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**Supreme**  
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attitudinal  
model

*Editor's Note: At the 1994 meeting of the American Political Science Association Susan E. Lawrence chaired a roundtable, "Authors Meet Critics: The Supreme Court and the Attitudinal Model" (Cambridge University Press, 1993) by Jeffrey A. Segal and Harold J. Spaeth. Susan edited a symposium for this issue of **Law and Courts**, containing revised versions of the remarks of the critics (Lawrence Baum, Jack Knight, Gerald N. Rosenberg, and Rogers M. Smith) and of the responses of the authors (Jeffrey A. Segal and Harold J. Spaeth).*

### **Introduction to the Symposium**

Susan E. Lawrence, *Rutgers University*

In 1948, C. Herman Pritchett fundamentally changed the field of public law by moving us beyond a sterile brand of doctrinal analysis of Court opinions toward deeper and more systematic attempts to predict and explain how and why the Justices—a set of political actors operating under a unique set of institutional constraints—decide as they do. Segal and Spaeth's *The Supreme Court and the Attitudinal Model* is a mature fruit of this paradigm shift.

The validity and usefulness of attempts to explain judicial decision making through quantitative analysis of the relationship between judicial attitudes and judicial votes has been debated, redebated, and debated again for over 35 years now. On the one hand, this persistent debating of the attitudinal model seems foolish in that it obscures the extent to which the attitudinal model's systematic, empirical shattering of the myth of mechanical jurisprudence permeates virtually all of our work on judges and courts today. On the other hand, precisely because the

truths revealed by the attitudinal model do permeate our work, serving as an underlying premise in almost all of our scholarship today, I believe that it is important for us to occasionally return to the model itself and examine it closely.

Segal and Spaeth's *The Supreme Court and the Attitudinal Model* is the first full-blown, thorough, up-to-date treatment of the attitudinal model of Supreme Court decision making that the field has seen in quite some time. As such, it provides an important resource service to the field and serves as a catalyst for a reexamination of the attitudinal model itself in light of contemporary scholarship across the field. Such a reexamination took place at the 1993 meetings of the American Political Science Association in the form of a roundtable on Segal and Spaeth's *The Supreme Court and the Attitudinal Model*. What follows are the panelists' edited and condensed versions of their remarks and Segal and Spaeth's reply.

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### **The Critics**

Lawrence Baum, *Ohio State University*

*The Supreme Court and the Attitudinal Model* is an important book, one that is essential for students of judicial behavior to read and to grapple with. The book raises a great many matters that merit discussion, but in these comments I will focus on one issue: what the evidence presented and cited by the book actually establishes about the determinants of Supreme Court behavior.

Jeffrey Segal and Harold Spaeth argue that the policy preferences of Supreme Court justices constitute close to a full explanation of the Court's decisions. Consistent with that position, they also argue that legal considerations—efforts to interpret the law accurately and well—play essentially no role in the Court's decisions. They present

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*The Supreme Court and the Attitudinal Model (continued)*

the logical premises on which this argument is based and then offer a variety of evidence to support it. Restricting my discussion to votes on the merits of cases, the heart of the book's concerns, I will consider the evidentiary basis for its argument—first, the evidence for the importance of policy preferences, and then, the evidence against the importance of other considerations in decisions.

Segal and Spaeth muster two types of evidence to show the primacy of policy preferences. One is a pattern in which votes within particular issue domains generally approximate a unidimensional structure, a structure they interpret as reflecting justices' policy preferences. The second is a set of strong statistical relationships between justices' votes and three kinds of independent variables: perceptions of the justices' views in newspaper editorials at the time of their selection, past votes of the same justices, and the facts of cases.

These bodies of evidence support the book's argument, but they are far from conclusive. Leaving aside case facts for the moment, neither editorial content nor past votes actually taps justices' personal policy preferences in pure form. Rather, their relationship with current votes demonstrates a high consistency of individual behavior over time, extending even to the period prior to a justice's career on the Court. In turn, both that consistency and the tendency of votes to take a unidimensional structure strongly suggest that justices vote on the basis of relatively fixed policy positions. But those policy positions could result from a variety of sources, not just policy preferences. And the book does not provide direct evidence for the importance of preferences in shaping individual policy positions.

For the most part, the evidence presented in the book relates to variation among justices in their responses to the same cases, not variation in a justice's responses to different cases in an issue area. The primary exception is the use of the facts of cases to help explain the Court's decisions. As Segal and Spaeth recognize, however, the impact of the case facts on decisions can be interpreted as reflecting either policy preferences or legal considerations.

Thus, the book's evidence for the impact of policy preferences leaves considerable room for other considerations, such as readings of the law and the Court's political environment, to influence decisions. This impact may come in any of three forms. First, these considerations might be important sources of the policy positions that produce mostly unidimensional voting patterns. Second, the deviations from unidimensional voting that occur might result from considerations other than policy preferences. Third, these considerations might be primarily responsible for differences in justices' votes from case to case within a policy domain. For this reason, the book's evi-

dence *against* the significance of these potential determinants of votes—especially legal considerations—is especially important.

The systematic portion of this evidence relates primarily to the book's argument that justices do not act on the basis of what it characterizes as judicial restraint—for the most part, deference to other political actors. Voting patterns in selected sets of cases are used to support this argument. But judicial restraint in this sense is not the same as using legal considerations to reach decisions. Rather, this type of judicial restraint involves what might be called structural policy positions, positions relating to the distribution of power among political actors. These can be distinguished from positions relating to the substance of government policy. A showing that substantive positions generally override structural positions is not the same as a showing that policy preferences generally override a justice's reading of the law. (The book does analyze decisions in which the Court overturned its precedents, and this evidence relates to—and shows limitations in—the impact of legal considerations in decisions.)

Segal and Spaeth also offer a good deal of what they characterize as anecdotal evidence that forces other than preferences, especially legal considerations, exert little influence on justices' votes. The primary reason for the reliance on anecdotal evidence, as they point out, is the difficulty of developing systematic tests for the impact of such considerations as adherence to the law. Even the book's anecdotal evidence points to the limits of such considerations as explanations of justices' choices, but it does not rule out a significant role for them—especially as a source of variation across cases in justices' votes and Court decisions.

How, then, do Segal and Spaeth get from their limited evidence to their broad conclusions about Supreme Court decision making? They seem to make an intuitive leap, resting on the unstated premise that the structure they find in justices' votes could have no basis other than the attitudes of justices about public policy. It is a highly reasonable leap, one that other students of the Court have made, but it is not compelled by the evidence presented in the book.

That Segal and Spaeth have to make such a leap does not result from any failures on their part in gathering or analyzing relevant evidence. While empirical research on Supreme Court behavior can do better in probing the determinants of that behavior, it is not clear whether *any* research can produce definitive judgments about the relative strength of these determinants, or—to focus more narrowly—about the sources of justices' policy positions. Thus it is hardly a weakness of this book that it does not prove the dominance of the “attitudinal model” of

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Supreme Court decision making.

This is a time of great advances in our knowledge of judicial behavior. Jeffrey Segal and Harold Spaeth are prominent among the scholars who are contributing to those advances. Their book documents how much our knowl-

edge about Supreme Court decision making has grown, and it advances that knowledge considerably further. But the limits to our understanding of the Court remain enormous. The more that we face those limits directly, the better we can clarify what we actually know about the Supreme Court and identify the questions that require more investigation.

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Jack Knight, *Washington University in St. Louis*

In *The Supreme Court and the Attitudinal Model* Jeffrey Segal and Harold Spaeth put forward a number of important arguments about law and the Supreme Court and about the ways in which social scientists should study the relationship between the two. One set of arguments is substantive in nature, emphasizing judicial decisionmaking as a political process and the Supreme Court as a policy-making body. A second set of arguments is methodological in nature, promoting an attitudinal approach to judicial voting and adamantly rejecting a causal role for law in that process. One can easily accept Segal and Spaeth's substantive claims about the political nature of legal decisionmaking and at the same time seriously question their methodological recommendations.

Segal and Spaeth forcefully recommend the attitudinal model as a complete and adequate explanation of judicial behavior. I admire the degree to which they make explicit the model and behavioral theory on which they propose to base their explanations. What I want to suggest here, however, is that they are unable to sustain an exclusive reliance on the attitudinal model in the construction of their explanations of Supreme Court decisionmaking. A close reading of Segal and Spaeth's book suggests that they often invoke other explanatory mechanisms to supplement their attitudinal models, mechanisms that they explicitly reject elsewhere in the course of their analysis.

In a longer version of this comment that I prepared for the APSA meetings I developed three examples of this explanatory strategy. In the limited space here I can merely cite these features of the Segal and Spaeth analysis and suggest that readers review these chapters to see how the authors use various explanatory mechanisms.

[1] In their discussion of the Supreme Court's control of its docket, Segal and Spaeth claim that there are many types of cases, such as "meritless" ones, which "no self-respecting judge would decide solely on the basis of his or her policy preferences" (70). Later, they suggest that "[t]he justices would not likely refuse to review a decision by a lower federal court that voided a major act of Congress, nor would it decline to consider a state court's decision that substantially redefined the scope of the First

Amendment, absenting extenuating circumstances" (179). A close inspection of their analysis shows that they do not explain this form of behavior in terms of a strict attitudinal model. Rather they invoke other mechanisms to explain why the judges sometimes do not act according to their substantive attitude towards the merits of the case.

[2] When Segal and Spaeth turn to the explanation of the actual decisions of the Court, they introduce a model which attempts to explain votes of justices in terms of two main categories of variables: the facts of the case and the attitudes of the justices. They state that "behavior may be said to be a function of the interaction between an actor's attitude toward an 'object' (*i.e.*, persons, places, institutions and things) and the actor's attitude toward the situation in which the object is encountered" (215). They operationalize the facts component of the decision by a measure of the relevant facts as articulated in previous Supreme Court decisions. But they are unclear as to exactly how and why the facts of previous cases should affect the attitudinally-driven decisions of future judges. The best justification that I can produce for their claims seems to invoke previous Court decisions as precedent, thus allowing for an independent causal role for law.

[3] In Chapter 8, Segal and Spaeth deal directly with the question of "judicial restraint," an idea that they take to be a central challenge to their main substantive thesis about the Supreme Court as a political body. In attempting to justify the fact that their empirical tests involve exclusively those cases in which the Court overturns a statute or a precedent (thus, biasing the test in favor of a judicial activism conclusion), they argue that the "unconstrained attitude-model" judge always agreed substantively with the cases which were affirmed. Intuitively, this seems highly implausible but this is exactly what Segal and Spaeth conclude: "We have found some evidence of judicial support for the decisionmaking of the other branches and levels of government. But, overwhelmingly, such support results because these nonjudicial actions comport with the policy preferences of the justices themselves" (332). Note clearly that while this may be true, such a conclusion does not follow from any of the empirical tests

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***The Supreme Court and the Attitudinal Model (continued)***

offered in this chapter. A more likely source of explanation rests with the types of mechanisms they implicitly invoke in example [1].

My reading of Segal and Spaeth's suggests that the attitudinal model fails to produce a complete and adequate explanation in those instances in which the justices are confronted with a situation which requires *effective* decisionmaking. That is, the attitudinal model fails to account for factors which complicate the relationship between an individual justice's vote and the effectuation of a particular outcome (in this case, the Court's decision.) If the decisions of justices are to be explained mainly in terms of their desire to affect public policy (an explanation with which I am in general agreement), then such accounts should at a minimum incorporate those factors which influence what is in effect a strategic calculation. Such factors appear, on Segal and Spaeth's own account, to implicitly include: constraints which the democratic process places on the decisions of the Supreme Court; constraints which jurisdictional rules (constitutional and statutory) place on the agenda, and thus on the decisions, of the Supreme Court; constraints which precedent, statute and constitutional provisions place on decisionmaking; and constraints which the anticipated votes of their colleagues place on their own voting calculus. Segal and Spaeth may counter that they have explicitly rejected some of these constraints, but the most sympathetic reading that I can give of their argument is that these factors reappear in an ad hoc manner as a way of explaining either (1) how and why attitudes enter into the decisions of judges (the underlying explanatory mechanisms) or (2) gaps in the attitudinal model's presentation.

If scholars who employ the attitudinal model want to move beyond prediction to the systematic provision of answers to these how and why questions, the best evidence of such answers comes from two basic sources, sources which Segal and Spaeth appear to invoke in an unsystematic way. Both are capable, in different ways, of reintegrating

the constraints of law into a justice's decisionmaking process. The first source is rational choice theory which treats legal rules as a prudential constraint on decisionmaking. This is the approach most consistent with the basic thrust of Segal and Spaeth's account of the strategic behavior of judges. Here the effects of legal rules enter a justice's strategic calculus on two explicit ways: as a direct effect on the incentives of the available choices and as an indirect effect in their effects on the strategic choices of the other justices. The second source is the non-quantitative empirical literature which investigates the importance of normative constraints on judicial decisionmaking. This literature is often presented in a way quite different from the micro-level orientation which I suggest here, but there is no a priori reason why these normative constraints cannot be incorporated into an intentional account of judicial policymaking. Such constraints might be conceived as normative constraints on the justices' feasible set as opposed to factors incorporated in an expected utility calculus. Or they might be combined with a strategic orientation in a more complex judicial decisionmaking framework. This is in fact one plausible way to read Segal and Spaeth's discussion at the beginning of chapter 8.

These two approaches take more systematic account of the effects of legal and normative constraints on judicial decisionmaking, but allow for the possibility that Supreme Court justices are mainly interested in promoting their own policy preferences. Thus, we are not left with an all or nothing choice: a deductive legal model or an attitudinal model which rejects any causal role for law. And, as I have tried to show, Segal and Spaeth do not consistently adopt the latter approach in their own explanations. Segal and Spaeth invoke features of these other approaches to make sense of their important analyses of the values of judges and their effects on judicial outcomes.

Of course there are problems in empirically assessing the different mechanisms. One test of their persuasiveness however might lie in self-reflection. What I would suggest all students of judicial politics do is to reflect on which mechanisms they actually invoke in their own work.

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Gerald N. Rosenberg, *University of Chicago*

*It is easier to debunk worn myths of judicial objectivity, however, than to replace them with realistic conceptions . . . that do not overstate the case. Recognizing that judges legislate is the beginning rather than the end of sophistication (Howard 1981, 15-16).*

*The Supreme Court and the Attitudinal Model* is the best work to date on the Attitudinal Model. It brings together an impressive amount of data and makes a strong case for the importance of attitudes as predictors of judicial decisions. The book also should be a wonderful teaching tool.

Segal and Spaeth's ability to shake up traditional legal thinking is nicely illustrated by a law student's comment on the Segal and Cover article that is part of chapter 4. She wrote, "Their conclusions undermine the integrity of the Supreme Court . . . [creating a] risk of crisis in American politics;" attitudinal studies are full of "danger" and should not be done. On the other hand, Segal and Spaeth's argument is weakened by straw-person arguments, over

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statement, and disrespect for the work of others.

The object of Segal and Spaeth's critique, what they label the Legal Model, "postulates that the decisions of the Court are based on the facts of the case in light of the plain meaning of statutes and the Constitution, the intent of the framers, precedent, and a balancing of societal interests" (32). Under this conception, law is discovered, not made. It follows from this, for example, that "it should not matter whom the President nominates or whether the Senate confirms, given a basic modicum of legal training and intelligence" (125). Not surprisingly, Segal and Spaeth have a field day in chapter 2 showing that the key features of the Legal Model—plain meaning, intent, precedent, and balancing—can be used to support any outcome. Thus, Segal and Spaeth argue that the Legal Model "rest[s] on myth instead of data" ( xv) and "serves only to rationalize the Court's decisions and to cloak the reality of the Court's decision-making process" (34).

There are several difficulties with this conception of the Legal Model that make it a straw person. First, practically no judge or scholar believes it. Rather, they tend to believe that the "*Legal Model Properly Understood*" draws a bright line distinction between an a priori commitment to policy preferences or outcomes, as the Attitudinal Model postulates, and an a priori commitment to a set of interpretive principles. Under the Attitudinal Model, judges examine the substantive issue of a case, select the result that most closely accords with their attitudinal (policy) preference, and write an opinion supporting it. In contrast, under the Legal Model Properly Understood, judges apply a set of interpretive canons, a set of principles that guide them in interpreting the Constitution, statutes, precedent, and derive an outcome. They are driven not by outcomes but by interpretive philosophy. And, in accord with the sort of scientific procedure that Segal and Spaeth apply, a group of "judges," if provided a set of facts and told to employ a given interpretive philosophy, should reach similar outcomes. It follows from the Legal Model Properly Understood, for example, that it matters a great deal who the President nominates to sit on the Court because different interpretive philosophies will produce different outcomes. Remove the straw person argument, re-characterize judicial motivations, and the Segal/Spaeth argument is both more powerful and less striking. Indeed, their data and analysis lend a great deal of support to the Legal Model Properly Understood.

Segal and Spaeth's Legal Model is also a straw person because, in contrast to the Legal Model Properly Understood, it requires that judges treat each and every constitutional clause or statutory provision similarly. But why, for example, must Article II, limiting presidential eligibility to those age 35 or older, be treated similarly to the 14th Amendment's requirement of equal protection? There may be good reasons to treat them differently, reasons that derive from a philosophically consistent position, not

from a policy-driven preference. In addition, the book's analysis is based on the premise that all cases are equal. But cases differ as to subject area, constitutional or statutory provision, etc., differences that allow for or even demand different treatment. The "inconsistencies" that Segal and Spaeth find may have more to do with their treating dissimilar cases as similar than with attitudes as they conceive of them. Thus, what they call "subjective preferences" may be nothing more than honest attempts to apply consistent interpretive philosophy to the facts. While the choice of interpretive principles is certainly subjective, and in this sense the Attitudinal Model and the Legal Model Properly Understood are similar, the choice is not driven by the preferred policy outcome in any given case.

The Legal Model Properly Understood and the Attitudinal Model produce similar results. In virtually every area where Segal and Spaeth find support for the Attitudinal Model, the Legal Model Properly Understood is potentially supported as well. For example, in chapter 5, Segal and Spaeth analyze judicial access and conclude that the decisions of the justices are driven by their policy preferences. But merely to show that access is "subject to the justices' control" (206) is not the same as showing that it is determined by the "individual justices' personal policy preferences" (206). It could well be the result of a consistent philosophical position. Similarly, to show a correlation between attitudes and votes in the areas of the death penalty ( 224), civil liberties cases (228), and a number of other issues (Appendix 6.1, 255-60), is consistent with both the Attitudinal Model and the Legal Model Properly Understood.

I would be remiss if I did not address the book's tone. *The Supreme Court and the Attitudinal Model* is marred by numerous disrespectful ad hominem attacks. They are inappropriate, counterproductive, and not supported by the evidence. For example, the characterization of Justice Blackmun's positions on the death penalty and abortion as "pure hypocrisy" and "simply bunkum" (235-36) is distasteful and unsupported. They present no evidence of Blackmun's thinking. What they do present, correlations, are suggestive, not conclusory.

Segal and Spaeth's conclusions of judicial deceit throughout the book are based on the correlations they produce between their set of attitude measures and votes. They have no supporting evidence as to the motivation of judges while they do have opposing anecdotal evidence that the Attitudinal Model is seen as inappropriate to the judicial role. Their response, however, is not to acknowledge the limitations of their methodology. Rather, they launch a full-scale attack on the truthfulness of judges and scholars who do not believe that the Attitudinal Model explains everything. Indeed, they simply deny the possibility that commitment to principle can override policy preferences. As they put it, "rational people. . . along with judges, only

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*The Supreme Court and the Attitudinal Model (continued)*

defer to the course of action or to the policies of which they approve” (300). They assume a conspiracy so immense as to involve all judges and most legal commentators. Either most members of the legal profession (and scholars who study the law) completely lack insight or self-knowledge, or they all share this dirty little secret. Such arguments are not persuasive. While Segal and Spaeth may be right, and their correlations are impressive, they have only begun to make the case.

By relying on straw-person arguments and overstating their conclusions, Segal and Spaeth offer an initially more striking but ultimately less powerful argument. A more careful argument, one less dismissive in tone and more sensitive to the richness of alternative approaches, would pack a much greater punch. Judicial politics scholars, like others, can learn a great deal from their colleagues. *The Supreme Court and the Attitudinal Model* makes an important contribution to our knowledge, but it is not the final word.

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Rogers M. Smith, *Yale University*

I. *The Value of the Book.* Let me first stress a fundamental agreement. If our task is predicting judicial votes, and the choice is between the “legal model” Segal and Spaeth describe and models of ideological attitudes, then I, along with virtually all other political scientists, am firmly on the side of Segal and Spaeth. As a doctrinally oriented political scientist, Robert McCloskey, argued long ago, “it is perfectly apparent to the detached observer that the Court’s decisions do tend to fall into patterns that reflect current judicial views of what ought to be done; and that these views, though heavily influenced by the nature of the forum that issues them, are nonetheless policy determinations.” McCloskey then contended that “since the constitutional questions that do successfully claim the attention of the Court are often those least answerable by rules of thumb, the predilections, the ‘values’ of the judges, must play a part in supplying answers to them” (McCloskey 1960, 27). My own research and writing over 20 years have only made me more confident of those premises. Segal and Spaeth differ from them only in emphasis. True, they downplay the influence of the legal “forum” and stress the role of judicial values almost exclusively. But, they do so while acknowledging that there are legally “meritless cases that no self-respecting judge would decide solely on the basis of his or her policy preferences.” They also contend that the Supreme Court generally refuses such cases and considers only those that “tender plausible legal arguments on both sides.” Then judicial preferences become decisive. That is little more than a paraphrase of McCloskey’s position (70).

The research on attitudinal models by Segal and Spaeth, and others, has, however, greatly strengthened this general political science view by operationalizing it and providing evidence for it over the last 30 years. Their book is the most thorough presentation of the attitudinal approach extant, and it is also an invaluable summary of a wide range of empirical work on the Court. Hence it is already a basic reference in our field.

II. *Main Concerns.* The book could nonetheless be stronger in several ways. First, it has a hectoring tone that makes it seem more defensive and less magisterially mature than it should. Even copy editors are praised only to the extent that they refrain from disagreeing with Segal and Spaeth (xviii)! This shrill style encourages unsympathetic readers to write the work off, mistakenly, as unduly extreme.

More importantly, the book attacks the wrong targets. Its official target, the “legal model” of “interpretivist jurisprudence,” is a straw man as far as modern judicial scholarship is concerned. Segal and Spaeth cite some judges, such as the anti-New Deal Four Horsemen, who said deductive reasoning from authoritative legal texts determined their results. But Segal and Spaeth also note that some judges reject this claim, and they admit that even many “legalist” judges engage in “balancing,” an approach that openly relies on subjective judicial assessments of social advantage (4-7, 52-53, 356). Most significantly, Segal and Spaeth do not cite a single scholar who now claims that courts actually behave largely in accordance with traditional “mechanical jurisprudence.” They only invoke writers like Raoul Berger and Robert Bork, who think the Supreme Court SHOULD act that way, but bemoan the fact that the Court rarely does. Bork levels this critique against every Supreme Court since John Marshall’s (55-57; Bork, 1990, 15-132).

Segal and Spaeth fail utterly to address what is really their most appropriate “legalist” target now: the sophisticated post-realist jurisprudence of legal scholars like Ronald Dworkin and Bruce Ackerman. These authors *reject* the old mechanical legal model and *acknowledge* the impact of judicial values on decisions. To preserve legal credibility, however, they still try to minimize the significance of judicial values in ways that may well be vulnerable to the Segal and Spaeth critique. But Segal and Spaeth fail to take on these potent current targets and instead tilt at

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long abandoned windmills. At times they make modern versions of legalist jurisprudence look more balanced and credible than their own. They often write as if legal factors played not the *slightest* role in judicial decision making, a claim they disavow in their careful moments (e.g. 1-3, 34, 206, 360-361).

Ronald Dworkin argues that justices reach decisions that may have merely an "adequate fit" with authoritative legal texts and precedents. They decide among the many views that have such minimally "adequate" conformity to legal materials by relying on the political and moral values that seem *to them* parts of the most coherent and persuasive constitutional theory they can find or construct (Dworkin, 1978, 106-107, 127-128; 1986, 65-68, 87-88). This account suggests that in most serious constitutional cases black-letter law is a lesser factor. The ideological values of justices play the decisive role, just as Segal and Spaeth and other political scientists contend. But Dworkin tries to preserve an aura of legal objectivity by stressing that judges must use their values to give content to concepts they find in the law, even when their preferences might be against those concepts being there; and by stressing that there is "one right answer" each judge must reach, even though that answer may be different for different judges. Whether these reassuring claims are true, and whether in any case they make judicial decision making significantly more constrained than Segal and Spaeth believe, are the sorts of pertinent questions Segal and Spaeth fail to address.

Similarly, Ackerman claims Supreme Court decision making is now largely a matter of synthesizing the "texts" of previous distinct constitutional moments. That task leaves room for judicial discretion and creativity but again, only within bounds set by clearly defined legal problems (Ackerman, 1991, 86-99, 159-162). It is unclear if one can or cannot operationalize this model of "synthetic problem-solving" in contrast to the attitudinal model; but either way, Ackerman offers an influential current position that is vulnerable to a Segal and Spaeth-style critique. Yet he escapes unnoticed.

III. *An Unofficial Target?* Finally, at times Segal and Spaeth seem to have another, unofficial target. They are vexed by the abundant scholarship in political science which accepts that Supreme Court justices vote in terms of their ideologies, but analyzes those ideologies interpretively, not quantitatively. Since my own work falls under this head, I may be unduly sensitive here. But their first paragraph says "most" of what political scientists have written about the Court "has been historical, anecdotal, legalistic, tendentious, or doctrinal." They dismiss this "stuff" as suited at best only to other disciplines, because none of it is "science," though they do rely on such sources on occasion (xv; cf. xviii, 67, 356).

Segal and Spaeth seem to think that most if not all

nonquantitative public law scholarship concedes too much to the "legal model." I believe they should instead recognize that interpretive studies of judicial ideologies can helpfully complement their own. They rightly say judicial "beliefs" or "attitudes" have "cognitive, affective, and behavioral components" and that they are concerned only with the "behavioral components," judicial votes (69). Their focus is fine, but a fully explanatory social science must also address the cognitive and affective dimensions of decision making. We should try to illuminate the modes of reasoning and logical tensions associated with certain sets of beliefs as well as the varying subjective attachments to those beliefs judges are likely to have. Such assessments can often only be reached via nuanced discursive interpretations that are not easily reduced to an attitudinal scale. Good interpretive accounts can then identify factors in decision making that more summary attitudinal models may neglect. They can, for example, highlight contradictions in a judge's ideological beliefs that may lead him to alter his voting pattern or even his structure of beliefs when faced with unusual cases that make those contradictions unavoidable. And an account of the affective power his conflicting beliefs have for him may predict which way he will then go.

To be sure, such interpretations should be well-specified and falsifiable, and too often they are not. It should always be possible for critics to show, for example, that a justice's opinions *rarely* exhibit the modes of reasoning and tensions an interpretive scholar identifies as characterizing his ideology. But, if well done, interpretive studies can shed light on the inner workings of the beliefs attitudinal models simply count, producing explanations that supplement the predictions those models achieve, and sometimes helping scholars to construct more powerful attitudinal models and to address further questions. In turn, interpretive work is likely to be much better done if the voting patterns discerned via attitudinal models are fully considered.

Hence I would urge Segal and Spaeth to view interpretive efforts as linked, not opposed, to their quantitative endeavors. And both belong in science. After all, as Segal and Spaeth note, it was a lawyer and judge, Oliver Wendell Holmes, who saw the law *simply* as predictions of what the courts would do (241 n. 128). Such predictions are often all that lawyers and their clients want to know. But scholars should not just be interested in predictions. We seek the most powerful explanations of human experience we can devise, including explanations of patterns of cognition and affection, and accounts of why people sometimes find their beliefs problematic and change them. The attitudinal model is not useless for those questions, but its strength lies in predicting votes. Let us applaud it for that and seek its elaboration and refinement. Let us not imply that it is the only aspect of public law research worthy of the name "political science."

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## The Authors Respond

Jeffrey A. Segal, *SUNY-Stony Brook* and Harold J. Spaeth, *Michigan State University*

Our critics argue both that the legal model is a straw man and that the legal model is true. They say we are both too devoted to the attitudinal model and not devoted enough to it. We consider their arguments in turn.

First and foremost, the legal model is hardly a straw man. While none today could credibly argue that legal factors are all that influence judicial decisions, the argument that legal factors do influence court decisions is made by some of the brightest minds in law and political science. In addition to the luminaries noted by Smith, one might add scholars such as John Ferejohn and Barry Weingast (Ferejohn and Weingast 1992; McNollGast 1992). Indeed, all of our critics except Smith accept various tenets of the legal model, though some (*i.e.*, Rosenberg) do so more than others (*i.e.*, Baum). Smith's review of modern legalist thought is useful and we agree with his contention that the book could have benefited from a discussion of these scholars.

Smith is also correct in noting that the attitudinal model has a difficult time accounting for changes in attitudes. More accurately, it does not even attempt to. Instead, the model treats attitudes as exogenous. If attitudes are crucial to the justices's decisions, then the source of attitudes and attitude change becomes crucial as well. Quantitative methods have not been exceptionally useful in explaining justices's attitudes and have shed extraordinary little light on the causes of individual attitude change. To the extent that justices such as Blackmun do shift over time, our empirical model is limited in that we treat attitudes as stable across a justice's career. This is more a measurement problem than a theoretical problem, for while attitudes must be *relatively* stable, long-term drifts and occasional random shocks are not precluded. Such shifts, unfortunately, are extremely difficult to measure on an a priori basis. Regardless, the explanatory ability of our model suggests we do not do too much damage by modeling a justice's attitudes as constant.

We must dissent from Smith's argument that we disrespect qualitative research. We frequently and positively cite the works of Henry Abraham, Judith Baer, Leif Carter, Sue Davis, H.W. Perry, Gerald Rosenberg, and Charles Warren. No author receives more extensive citation than Robert McCloskey. (We nevertheless trace our intellectual roots not to McCloskey, as Smith suggests, but to the legal realists who wrote decades before McCloskey. See 65-66.) While we find much of merit in cited qualitative works, let us not be misunderstood: we still believe that evidence as to the factors that affect Supreme Court decisions must be systematically demonstrated.

Rosenberg's arguments about the Legal Model Properly

Understood is a perfect example of why the legal model has, to date, failed as a scientific explanation of the Court's behavior. If justices can be textualists in some areas, intentionalists in others, and social engineers in still others, then there is no decision that would be *inconsistent* with Legal Model Property Understood. *A model that is so broad that it can be used after the fact to explain everything necessarily explains nothing.* Alternatively, if we actually restrict justices to a consistent jurisprudential philosophy, then we have a falsifiable hypothesis that has already been falsified. We recommend to all who may have missed it the Phelps and Gates article (1991), which demonstrates that Justice Brennan, just like Justice Rehnquist, relies on intentionalist arguments when it suits his purposes, and Justice Rehnquist, just like Justice Brennan, relies on nonintentionalist arguments when it does the same.

We make no apology, by the way, for pointing out similar inconsistencies in the opinions of other justices and labeling them as such, *e.g.*, Blackmun's intentionalist approach in *Furman* as compared to his substantive due process approach in *Roe*. Blackmun's recent statement that the death penalty is unconstitutional demonstrates perfectly the problem we had with his initial statements in *Furman*. Since his 1972 opinion stating that the Constitution forced him to uphold the death penalty—despite his purported personal abhorrence of it—the text of the Constitution with regard to the death penalty has not changed, no significant new evidence about the intent of the framers has been found, and Court precedents on capital punishment have gotten substantially more conservative. The public supports capital punishment by even greater margins than it did in 1972 (Epstein et al., 1994, 591) and crime certainly has not become a less important national concern. What then could have caused Blackmun's change of heart? All court watchers agree that Blackmun has become substantially more liberal over the past two decades. It was his conservative ideology on criminal procedure issues that prohibited him from striking the death penalty in 1972, and his more liberal ideology that allows him to do so now. The Constitution never prevented him from declaring the death penalty unconstitutional in the first place, regardless of his protests in *Furman*.

Needless to say, we posit no Oliver Stone-like conspiracy in regards to the legal profession. Judges consistently recognize policy-based decision making in the votes of justices with whom they disagree. Rarely, if ever, though, does a judge acknowledge the same in his or her own legal opinions. This hardly requires an immense conspiracy. Legal socialization, to say nothing of self-preservation, keeps even self-aware judges from admitting

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their attitudinal biases. It requires no conspiracy to note that members of Congress are influenced by PAC contributions, even though none would admit it about her or himself. Just as a member of Congress who admitted voting based on PAC contributions might well be defeated, a judge who admitted voting based on policy preferences might well be impeached. With regard to legal scholars the dominant mode of analysis is the legal model. In the law schools the job of pointing out the role of attitudes and values has essentially fallen to the critical legal theorists, albeit from their own ideological perspective.

Rosenberg's claim that we are too wedded to the attitudinal model is directly contradicted by Jack Knight's claim that we are not wedded enough to it. In particular, much of our discussion about docket control and the certiorari process is not strictly attitudinal. Knight believes that this is inconsistent with our purported claim that the attitudinal model is "a complete and adequate explanation of judicial behavior." If we made such a claim we would be due for a great deal of criticism but this clearly misrepresents our view. While we argue that the attitudinal model is a complete and adequate *model of the Supreme Court's* decisions on the *merits*, this is a far cry from what Knight claims we say. We do believe that the attitudinal model has *implications* for cert votes, for opinion assignment, and for the behavior of lower court judges, but we would be closing our eyes to reality if we argued that that's all there is to those decisions. Nor are such conclusions *ad hoc*. The institutional rules and incentives that allow Supreme Court justices to engage in attitudinal decision making in votes on the merits simply do not apply in full to other courts or to other stages of the Supreme Court's processing of cases. Certainly justices engage in strategic behavior in certiorari voting, and just as certainly opinion assigners pay careful attention to ideological proclivities when handing out assignments. But nothing in the attitudinal model, which was developed explicitly to explain the decision on the merits, requires these factors to be sole explanations of the justices's behavior at other stages. Legalistic questions of jurisdiction and standing clearly have some importance in cert decisions. Nor could anyone credibly argue that the need to share workload has no importance in opinion assignment. Burger, after all, did assign some cases to Brennan and Marshall. In short, different types of decisions are not fungible with one another. Context matters. Justices overloaded with cases to hear use legalistic criteria to help weed them out. Chief justices wishing to maintain efficient working conditions assign some opinions to ideological opposites. We make no apologies for considering extra-attitudinal factors in these areas. Finally, with regards to the Supreme Court's decisions on the merits, we remind readers that it is a model we are expounding. We do not say that attitudes are all that matter in the vote of every justice in every case. We do present a model that says that attitudinal factors are all that systematically explain the votes of the justices (chapter 2). With the exception of the

Solicitor General our empirical analyses find no other extra-attitudinal influences operating on a systematic level (chapters. 6 and 8). Neither Knight nor any of our other critics provide any evidence to the contrary.

We also note that case stimuli are consistent with both attitudinal and legal decision making, and thus cannot help us distinguish between the two. Those parts of the attitudinal model that are independent from the legal model have been supported; those parts of the legal model that are independent from the attitudinal model have not been.

Knight claims that our empirical tests of judicial restraint are biased in that they "involve exclusively those cases in which the Court overturns a statute or a precedent." Again, if that were true he would have a valid point. In addition to the cases he mentions, though, we examine each of the following in an attempt to find evidence of judicial restraint: all cases in which the Supreme Court reviewed NLRB decisions between 1953 and 1959, 1969 and 1977, and 1981 through 1989 (305-308); all cases in which the Court reviewed state economic regulation between 1981 and 1989 (308-310); all state and federal First Amendment, double jeopardy, search and seizure and poverty law decisions between 1969 and 1979 (310-311); all state and federal civil liberties decisions between 1981 and 1989 (311-312); all Solicitor General briefs between 1983 and 1988 (313); and *all* challenges to state and federal laws between 1986 and 1989 (320). We believe we have conducted the most exhaustive search for judicial restraint of any scholars to date.

Knight then argues that we could have retained a single unifying theory of judicial behavior had we adopted rational choice theory (aka positive political theory) as our paradigm. Rational choice theorists are split on the role of the Supreme Court in the American political system. One school of thought (e.g., McNollGast 1992) considers judges, at least in part, to be neutral arbiters of original intent and is thus consistent with the legal model. A second school considers judges to be purely policy-motivated actors who are nevertheless constrained by preferences of other political actors (Ferejohn and Shipan 1990; Marks 1988; Gely and Spiller 1990). Indeed it is easy to demonstrate using formal mathematical logic that a politically motivated Supreme Court *must* carefully consider the views of Congress in reaching its decisions. We warn readers though that such results are typically achieved by forcing the Court into a statutory interpretation mode and granting Congress the last move (Ibid).

There is enormous power to the formal logic of positive political theory. The policy-motivated school may someday emerge as the strongest competitor to the attitudinal model as an explanation of the Supreme Court's decisions. To date, however, empirical verification remains virtually nonexistent. As Lee Epstein so cogently put it,

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“the modus operandi of the theorem provers who have studied these questions will not suffice. The standards of social science simply require more than reading some cases (e.g., *Grove City* seems to be a favorite), developing a model, and then testing the model against the same cases used to develop it (again, *Grove City* comes to mind)” (1993, 4). With the single exception of Spiller and Gely’s article on the Supreme Court’s NLRB decisions (1992), the positive political theory of courts has no more systematic empirical evidence supporting it than does the legal model.

Knight’s call for examination of normative constraints is no more persuasive. As we note in our book, role models have been successful in examining lower court behavior, but the unique status of the Supreme Court makes such behavior there unlikely. Again, we sound a familiar refrain: there exists no systematic evidence of their usefulness at the Supreme Court level.

Finally, Baum’s analysis of our evidence raises some important points that need to be answered. First, he argues that our independent measures of the justices’ ideology do not actually tap the justices’ policy preferences in pure form. This is necessarily true since “policy preferences” or “attitudes” or “values” are constructs: they do not actually exist. As such, all measures of attitudes, including ours, are indirect. Relatively speaking, though, one could argue that our indirect measures are more indirect than others typically used in survey research, such as attitude questionnaires. Note though that the use of such questionnaires, even if the justices would agree to their use, would not necessarily give us better measures of their attitudes. Self-deception, social desirability effects, and flat-out lying, would mar any such analysis. Judicial nominees who can state under oath before the entire nation that they had never thought about *Roe v. Wade* can hardly be fruitful candidates for traditional survey measures.

Baum, while acknowledging that we find strong relationships between our attitudinal measures and the justices’ voting, questions whether our attitudinal measures might be confounded by factors other than policy preferences that also would affect the justices’ voting behavior. For instance, consistency is a necessary but not sufficient condition for attitudinal voting, as we point out in our book. Additionally, newspaper editorials might be influenced by the justice’s legal attitudes as much as by their political attitudes, and it might be these legal values that influence the justices’ votes. Again, as we point out in the book (228), such a hypothesis fails to explain why our measure works demonstrably better for justices without prior judicial experience ( $r = .94$ ) than for justices with prior judicial experience ( $r = .69$ ). These results suggest that any inferred legal values provide disinformation about both the true values of the justices and how they will vote once on the Court.

Baum is correct that we do not provide systematic evidence against the legal model. But then, *no one* to this point has been able to provide testable hypotheses about the legal model. As we noted above, it is a basic tenet of science, whether social, political or natural, that an untestable model has no explanatory power. The inability of our critics to cite any scholarly evidence demonstrating the validity of alternative approaches with regard to the Supreme Court’s decisions on the merits buttresses our claims more than anything we could say in response. The leap of faith is not made by those who accept that which has been empirically verified, but by those who believe in alternatives despite the absence of supporting evidence.

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