriety lead to an uncertainty over the reality and meaning of results. The specialized technical study seems not able to stand purely on its own terms, as technical discussions alternate with ordinary language accounts of historical cases, hypothetical situations, or traditional political theory. The studies seem to be hanging under the question, "What does this all have to do with the real world?" Even though the data of molecular biology or spectroscopy are much further removed from everyday experience than voting statistics or per capita welfare expenditures, authors in the natural sciences do not seem to need to defend the reality of their data beyond presenting acceptable technical methods for the data production. Jennings, however, begins with a commonsense paraphrase and quotation from Key and in the later statistical passages keeps converting the statistics into historical descriptions. Page and Jones rely on commonsense observations about recent presidential elections to reinforce and interpret their data manipulations, and they let a series of plausible hypothetical statements carry an argument. Finally, the gender role article steps back from a specialized statistical approach to offer general speculations in ordinary language.

The insecurity about the force of a purely technical argument is related to difficulties in identifying just what is being indicated by statistical indicators and what real world behaviors are identified in the nomenclature. Although a vote is an isolatable measurable action, vote decision making is, by its nature, invisible to the outside observer; at the same time we have a wealth of anecdotal, testimonial, historical, and introspective data about the phenomenon. Any model we put forward is a speculation about an internal process that begs for comparison with our experience, knowledge, and intuition on the subject. No matter how

detailed and concrete the data manipulations one can perform, one may suspect that the model does not really reflect the way things are. Even more of an indicator problem arises when you try to connect survey responses to actual behavior. The practical meaning of the terminology in the gender roles article is particularly befuddling. Suggestive psychological terms, although vague and not based on widely accepted theory, are made the basis of rather concrete distinctions. Thus, although the conclusions are intelligible in general terms, some of the specifics about how the conclusions refer to actual political behavior are elusive. Page and Jones directly address a similar issue when they try to identify the factors and relationships in their model of voter decision making:

> We cannot estimate any of the coefficients in Figure 6, as it stands, because the model is hopelessly underidentified. That is, there are only three empirically observable relationships among the central endogenous variables available to estimate the six causal processes of theoretical interest. (1079)

The observable behaviors are not rich enough to tell them about the internal processes they are interested in. Page and Jones then try to define the internal machinery, but they run into further obstacles:

> It is in the search for suitable exogenous variables that difficulties mount, for most of the *pertinent social theory is either not very powerful or not universally accepted.* The grounds for specifying that a given variable theoretically cannot affect or be affected by another are seldom overwhelming. The situation is *worse than usual when one deals with psychological measurements or attitudinal variables,* since practically any attitude might conceivably affect any other. There are times when we seem to be studying *relationships between mush and slush.* (1080, emphases added)

The authors escape from their dilemma only by an eclectic synthesis of plausible factors suggested by the literature, history, and common sense. But no grounding theory or unifying approach make the factors and relationships anything more than assertions. To their credit, Page and Jones recognize their conjecture.

The Authorial Vision

This last case exemplifies the exposure of the author's intellectual processes, typifying an authorial role for political scientists that both resembles and differs from the authorial role of natural scien-

tists. In political science papers, as in natural science papers, the first person frequently is used to express the author's active role in constructing ideas and collecting data as well as to claim credit for the research process and results. For example, Page and Jones use such phrases as, "we intend to specify and estimate," "we first consider," "we conceptualize," "we prefer to analyze," "we measured reactions to candidates' personalities by counting the net number," "in short we are suggesting," "we can with some confidence specify," "to us the most striking aspect," and "perhaps the theoretically most important of all our estimates." The authors of the other two articles also represent themselves as the doers, interpreters, and owners of the research.

Yet in the natural science articles, the results tend to rise above all the separate doings of the authors. As Latour and Woolgar note, the claim seeks to rise above the condition of its begetting. The claims of political science may have the same ambitions of disembodied knowledge, but because of all the problematic conditions discussed earlier, the claims cannot easily rise above the author's perception of the literature, definition of problem, choice of methodology, naming and division of the phenomena investigated, and development of the argument. The author as conceiver, doer, and owner of the claim cannot so easily shift responsibility to nature for the truth of the claim. The authorial stance can be no more than "I have an interesting and revealing way of looking at political behavior and institutions. Look at them my way." Some readers come to share the vision and others do not.

In this way the discourse of political scientists still bears some resemblance to the discourse of political philosophers, who also ask the readers to see it their way, although the philosopher's vision is less constrained by empirical methodology. Rhetorically, political science is somewhere in the middle—whether that middle is part of a historical development or of a permanent dilemma I leave to epistemologists and future historians of knowledge. In the meantime political science needs the resources of both forms of discourse.

The gender roles article suffers from sidestepping its need for traditional discourse and using the stereotype of the scientific paper as a persuasive resource; we can see the rhetorical ambitions from the section divisions—untitled introduction, "Methodology," "Data Analysis," "Findings," "Discussion."

The welfare policies article shows greater concern for the problem of translating the terms of ordinary political discourse into mathematically more solid form, as evidenced by the broader conceptual discussion preceding formulation of hypotheses and by interplay of historical descriptions and statistical indicators. Again the section headings reveal

the stance; although the underlying structure of the paper follows the typical pattern of introductory review of the literature, hypothesis, methodology, data analysis, discussion, and conclusions, the division titles are more discursive: untitled introduction, "Party Competition and Welfare Policy," "State Welfare Policy and the Lower-Class Electorate," "Changes in Politics and Policy in Eight States," "System Differences and Policy Differences," "Electoral Support and Change in Policy," "Further Considerations," and "Conclusions." Thus Jennings preserves the appearance of a commonsense political discussion even as he moves the argument into mathematical terms.

Finally, the study of voter decision making treats the scientific mathematical discourse it relies on as problematic. The underlying structure remains the typical one, but the review of literature is expanded into an extended theoretical methodological discussion. The division titles are then drawn from the methodological problem: untitled introduction, "One-Way Causation: Recursive Models of Voting," "Two-Way Causation: Non-recursive Models of the Vote," and "Conclusions."

Each of the three political science articles discussed employs a strategy to maintain a stable rhetorical base on which to frame statements about real world political behavior and institutions. The articles share some points of strategy, but the overall stances toward the discourse differ. This rhetorical variety suggests that political science has yet to forge a consistent rhetoric. Whether such a consistent rhetoric that addresses all the relevant dynamics of political studies is possible or advisable will only be decided by the collective wisdom of the discipline over time. At the moment, the one certainty is that mandating a rhetoric, borrowed (and reduced) from the practices of a different community does not make the real rhetorical complexity of a community vanish. The ambitions expressed in the transplanted rhetoric only add to the complexity of the rhetorical task. Writing a science of politics may be a worthwhile task, but it is no easy task.