

Contemplating Courts

Lee Epstein, Editor

Washington University in St. Louis



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1 Studying Law and Courts

Lee Epstein

In the 1963 case of *School District of Abington Township v. Schempp*, the U.S. Supreme Court ruled that the First Amendment to the Constitution prohibits prayer and Bible-reading exercises in public school. This ruling was immensely controversial. Many public schools throughout the United States—but especially in the South—had historically engaged in some form of prayer in school. The public outcry against *Schempp* was so great that members of Congress immediately introduced nearly 150 proposals to overturn the decision.

Given the hostile political environment surrounding the Court's pronouncement, a natural question arises: Did school districts comply with *Schempp*? In his textbook *The Supreme Court*, the political scientist Lawrence Baum (1992b, 214) provides one answer when he writes that “[w]ithin three years . . . the frequency of school prayer and Bible readings had declined by about one-half; . . . this decline is noteworthy. But equally striking is the continuation in so many schools of practices that the Supreme Court had declared to be unconstitutional.”

Although Baum's statement provides useful information about public response to *Schempp*, it points to an interesting question: How does Baum know that only about one-half of all schools complied with the Court's rulings? To put the question more generally, from where do the authors of textbooks obtain the facts and figures that populate their works? The answer to these questions is quite simple. Usually textbook writers rely on studies conducted by other researchers working in their field. In making the claim about the effect of the school prayer decisions, for example, Baum cited a study by H. Frank Way, Jr. (1968), who surveyed school districts and found that compliance was occurring in only about half.

From this perspective we can begin to understand the importance of research. The most obvious benefit is the useful information that scholarly studies provide to society. The

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results of Way's survey, for instance, highlight the Supreme Court's lack of power to enforce its decisions; it may be unable to generate compliance with its rulings if we—the people—reject them. Somewhat less obvious, though, is the role played by research and how it influences what instructors teach and what students learn. It was Way's study of prayer in school that enabled Baum to write about compliance with the Court's decision. And it is Baum's textbook, not necessarily Way's study, that instructors assign to their students to read.

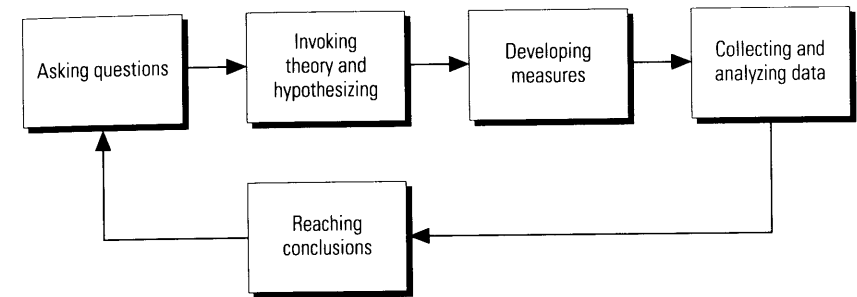
This point—that an intimate link exists between research and teaching—is often obscured in debates that pit one against the other. We have all heard the voices of the commentators who complain that professors spend too much time conducting research and too little teaching students. Without denying the validity of these claims, I would say that at the very least they set up a false and unfair dichotomy. As the Way-Baum connection illustrates, much of what scholars teach and students learn comes from research. The converse is true as well. Students, through their questions, often generate research ideas. My own experience bears this out. While I was teaching a course on defendants' rights, a student asked me a penetrating question about the death penalty. When I found I could not supply a definitive answer, I asked one of my colleagues the question. He, in turn, suggested that we conduct some research to derive a solution. We did, and a book that I now use in that very class (Epstein and Kobyłka 1992) resulted.¹

As readers can probably tell, I have strong feelings about the relation between teaching and research, feelings that occasionally run counter to fashionable commentary. The purpose of this book, however, is not to provide an abstract response to those who set teaching against research. Rather, by example it lends support to their interrelatedness. In each chapter, authors raise significant substantive questions and address them through appropriate analytic research strategies. One of the goals of the book is to bring the research process to students and instructors *directly*, not by the usual route of researcher to textbook writer to student (for example, Way to Baum to student).

A second and related objective is to give students some sense of the mechanics behind conducting research. Undergraduates, in particular, often see their professors furiously typing something into a computer or sifting through various collections in the library. But they have little idea about what they are up to or about the research process.

The contributors to this book provide a window to this process by exploring specific topics related to law and courts. Jeffrey A. Segal, Donald R. Songer, and Charles M. Cameron, for example, consider the relation between lower and upper appellate courts; Richard Pacelle examines the marked change in the Supreme Court's agenda; and so on. In what follows, I take a more general course. I outline the research process, leaving the details up to the essayists to fill in.

Figure 1-1 The Research Process



Sources: Bernstein and Dyer 1984, 3; Williamson et al., 1982, 7.

An Outline of the Research Process

The social-scientific research process is not a monolith; instead, it reflects the diversity of theoretical and substantive interests of the various disciplines. Consider the contributions to this volume. All the authors are concerned with issues relating to law and courts, but a mere glance at their essays attests to the pluralism of approaches. For example, although the contributions of Harold J. Spaeth and Leslie Friedman Goldstein both concentrate on judicial decision making, they ask very different questions. Spaeth considers how judges decide cases; Goldstein asks how they *should* decide cases. The subtle but crucial difference in the wording of their questions gives rise to divergent research strategies: Spaeth takes a data-intense approach, and Goldstein takes a more contextual one.

For all the differences, however, patterns exist. Virtually all research on law and courts—and, for that matter, on political and social phenomena more generally—shares some common features. Studies tend to (1) focus on specific research questions, (2) invoke theories to provide initial answers (or predictions), (3) develop ways (or measures) to turn predictions into testable hypotheses, and (4) examine the hypotheses against observable facts to reach some conclusions. Figure 1-1 provides a visual description of this process; below, I consider each of these features, with emphasis on their relation to legal social scientific scholarship.²

Asking the Question

Almost all research starts with a basic question or set of questions to which the scholar wants to learn the answer. In the field of law and courts, questions can be as broad in scope as, What factors generate crime in the United States? to ones more narrowly focused, such as, Does the death penalty serve to deter murder? They can center on aggregates (Why does the Supreme Court vote the way it does?) or individuals (Why does Chief Justice Rehn-

quist vote the way he does?). And they may be focused on events occurring at a specific point in time (What explains the senators' votes in the confirmation proceedings for Robert Bork in 1987?) or over time (What explains senators' votes in confirmation proceedings between 1900 and 1994?).

How do scholars select their research questions? An important factor is interest. Scholars, like all people, are interested in some topics more than others. And since research questions are a way to narrow a topic, it is not too surprising that scholars focus on things that interest them. Suppose I am intrigued by the relationship between international crises and judicial decision making in cases concerning civil liberties. If that is my interest, I might then narrow my question to something along these lines: Are jurists more likely to repress rights and liberties during times of war?

Another, related factor is curiosity. Many things can lead scholars to become curious about particular phenomenon. When my student asked me the question about the death penalty that I could not answer, I became curious—almost insatiably so—about the appropriate response. Curiosity can also emerge from discrepancies between what scholars think they know and what they observe. To return to the issue of war and liberties, I thought I knew the answer to the question posed above: according to research by Jeffrey A. Segal and Harold J. Spaeth (1993), external events, such as wars and international crises, should not influence judicial rulings unless the litigation itself deals with such events. That is because justices base their decisions on the facts of cases in light of their own ideological attitudes and values. They are “single-minded seekers of legal policy” whose ideology dictates their votes. Or, as Segal and Spaeth (1993, 65) put it, “Rehnquist votes the way he does because he is extremely conservative; Marshall voted the way he did because he is extremely liberal.” Thus, this view—what Segal and Spaeth label an *attitudinal* approach—leaves no room for external events, such as wars, that are not explicitly part of the facts of the case; in general, Rehnquist will take the conservative position and Marshall will take the liberal one, regardless of whether or not a war is occurring.³

Although the logic invoked by Segal and Spaeth seems to make sense, I became troubled by the presence of certain “facts” that do not sit comfortably with their explanation. For one thing, the justices have implied that they, like “all citizens . . . both in and out of uniform, feel the impact of war in greater or lesser measure” (*Korematsu v. United States* 1944). According to some scholars (for example, Emerson 1970), this kind of statement is suggestive. It implies that the Court thinks of cases—even those unrelated to the war effort—differently during times of war and peace. There is yet another piece of the puzzle that did not add up to me. That is, analysts (for example, Dahl 1957) have long argued that as part of the ruling regime, the Court usually upholds the interests of the majority, a phenomenon that may be even more attenuated during times of crisis or when national security interests are at stake. Under this view, the initiation of crisis conditions jolts the

Court into the necessity of being more sensitive to the interests and wishes of the public and other government institutions. The discrepancy between the explanation of Segal and Spaeth and these “facts” only heightened my curiosity; I became genuinely puzzled.

Theorizing about Possible Answers

Once scholars hit upon questions they find interesting or about which they are curious, the next step is to think about possible answers, which can be used to generate predictions or expectations. Where do scholars find these potential answers?

Sometimes they discover them in the scholarly literature; they scrutinize the results of past research and apply them to their problem. That is why most academic publications, including many of the chapters in this book, include reviews of the relevant literature. For example, in thinking about decision making during times of war, I would want to know if any other scholar had attempted to answer precisely the same question. And, in fact, a search I conducted of journals and books turned up several studies directly on point. *The System of Freedom of Expression*, by Emerson (1970), for one, provides a descriptive analysis of cases decided before and after periods of threat to the nation's external security. Emerson finds that the Court is more likely to repress rights and liberties when the United States is at war, even in cases unrelated to the particular international crisis.

Emerson's work provides me with valuable leverage on my *specific* research problem. And I certainly would mention the study in my literature review. But since social scientists strive to produce *general* explanations, more often than not they seek broader answers to their research questions. That is why we turn to “theories.” In the context of social science research, theories are sets of “principles” that provide us insight into why actors behave the way they do (see Williamson et al. 1982). Scholars often use these insights to develop crisp expectations about the relations that intrigue them.

By way of illustration, reconsider the object of my interest: the relation between war and judicial decision making. How might I use theory to inform my research? I would start by identifying appropriate theories, those that speak to the issue of political decision making. Then, I would use those theories to generate expectations about the relationship between war and judicial decisions in civil liberties cases. For my research, I would have no trouble in finding such theories, since scholars in political science have paid a good deal of attention to the subject of decision making. We already know about one such theory—the attitudinal approach offered by Segal and Spaeth—and the expectations about the relationship between war and judicial decision making that it would generate (that is, there would be *no* relation independent of the facts of the case). But there are others. Many *rational choice* theories of judicial decision making, for example, begin with the same assumption as Segal and Spaeth—that justices are goal-directed, single-minded seekers of legal policy (Eskridge 1991a and 1991b). But, unlike Segal and Spaeth, choice theories of

judicial decisions emphasize that these goal-directed actors operate in strategic or independent decision-making context: the justices know that their “fates” depend on the preferences of other actors—such as Congress, the president, and their colleagues—and choices they expect these other actors to make, not just on their own actions (Ordeshook 1992, chap. 1).⁴ This notion of interdependent choice is important for the following reason. If justices really are single-minded seekers of legal policy, then they necessarily care about the “law,” broadly defined. And if they care about the ultimate state of the law—about generating policy that other institutions will not overturn—then they must act strategically, taking into account the preferences of others and the actions they expect others to take. Occasionally, such calculations will lead them to act in a sophisticated fashion (that is, in a way that does not reflect their sincere or true preferences) so as to avoid the possibility of seeing their most preferred policy rejected by their colleagues in favor of their least preferred one, of Congress replacing their preference with its own, of political noncompliance, and so forth (Murphy 1964; Rodriguez 1994).

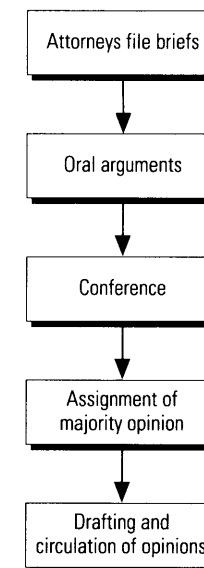
If we adopt this rational choice theory, then we would obtain a prediction about the relationship between war and judicial decision making very different from the one generated by Segal and Spaeth. Suppose Justice X were to select among three possible outcomes in an ordinary criminal case, unrelated to any war effort. Further suppose that Justice X preferred outcome 1 to 2 and outcome 2 to 3. In such a case, we would posit that the attitudinal Justice X would always choose outcome 1, regardless of whether or not America was fighting a war, while the strategic Justice X might choose outcome 2 if—depending on the political context (for example, a time of war), the preferences of the political actors involved (for example, Congress), and the actions those actors are expected to take—that would allow her to avoid outcome 3. In other words, while Segal and Spaeth’s attitudinal theory posits that no relation will exist between judicial decisions and wars (unless, of course, the war is part of the case facts), the rational choice view of the Court suggests that a relationship could emerge, depending on the preferences of the key political actors and the actions they are expected to take.

In the pages that follow, students will see how other researchers invoke various theories to guide their research, to help them to formulate expectations about the relations under investigation. In many of these chapters, however, students will find that essayists use the term *model* instead of *theory*. Indeed, in his essay, Spaeth refers to the attitudinal approach as a model, not a theory. What are the differences between these two terms?⁵

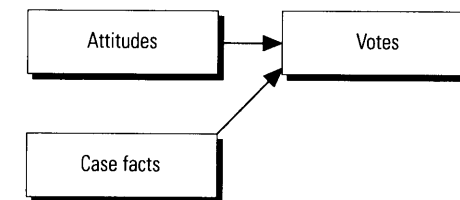
The answer to this question, as Figure 1-2 suggests, depends on the nature of the model. As we can see, the two parts of Figure 1-2 are models in the sense that they are “visual depiction[s] of how something works” (Williamson et al. 1982, 20); yet, they provide us with very different kinds of information. The first part of the Figure *describes* how cases proceed through the U.S. Supreme Court, but it does not explain much about that process.

Figure 1-2 Descriptive and Explanatory Models

Descriptive Model: The Processing of Supreme Court Cases Placed on the Plenary Docket



Explanatory Model: The Attitudinal Model of Judicial Decision Making



From this picture we could not, for example, say why the Court affirms some cases and reverses others; all we learn from the diagram is that the Court makes decisions on the merits of cases after it hears oral arguments, holds conferences, and so forth. The attitudinal model depicted in the second part of Figure 1-2 is quite different. It shows that a justice’s decision in a given case is influenced by the facts of that case in relation to the justice’s ideology. To put it differently, attitudinalists assert that two factors—case facts and ideology—*explain* judicial decisions.

Seen in this way, the first part of Figure 1-2 is a descriptive model, and the second part is an explanatory one. Undeniably, both are useful for scholars conducting research on law and courts. But descriptive models can only help us to build theory, whereas explanatory models possess all of the attributes of theory. Hence, for our purposes, when models—such as Segal and Spaeth’s attitudinal model—seek to explain some phenomenon, *models*

and *theories* are synonymous. It is only those models that seek to describe phenomenon that are wholly distinct from theory.

The chapters to come serve to reinforce the significance of theories and models (from this point on, I use the terms *theories* and *models* interchangeably with the understanding that I am talking about explanatory models).⁶ The essays also serve to shore up an important point: however useful theories and models may be, they have their limits. In particular, students should keep in mind that they are simplified versions of reality, much like an airplane model is a simplified version of a real airplane (Schrodtt 1991, 294). Just as an expertly constructed model airplane may look—though not act—exactly like the real thing, models in social science are not meant to constitute reality. To the contrary, they are purposefully designed to ignore certain aspects of the real world and focus instead on a crucial set of explanatory factors. In seeking to explain the relation between war and judicial decision making, for example, I might be able to list dozens of reasons why a given justice in a given case during a given war would, say, repress rights and liberties: the justice served in the military, the justice admired the president who was serving as commander in chief, and so forth. But suppose I could identify one major underlying explanation, such as the one proposed by the attitudinal model, and I could deploy that model to show that 80 percent of all judicial votes cast during all wars conducted during our nation’s history could be explained by the case facts and political ideology. If this was the case, then the attitudinal model would provide us with an extraordinarily useful tool for explaining and understanding not just one justice’s vote but most votes cast by most justices.

This example reflects both the disadvantages and advantages of theory. When we try to make generalizations about a phenomenon—say, judicial decision making—we lose the specifics that are part of reality (those specifics may form the 20 percent of the votes left unexplained by ideology and case facts in my illustration above). Yet, the simplifications inherent in models provide social scientists with useful handles for understanding the real world and for reaching general conclusions about the way that world works.

Operationally Defining Concepts

As I noted earlier, theories and models also provide us with ways to develop expectations about the relation we are seeking to explain. If I were to invoke the attitudinal model to explore the relation between judicial decision making in civil liberties cases and wars, I would expect to find that such external events—unless they were part of the case facts—had no effect on judicial voting; justices would simply vote their attitudes. In contrast, if I were to adopt a rational choice approach, I would predict that wars may have some effect on the vote, depending on the preferences of other actors, such as Congress and the president.

We often refer to these kinds of predictions—ones that our models and theories gener-

ate—as hypotheses. Hypotheses are claims about the relationships between variables. By *variables*, I mean “observable characteristics” of some phenomenon that, as the name suggests, vary. Sex is a variable: it can take on one of two values, male or female. We can also think of judicial decisions as a variable. Depending on the nature of the study, for example, decisions can take on the value of liberal or conservative, important or unimportant to the public, repressive or tolerant, and so forth. In social science research, we often talk about two kinds of variables, *independent* and *dependent* variables. *Dependent* variables are the things we are trying to explain; *independent* variables are those properties that we think cause the variation in our dependent variables. Hence, in my example about the relationship between war and judicial decision making, the political environment (whether we are engaged in a war or not) would constitute my independent variable, and judicial outcomes (whether the Court decided for or against the civil liberties claim), the dependent variable.

But hypotheses do more than state the variables under examination; they also posit a relationship between the dependent and independent variables. To return to my example, the attitudinal model would lead to the following hypothesis: judicial outcomes (the dependent variable) will not necessarily be any more or less supportive of civil liberties during times of war than in times of peace; only attitudes and case facts will determine outcomes. The strategic component of rational choice theories would suggest a very different kind of prediction: even after we consider (or control for) case facts and attitudes, judicial outcomes will be less supportive of civil liberties during times of war than during times of peace if that is the preference of other key political actors, and if the justices expect them to act on their preference (that is, override decisions to the contrary).

Making predictions or hypothesizing about, in my case, judicial decisions is one thing; determining the accuracy of those predictions is quite another. Indeed, in systematic sociolegal research, as in all social science work, scholars face particularly acute problems in testing their predictions. One of the most severe problems concerns measurement. Think about it this way. Suppose I was a chemist who wanted to determine the relation between water and temperature. Theory leads me to the prediction that when water freezes it turns to ice. Measurement is no obstacle: we can simply define *water* as H₂O and *freezing* as 32 degrees Fahrenheit. These are “operational” definitions on which all chemists and even schoolchildren would agree. Now reconsider the topic of judicial decision making in civil liberties cases during times of war. Unlike chemists, who enjoy consensus over the definitions of terms like *water*, legal scholars do not necessarily agree on what constitutes “civil liberties” cases. Should we define them strictly with regard to the First Amendment or should we consider cases involving criminal justice and equality, as well? What about wars? Are “wars” only those declared by Congress, or do they include long-term, but undeclared, military efforts, such as those that took place in Korea and Vietnam?

Because reasonable people might disagree over how to define operationally the concepts

of civil liberties or wars, these are decisions I, as the researcher, must make. Making choices about appropriate measures, thus, constitutes part of the research process. But the decisions scholars make are not unfettered; in fact, their colleagues will judge them by two criteria, *reliability* and *validity*. A *reliable* measure “is one that can be used over and over again with comparable results,” regardless of who does the measuring (Williamson et al. 1982, 70). Say I classified President Reagan’s air strike on Libya as a war, but another researcher, *using my measurement procedure*, did not classify the same air strike as a war. That would provide some evidence that my measure was unreliable and, hence, should not be used.⁷

Why are some measures unreliable? The most common reason is that they are vague. As Bernstein and Dyer (1984, 60) note, “All too often in the political science literature, measurement procedures are not described or are only vaguely noted. . . . Fellow scientists cannot tell how the original researcher obtained his results, nor can they replicate (repeat) the work to check its accuracy.” To put it another way, if I classified the Libyan air strike as a war and the next researchers, using my procedures, did not, the problem is mine, not theirs. I failed to describe adequately my operational definition or measure of war. To prevent this problem, I would be best off to use a crisp definition, such as, “Wars are those military efforts that have been declared by Congress.” Since Congress never declared war against Libya, there would be no reason why I or the researchers seeking to replicate my work would classify Reagan’s air strike as a war.

In addition to reliability, scholars look for measures that are *valid*. A measure is valid if it “measures a property as it was defined” (Bernstein and Dyer 1984, 61). Operationally defining war by the criteria of congressional declaration is a valid measure of war: few would disagree that a war is occurring when Congress declares one.⁸ But suppose I define a state of war as the condition that exists each day on which 500 or more people die in the United States. This would not be a valid measure of war because U.S. deaths come about from many factors—murders, heart attacks, suicides—and not necessarily from wars. If I defined *war* in this way, then, other researchers could attack my work—and rightfully so—on the ground that I would be testing the Court’s response to deaths, not wars.

When reading the chapters to come or, for that matter, any social scientific research, keep these points in mind. It is not enough for researchers to ask interesting questions and to invoke appropriate theories; they must also develop measures that reliably and validly tap the concepts that make up the posited relationships.

Testing Expectations against Data

The next stage in the research process is one of the most delicate: it involves amassing evidence to explore expectations generated by models and then analyzing that evidence.⁹ As students will see, virtually all the chapters in this volume have such an *empirical* compo-

nent, meaning that the writers seek to evaluate their hypotheses against observations in the real world. But there is great variety in the kinds of evidence they bring to bear on their research questions and in the types of analyses they invoke.

To clarify these points, let us return to the question of war and judicial decision making. Suppose I wanted to test the attitudinal model’s prediction that only facts and ideology—and not external events on issues unrelated to the case, such as wars—affect the votes of the justices. What kinds of evidence or data could I collect? And how might I analyze that evidence? One approach would be to examine contextually specific Court decisions. For example, I could select a few cases that occurred, say, directly before the onset of World War II and a few cases decided during the war in which the same set of justices participated. Then, I might closely read the opinions in those cases to determine whether the war had any effect. If I followed this approach, my test would take the form of a descriptive comparison of the opinions handed down before and during the war.

Another approach would be to examine Court outcomes systematically. For example, on the basis of some clear criteria, I could classify the outcome of every Court decision handed down, say, during the past sixty years, as pro-liberties or anti-liberties. These outcomes, thus, would constitute my dependent variable, or the property I was trying to explain. I would also note certain attributes of the outcomes I was classifying, such as whether or not the cases occurred during a time of war; whether they involved religion, free speech, or free press; whether the Court was a liberal or conservative one; whether the case facts centered on a war-related issue; and so forth. These attributes are my independent variables, or those properties that I think cause variation in my dependent variable. Naturally, the independent variables emanate from the model under analysis. For example, to explore (that is, test) the attitudinal model’s prediction that only the facts of the case and the ideology of the Court affect judicial decisions, I must have knowledge of those independent variables—case facts and court ideology.

This last step—testing the model—I might accomplish by invoking a statistical procedure—one that would allow me to determine whether war had any effect on Court outcomes after I considered (or controlled for) those variables that the attitudinal model considers critical: Court ideology and case facts. In other words, I would want to employ statistical tools to probe the relation between judicial outcomes and the political environment (times of war or not), ideology, and case facts. Such statistical tools come in many forms, but all center on an important concept in social science research: *probability*. Scholars use this term in much the same way as laypersons, to refer to the likelihood that something will occur. But to scholars, probability means more than simply *guessing* about whether something will or will not happen. It is often used to establish—with some degree of precision—“the likelihood of a given set of data emerging by chance” (Hoover 1988, 98). To see this usage of probability, think about jurors examining blood on a murder

weapon. Suppose a medical expert told them that the chance of a random relationship between the blood found on the weapon and the defendant's blood was only one in a hundred. The jurors would surely feel a good deal more confident in making the inference that the defendant wielded the murder weapon than if the expert had told them that there were fifty chances out of a hundred. The same is true of social scientists. Quite typically, we consider data important at a *level of significance* of .05 or less (represented in tables throughout this book as $p \leq .05$), which means that there are only—at maximum—five chances in a hundred that the relation would occur by chance. To put it another way, we want to believe that there is a 95 percent chance or better that our dependent variable is related to our independent variable (or, conversely, that there is a 5 percent chance or less that the independent variable is unrelated to the dependent variable). Statistics allow us to make these kinds of claims.

In the appendixes to this book, David Nixon and I provide information on the statistical approaches commonly used in the field of law and courts (Appendixes A and B) and on where students can find data used in the field (Appendix C). For now it is enough to note that the two research strategies outlined above—the contextual and the systematic—are quite distinct, with each having its assets and deficits. On the one hand, the detailed analysis of case opinions does not necessarily lend itself well to generalizations. In other words, after conducting this kind of analysis, I might be able to make some claims about the attitudinal model's ability to account for certain judicial decisions during certain periods of time. But those claims would be limited in time and scope and would be largely descriptive in nature. On the other hand, the more systematic examination of case outcomes would provide me with little information about the policies enunciated by the Court and about the ways in which the justices themselves constructed and interpreted the facts and law in cases. In the end, I would be able to explain something about judicial decision making and, perhaps, reach some generalizations. But I would miss nuances in that process; nor would I capture specific statements on the part of the Court that might have some bearing on my theory.

What is more (and what is probably obvious by now), different approaches can occasionally lead to different conclusions. To return to my research interest, it is quite possible that an in-depth investigation of Court opinions would lead me to question the attitudinal model, while my systematic classification of Court outcomes would lead me to accept its premises. Or vice versa. The general point is that students should take note of the approaches used by authors and realize that their results may be dependent on the kinds of evidence they amass and the analytic tools they invoke. To see why this is so, students could conjure up different ways to conduct the very same research and explore their ideas with the kinds of data identified in Appendix C.

Discussion

In the final analysis, then, the research process confronts the scholar with a series of choices: On what question should I focus? What model should I invoke? How should I go about measuring key concepts? What kinds of evidence should I amass and how should I examine that evidence? To these questions, as we have seen, there are no necessarily “right” answers; only possibilities.

That, though, is what makes the social scientific research process both challenging and, at times, frustrating. As scholars, we know that it is very difficult to get at the “truth,” at the answers to the questions we seek to address. Yet, we also know that the results of our work are important, for they can structure the way society views its political institutions and the way in which students—our future scholars, teachers, and leaders—think about political phenomena. So, as the essays in this volume reveal, we trudge on, using the research process in the hope that we will uncover the truth of whatever it is we study.

Notes

1. The question had to do with U.S. Supreme Court decisions in two death penalty cases, *Furman v. Georgia* (1972) and *Gregg v. Georgia* (1976). In *Furman*, five justices struck down as unconstitutional the procedure used by most states for imposing capital punishment; but four years later (in *Gregg*) seven justices reasserted the constitutionality of the death penalty, upholding a newly devised schema for its execution. The question the student asked was, Why did the law experience such an abrupt change in only a few short years? At the time, I didn't have a particularly good answer. In *The Supreme Court and Legal Change* (1992), Joseph F. Kobylka and I try to provide one.

2. My goal here is to point out some of the general thinking behind the research process, rather than provide an in-depth perspective. Interested students might consult Hoover (1988), which treats the logic of the scientific method lucidly. Manheim and Rich (1991) and Babbie (1992) are excellent textbooks on the subject.

3. This point deserves some clarification. Suppose the Supreme Court heard two criminal cases during World War II, one involving a criminal conviction for treason against the U.S. government and the other an ordinary burglary case, unrelated to the war effort. According to attitudinalists, the existence of a war would be relevant to the Court's decision in the first case because it would be part of the facts; but it would not influence the vote in the second example.

4. In rational choice models of judicial decisions, thus, it is not enough to say, as the attitudinal model does, that Justice X chose action 1 over 2 because she preferred 1 to 2. Rather, the strategic assumption suggests the following proposition: Justice X chose 1 because X believed that the other relevant actors—perhaps Justice Y or Senator Z—would choose 2, 3, or another action, and given these choices, action 1 led to a better outcome for Justice X than did other alternative actions (see Ordeshook 1992, 8).

5. I adapt this discussion from Williamson et al. 1982, 20-21.

6. I thank Jeffrey A. Segal for his help in writing this paragraph.

7. I adapt this example from Bernstein and Dyer 1984, 60.

8. Still, we might question the validity of this measure because it would exclude military efforts, such as those in Korea and Vietnam, that many analysts consider wars.

9. The logic in this paragraph comes from Manheim and Rich 1991, 28.