



The Craft of
POLITICAL
RESEARCH

Fifth Edition

W. PHILLIPS SHIVELY

THE CRAFT OF POLITICAL RESEARCH

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TO BARBARA

PREFACE

I wrote this little book in 1970, when I was an assistant professor at Yale University. In teaching a number of sections of “Introduction to Research” to undergraduates there, I had found that the students benefited from an introduction that emphasized the internal logic of research methods and the collective, cooperative nature of the research process. I could not find a book that presented things in this way, however, at a sufficiently elementary level to be readily accessible by undergraduates. And so I wrote this book.

It has followed me through the rest of my career so far, and has given me enormous pleasure. It has always seemed to me that it fills a needed niche, and it has been a thrill when students have told me that they have benefited from it. I am pleased that it still seems to be working for them.

While the general principles of good argument and investigation don't change, I have made a number of additions and deletions over the last couple of editions to reflect new possibilities in technique. Most importantly, I have eliminated several nonparametric measures of relationship, as these have been supplanted over the last decade by advances in least squares analysis. I have also added a very elementary introduction to logit and probit analysis, and have further strengthened my treatment of experimentation, which has been evolving in this book over the years. I have also updated a number of the examples. While I still love Converse's “Of Time and Partisan Stability,” for example, it seemed to me that today's students would find more to respond to in Robert Putnam's *Making Democracy Work*.

As you can no doubt tell from the tone of this preface, this is a book for which I have great affection. I hope you will enjoy it as much as I have enjoyed it!

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W. Phillips Shively

THE CRAFT OF POLITICAL RESEARCH

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DOING RESEARCH

Scholarly research is exciting and is fun to do. Some students, caught in the grind of daily and term assignments, may not see it this way. But for people who can carry on research in a more relaxed way, for professors or for students who can involve themselves in a long-range project, research may be a source of fascination and great satisfaction.

Francis's preoccupation with DNA quickly became full-time. The first afternoon following the discovery that A–T and G–C base pairs had similar shapes, he went back to his thesis measurements, but his effort was ineffectual. Constantly he would pop up from his chair, worriedly look at the cardboard models, fiddle with other combinations, and then, the period of momentary uncertainty over, look satisfied and tell me how important our work was. I enjoyed Francis's words, even though they lacked the casual sense of understatement known to be the correct way to behave in Cambridge. (Watson, 1968, p. 198)*

This is the way James D. Watson describes his and Francis Crick's search for the structure of the DNA molecule. *The Double Helix*, his account of their work, gives a good picture of the excitement of research. It is more gripping than most mystery novels.

Although research can be exciting in this way, the sad fact is that writing papers for courses is too often something of a drag. First of all, course papers are tied to all sorts of rewards and punishments—your future earnings, the approval of others, and so on. All of the anxiety associated with these vulnerabilities comes, indirectly, to lodge on the paper. Yet this is probably the lesser cause for frustration in student research. After all, each of these anxieties also may be present for professional scholars. A more important reason for the student's lack of enthusiasm is the

*Reprinted with permission from Watson, James D. *The Double Helix* (New York: Atheneum, 1968).

simple fact that a paper is generally regarded, by both teacher and student, as a practice run, going through the motions of scholarship. Usually, not enough time is allowed for the student to think long and seriously about the subject, especially with other papers competing for attention. And even when adequate time is allowed, there usually is a feeling on both sides that this is “just a student paper”—that it doesn’t really matter how good it is, that a student will learn from doing the thing wrong. Students must have the chance to learn from their own mistakes, but this attitude toward the work cheats them of the pleasure and excitement that research can bring, of the feeling of creating something that no one ever saw before.

There is probably no way out of this dilemma. In a book such as this, I cannot give you the drama and excitement of original research. I can only give my own testimony, as one for whom research is very exciting. But I can introduce you to some selected problems you should be aware of if you want to do good research yourself or to evaluate the work of others. I also hope to make you aware of what a challenging game it can be, and of how important inventiveness, originality, and boldness are to good research.

SOCIAL RESEARCH

Social research is an attempt by social scientists to develop and sharpen theories that give us a handle on the universe. Reality unrefined by theory is too chaotic for us to absorb. Some people vote and others do not; in some elections there are major shifts, in others there are not; some bills are passed by Congress, others are not; economic development programs succeed in some countries, but fail in others; sometimes war comes, sometimes it does not. To have any hope of understanding why such things happen, to have any hope of controlling what happens, we must simplify our perceptions of reality.

Social scientists carry out this simplification by developing theories. A theory takes a set of similar things that happen—say, the development of party systems in democracies—and finds a common pattern among them that allows us to treat each of these different occurrences as a repeated example of the same thing. Instead of having to think about a large number of disparate happenings, we need only think of a single pattern with some variations.

For example, in his book on political parties, Maurice Duverger was concerned with the question of why some countries develop two-party systems and others develop multiparty systems (1963, pp. 206–280). The initial reality was chaotic; scores of countries were involved, with varying numbers and types of parties present at different times in their histories. Duverger devised the theory that (1) if social conflicts overlap and (2) if the electoral system of the country does not penalize small parties, the country will develop a multiparty system; otherwise, the country will develop a two-party system.

His idea was that where there is more than one sort of political conflict going on simultaneously in a country, and where the groups of people involved in these conflicts overlap, there will be more than two distinct political positions in the

country. For example, a conflict between workers and the middle class might occur at the same time as a conflict between Catholics and non-Catholics. Then, if these groups overlapped so that some of the Catholics were workers and some were middle class, while some of the non-Catholics were workers and some were middle class, there would be four distinct political positions in the country: the Catholic worker position, the non-Catholic worker position, the Catholic middle-class position, and the non-Catholic middle-class position. The appropriate number of parties would then tend to arise, with one party corresponding to each distinct position.

However, Duverger thought that this tendency could be short circuited if the electoral system were set up in such a way as to penalize small parties—by requiring that a candidate have a majority, rather than a plurality, of votes in a district, for instance. This requirement would force some of the distinct groups to compromise their positions and merge into larger parties that would have a better chance of winning elections. Such a process of consolidation logically would culminate in a two-party system. To summarize the theory: A country will develop a two-party system (1) if there are only two distinct political positions in the country, or (2) if despite the presence of more than two distinct political positions, the electoral law forces people of diverse positions to consolidate into two large political parties so as to gain an electoral advantage.

Having formulated this theory, Duverger no longer had to concern himself simultaneously with a great number of idiosyncratic party systems. He needed to think only about a single developmental process, of which all those party systems were examples.

Something is always lost when we simplify reality in this way. By restricting his attention to the number of parties competing in the system, for example, Duverger had to forget about many other potentially interesting things, such as whether any one of the parties was revolutionary, or how many of the parties had any chance of getting a majority of the votes.

Note, too, that Duverger restricted himself in more than just his choice of a theme; in addition, he chose deliberately to play down exceptions to his theory, although these exceptions might have provided interesting additional information. Suppose, for instance, that a country for which his theory had predicted a two-party system developed a multiparty system instead. Why was this so? Duverger might have cast around to find an explanation for the exception to his theory, and that explanation could then have been incorporated into the original theory to produce a larger theory. Instead, when faced with exceptions such as these, he chose to accept them as accidents. It was necessary for him to do this in order to keep the theory simple and to the point. Otherwise, it might have grown as complex as the reality that it sought to simplify.

As you can see, there are costs in setting up a theory. Because the theory simplifies reality for us, it also generally requires that we both narrow the range of reality we look at and oversimplify even the portion of reality that falls within that narrowed range. As theorists, we always have to strike a balance between the simplicity of a

theory and the number of exceptions we are willing to tolerate. We do not really have any choice. Without theories, we are faced with the unreadable chaos of reality.

Actually, what social scientists do in developing theories is not different from what we normally do every day in *perceiving*, or interpreting, our environment. Social scientists merely interpret reality in a more systematic and explicit way. Without theories, students of society are trapped. They are reduced to merely observing events, without comment. Imagine a physicist—or a fruit picker for that matter—operating in the absence of theory. All she could do if she saw an apple falling from a tree would be to duck, and she would not even know which way to move.

Social theory, then, is the sum total of all those theories developed by social scientists to explain human behavior. Political theory, a subset of social theory, consists of all theories that have been developed to explain *political* behavior.

Types of Political Research

The way a particular political scientist conducts research will depend both on the uses that the political scientist visualizes for the project and on the way evidence is marshaled. Research may be classified according to these two criteria.

The two main ways by which to distinguish one piece of research from another are:

1. Research may be directed toward providing the answer to a particular problem, or it may be carried on largely for its own sake, to add to our general understanding of politics. This distinction, based on the *uses for which research is designed*, may be thought of as applied versus basic research.
2. Research may also be intended primarily to discover new facts, or it may be intended to provide new ways of looking at old facts. Thus, political research can be characterized by the *extent to which it seeks to provide new factual information* (empirical versus nonempirical).

A glance at Table 1-1 shows us the four types of political research based on different combinations of these two dimensions. *Normative philosophy* consists of argument about *what should be* in politics. Probably the oldest form of political research, it includes among its practitioners Plato, Karl Marx, Ayn Rand, John Kenneth Galbraith, William F. Buckley, and others. It is *applied research*; that is, its goal is problem solving. This means that it is not intended so much to develop polit-

TABLE 1-1 Types of Political Research

	Applied	Recreational
Nonempirical	Normative philosophy	Formal theory
Empirical	Engineering research	Theory-oriented research

ical theory as to use what political theory tells us about society and politics as a basis for making political decisions. It is also nonempirical in that it does not consist primarily of investigating matters of fact. It typically takes certain political facts as given and combines them with moral arguments to prescribe political action. A good example is John Stuart Mill's argument in "Considerations on Representative Government," in which he urges the adoption of democratic representative government because (1) the chief end of government should be to facilitate the development in each citizen of his full potential (moral argument), and (2) democratic government, by giving the people responsibility, will do this (factual assumption).

Like normative philosophy, *engineering research* is geared to solving problems. However, its stance is empirical; it is concerned with ascertaining the facts needed to solve political problems. Some examples would be measuring the effects of various reapportionment methods, trying to design a diplomatic strategy to effect disarmament procedures, and designing methods of riot control.

These two forms of applied research exist in some estrangement from academic political science. Political engineering is a thriving industry and many courses relevant to it are taught in political science departments, but research in it is often relegated to a separate institute or "school of public policy." Normative philosophy is taught extensively, and research is carried on under that name, but generally this means the *history* of normative philosophy and its development, not the active formulation of normative arguments. For both forms of applied research, we must look largely outside academic life to such sources as the RAND Corporation and the *New York Review of Books*.

At the other end of the continuum from applied research is recreational research. It is usually called "pure" or "basic" research, but this carries the unpleasant implication that applied research is either impure or of limited value. The choice of the term "recreational" to describe this type of research is really not as flip-pant as it might seem, for this is research carried on for its own sake, to improve political theory. Political scientists pursue this type of research for the twin pleasures of exercising their minds and increasing their understanding of things. In a high sense of the word, it is "recreation."

Formal theory, largely a post-World War II phenomenon, is the most recently introduced form of political research. Like normative philosophers, formal theorists posit certain facts about politics. Their concern is to take these posited facts, or assumptions, and derive theories from them. Their end goal is to develop reasonably broad and general theories based on a small number of agreed-upon assumptions. To accomplish this, they work with precise, usually mathematical statements of their assumptions.

A good example of formal theory—indeed, a work by which many would date the emergence of formal theory as a distinct field in political science—is Anthony Downs's *An Economic Theory of Democracy* (1957). Downs builds a wide ranging theory from a set of assumptions that include, for example: (1) voters and parties behave rationally; (2) political conflict occurs on only one issue at a time; and (3) political events are not perfectly predictable. Some of the predictions generated from

his theory are (1) in a two-party system, parties will tend to agree very closely on issues, whereas in a multiparty system, they will not; (2) it may be rational for the voter to remain uninformed; and (3) democratic governments tend to redistribute income. (Of course, one must recognize that excerpts such as these do even more than the usual violence to a rich net of theories.) It is important to emphasize that this sort of work is almost solely an exercise in deduction. All of the conclusions derive directly from a limited set of explicit assumptions. Downs's purpose in this is simply to see where the assumptions he started with will lead him. Presumably, if the assumptions produced an untenable result, he would go back and reexamine them.

The main use of formal theory, as in the example above, is explanation; the formal theory is used to construct a set of conditions from which the thing we wish to explain would have logically flowed. Such explanatory formal theories are then often tested empirically through theory-oriented research. But because formal theory consists of taking a set of assumptions and working out where they lead—that is, what they logically imply—it is also useful for developing and analyzing strategies for political action. That is, we can use formal theory to construct analyses of the form: If we want to achieve *X*, can we devise a set of reasonably true assumptions and an action which, in the context of those assumptions, will logically lead to *X*? Formal theory is used in this way, for example, to argue for various ways to set up elections; or for various ways to arrange taxes so as to get the outcomes we want. Flat-tax proposals are a good example: they originated in argument of the following form: (a) If we want to maximize investment and economic growth, and (b) if we assume that governmental investment is inefficient and that individual taxpayers act so as to maximize their income, then (c) can we deduce what sort of taxes in the context of the assumptions of (b) would best achieve (a)?

Like normative philosophy, formal theory interacts with empirical research. Formal theorists usually try to start with assumptions that are in accord with existing knowledge about politics, and at the end they may compare their final models with this body of knowledge. But they are not themselves concerned with turning up new factual information.

Good work in formal theory will take a set of seemingly reasonable assumptions and will show by logical deduction that those assumptions lead inescapably to conclusions that surprise the reader. The reader must then either accept the surprising conclusion or reexamine the assumptions that had seemed plausible. Thus, formal theory provides insights by logical argument, not by a direct examination of political facts.

Following from Downs, a great deal of formal theory in political science has based itself on the economists' core assumption of *rational choice*: the assumption that individuals choose their actions in order to maximize some valued object, and minimize the cost expended in achieving it. (In economics the valued object is generally taken to be money; in political science it may be money—as in theories of why and how communities seek pork-barrel spending—but theories may also posit that the valued object is a nonmonetary policy such as abortion, or political power itself. Sometimes the object may even be left unspecified in the theory.)