



Law & Courts

NEWSLETTER OF THE LAW & COURTS SECTION OF THE
AMERICAN POLITICAL SCIENCE ASSOCIATION

A Letter from the Section Chair

Paul Wahlbeck
George Washington University



Upcoming APSA Meeting

I hope that you will join us at the meeting of the American Political Science Association in Philadelphia. This meeting marks the 100th anniversary of the association. In addition to attending the panels listed elsewhere in the newsletter, I hope that you are able to join us for the business meeting on Friday, August 29th, at 6:15 in Room 103A of the Pennsylvania Convention Center. There will be a reception following the meeting at 7:00 in Room 103B of the Convention Center.

Law and Court Section Members' Scholarly Activities

One of the tasks accorded to the section chair is the writing of this column in the newsletter. In preparing my first column last fall, I revisited columns written by my predecessors in this position. There were many models to follow: a newsy account of events in the section, columns summarizing and encouraging work in particular areas of inquiry, and so on. I chose instead to indulge my curiosity with who we are as a section. Although section membership ebbs and flows from month to month, the Law and Courts Section is one of the largest (currently second largest) in the APSA with 881 members. One of the principal questions on my mind was with the state of research in law and courts: Where do we publish our research on law and courts? Who is producing these research findings?

Let me start by saying a word on how I gathered data to address these questions. I began this project with every intention of gathering the publication record of each section member. As many researchers discover, my initial objective was overly ambitious. Instead, I gathered data on 500 of the 836 members (59.8%) listed on the spreadsheet I received from APSA last fall.¹ My data source was the OCLC FirstSearch service (<http://newfirstsearch.oclc.org>), which has a database for scholarly works published as books (WorldCat) and articles (ArticleFirst). The scope of the article database is more constrained than the book database as it only includes articles published since 1990, while WorldCat claims to have books published since 1000 BC. I doubt any of our current membership published books before that date.²

Some departments describe themselves as book-oriented or article-oriented. If I were to characterize Law and Courts as a subfield, I would be compelled to say that it leans toward

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General Information

Law and Courts publishes articles, notes, news items, announcements, commentaries, and features of interest to members of the Law and Courts Section of the APSA. **Law and Courts** is published three times a year in Winter, Spring, and Summer. Deadlines for submission of materials are: November 1 (Winter), March 1 (Spring), and July 1 (Summer). Contributions to **Law and Courts** should be sent to the **NEW** editor:

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Articles, Notes, and Commentary

We will be glad to consider brief articles and notes concerning matters of interest to readers of **Law and Courts**. Research findings, teaching innovations, or commentary on developments in the field are encouraged.

Footnote and reference style should follow that of the *American Political Science Review*. Please submit two copies of the manuscript; enclose a diskette containing the contents of the submission; provide a description of the disk's format (for example, DOS, MAC) and of the word processing package used (for example, WORD, Wordperfect). For manuscripts submitted via electronic mail, please use ASCII or Rich Text Format (RTF).

Symposia

Collections of related articles or notes are especially welcome. Please contact the Editor if you have ideas for symposia or if you are interested in editing a collection of common articles. Symposia submissions should follow the guidelines for other manuscripts.

Announcements

Announcements and section news will be included in **Law and Courts**, as well as information regarding upcoming conferences. Organizers of panels are encouraged to inform the Editor so that papers and participants may be reported. Developments in the field such as fellowships, grants, and awards will be announced when possible. Finally, authors should notify *Helena Silverstein* at silversh@mail.lafayette.edu, of publication of manuscripts.

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journal publications. Nearly half (238 or 47.6%) of the 500 members I surveyed have published at least one journal article. Of those who have published journal articles, the average number of publications is 4.7 articles. In contrast, almost forty percent (194 members) have published a book. Half of our section’s research producers have published both a book and a journal article (50.5 of 287).

Where do we publish our research? As displayed in Table 1, section members publish in the discipline’s leading journals. Using the standard definition of “major” journal (*American Political Science Review*, *American Journal of Political Science*, and the *Journal of Politics*), 38 members have published articles in one or more of these prestigious journals. Thirty-three members published articles in a second tier political science journal (i.e., *Political Research Quarterly* and *American Politics Quarterly*). In total, 55 members (11%) published at least one article in one of these five venues. The Law and Courts section is somewhat unique among sections as our work is sometimes suitable for publication in multidisciplinary journals (e.g., *Law and Society Review*) or even law reviews. This is reflected in our publication placement as 50 individuals published in *Law and Society Review*, *Law and Social Inquiry*, or *Judicature* (the most common multidisciplinary journals). Are these individuals from a different set of scholars than those who published in political science journals? The short answer is “not as much as one might expect.” Forty percent of these 50 scholars (20) also published articles in a top tier or second tier political science journal.

Table 1. Members’ Placement of Journal Articles

Journal	Number of Law & Courts Section Members
American Political Science Review	15
American Journal of Political Science	24
Journal of Politics	21
Political Research Quarterly	29
American Politics Research (American Politics Quarterly)	13
Judicature	21
Law and Society Review	29
Law and Social Inquiry	12

What about placement of books with university and trade presses? Table 2 presents the results of my survey of judicial publications by press. Among university presses, Cambridge University Press and Oxford University Press are the most popular outlets by section members, which is a good indicator of the quality of work conducted by our membership given the high regard in which the discipline holds these presses (see Goodson, Dillman, and Hira 1999). Nearly a quarter of the section membership (120 members) has published a book with a university press. Even more members have published books with trade presses (143 members). Among the trade presses, the most common is Greenwood Press, which has attracted more than twice as many member authors than the runner-up, St. Martin’s. One of the most staggering numbers, though, is that these 500 section members have published books with 165 presses.

Table 2. Members’ Publication of Books by Press

University Presses	Number of Law & Courts Section Members
Cambridge University Press	16
Oxford University Press	15
University of Kansas Press	14
Princeton University Press	12
University of Chicago Press	12
New York University Press	9
Cornell University Press	8
Harvard University Press	8
Johns Hopkins University Press	8
State University of New York	8
University of Michigan Press	8

Trade Presses

Greenwood Publishing Group	27
St. Martins Press	13
Sage Publications	12
CQ Press	10
Peter Lang	10
Prentice Hall	10
Garland	9
Rowman & Littlefield	9
McGraw-Hill	7
West Publishing	6

This discussion of publishing outlets begs the question of how have Law and Courts members fared in the race for prestigious homes for our research? To assess this question, I use ratings developed of journals and book publishers (Goodson, Dillman, and Hira 1999; Hix 2003).³ Seventy-four members published articles in political science journals ranked by Hix. Of these, the typical article landed in what Hix characterizes as the second tier (comparable to the *British Journal of Political Science*).⁴ One-hundred-fifty-three members published books ranked by Goodson et al. The median book ranking would be sufficient to place 20th on the list of 65 presses (which was shared by St. Martin's Press and Sage Publications). My conclusion from this analysis is that law and courts scholars are publishing their work with prestigious presses. Could we or should we do better in future years? I would hope so, but we are hardly a stagnant field.

Who Publishes? Explaining Member Publication Records

Standard explanations for publication success include quality of a person's doctoral training, collegial recognition of research, reinforcement by reward structures, departmental effects, and gender (see, e.g., Clemente 1973; Hogan 1986; Allison and Long 1990; Reskin 1977).

A person's doctoral training might affect later research productivity in several alternative ways (Clemente and Sturgis 1974, 288-290). First, some have argued that productivity is a function of a value system into which one is oriented in graduate school. Second, successful graduate programs offer productive scholars as role models to its students. Third, success breeds success by providing superior methodological and research training to students, which is enabled by that program's greater funding and resources. The importance of a person's graduate training was supported by Hogan's study of economists. Hogan (1986, 217) found that graduates of the top four economics programs accounted for 43 percent of the pages in the *American Economics Review*, the *Journal of Political Economy*, and the *Quarterly Journal of Economics* during the 1970s. Economists trained at the top 25 programs contributed more than 80 percent of the total. I use the National Research Council's ranking of doctorate-granting political science departments (see APSA 1995).

A scholar's institution also is able to encourage research productivity. This may come in the form of incentives given to faculty to be professionally active. As discussed by Reskin (1977), "organizations that value the production of knowledge should reinforce such behavior with professional rewards that will facilitate future research." Of course, universities and colleges are not equally committed to "production" as a principal goal. Liberal arts colleges, for instance, frequently place a higher priority on the teaching and mentoring of students than a major research-oriented state university. These institutions, thus, may reward excellence in the classroom or administrative service by giving them greater weight in determining pay raises and promotions compared to research productivity. In addition to the incentive structure at our institutions, universities and colleges may vary as to the quality of facilities and the presence of intellectual stimulation that would encourage research productivity (Allison and Long 1990).⁵ To measure each member's organizational context, I use the 2000 Carnegie Classification of Higher Education (<http://chronicle.com/stats/carnegie>). In particular, I identify members who are at universities with extensive or intensive doctoral programs.

A number of scholars have explored the role of gender on publication success and research productivity. It has been hypothesized that women face several impediments to establishing a successful research record, including institutional locations, domestic responsibilities and private-life identities, choice of research methods, and differential attribution of the

reason for rejection of a manuscript (Grant and Ward 1991; Wiley, Crittenden, and Birg 1979). The results of studies examining gender effects have been contradictory and inconclusive. Nevertheless, I identify members who are women.

Finally, I include two control variables. I created a series of variables that indicate each member's rank. This variable controls for the length of each member's period of research activity.⁶ I also created a variable that indicates whether the member has received a Ph.D. as his or her highest degree.

Table 3. Logit Model of Book and Journal Publication Records

Variable	Coefficient	Standard Error
Employed at University with Doctoral Programs	.99***	.22
Woman	-.22	.24
Full Professor	1.63***	.36
Associate Professor	1.48***	.48
Assistant Professor	.32	.35
Possesses Ph.D.	1.52***	.25
Constant	-1.37***	.22
Number of Observations-2 x Log LikelihoodPseudo-R ²	500507.85***.26	

* p<.05; ** p<.01; *** p<.001 (one-tailed test)

Before moving beyond the independent variables, I would be the first to acknowledge that there are other variables that I have not incorporated in my model. For instance, the literature on research productivity has included factors such as the time a person takes to complete his or her graduate training and whether a person publishes an article before graduation as an indicator of personal productivity (Clemente 1973). Unfortunately, I do not have these data and therefore cannot test these hypotheses.

The dependent variable is whether a member has published either a book or a journal article. It is measured as a dichotomous variable.

Using the full sample of 500 members, which includes students, political science faculty, law school faculty, and other individuals, the data confirm almost all of the hypothesized relationships (I omit the graduate program ranking from this model since it only applies to persons with a Ph.D.). Members at universities with extensive or intensive doctoral programs are more likely than other members to be research producers. The probability of publication is .84 for doctoral university faculty and .66 for other members.⁷ Full and associate professors are more likely to have publications than members who do not record their rank as a full, associate, or assistant professors. The probability of publications among full professors is .94, while it is .93 among associate professors. Finally, the probability of being a research producer increases 55.6% if a member possesses a Ph.D., compared to all other members. It is worth noting that research productivity is not significantly affected by gender.

To test the hypothesis regarding the quality of one's graduate education, I limit the analysis to members holding a Ph.D.⁸ As you can see in Table 4, the relationships among members who hold a Ph.D. are comparable to the results presented in Table 3. The one additional variable, which captures the rating given to each graduate program, provides support to the proposition that one's training affects a person's later productivity. For instance, graduates of Harvard's Political Science Department, the highest rated program in NRC's 1993 rankings, have a publishing probability of .95. This is in contrast to a probability of .89 for graduates of the University of Iowa, the 25th ranked program, or the probability of .57 for the graduates of the lowest rated program.

Table 4. Logit Model of Book and Journal Publication Records of Ph.D. Holders

Variable	Coefficient	Standard Error
Employed at University with Doctoral Programs	1.31***	.37
Woman	.35	.43
Full Professor	1.27***	.48
Associate Professor	.90*	.54
Assistant Professor	.02	.49
NRC Rating of Graduate Program	.62***	.19
Constant	-1.84**	.64
Number of Observations-2 x Log LikelihoodPseudo-R ²	233194.90***.19	

* p<.05; ** p<.01; *** p<.001 (one-tailed test)

This analysis has been conducted on the rather blunt instrument of publication of either a journal article or a book. Of course, there are many other ways to cut the publication data, and I will not burden you with endless tables. However, one does discern interesting differences in the causal factors that explain publication. The most notable is that the NRC rating of a person's graduate program is insignificant in models of publication of articles in major journals or trade books, while it is a significant factor in explaining publication of university press books. The distinction between producers of articles in the leading journals and authors of university press books seems to confirm stereotypes of book-departments and article-departments. Some sociologists have argued that the decision of which publishing format to pursue is a reflection of the kind of intellectual enterprise one believes the discipline to be (Wolfe 1990, 478-479). While we could debate the accuracy of this view or the relative merits of books and articles, it is worth noting that the evidence seems to support Wolfe's proposition. A perusal of the NRC rankings reveals that the top programs are much more likely to fit the mold of book departments (i.e., exclusive private universities in or near big cities on both coasts rather than land grant universities in the South and Midwest [Wolfe 1990]): Harvard, Yale, Stanford, Chicago, and Princeton.

Where Do We Stand?

This discussion may raise more questions than it answers. For instance, what research questions are members exploring? What type of evidence or analysis is being used in this research? Most compelling in my mind, however, is what is the health of research on law and courts. Are we making a contribution to the discipline of political science or to the study of law and legal systems more broadly? Answers to these questions would take more time and space than I have for this newsletter article. Nevertheless, a brief comparison of articles on the Supreme Court and another major institution, Congress, in the three major Political Science journals may prove useful.⁹

Between 1950 and 1979, the three major political science journals averaged a grand total of 1.5 articles exploring Supreme Court behavior each year. I think even those members who bemoan our "disproportionate" focus on the national high court would acknowledge that this is sparse coverage, albeit more than other courts received. The trend began to shift in the 1980s with more articles on the Supreme Court as available journal space increased (i.e., the total number of articles published in these three journals grew in the 1970s and 1980s). The increase became more substantial in the 1990s with the three major journals publishing a total of 5.3 articles per year (of course, that is still less than 2 articles per year in each journal or less than .5 article per journal issue). While we might celebrate increased attention to the Supreme Court and other topics of judicial politics, it is sobering to note that Supreme Court scholars have and continue to trail congressional scholars in article placement. The gap narrowed in the 1990s, but prior to that decade articles devoted to studies of congressional behavior outnumbered Supreme Court articles by more than 2 to 1.

What accounts for this disparity? It might be that journal editors are faced with a greater supply of manuscripts examining Congress. This could be due to more congressional scholars, although the Law and Courts Section is now quite a bit larger than the Legislative Studies Section (881 members to 622 members). It might be that congressional scholars are more apt to present their research results in articles than books. Of course, just as many landmark judicial works were published in book form (Glendon Schubert's *Judicial Mind* or C. Herman Pritchett's *Roosevelt Court*), congressional scholars like Richard Fenno, author of *Home Style*, published their research findings in books.

A less pleasant explanation is that students of Congress are more likely to produce research that advances the discipline. Certainly, there have been notable contributions by scholars studying Congress, like Kenneth Shepsle, that span the fields, but the same could be said of students of the Supreme Court. One could gain a glimpse of the relative cross-fertilization of ideas by examining citations patterns. My hunch, however, is that Supreme Court scholars would not fare especially well in that comparison. Yet, we examine questions and explore theoretical arguments that have currency outside the realm of law and courts.

What, if anything, can we do to advance the role of our subfield in the discipline beyond the plea to continue doing good work? [Warning: this is where I get up on my soapbox!] We should not be modest as authors or reviewers about the contribution of our studies to the discipline. At a practical level, we should continue to test theoretical arguments that are relevant to law and courts scholars and to others. By definition, our research will advance our fundamental knowledge in political science and not simply of something unique to the courts. As reviewers, we should refrain from diminishing work by stating that it is only of interest to a small group of scholars. After all, the Law and Courts Section is the second largest section in the APSA; we are not a small constituency within APSA. Let the editors and discipline know that Law and Courts members are a vital part of the political science research enterprise!

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Notes

1 These observations were not randomly selected, but arbitrarily selected – I gathered data on the first 500 members listed on the spreadsheet I received as section chair from APSA. I have tried to discern the ordering of members, but it appears to not follow a systematic pattern. The best I can suggest is that I most likely sampled members whose first names begin with a letter between A and M.

2 I should also note that I learned how to conduct efficient searches over the course of this project. My search for books benefited from this experience as I learned how to narrow my search by author's name. My search for articles was with a blunter search strategy where I simply inspected articles authored by individuals with a particular name. If a name was common, I counted only articles that seemed likely to be written by a political scientist. This meant that I excluded articles on landscaping and physics, for example. For this reason, I also excluded articles and books not written in English. I should also note that I excluded books that are not held by more than 10 libraries or that were published as research reports by government agencies or university institutes.

3 Hix ranked an extensive list of political science journals based on the number of citations to articles in each journal. Using this rating, the *American Political Science Review* landed the top spot among political science journals followed by the *American Journal of Political Science*, *International Organization*, *Journal of Politics*, *Journal of Conflict Resolution*, and so on. Goodson, Dillman, and Hira surveyed a sample of APSA members, asking them to assess the quality of books published by 65 different presses (1999, 257). The ratings were arrayed along a five-point continuum ranging from excellent (4) to poor (0). One can certainly quibble with the use of these rankings. Most notably, they do not include all publishing outlets despite fairly broad coverage. For instance, the journal rankings do not include multidisciplinary journals, including *Law & Society Review* or *Public Choice*. The book rankings omit the University of Kansas Press.

4 Hix has four groupings of journals – the top category includes APSR and AJPS; the second category includes JOP, BJPS, and *European Journal of Political Research*; the third category includes PSQ, APQ, and PRQ; the fourth category includes *Political Communication* and *Journal of Public Policy*.

5 It may be interesting to note that studies have found that the open yet competitive nature of our higher educational system prevents publication productivity to be concentrated at a few institutions (Dey, Milem, and Berger 1997).

6 I have data on the year that members received their highest degree. Unfortunately, though, a significant number of members (89 of the 500 members) did not report this information to APSA on their membership form.

7 In calculating these and other probabilities, I use the clarify routine in Stata and set other variables at their mean or modal values.

8 I also excluded 38 members who either attended graduate school outside the United States or did not provide APSA with the university from which they received their doctorate.

9 I searched JSTOR (www.jstor.org), an online archive of scholarly journal articles, for articles that contained the phrase “supreme court” in the abstract.

SYMPOSIUM ON *THE SUPREME COURT AND THE ATTITUDINAL MODEL* REVISITED

INTRODUCTION BY LEE EPSTEIN

ESSAYS CONTRIBUTED BY:

HOWARD GILLMAN, HERBERT M. KRITZER, JAMES F. SPRIGGS, II, STEPHANIE A. LINQUIST

RESPONSE BY JEFFREY A. SEGAL & HAROLD J. SPAETH

Editor's Note: This Symposium is based in part on an "Author Meet Critics" Roundtable at the 2003 Annual Meeting of the Midwest Political Science Association, Chicago, April 3, 2003. The panelists substantially revised and formalized their remarks for this issue of *Law & Courts*.

Introduction

Lee Epstein

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In the Spring 1994 edition of *Law and Courts*,¹ four distinguished scholars—Larry Baum, Gerry Rosenberg, Jack Knight, and Rogers Smith—drew our attention to voids in Jeffrey A. Segal and Harold J. Spaeth's *The Supreme Court and the Attitudinal Model (SCAM)*.² Among the most noteworthy:

- (1) the failure of the authors to undertake a systematic assessment of whether legal considerations influence the votes of justices (Baum, Rosenberg);
- (2) the lack of serious attention to the constraints the "democratic process" may impose on the Court (Knight); and
- (3) a disregard for the views of "sophisticated" contemporary legal scholars (Rosenberg, Smith).

Now, roughly a decade later, Segal and Spaeth seek to fill these and other holes. In *The Supreme Court and the Attitudinal Model Revisited (SCAMR)*,³ they undertake that systematic assessment of legal considerations (or, more precisely, of precedent); they are now attentive to the separation-of-powers system; and Dworkin and Ackerman both earn entries in book's index. Of course the bottom line—encapsulated in the oft-quoted line, "Rehnquist votes the way he does because he is extremely liberal; Marshall voted the way he did because he was extremely conservative"⁴—remains. But, to get there, the authors were not unmindful of Gerry Rosenberg's challenge in his 1994 critique of *SCAM*: "While Segal and Spaeth may be right...they have only begun to make the case."

Have the authors made it in *SCAMR*? That is the overarching question another group of distinguished scholars addressed in April 2003, at the annual meeting of the Midwest Political Science Association. Their answers, published below, may be mixed but they are hardly dull. Quite the opposite: The responses make all too clear that, regardless of our own theoretical and methodological predilections, the attitudinal model continues to provide the fodder for serious, searching, and wide-ranging debates about a primary focus of so much of our energies—judicial decisions.

Why the attitudinal model generates such a reaction, I am not sure. It is, after all, a rather modest, even unambitious, approach to judicial decisions. It simply says this: the votes of judges on the merits of cases will reflect their sincerely-held ideological (read: liberal or conservative) attitudes over particular matters of public policy *if*: (1) those judges lack political

or electoral accountability, (2) have no ambition for higher office, and (3) serve on a court of last resort that controls its own agenda. The primary (perhaps even sole, depending on which author you ask) goal of model is to explain the *votes justices of the U.S. Supreme Court* cast on the *merits* of cases. It does not attempt to account for the votes of judges on other American courts because no jurists other than U.S. Supreme Court justices meet the conditions of the model. And it only attempts to account for votes cast on the merits of cases; few, if any (again depending on the author), other judicial choices come under its reach.⁵

Then again, what the model does do, it does well. If we were limited to developing an account of the votes of but one U.S. tribunal, we'd probably pick the Supreme Court. And if we were inclined to assess that model against data, we would be delighted if it performed as well as the attitudinal account. Indeed, these days it is difficult to argue credibly that the model utterly fails to perform its primary task. The evidence in support of its one observable implication—namely, that the policy preferences of the justices help predict their merits votes—is overwhelmingly in its favor.⁶

And therein perhaps lies the rub: The model performs so well that it inevitably evokes the sorts of responses Rosenberg quoted in his 1994 critique of *SCAM*: Segal and Spaeth's "conclusions undermine the integrity of the Supreme Court...creating the risk of a crisis in American politics; attitudinal studies are full of 'danger' and should not be done." Or the sorts of reactions that run rampant in the legal academy: that the model "caricatures the Supreme Court as nothing more than a body of nine legislators unconstrained by the need to stand for election;"⁷ that it is "unappealing, at best, from a normative perspective;"⁸ and that it contains "methodological, contextual, and interpretive problems that limit [its] value."⁹

Whatever the explanation for the attitudinal model's rather uncanny ability to generate passionate rebukes and equally passionate defenses, one thing seems a near certainty: The 2003 "critics" have raised enough questions and identified enough holes that in a decade or so yet another distinguished panel will be writing in these pages—this time, on *SCAM III*.

Notes

1 Available <http://www.law.nyu.edu/lawcourts/pubs/newsletter/spring94.pdf> (last accessed on April 25, 2003).

2 Jeffrey A Segal & Harold J. Spaeth, *The Supreme Court and the Attitudinal Model* (1993).

3 Jeffrey A. Segal & Harold J. Spaeth, *The Supreme Court and the Attitudinal Model Revisited* (2002).

4 Segal & Spaeth, *The Supreme Court and the Attitudinal Model*, p. 65; Segal & Spaeth, *The Supreme Court and the Attitudinal Model Revisited*, p. 86.

5 My colleagues, Jack Knight and Andrew D. Martin, and I make these general points in "Childress Symposium: The Political (Science) Context of Judging," 47 *Saint Louis University Law Journal* (2003), forthcoming

6 Of course some of this evidence resides in *SCAMR* but more is accumulating as I write. For information on an on-going forecasting project—one that grounds its logic in the attitudinal model—visit: <http://wusct.wustl.edu/> (last accessed on April 25, 2003).

7 Thomas W. Merrill, "Childress Symposium: The Making of the Second Rehnquist Court," 47 *Saint Louis University Law Journal* (2003), forthcoming (summarizing the views of law professors and lawyers on the attitudinal model).

8 Stephen M. Bainbridge and G. Mitu Gulati, "How Do Judges Maximize? (The Same Way Everybody Else Does—Boundedly): Rules of Thumb in Securities Fraud Opinions." 51 *Emory Law Journal* 83 (2002).

9 Richard J. Pierce, Jr. "Is Standing Law or Politics?" 77 *North Carolina Law Review* 1741 (expressing the sentiments of, among others, Frank B. Cross, "Political Science and the New Legal Realism: A Case of Unfortunate Interdisciplinary Ignorance." 92 *Northwestern Law Review* 251 (1997)).

SEPARATING THE WHEAT FROM THE CHAFF IN *THE SUPREME COURT AND THE ATTITUDINAL MODEL* *REVISITED*

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As was the case with its predecessor volume,¹ the lessons in judicial politics presented in *The Supreme Court and the Attitudinal Model Revisited* are simultaneously axiomatic and banal. The arguments are sometimes impressively cautious but many of the central claims are overblown. It is a distinctive combination of rigorous data (carefully selected to focus on the cases that work best for the model) and idiosyncratic stories chosen mostly to mock opponents. On one page it will display the great virtues of scientific dispassion and fair-mindedness, and on the next it will infuriate the thin-skinned with explosions of condescension. It takes pains to enter into dialogue with its surrounding community of scholars, but it does so with the same humility that characterizes the Bush administration's foreign policy. For many in our field the model operates as both science and as religion, in the sense that its central tenets are accepted as articles of faith.² For better or worse it is a canonical work in contemporary public law scholarship—containing one very important idea (which is one more than most), updates of vital descriptive information, very useful and smart summaries and commentaries on the most recent behavioralist and rational choice literatures, and a fierce determination to undermine critics. Yet, for all its considerable virtues, it is likely to have almost no influence beyond the faithful.

While I think of myself as one of the faithful, I also understand that the priests of this faith are quite demanding, and it is likely that they would consider me insufficiently orthodox. (Like Garry Wills among conservative Catholics, I probably have the kind of faith that the orthodox consider heretical.) In an effort to establish my credentials as one of the faithful I am willing to confess that Supreme Court justices often (but not always) associate themselves with legal views that carefully trace their conventional political ideologies, at least when the legal disputes are over the sorts of issues that divide ideologues. Of course, this kernel of truth is not worth arguing about because scholars of law and courts have known this for a very long time. Thomas Reed Powell wrote about the influence of a justice's personal policy preferences on the constitutionality of minimum wage legislation in the early 1920s.³ During the New Deal the whole country was told that conservative justices voted one way and more liberal-minded justices voted a different way. Almost forty years ago, Martin Shapiro (who was sympathetic to behavioralism but wisely practiced more traditional methods) declared that most of the "myths" that attitudinalists claimed to "puncture" involved claims "that no well informed traditionalist currently believes," and "after a thorough review of the new literature, I cannot honestly say that it has so far made any startling new contribution to our knowledge of courts and judges. Its value has largely been in presenting further evidence for already well formulated hypotheses."⁴ Every modern constitutional law professor knows that some judges are liberal and some are conservative; the more ambitious and politically-connected among them shape their work to establish their own bona fides as reliable ideologues. Every lawyer practicing before the Court knows this and constructs arguments with an eye on the divisions among the justices. Presidents, senators, and interest groups are also in on the non-secret, and virtually all of them figured it out without reading one word of Segal and Spaeth.⁵

But Spaeth and Segal want more from us than an acknowledgment that Supreme Court decision making is sometimes influenced by ideology. They also want readers to accept that their "attitudinal model" represents the one and only truly scientific theory of U.S. Supreme Court decision making, and that law (or what they call the "legal model") has almost nothing to do with how the justices behave. Thus, when they say that "Rehnquist votes the way he does because he is

extremely conservative [and] Marshall voted the way he did because he was extremely liberal” [p.86] they are not attempting to state a modest truism; they are announcing what they consider to be the central reality of Supreme Court politics. The implication is that only those who are naïve, ignorant, or hopeless romantics would insist that justices are something other than unrestrained (and sometimes even shameless) promoters of their personal policy preferences. It is a bold and aggressive claim and it deserves to be addressed head on.

While it is pointless to dispute the weak or minimalist version of the attitudinal model—that is, that political ideology often influences Supreme Court decision making in certain categories of cases—I want to suggest that the rest of their orthodoxy is entirely dispensable.

There is a lot in the book that readers will dispense with without thinking much about it.⁶ The book is packed with questionable observations about matters that have nothing to do with the central model. When Spaeth and Segal ask, “Why do American judges have such virtually untrammelled policy making authority?,” we do not get a careful test of competing hypotheses; we get 12 pages on “fundamental law, distrust of government power, federalism, separation of powers, and judicial review,” none of which is an especially convincing “reason for judicial policy making” [p.12]. The authors also share their view that the “reality of American life” is to have a “schizoid orientation” toward change [p.13]. There is no reason to complain about these sorts of observations—they provide much of the book’s seasoning (with the rest coming from the authors’ sarcastic analyses of judicial opinions and flamboyant vocabulary)—but it is difficult to take seriously all the solemn talk about scientific method when much of this book is made up of these sorts of seat-of-the-pants comments. Also, because no one will read the book for its “political history of the Court,” it is hardly worth spending time debating whether that chapter is sufficiently well-informed by contemporary historical scholarship.⁷

Also disposable is their demand that scholars worship at the altar of quantification, hypothesis testing, and falsification. I understand there is no arguing with them about this; it is central to their identity as professionals. Still, given how much the “attitudinal model” is associated with judicial “behavioralism,” it is worth emphasizing that one can accept some of the substantive conclusions of attitudinalism without associating oneself with any particular research tradition. In other words, acknowledging the influence of ideology in Supreme Court decision making does not make you a behavioralist. Happily, this feature of their orthodoxy is easy enough to jettison, because Spaeth and Segal are more methodologically eclectic than they let on. Their previous book, *Majority Rule or Minority Will*,⁸ was chock-full of wonderful and persuasive traditional interpretations of judicial opinions. In this book, they are willing to amass interpretive data when it is useful, such as in response to Ron Kahn’s unpersuasive claim that O’Connor voted as she did in *Casey* because she respected precedent rather than because she has a more moderate position on abortion. (The chart on p.293, designed to refute Kahn’s argument, reflects very solid interpretive scholarship.) Even Segal and Spaeth know that you cannot flatten the story of *Bush v. Gore* to a simple correlation between attitudes and votes, and their overview is pretty good [pp.171-3]. Still, given their extensive reliance on historical and interpretive methods—not just in these sorts of examples, but more fundamentally as the basis for coding all their data—it is unfortunate that their methodological posturing all-too-often acts as a conversation-stopper, allowing them (and their co-horts in the discipline) simply to ignore lots of non-quantitative scholarship that might cast doubt on some of their assumptions and findings.

This leads us to the central question: what exactly are their central findings?

Their principal claim is that their model proves that Supreme Court justices are unconstrained promoters of their personal policy preferences and thus their voting behavior reflects their conventional political ideologies. The claim is not qualified, but it should be. In what we can now call SCAM I there were some wonderful tables (6.6-6.8) summarizing judicial voting patterns across a wide variety of issue areas, and they were very useful because readers could see that the influence of ideology was greatest when the Court addressed those legal issues that divided ideologues but was less strong (and sometimes invisible) in other types of cases; moreover, the tables showed clearly that the justices did not have the same predispositions toward/against liberalism on every issue area.⁹ Those tables made it clear that there was more to say about Supreme Court decision making than that the justices voted their ideologies. Those tables are now gone. The authors still

insist that Supreme Court decision making is exclusively ideological, but this time the evidence they present is made up almost entirely of cases that divide ideologues. Time and time again the data set focuses on civil liberties cases, civil rights cases, criminal procedure cases, or economic regulation. One waits in vain for some explanation of what this model has to say about (for example) the Court's approach to something like separation of powers cases.

In fact, in this long book, the core evidence in favor of the attitudinal model with respect to decisions on the merits is outlined in just *two little paragraphs* on pp.322-323, and summarized in Table 8.2. Spaeth and Segal compare the (unfortunately imprecise but apparently close enough) Segal-Cover scores¹⁰ (measuring judicial ideology) to... What? All Supreme Court cases? A random sample of all Supreme Court decisions? A random sample of all Supreme Court decisions since Warren? No. The Segal-Cover scores are correlated with "*all formally decided civil liberties cases*" decided from 1953 through 1999. In case you are interested the correlation is 0.76 with an $r^2 = 0.57$. Believe it or not, that is the sum and substance of the book's proof for the model, as applied to final decisions on the merits, where the model is said to work best. As if the authors sense that a close reader might begin to worry that the model is actually of more limited scope than we have been led to believe they quickly add that they have also successfully predicted the majority and dissenting coalitions in 19 of 23 death penalty cases [p.324].

So far there should be little for even the most chastened anti-attitudinalist to take issue with, at least as a descriptive matter. But we are far from a general model of Supreme Court decision making. Even if (just to play ball) we stick with the reported results in these highly ideological cases, there is reason to think that there is more going on than justices just voting their policy preferences. For example, in another part of the book, we find out that Marshall votes to strike down 21.4% of liberal laws, Brennan strikes down 36.4% of liberal laws, and Rehnquist only strikes down 40% of liberal laws; similarly, Thomas strikes down 40% of conservative laws and Scalia strikes down 37.8% of conservative laws [p.416]. Segal and Spaeth interpret this data as consistent with their model, since the rate of hostility to certain laws seems partially related to a justices' ideology, but it also leaves one to wonder why some of these justices are as hostile as they are toward laws that they should be predisposed to uphold.

The patient reader has to wait until p.417 before being told that there are no obvious ideological patterns to explain the approach of Rehnquist Court justices to federal agency review: "...the substantially participating Rehnquist Court justices, with only four exceptions, support agency action whether liberal or conservative with at least 50 percent of their votes." There are similarly crummy results (crummy for the model) for Supreme Court review of federal agency action affecting civil rights and liberties between 1986-1999 [pp.419-20]. The role of ideology in economic federalism cases is also much less striking [p.421].

If the trumpets sounded as loudly at the arrival of these unimpressive results as when civil liberties cases are reported we might all come to understand that, even on its own terms, *the model is not a general explanation for all Supreme Court decision making; at best it is a partial explanation for a subset of votes in cases that raise the most ideologically divisive issues of law*. Other political scientists who refer to the model would be well advised to incorporate this moderating point in their own work, so as not to leave a misleading impression about the state of the evidence.¹¹ In fact, it would be a service to the discipline if Segal and Spaeth would provide statistics on the correlation between Segal-Cover scores and a random sample of all Supreme Court decisions over the past half century. That would put all of us in a much better position to get a balanced perspective on the role of conventional political ideology on the justices' behavior.

(I was tempted in these comments to raise the methodological complaint about whether it is optimal for the person who has made the attitudinal model his life's work [Harold Spaeth] to be the one deciding how to code the ideological direction of the justices' votes. After all, Segal and Spaeth warn us about the effects of motivated reasoning [p.433], and Spaeth certainly has the motivation to ensure that the votes of justices he knows to be "conservative" are coded "conservative" as much as possible; it makes the correlations of the model work better. This may help explain why that incredibly liberal equal protection decision by the majority in *Bush v. Gore* was coded by Spaeth as "conservative"—any other code would lower the correlations. But that is a complaint for methodological purists to worry about. So maybe the numbers are not perfect; maybe the books have been cooked a little bit to make the correlations a little better; maybe other coders would have coded

ideological direction differently in some cases, especially if they had no dog in this fight, or were forced to code the case before the justices revealed their votes, or evaluated ideological direction on a 1-5 scale rather than as a dichotomous variable. The main point is that, once it is understood that the model only applies to the resolution of ideologically-divisive legal issues, there should be little doubt about the fundamental accuracy of the results. Others who have more of a stake in the niceties of scientific procedure are free to pursue this coding issue at their convenience.¹²⁾

Finally, and maybe most importantly, we may also dispose of their claim that we should stop talking as if law matters. A reexamination of the “legal model” is one of the principal differences between this version of their argument and the version elaborated in SCAMI. However, the authors still have an almost impossible time formulating a version of the “legal model” that anybody believes. They can find a few legal philosophers who make claims about legal determinacy, but they also seem to acknowledge that most contemporary scholars do not believe law operates anything like the formalists thought [p.48 n.11, citing Frank Cross]. They ridicule Judges Harry Edwards and Patricia Wald when they insist that personal policy preferences have almost nothing to do with the way that most court of appeals decisions are handed down, but they also acknowledge that the attitudinal model does not work as well for the court of appeals. Moreover, both of these judges acknowledge that personal ideology has a greater impact on the U.S. Supreme Court. Thus, it is unclear that these judges have ever said anything inconsistent with Segal and Spaeth’s evidence.¹³ Segal and Spaeth also claim that Dworkin makes empirical (and not just normative) statements about the influence of law, but their examples are statements of his such as, “judges characteristically feel an obligation to give what I call ‘gravitational force’ to past decisions,” which are empirical statements about how judges *feel* and thus have nothing to do with the sort of causal theory of voting behavior that they want to use as a punching bag [p.50].¹⁴ By p.432 this strained collection of quotes transforms into the claim that some scholars (apparently myself included) “argue that the Court merely follows established legal principles in deciding cases”; but again, this is a phantom opponent. No reasonable person believes this, and so disproving this sort of claim makes no headway against any actual argument about how the law may or may not influence judicial decision making.

The truth of the matter is that most scholars who think law matters also acknowledge that judges with different ideologies will have different understandings of the law.¹⁵ As I argued in an earlier review of Segal and Spaeth’s approach to the legal model, “These scholars do not reject behavioralist descriptions of decision-making patterns, but they insist that behavioralists should not infer that these patterns mean an absence of legal motivations....”¹⁶ Thus, alongside attitudinalism (and not in contradiction to it), there should be room for the claim that, when it comes to legal disputes that divide ideologies, the justices often translate their political attitudes into relatively stable jurisprudential regimes, and that, in some non-trivial way, these legal regimes are taken seriously.¹⁷

In that same review of the way in which behavioralists conceptualize “the law” I suggested that the influence of law may be present whenever a judge makes a decision based on a good-faith understanding of the law, and conversely, that legal influences are absent when judges disregard their best understanding of the law in favor of other considerations.¹⁸ This leaves open the possibility that judges may draw on ideological considerations in cases where the law seems to allow for such considerations, and if this is so then ideology may be seen (sometimes) as “internal” to the law rather than as a competing independent variable. Toward the end of SCAMR this suggestion is characterized by Spaeth and Segal as a “strategic retreat” [p.432] for scholars who believe that law (sometimes) matters, one that “fails to appreciate the fundamental influence of motivated reasoning in human decision making” [p.433]. They claim that “the attitudinal position on motivated reasoning is one of agnosticism” because it is nonfalsifiable [p.433]. Well, if turnabout is fair play, then let me say that I view their agnosticism as a welcome strategic retreat by attitudinalists on the vital question of whether law still matters. Among other things it should mean that attitudinalists will stop saying that their science proves that judges do not take law seriously, or that judges lie when they claim that they feel a sense of fidelity to law. Apparently, Segal and Spaeth no longer have a position on these questions, and so other scholars should no longer assume that the model presented in this book is inconsistent with the idea of legal obligation.

And this leads to my final point, about *Bush v. Gore*. Segal and Spaeth open the book with the case, and by the end of their first paragraph they trumpet their noteworthy 1993 prediction that the justices may vote their political preferences if they were ever asked to resolve a presidential election dispute. The authors rightly suggest (based on interpretive-doctrinal

evidence) that it is unlikely that the conservative majority in that case actually believed what they wrote, and thus made their decision based on political considerations rather than legal considerations.¹⁹ They want to treat this case, not as exceptional, but as routine; in fact, as exemplary for their entire discussion. But despite their prediction, this has to be a mistake. While Segal and Spaeth claim to be agnostic about the justices' attitude toward law, for the rest of us it makes all the difference in the world whether we have a model of Supreme Court politics that asserts that justices normally translate their sincere preferences into legal policy or whether it asserts that justices normally announce legal policies they do not sincerely believe in order to help out favored litigants. It is the difference between acknowledging that good-faith judging is often inherently ideological—which is perfectly consistent with a belief in the rule of law and with an insistence that judges conduct themselves in an ethical way—and believing that we should all consider the lies of those hard-ball partisan activists to be a normal and inevitable feature of our system of government. I hope Segal and Spaeth are content with the former point. If instead they insist on interpreting their attitudinal model in a way that makes a mockery of any outrage at *Bush v. Gore*, or that undermines our more general expectation that judges decide cases in accordance with their best understanding of law, then despite all their extraordinary talents as social scientists, we would all be better off dispensing with the model altogether.

NOTES

- 1 *The Supreme Court and the Attitudinal Model* (New York: Cambridge University Press, 1993). There may be some debate whether “SCAMR” (which, according to the authors, is pronounced “scammer”) volume should be considered merely a second edition to the original or (as the authors prefer) a new volume. To the authors' credit this new version has been substantially updated and reworked. However, the template of the original is still unmistakably present in the core argument, the tone of the volume, and the structure of the presentation.
- 2 For a similar characterization of the “rhetoric of fundamental belief” in another branch of the social sciences, see Robert H. Nelson, *Economics as Religion: From Samuelson to Chicago and Beyond* (University Park, PA: Penn State Press, 2003).
- 3 See Thomas Reed Powell, “The Judiciality of Minimum-Wage Legislation,” *Harvard Law Review* 37 (1924):545-73.
- 4 Martin Shapiro, “Political Jurisprudence,” *Kentucky Law Journal* 52 (1964): 294-343, 313, 329. Shapiro finished this thought by noting that “it must be insisted that this is not a negligible accomplishment” (*ibid.*, 329).
- 5 Segal and Spaeth are able to find quotes from various people (mostly judges and a handful of legal theorists) who talk as if they believe something different, but it is always possible to find people who say silly or self-serving things. The authors acknowledge [p.6] that it is getting increasingly difficult to find people who need to learn their lesson.
- 6 In addition to the dispensable material I will be discussing there is an interesting central tension in their theory, which is that a model premised on the justices' desire to pursue policy preferences focuses exclusively, not on the *policies* they announce, but on their *votes*—a variable that tells us surprisingly little about the policies being promulgated by the Court. In light of their dependent variable one might mistakenly conclude that the attitudinal model is built on the assumption that the main goal of Supreme Court justices is to cast votes—that the justices intend merely to send signals like “the state wins” rather than signals about (for example) the extent to which state governments should be vulnerable to lawsuits. Jim Spriggs elaborated this point in his presentation during the “SCAMR” roundtable. He argued that flattening judicial behavior into a dichotomous choice ignored the actual scope of the justices' decision making, since even within “liberal” and “conservative” options there were ranges of policy options available to the justices. In *Crafting Law on the Supreme Court: The Collegial Game* (New York: Cambridge University Press, 2000), Forrest Maltzman, James F. Spriggs II, and Paul J. Wahlbeck illustrate how the justices spend most of their time working on these issues.
- 7 I am sure there are explanations based on the authors' personal preferences (as well as the sociology of the discipline) for why (admittedly top-notch) political scientists who do not specialize in historical research, such as Lee Epstein, Jack

Knight, and Greg Caldeira, earn a citation in the chapter on the history of the Court but not well-regarded political scientists who specialize in constitutional history, such as Mark Graber or Keith Whittington. Barry Cushman, author of *Rethinking the New Deal Court: The Structure of a Constitutional Revolution* (New York: Oxford University Press, 1998), may be the only contemporary Court historian who is cited [p.139 n.91], and that is because he helps the authors underscore their larger point about how the justices rarely engage in a strategic retreat (even during the New Deal). Needless to say, Cushman's arguments about the justices' fidelity to preexisting jurisprudential traditions is not addressed.

8 Harold J. Spaeth and Jeffrey A. Segal, *Majority Rule or Minority Will: Adherence to Precedent on the U.S. Supreme Court* (New York: Cambridge University Press, 1993).

9 For more on these tables see Craig S. McClure, "A Critical Analysis of the Attitudinal Variable and Data Reconfiguration of Tables 6.6-6.8 in: The Supreme Court and the Attitudinal Model by Segal and Spaeth," paper presented at the annual meeting of the Midwest Political Science Association, Chicago, IL, April 3-6, 2003.

10 For more on the Segal-Cover measurement of the justices' ideologies (based on newspaper characterizations of a nominee's political attitudes), see Lee Epstein and Carol Mershon, "Measuring Political Preferences," *American Journal of Political Science* 40 (1996): 261-294. For an alternative approach to the measurement of judicial ideology, which takes into account the possibility that a justice's estimated ideal points may change over time, see Andrew Martin and Kevin Quinn, "Bayesian Learning About Ideal Points of U.S. Supreme Court Justices, 1953-1999" (APSA paper, 2001).

11 In his response to an earlier version of this argument at the MPSA roundtable, Jeff Segal acknowledged that the current version of the model is not a general model of all Supreme Court decision making; instead it explains civil liberties cases and related cases that raise legal issues that typically divided ideologues.

12 In response to questions about this issue during the MPSA roundtable Spaeth acknowledged that he exercised discretion in determining whether to code votes as "conservative" or "liberal." When asked if he followed any rule in deciding whether to focus on the justices' attitude toward the "object" of the litigation (such as the identity of the litigant) or the "situation" of the litigation, he answered that there was no rule; it was an "ad hoc" decision on his part. The issue comes up in cases like *Bush v. Gore*, where one could code the result "liberal" if one chose to focus on the Court's equal protection policy or "conservative" if one chose to focus on who won the case. In light of the influence of the Spaeth Database, it might be useful for someone to determine whether Spaeth's coding of the ideological direction of the justices' votes can be reproduced. Alternatively, Spaeth may be encouraged to make decisions about the ideological direction of certain votes before the justices reveal their positions. For an example of a coding procedure that determines the ideological direction of a vote in a case even before the justices participate in oral arguments, see The Supreme Court Forecasting Project at <http://wusct.wustl.edu>.

13 In "Collegiality and Decision Making on the D.C. Circuit," *Virginia Law Review* 84 1998:1335, Judge Harry T. Edwards reports that during the three year period from 1995-1997 the dissent rate on the D.C. Circuit in all dispositions "has been between 2% and 3%. In cases where the court published an opinion, the dissent rate was between 11% and 13%. Of those dissents, between one-third and two-thirds involved cases in which the dissenter and the two judges in the majority were appointed by Presidents of different parties. In aggregate, only 47 out of 94 dissents over the last three years followed presumed 'party' lines. Thus, even where there was dissent, the dissent only occurred along presumed 'party' lines around half of the time. This is, in my view, extremely strong prima facie evidence of consensus among judges about the correct judgment in a given case."

14 Similar attempts to find opponents also fail. They cite Ackerman for the claim that *Lochner* era justices attempted to "develop a comprehensive synthesis of the meaning of the Founding and Reconstruction out of the available legal materials," and they cite me for the claim that these justices were "giving voice to the founders' conception of appropriate and inappropriate policymaking" [pp.50-51]. But these are interpretive statements about how the jurisprudence related to other bodies of thought, not causal arguments about the Court's decision making. Just so that it is clear: while I think that the *Lochner* justices drew on preexisting traditions in constructing their jurisprudence, I also believe that they were motivated to do that because they were conservatives. When more progressive justices were appointed they drew on

other traditions of thought. See “The Collapse of Constitutional Originalism and the Rise of the Notion of the ‘Living Constitution’ in the Course of American State-Building,” *Studies in American Political Development* 11 (1997):191-247, and “How Political Parties Can Use the Courts to Advance Their Agendas: Federal Courts in the United States, 1875-1891,” *American Political Science Review* 96 (2002):511-524.

15 As Dworkin put it in *Freedom’s Law: The Moral Reading of the American Constitution* (Cambridge: Harvard University Press), p.37, “It is no surprise, or occasion for ridicule or suspicion, that a constitutional theory [and interpretive judgments made from it] reflect[] a moral stance. It would be an occasion for surprise—and ridicule—if it did not. Only an unbelievably crude form of legal positivism—a form disowned by the foremost positivist of this century, Herbert Hart—could produce that kind of insulation.”

16 “What’s Law Got to Do With It? Judicial Behavioralists Test the ‘Legal Model’ of Judicial Decision Making,” *Law and Social Inquiry* 26 (2001):465-504.

17 This is what I tried to say in *The Constitution Besieged: The Rise and Demise of Lochner Era Police Powers Jurisprudence* (Durham: Duke University Press, 1993); it is what Mark J. Richards and Herbert M. Kritzer suggested more recently in “Jurisprudential Regimes in Supreme Court Decision Making,” *American Political Science Review* 96 (2002):305-320; and it’s what Jeff Segal used to say as well, until his search and seizure project was made politically correct through absorption into simple attitudinalism [pp.316-320].

18 For a terrific analysis of the influence of legal and non-legal considerations in the decisions of each of the justices in the case of *Brown v. Board of Education*, see Michael J. Klarman, “*Brown v. Board of Education*: Law or Politics?,” University of Virginia School of Law Research Paper No. 02-11, available for download from the Social Science Research Network Electronic Paper Collection at http://ssrn.com/abstract_id=353361.

19 I agree that there is plenty of evidence for this position. See Howard Gillman, *The Votes That Counted: How the Court Decided the 2000 Presidential Election* (Chicago: University of Chicago Press, 2001).

HAVE SEGAL AND SPAETH FINALLY DRIVEN A STAKE THROUGH THE HEART OF THE LEGAL MODEL?

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Segal and Spaeth's (1993) *The Supreme Court and the Attitudinal Model* (SCAM) has deservedly been a much discussed and debated book. Importantly, the key point of controversy is not the argument that attitudes are extremely important in explaining what the Supreme Court decides, but on the very specific point that "attitudinal model is a complete and adequate model of the Supreme Court's decisions on the merits" (1994, 11).

I think it is crucial to note that in neither SCAM nor *The Supreme Court and the Attitudinal Model Revisited* (SCAM-R) do Segal and Spaeth argue that *attitudes* constitute a complete and adequate explanation of the Supreme Court's decisions on the merits. Rather they argue that the "attitudinal model" is a complete and adequate explanation/model. Importantly, the attitudinal model is not just attitudes, but combines justices' attitudes and key factual elements about the case. Those factual elements, in Segal and Spaeth's view, cannot be labeled as legal factors because they could just as easily be attitudinal elements that define a case's position in an attitudinal space.

Part of what made *The Supreme Court and the Attitudinal Model* both fascinating and frustrating was that it falls somewhere between a piece of very well done social science and a legal brief. The many pieces that have been written challenging Segal and Spaeth's bold contention about the attitudinal models adequacy are in some ways akin to reply briefs. *The Supreme Court and the Attitudinal Model Revisited*, is, as the title implies, Segal and Spaeth's rebuttal brief, bringing together earlier analyses and arguments they have made in reply to their critics.

The rebuttal brief reprises the core arguments of the original, presenting those arguments in a more concise and focused fashion. Segal and Spaeth's rebuttals focus on the arguments of two groups of critics: those who argue that Segal and Spaeth are incorrect in dismissing law as an influence and those who argue that Segal and Spaeth are incorrect in dismissing strategic concerns, particularly concerns about the justices' attention to the response of actors with the power to trump the justices' decisions.

I want to focus on the legal. I leave the strategic issue to others contributing to this symposium who are better qualified to comment on that aspect of the revisitation.

Let me first say that I find it best to think about modeling the decisions of the justices as a two-level phenomenon. At one level are case-specific factors including factual elements, amicus behavior, strategic concerns, and possibly legal influences. At the other level are justice-specific factors, which ultimately boil down to attitudes. When the justices differ on a given case, those differences must eventually link back to attitudes to the extent that there is anything systematic about those differences. Everything else is a case-level factor, and cannot explain differences among justices in a specific case except in so far as those case-level factors are mediated through the justices' attitudes. Note, however, that I have not specifically limited attitudes to those dealing with the justices' policy preferences. There may also be attitudes regarding the level of deference due to the other branches, attitudes about the deference due to earlier decisions of the Court, etc.

Thus, with one caveat, I would be inclined to accept an argument that some type of attitudinal model is a complete model of the *differences* among justices in how they decide individual cases. The one caveat might be for the median justice in a given case, who *might* be attentive to whether or not her preference would be overturned by Congress, and thus might be willing to calibrate the nature of her difference with her colleagues to avoid being overturned. I see no way that law, as a factor that must exist at the case level if it is to be conceptually distinct from attitudes, can *independently* explain differences among the justices in a given case.

One might take this position and advance the argument that what is important in studying Supreme Court decision making are differences among the justices, and hence I am accepting Segal and Spaeth's core argument. This would be incorrect, because while individual differences are important, differences across cases are probably more important from an institutional perspective, because the institutional role of the Court is to provide guidance to other actors, and those actors are most concerned with understanding what the Court is saying about the central issues upon which the Court has spoken (see Shapiro 1964).

With that said, let me turn to law as an explanation for Supreme Court decision making on the merits. In the revisitation, Segal and Spaeth fine tune their explanation for their rejection of what I would call "legal determinism": judicial decisions are controlled by precedent, plain meaning, and/or intent of the drafters. To their refinement of their earlier discussion, they add a reprise of the analyses of conformance to precedent published in the *American Journal of Political Science* (Segal and Spaeth 1996) and extended in *Majority Rule or Minority Will* (Spaeth and Segal 1999). The question is whether the arguments and analyses presented by Segal and Spaeth are sufficient to eliminate law as an important influence in decision making on the merits?

Mark Richards and I (2002) have suggested an alternative way of thinking about the role of law in the context of Supreme Court decision making. We accept much of Segal and Spaeth's institutional argument about the situation of the Supreme Court, and we accept much of their argument about why legal determinism fails as an explanation for most of what the Supreme Court decides. Specifically, the cases the Court decides it decides precisely because precedent, plain meaning, and intent fail to provide clear answers about what the law demands. However, we part company from Segal and Spaeth in their position that the institutional situation of the Court means that the justices can and do essentially ignore law.

There are extremely important institutional reasons why the justices should not ignore law in their decision making. Specifically, as noted above, the influence of the Court turns in significant part on the guidance it provides for other actors. For that guidance to be effective, the Court must decide cases in consistent ways and its decisions must reflect the guidance provided for other actors.

To this end, Richards and I argue, the Court creates decision structures to guide both itself and other actors. At the Supreme Court level, law is to be found in these decision structures, which we label "jurisprudential regimes." Such regimes constitute another way of looking at the influence of precedent, a way that is consistent with the fact that the Supreme Court seldom revisits the same issue in the same form, and that a decision to revisit an issue directly occurs in situations where the Court is least likely to uphold that precedent established by that prior decision.

Detecting jurisprudential regimes requires that regimes change so that one can compare patterns in decision making under different regimes. The expectation is that the influences on the justices will differ if jurisprudential regimes change. In our *APSR* article, we applied our argument about jurisprudential regimes to free expression cases and found patterns that support the argument we advance.

What happens if we take our construct of jurisprudential regimes and apply it to the area that has been a central focus in the analyses presented by Segal and Spaeth, search and seizure? We hypothesize that during the post-*Mapp* period, there were significant changes in how the Court viewed search and seizure cases around 1983-84:

- There was a movement toward assessing the presence of "probable cause" using the "totality of the circumstances" rather than more specific requirements established soon after *Mapp v. Ohio* (1961). Specifically, *Illinois v. Gates* (1983) began the move toward a new jurisprudential regime by holding that two independent requirements specified in *Aguilar v. Texas* (1964)—that police had to explain how informants know what they know *and* why the policed believe the accuracy of the information—were no longer independent. That same year, in *Massachusetts v. Upton* (1983), the Court went on to hold that the "totality of the circumstances" is enough to justify a finding of probable cause.
- The next year, the Court created two specific exceptions to the exclusionary rule, the good faith exception (*United States v. Leon* [1984] and *Massachusetts v. Sheppard*, [1984]) and the inevitable discovery exception (*Nix v. Williams* [1984] and *Segura v. U.S.* [1984]).

We proceeded to analyze cases before and after the beginning of these changes, using *Gates* as the demarcating case. Our central hypothesis was that, even after controlling for personnel change, the factors influencing the justices should be different for the two time periods. Jeff Segal kindly provided us the data through 1990 which was basis of the analysis in *SCAM*; we updated the data through last year's term.

Our results show significant differences in the two time periods, and the differences hold up if we restrict our analysis to justices on the Court in 1983-84.¹ The magnitude of the differences we found were substantial, with the tests for significant differences between the two time periods producing chi squares roughly equal to the chi square testing the overall significance of a model combining the two time periods. In one of his earliest analyses of the search and seizure cases, Jeff Segal (1985) looked at change; however, his best model, which focused on personnel change, just barely achieved statistical significance (*ibid.*, 471). We believe this is suggestive of the argument that relatively little change took place in the years after *Mapp* up to 1981, and would be consistent with the argument that the major changes reflected a shift in what we have labeled jurisprudential regimes.

Not only are the differences substantial in magnitude, but more importantly, they are consistent with the types of specific changes one would hypothesize based on the changes in law:

- In Period 1, the lower court's finding of probable cause had no measurable influence on the justices' votes. In Period 2, the lower court's finding of probable cause significantly increased the likelihood that a justice would vote to uphold the search.
- In Period 1 the justices appear to be much less likely to uphold searches of homes, business, persons, and cars than locations where the subject does not have a property interest. In Period 2 the location/object of the search has much less importance; the only location that is afforded some extra protection appears to be homes, and the protection is much reduced compared to Period 1. Where in Period 1, justices might have looked to specific location/object criteria, the diminution of influence of these criteria in Period 2 would be consistent with the movement toward a "totality of circumstances" evaluation.
- A search incident to an unlawful arrest was significantly more likely to be upheld in Period 2 than in Period 1. This is consistent with the development of the good faith exception. Even if an arrest was unlawful, if the officers conducted a search on the basis of a good faith belief that the arrest was lawful, the search could be upheld.

We believe that this evidence helps resolve the question of whether factual elements should be seen as representing legal or attitudinal influences. If the influence of factual elements is structured by law in the form of jurisprudential regimes, then the influence of those factual elements must, in significant part, reflect the influence of law.

Let me close with one final point, and that is the attitudinal model's inadequate treatment of unanimous decisions. As I said before, attitudes, directly or indirectly, must be the explanation for systematic differences among the justices in a specific case. However, if the justices are unanimous in a case, might it not be that legal factors account for that unanimity? The standard attitudinal explanation for a unanimous decision is that a case falls to the right or left of all justices' indifference points. However, when I have looked at unanimous cases (see Kritzer, Pickerill, and Richards 1998), I find it very hard to accept this proposition. Something else must be going on in these cases.

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Notes

1 The full results of this analysis can be found at on my website at http://www.polisci.wisc.edu/~kritzer/research/supcourt/Role_of_Law.pdf. On my website readers can also find analyses extending the jurisprudential regime analysis to Establishment Clause cases (in a paper that is forthcoming in *Law & Society Review*) and unpublished analyses applying the model to administrative law cases. Links to all of these papers can be found at: <http://www.polisci.wisc.edu/~kritzer/research/research.htm#supcourt>.

THE ATTITUDINAL MODEL: AN EXPLANATION OF CASE DISPOSITIONS, NOT SUBSTANTIVE POLICY OUTCOMES

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One of the key distinctions between *The Supreme Court and the Attitudinal Model Revisited* and its predecessor is that Segal and Spaeth now unequivocally state that the attitudinal model applies only to the U.S. Supreme Court's final decision on the merits. All other choices on the Court, they state, can result from factors other than attitudes.

I agree that justices principally pursue their policy goals. My point of departure with Segal and Spaeth has to do with whether it is the *only* factor that influences the decision on the merits. My central argument is that the way in which this book (and, more generally, the attitudinal literature) conceptualizes and operationalizes the final decision on the merits is problematic. My first claim is that the focus on the ideological direction of case outcomes simplifies the final decision on the merits in a way that masks important variation in the justices' behavior. My second claim is that, even when considering the final decision on the merits, justices take non-attitudinal factors into consideration. I ultimately conclude that the attitudinal model is best suited for explaining the final disposition of a case, rather than the substantive policy choice at the heart of the final decision on the merits. I base these conclusions on four premises, to which I now turn.

1. Justices have preferences over substantive policy outcomes, not just dispositions.

According to the attitudinal model, justices have attitudes over objects (the direct and indirect parties to a dispute) and situations (the dominant legal issue(s) in a case). When deciding the merits of a case, the justices vote based on their value(s) (an interrelated set of attitudes) in the relevant issue area(s) (Segal and Spaeth 2002, 91). The key observed dependent variable for Segal and Spaeth (and thus the object of the justices' attitudes) is the ideological direction of a justice's vote. A liberal vote is one that supports the litigant advocating the liberal position in a case (and vice versa).

This measurement model therefore implies that justices have preferences over the *disposition* in a case (i.e., which litigant wins and loses). However, I contend that justices' care most about the underlying legal principles in an opinion, rather than just which side wins the case. The justices want legal policy to reflect their policy preferences because they understand that it is those policies that ultimately influence distributional consequences in society. It is the legal rule announced in an opinion (not which party won the case) that ultimately serves as referents for behavior and alters the perceived costs and benefits decision makers attach to alternative courses of action. That is why, for example, the bargaining that occurs during the opinion-writing process is largely concerned with the language of opinions (e.g., Epstein and Knight 1998; Maltzman, Spriggs and Wahlbeck 2000).

I would even suggest that this is how Segal and Spaeth think about the justices' policy goals. The version of the attitudinal model presented in their book derives from Rohde and Spaeth (1976), who changed the logic underpinning the attitudinal model from social-psychological to rational choice. Following Rohde and Spaeth, Segal and Spaeth (2002, 92) define the justice's goals as "preferences concerning the policy questions faced by the Court." This language suggests that justices are motivated by their preferences over the underlying substantive policies in their opinions.

Let us consider the consequences of defining the justices' preferences as being oriented towards substantive policy, rather than dispositions. Figure 1 portrays voting over two alternatives in a case. As is customary, I assume an unidimensional ideological space in which justices have continuous, single-peaked, and symmetric preferences. I represent the ideal points for three justices as J_1 , J_2 , and J_3 , and I represent the liberal and conservative dispositions as X_L and X_C , respectively. In this spatial model, a justice votes for the alternative closest to his or her ideal point, and thus any justice to the left of the

cutting line (the mid-point between the two alternatives) votes for X_L and any justice to the right of the cutting line votes for X_C .

FIGURE 1. Supreme Court Voting on Case Dispositions

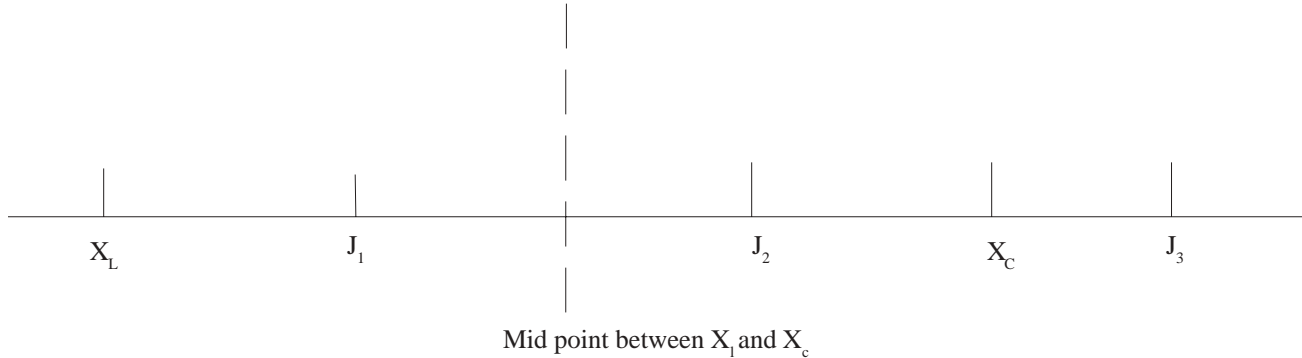


FIGURE 2. Supreme Court Voting on Policy Alternatives

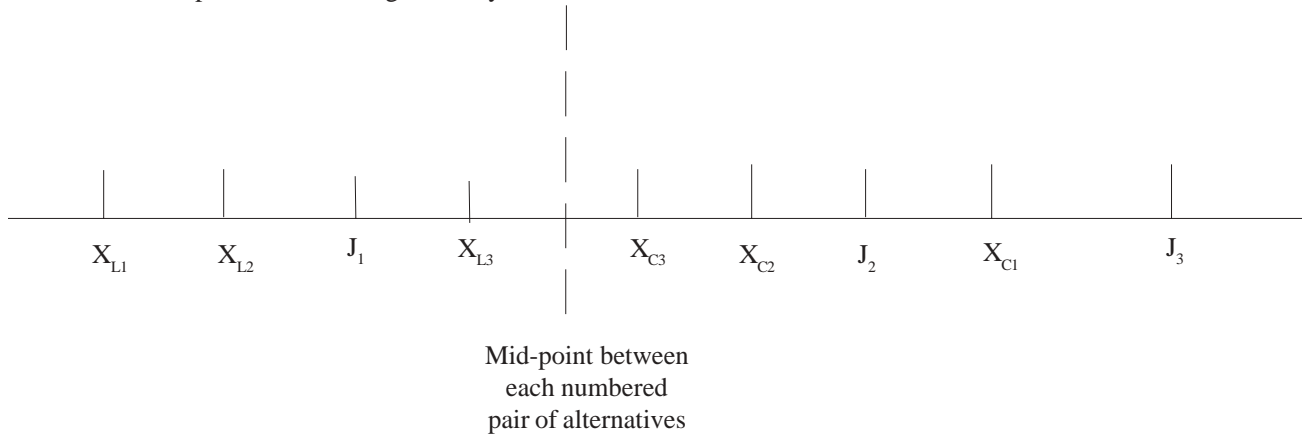


Figure 2 presents the spatial model with a range of policy options (represented by, for example, X_{L1}) associated with the liberal and conservative dispositions. This figure recognizes that a liberal vote can result from a justice supporting a variety of different legal holdings and supporting reasoning. It therefore suggests that if the justices' goals are about legal policy then we need to study aspects of the final decision on the merits that are more directly related to those underlying goals. These dependent variables include, but are not limited to, legal change (e.g., Spriggs and Hansford 2001; Wahlbeck 1997), coalition formation, and the development of majority opinions (e.g., Maltzman, Spriggs, and Wahlbeck 2000; Epstein and Knight 1998).

The obvious response to what I have written so far is that I have redefined the attitudinal model's dependent variable in a way that is beyond its scope—which is the final decision on the merits. This leads to my second point.

2. *The final decision on the merits consists of more than the disposition of a case.*

Case dispositions are obviously meaningful to the parties to a dispute and provide a reasonable indicator of the outcome in a case. Yet, I contend that a more important aspect of the final decision on the merits is the substantive policy in an opinion. The central reason, as I have already stated, is because it is the legal rule in a case that can influence social, political, and economic outcomes. Moreover, I argue that which litigant won is a simplified version of the full range of choices available to the justices when deciding the merits of a case.

Segal and Spaeth (2002, 96) contend that the decision on the merits is distinct from all other behavior on the Court in that it, but not the other choices made by the justices, is exclusively a function of ideological considerations. By contrast, I

contend that the data-generating process underlying the ideological direction of the justices' votes is not distinct from other choices made during the coalition-formation process (such as joining the majority opinion or concurring specially). I can demonstrate this point most clearly by considering the process by which justices cast their final votes on the merits. The final decision on the merits occurs as the justices choose whether to join the majority opinion and/or write or join other opinions. Thus, the memorandum a justice circulates that indicates which opinion he or she joins, or the circulation of the final draft of a separate opinion, constitutes his or her final decision on the merits.

Let us reconsider Figure 2. You can think of each of the alternatives in this figure as representing different legal positions (as embodied in different opinions). Assume that X_{L1} is the position of the majority opinion, while X_{L2} and X_{L3} represent the positions of two different special concurrences and X_{C1} and X_{C2} depict the locations of two dissenting opinions. The ideological direction of a vote is therefore an aggregated version of the justices' final vote on the merits. Support for one alternative (in this case, the liberal alternative) occurs when a justice joins the majority opinion or writes or joins a concurrence, while support for the other alternative is observed when he or she writes or joins a dissent.

Thus, the ideological direction of the justices' votes is actually constructed by eliminating some of the variation in their final decisions on the merits. In this sense, the ideological position of the winning litigant is not a separate choice; rather, it is endogenous with the justices' decisions about which legal position to support (as represented in a particular opinion). This leads to my third point.

3. Measuring the final decision on the merits as the ideological direction of a justice's vote masks underlying strategic behavior.

The key problem with the attitudinal model's measure of the final vote on the merits is that it treats justices who choose different policy positions, but vote for the same disposition, as interchangeable. I submit that the final decision on the merits is as much (or more) about which policy the justices chose than which disposition they support. At a minimum, the final vote on the merits can be more fully represented by which opinion a justice joins. The reason is because the joining of an opinion signifies which policy a justice chose in the case.

What does the literature tell us about Supreme Court decision making when considering this version of the final decision on the merits? Past research clearly demonstrates that justices do not simply make this decision based on ideology. For example, Wahlbeck, Spriggs, and Maltzman (1999) show that a justice's decision to specially concur, as opposed to join the majority opinion, depends on strategic considerations (such as the size of the majority conference coalition), as well as contextual features of the case (such as case complexity and salience). Maltzman, Spriggs, and Wahlbeck (2000) also show that the timing of a justice's decision to join the majority opinion (which is an aspect of the final decision on the merits) reflects strategic calculations. A justice, for example, is less at risk to join the majority opinion before the majority opinion author has the support of a majority of the justices or after other justices have bargained with the author. I now turn to my fourth point.

4. The attitudinal model is more applicable to the final disposition in a case than the substantive policy chosen at the final decision on the merits.

I have argued that the final vote on the merits includes a justice's choice about which policy to support. I have also discussed research indicating that this choice involves both strategic and contextual considerations. As a result, the attitudinal model is not an adequate explanation for the final vote on the merits.

I am comfortable concluding, however, that the attitudinal model is an effective explanation of the final disposition in a case. I base this suggestion on the following conjecture: even though the justices' policy choices are strategic in nature, they will generally vote for a policy that is on the same side of the cutting line as their ideal point. Consequently, as research consistently indicates (Rohde and Spaeth 1976; Segal and Spaeth 2002) the justices' policy preferences are highly correlated with case dispositions.

Think about it like this: what would convince a justice whose ideal point is to the right of the cutting line (see Figure 2) to vote for a policy that does not reflect his or her preferred disposition. Justices would only vote in this way if they thought

it would move Court policy closer to their ideal point. I conjecture that such a scenario will be relatively rare because, in most cases, voting in this way is not going to lead to a justice getting a better outcome than would otherwise obtain. Put differently, I expect for the bargaining in a case to largely occur among justices on the same side of the cutting line.¹

Let me offer one reason for this latter suggestion. In most cases, a justice who is ideologically distant from the opinion author is unlikely to influence the formation of the majority opinion. Thus, such a justice has little incentive to vote for a policy on the opposite side of the cutting line from his or her ideal point. Research provides initial support for this speculation. For example, we know that majority opinion authors are less likely to accommodate ideologically distant and heterogeneous majority coalitions (Maltzman, Spriggs, and Wahlbeck 2000). Research also indicates that justices who are ideologically distant from the majority opinion author are more likely to specially concur or dissent (Wahlbeck, Spriggs, and Maltzman 1999).

In summary, my argument is that the attitudinal model focuses on a simplified aspect of the final decision on the merits that overlooks important variation in the justices' behavior. I made this case in four steps. First, justices' goals are about more than case dispositions. Instead, justices have preferences over substantive legal rules. Second, the final decision on the merits includes more than the disposition of a case and, at a minimum, consists of which opinion a justice joins (which represents which policy he or she chose). Third, a focus on case dispositions camouflages the influence of non-attitudinal factors. It does so by aggregating the final decision on the merits and thus obscuring important variation in this choice. Fourth, the attitudinal model is best suited for explaining the disposition in a case, rather than the substantive policy underlying the vote. Thus, by conceptualizing the final decision on the merits as being about more than the disposition in a case—for example, which policy justices choose as seen in which opinions they join—we see that Supreme Court justices' final decisions on the merits are indeed influenced by more than their policy goals.

OF TURNING WORMS, KAKISTOCRATS, & RESEARCH PROGRAMS: SEGAL & SPAETH REDUX

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In *SCAM II*, Jeffrey Segal and Harold Spaeth have produced a thorough and thought-provoking book that is clearly a must-read for any student of the courts. The authors expand the scope of their inquiry substantially beyond that covered in *SCAM I* and present new evidence in support of the attitudinal model of Supreme Court decision making. On a substantive level, the book is chock-full of interesting information, on which I will comment in the latter part of this essay. Initially, however, I would like to highlight some other dimensions of the book that are also worth noting.

A dull academic tome *SCAM II* is not. In this book, not only did Segal and Spaeth flex their muscles as political scientists, but they obviously had fun offering insights on a number of other matters. Stanley Fish has noted that interpretation is not constrained by the “conventional” view of a text. For example, Fish claims that Agatha Christie’s mysteries can reasonably be viewed as philosophical treatises on the meaning of life and death (Fish 1982; 1983). In this tradition, then, I begin by offering some alternative readings of Segal and Spaeth’s *SCAM II*.

SCAM II as Linguistic, Literary and Grammatical Primer. In reading *SCAM II*, prepare yourself to encounter new vocabulary with a dictionary handy by your side. In commenting on continued popular support for the Court, for example, the authors treat us to the following sentence: “The fact that the legalists’ faith in the rule of law may have gone the way of the phlogiston will not affect the popular conviction that the emanations from the marble palace alone safeguard the American way of life, and not the pestiferous effluent generated by the political branches of government” (177). William F. Buckley, eat your heart out! You will also learn of “kakistocracy” (rule by the worst), “plunderbunds,” “cancerous accretions,” “rebarbative decisions,” and “pellucid interpretations,” among many other unique word choices. Furthermore, the book is erudite: it incorporates Shakespearean and other references from literature and even a poem or two. And metaphoric language livens the prose throughout. For example, the Rehnquist Court frequently “turns the constitutional worm,” and geeses and ganders are sauced and served up for our symbolic consumption throughout the text. These lively turns of phrase make reading *SCAM II* a literary as well as an academic delight.

SCAM II as Anthropology, Sociology and Psychology Text. In *SCAM II*, we are also offered the authors’ assessment of human nature. On why we in America are so attached to our Constitution, Segal and Spaeth suggest that human beings are creatures of habit who are rattled by change. Or as the authors observe: “Life becomes frightening to those who find events in the saddle riding herd on them” (13). When we are so destabilized, “the Constitution and its system of government furnish us with our [comforting] link to the invariant” (14). Constitution as security blanket, pacifier, thumb? As for sociology, suffice it to say that the authors view those of us living below the Mason Dixon line somewhat less favorably than our northern brethren. Indeed, the only “kakistocrats” described in the text—on more than one occasion—are members of the Georgia legislature. In addition, the book provides a heartwarming anecdote about Jeff Segal’s angst over discovering his inability to answer a fourth-grader’s question on a matter of constitutional law. How comforting to learn that fallibility is a universal human condition.

SCAM II as Political Theory and Commentary. The authors’ distaste for recent decisions by the conservative Rehnquist Court is not even thinly disguised. In what I viewed as devilishly good fun, the authors take aim and blast a volley at the “Rehnquist Five” and their recent decisions “wreaking carnage on federal authority.” *Bush v. Gore* is characterized as particularly symptomatic of a partisan court—and a power-hungry one at that. Indeed, the Rehnquist Five is noted as exercising the “atavistic activism of divine right monarchism” in *Bush v. Gore*, and as using reasoning that is “ingeniously preposterous.” Rehnquist and Scalia are singled out for particularly skeptical scrutiny. Even Scalia’s purported intelligence

“does not preclude his self-deception” on matters of plain meaning and constitutional interpretation, and Rehnquist, bright as he may be, can’t quite grasp the scientific method and the principle of falsifiability. The moral of the story: behind their mysterious black robes, these emperors are quite naked indeed.

SCAM II as Comedy. Perhaps most surprisingly, *SCAM II* is sprinkled with humor, as the authors’ rapier wit and sometimes gallows humor emerge when the reader least expects it. For example, after noting that Justice Douglas denounced polygamy as a “notorious example of promiscuity,” Segal and Spaeth observe that “[p]resumably, the crucial consideration for Douglas, who was married four times, is that plural wives are permissible so long as a man has them consecutively, rather than concurrently” (55). Along with the “Rehnquist Five,” O’Connor is referred to as the “Madame in the Middle” (presumably no reference to a previous life of moral turpitude). And after discussion of a capital case (from Georgia) in which the defendant’s appeal was dismissed by the Supreme Court for failing to raise a federal question, Segal and Spaeth wryly remark: “The upshot: Williams was duly executed” (32). Although perhaps the dry world of academia has lowered my humor threshold, I nevertheless found these tongue-in-cheek remarks funny and refreshing.

The aspects of *SCAM II* described above should not go “unharolded” (shame on me). The authors deserve credit for producing a book that is a pleasant read as well as an impressive work of scholarship.

SCAM II as Political Science. As for the substance of the book, the authors not only “revisit” the attitudinal model in *SCAM II*, but also seek to shore up the evidence in support of the model’s now hegemonic position in political science. On the general debate over whether the attitudinal model represents the best predictor of individual justices’ voting behavior, the evidence is persuasive. The attitudinal model clearly accounts for much of the variation in the justices’ voting patterns. But as the authors rightly point out, a model is, by its very nature, a blunt tool that cannot and probably should not account for every aspect of the human events under examination.

So where do we go from here? Borrowing from concepts developed by philosopher of science Imre Lakatos, one might argue that the theoretical premise that Supreme Court justices’ voting behavior is primarily governed by their attitudes now forms the “hard core” or “negative heuristic” of the research program undertaken by Segal and Spaeth, as well as by earlier behavioralists such as Pritchett and Schubert. In his explanation of scientific development, Lakatos further explains that a theoretical hard core is protected by a “positive heuristic,” which is a protective belt of auxiliary assumptions and hypotheses to guide research that addresses counter-examples or anomalies to the theory’s hard core.¹ A research program is successful (or “progressive”) if research within the protective belt generates “ever more complicated models simulating reality,” where “each successive model is developed from the preceding one by altering some of the restrictive or simplifying assumptions of the earlier model in accordance with the directives of the positive heuristic” (Moon 1975, 152). As Moon explains:

In the case of the Newtonian research program, for example, Newton began with a highly simplified model of the solar system consisting of only one point-like planet orbiting the fixed point-like sun, and as he developed a satisfactory analysis of this case, he relaxed these assumptions and developed a series of ever more complex models. In a similar fashion, Down’s theory of political action in a democracy began with a highly simplified model of perfectly formed parties, and those restrictive assumptions were gradually relaxed until a reasonably complex, explanatory theory emerged (Moon, 1975, 152).

In many ways, political scientists’ research on Supreme Court decision making has proceeded in the fashion described by Lakatos. For example, the strategic model of the justices’ behavior takes as a given the attitudinal model’s “hard core” principle that the justices are motivated to embody their policy preferences into law (see, e.g., Epstein and Knight 1998; Maltzman, Spriggs and Wahlbeck 2000). However, strategic accounts then move beyond the highly simplified attitudinal model to describe and explain additional behavioral complexities resulting from external constraints imposed by other justices or by the political environment. Although Segal and Spaeth demonstrate that the latter form of environmental constraint fails to alter the justices’ attitudinally-driven behavior (2002, 326-351), the evidence is convincing that in the opinion-writing process the justices do act strategically to achieve their policy goals (Maltzman et al. 2000). Since by definition strategic behavior involves action that does not accurately reflect the actor’s true preferences (Knight and Epstein 1998, 12), this research falls squarely within the progressive domain envisioned by Lakatos, as it addresses an anomaly to the theoretical hard core and, without altering the core itself, provides a more sophisticated model of reality.

The lesson here is that highlighting and evaluating empirical anomalies need not involve falsifying the theoretical hard core. Rather, a progressive research program focuses on the protective belt of assumptions and hypotheses and produces

progressive change if it “permits the prediction of new facts even as it accounts for old anomalies” (Ball 1976, 36 n.14). On the other hand, a research program is “degenerating” if it resolves “old difficulties by means of verbal and/or ad hoc stratagems which do not point out new directions for research” (ibid). Critics and defenders of the attitudinal model must bear these lessons in mind. Ball notes that critics should be tolerant of efforts to “save” theories from refutation, and must ask whether adjustments to the protective belt to account for empirical anomalies are progressive or degenerating “*within the context of this particular research program*” (Ball 1976, 37, emphasis in original).

The next step in the context of the attitudinal model, then, would be to carefully identify anomalies that are not explained or predicted by the justices’ policy preferences. Once those counter-examples are identified, defenders (or critics) may choose to offer adjustments to the positive heuristic that account for the anomaly and also allow for the prediction of new facts. In the context of the MPSA roundtable, many of the “critics” comments addressed anomalies to the attitudinal model. They also acknowledged that offering counter-examples did not necessarily constitute an effort to refute the theoretical hard core. Attitudinalists might view these efforts to identify anomalies as offering opportunities to refine the model in the Lakatosian sense.

For example, the most persuasive evidence of attitudinal effects comes from decisions in the area of civil rights and liberties. Voting behavior in some other issue areas is less well predicted on the basis of the justices’ attitudes. Although this counter-example might stem from inaccurate measures of attitudes, a less ad hoc explanation would be that the influence of attitudes is conditional. This is not a new idea. But if the influence of attitudes is conditional, how do the justices render decisions in these non-attitudinally governed issue areas? Another counter-example involves non-scalar votes. In their Table 10.1, Segal and Spaeth present the justices’ votes on declarations of unconstitutionality. That table demonstrates that, during the period examined, Brennan voted to strike 4 of 11 liberal statutes he evaluated, and Ginsberg voted to strike 9 of 21 liberal statutes (2002, 416). What do these anomalous votes tell us about the justices’ voting behavior, even assuming that their behavior is largely driven by their attitudes? Similarly, Supreme Court justices occasionally form coalitions involving strange bedfellows. As Table 9.12 reflects, from 1986 to 1991, Marshall joined nine special opinions written by Scalia and Justice Brennan joined six Scalia special opinions (2002, 397). Why do these strange bedfellow coalitions emerge in the context of an attitudinal model that would generally suggest that Scalia and Marshall anchor opposite ends of the ideological spectrum? One explanation may be that ideology is multi-dimensional, an idea explored in earlier research by Rohde and Spaeth (1976). On the other hand, the strange bedfellows phenomenon might reveal something theoretically enlightening about the influence of attitudes in particular contexts, and thus provide progressive avenues for future “content-increasing” research.

Where does law figure in this progressive research agenda? Taking a Lakatosian perspective allows us to avoid the either-or debate over law and attitudes. A more productive approach is to ask whether any anomalies not well explained by attitudinal theory can be accounted for by legal considerations, not because law trumps attitudes but because the impact of attitudes is conditional in some way. At the risk of a shameless plug, research David Klein and I have conducted on intercircuit conflict cases in the Supreme Court suggests that such an approach may be fruitful (see Klein and Lindquist 2001). In intercircuit conflict cases, most of the conservative justices vote in a significantly more liberal direction than in other, non-conflict cases—suggesting an anomalous situation. And while attitudes still influence their votes to some extent, we also find that many of the justices are influenced by the nature of the legal deliberations undertaken in the circuit courts below. Similarly, work by Richards and Kritzer (2002) proposes a theory of jurisprudential regimes that accommodates attitudinal effects but that adds more complexity to the attitudinal explanation by allowing for the influence of jurisprudential considerations.

In conclusion, debating the relative merits of the legal and attitudinal models is not fruitful if they are conceptualized as competing theories in the broad sense. The attitudinal model is clearly persuasive enough to warrant considerable tolerance, as Ball suggests. Instead, focusing on anomalies and counter-examples to the attitudinal account may provide us with opportunities to enhance the theory’s sophistication, discover and explain novel facts, and provide, in the words of Lakatos, “higher corroborated theoretical content.”

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Notes

- 1 For a complete discussion of the work of Imre Lakatos and its application to political science, see Moon 1975; Ball 1976.

REPLY TO THE CRITICS OF THE SUPREME COURT ATTITUDINAL MODEL REVISITED

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Theoretical and substantive developments following the publication of *The Supreme Court and the Attitudinal Model* convinced us that the time was ripe for a successor volume. On the theoretical front, *The Supreme Court and the Attitudinal Model* confronted neither rational choice theory nor modern jurisprudential thought. On the substantive front, we looked to take advantage of conditions created by our most persuasive ally: the Rehnquist Court. Between *Bush v. Gore* (2000) and the Rehnquist Court's sovereign immunity cases, the case for attitudinalism was increasingly being made for us.

We thank Isaac Unah for putting together the Author Meets Critics Panel at the 2003 meeting of the Midwest Political Science Association, John Gates for publishing this forum, and our critics for their constructive criticisms. We respond to their comments as follows.

Epstein

Epstein discusses the domain of the attitudinal model. To us, the model presents an elegant but rigorous explanation of the Supreme Court's decision on the merits. The model does not apply in full to courts below the Supreme Court, nor does it fully apply to the complete range of Supreme Court behavior. Nevertheless, the model does have important implications for the behavior of judges below the Supreme Court, and, as we demonstrate in our book, for Supreme Court behavior other than on the merits.

In one sense, the tight fit to the Supreme Court's decisions on the merits makes the attitudinal model a narrow one, compared, say, to rational choice theory. Rational choice considers its domain to be no less than the entire range of human and social relations, and since it makes no limits on goals, even things like suicide are within its reach (p. 98).¹ Within judicial behavior, rational choice theory allows scholars to specify any goals they choose: social, legal, political, attitudinal, or anything else. This means that while specific rational choice models are falsifiable, rational choice theory is not, for there will always exist goals that can explain whatever behaviors we find. We prefer, instead, the leverage that comes from specifying a single goal for the Court, particularly when that single goal explains a remarkable proportion of the Court's behavior.

As to the fact that the attitudinal model does not apply *in toto* to the full panoply of courts below the Supreme Court, we wonder: how often are theories of presidential behavior subject to the criticism that they don't fully explain governors,' county executives,' or public school superintendents' decisions?

Gillman

Gillman churlishly contends – à la the original bete noire of behavioralists, Wallace Mendelson (1964) – that our book is “tragically destined to have almost no influence beyond the faithful” (Gillman 2003, p.). Everyone knows, he continues, that justices' decisions are based on their political ideology. Perhaps only most people know, for as recently as May 2003 Professor Gillman was politely chastised on the conlawprof listserv (conlawprof@listserv.ucla.edu) for suggesting a connection between the judges' ideology and their votes in the McCain-Feingold case.² Not to be outdone, another law professor complained that charges that the Rehnquist Court chose the incumbent president were incorrect. To this professor, the Supreme Court in *Bush v. Gore* did no more than remand the case to the Florida Supreme Court. Another commentator ridiculed the notion that the justices' ideology could be handicapped, as in a horse race. Still another insisted that ideology be considered a possible motivation for the justices' behavior only after all plausible legal considerations have been

rejected. As for published work, we might add that one of the fundamental tenets of the ever-present constitutive theory, according to an essay chosen by Gillman to represent this point of view, holds that “justices do not follow their own policy wants” (Kahn 1999, p. 177).

And if we simple-mindedly accept Gillman’s assertion that “Everyone knows,” what are we supposed to make of those deluded souls – traditionalists all – who apodictically assert that Justice Frankfurter personified judicial restraint (Spaeth 1964) and that today’s conservative jurists embody strict construction?

While we wish these examples were exceptional, that certainly was not the case when *The Supreme Court and the Attitudinal Model* (1993) was published. Many readers of this forum will recall the attitudinal wars on Gillman’s Law and Courts listserv (lawcourts-1@usc.edu).³ While we have conducted no systematic analysis of this, our own sense is that the attitudinal model does not meet with nearly the opprobrium it once did. The Rehnquist Court’s behavior, highlighted by *Bush v. Gore*, may have shattered the faith of many, but we believe that our own work has had no small role in the greater acceptance of attitudinalism. We hope this book will continue that trend.

But in addition to convincing those, unlike Gillman, who are yet unconvinced of the dominant role of policy goals in Supreme Court decision making, we might also convince scholars of the merits of a modeling approach to understanding the Court (ch. 2), for too many legal scholars are content to critique the attitudinal model by finding a case that doesn’t fit particularly well.⁴ We might also convince scholars of the importance of ideological preferences outside of the decision on the merits, and of the overwhelming lack of systematic evidence for alternative models of Supreme Court decision making, such as separation of powers, public opinion, or legal influence.

While we value a modeling approach to the Court, Gillman claims that we demand that scholars worship at the altar of quantification, hypothesis testing, and falsification. Since Gillman acknowledges that we frequently use qualitative evidence (although derived from sources of which he disapproves) and that our previous book (Spaeth and Segal 1999) relied almost completely on qualitative evidence, we must be more catholic than he claims. Moreover, he provides not a single statement from our book to back his anti-qualitative contention. Nevertheless, we do insist on falsifiability (not falsification), as falsifiability distinguishes scientific explanations from orthodox as well as heretical faith. Even the Supreme Court accepts this (*Daubert v. Merrill Dow* 509 U.S. 579, but see Rehnquist et al. dissenting).

Gillman contends that we cannot come up with a legal model that anyone would accept, suggesting our adoption of a formalistic determinism. Our conception of the legal model, though, explicitly rejects determinism, while noting, with attribution, that not all contemporary legal scholars do (p. 48).⁵ What we require of a useful legal model is not that legal factors *determine* decisions, but that legal factors significantly *influence* decisions (p. 48). This is similar in many ways to Dworkin’s gravitational force (p. 294). If arguments based on precedent, text, and intent cannot even be said to influence judicial decisions, if all that can be said is that judges convince themselves that they are acting in good faith, then we are left with a model that has no explanatory ability whatsoever.

In an attempt to box our description of the legal model into something “that no reasonable person believes,” Gillman ignores distinctions we make throughout the book between formalists and those who hold subtler views. As a prime example, Gillman lists himself as among those whom we allegedly claim “argue that the Court merely follows established legal principles in deciding cases” (p. 432). Of course, we do no such thing, and *in the very next sentence of the book* we cite Gillman as a counter-example of this position. Like the book’s purported attacks on qualitative research, this criticism seemingly represents what Gillman expected us to say, not what *The Supreme Court and the Attitudinal Model Revisited* actually said.

Gillman does correctly note that we largely limit our systematic analyses to civil liberties cases. This is not, as he suggests, because these are the only cases that fit the model. Consider national supremacy, an issue rife with Rehnquist Court declarations of unconstitutionality. Nor is his comment that Segal acknowledged that the model only applies to civil liberties and related cases correct (Gillman 2003, p. fn. 11).⁶ Rather, we typically limit our analyses to civil liberties cases because, as Segal and Cover (1989) originally noted, the editorial comments that make up the Segal-Cover scores are based overwhelmingly on judgments about the nominees’ views on civil liberties issues.⁷ Gillman states that in doing so we have left a “misleading impression about the state of the evidence.” He wishes we would correlate our ideology scores on a

sample of all Supreme Court decisions over the past 50 years. In response, we have now replicated our analysis on the population of Supreme Court decisions since the start of the Warren Court.⁸ We provide the data in Table 1. The second column provides each justice's ideology, using the Segal-Cover scores (Segal and Cover 1989). The third column presents newly derived data on the justices' liberalism in all cases. The final column represents the justices' liberalism in civil liberties cases (p. 322). In the book we reported a correlation between ideology and civil liberties votes of .76 (p. 323), rounding up from .758. We now report that when we look at all cases, the correlation coefficient does indeed drop, all the way to .757 – a whole one-thousandth of a point!⁹ So much for Gillman's claim that the data were "carefully selected to focus on the cases that work best for the model."¹⁰

We do not know what to make of Gillman's complaint that the justices don't "have the same predisposition toward/against liberalism on every issue area." Why a justice should be expected to occupy the same point on a liberal/conservative continuum, regardless of issue escapes us. They certainly did not and do not. Consider only the due process issue area. Scalia and Thomas are the most liberal on the takings clause if one views the direction of these cases conventionally; i.e., pro-individual and anti-government. Conversely, Justice Brennan was the most conservative on the contacts needed by state courts to exercise jurisdiction over out-of-state defendants. And let us not overlook Douglas, arguably the Court's all-time most extreme liberal, who behaved more conservatively than any of his colleagues on straightforward interpretations of the Internal Revenue Code. Bush and his handlers should worship at his anti-tax shrine.

Finally, Gillman attacks the integrity of the Supreme Court database because of his disagreement over Spaeth's coding of *Bush v. Gore*. He hopes that someone will reproduce the ideological coding of the database. Well, unlike *any* of the research that Gillman so admires, the Supreme Court database does include reliability scores. The reliability of the direction of the Warren Court's decisions is 98.6 percent (two errors) and 99.5 percent for the Burger Court (one error). And if Gillman nevertheless dislikes Spaeth's directional coding, he may change any record with but a single keystroke, as the documentation points out. As for the ideological coding, all of the Spaeth databases specify the legal provision(s) as well as the issue(s) that decisions address. Nothing prevents users from redirecting these variables into legalistic categories.

Kritzer

Kritzer, based on work with Mark Richards, proposes that Supreme Court decisions are influenced by what they label "jurisprudential regimes." Jurisprudential regimes can be detected when the Court shifts its decision making in a consistent direction following a leading decision or decisions. Because the Supreme Court rarely revisits the exact same issue (except perhaps when it wishes to overturn that issue), these shifts need not be in the same areas as the leading decisions.

We appreciate Kritzer's efforts but we nevertheless question these findings both conceptually and empirically. While Kritzer labels these changes "jurisprudential regimes," they might just as appropriately be labeled "attitudinal regimes." If the Court reaches a leading conservative decision on one issue (e.g., probable cause), and then follows that up with conservative decisions on other issues (e.g., lessening the protection granted to house searches), that could just as easily be due to attitudinal considerations as to legal considerations.

We initially note, for example, that personnel change arguably accounts for the change in search-and-seizure. Hence, the 1983-84 changes in how the Court viewed search-and-seizure plausibly occurred because of the presence of Justice O'Connor. She counted herself with the conservative majority in each of the cases Kritzer cites. And in her first two terms (1981-82), she voted liberally in only one search-and-seizure case: a unanimous decision holding that 90 minutes was too long to detain luggage for a dog sniff. We doubt that Kritzer's conservative outcomes would have occurred with a Douglas, Warren, and Fortas on the Court. Indeed, if the justice she replaced – Potter Stewart – were still on the Court, decidedly fewer conservative decisions would have resulted.

What are needed are tests that would be solely consistent with legal regimes. We suggest two: we start with a search and seizure factor that, as of a certain date, has not been declared influential, such as a totality of the circumstances test for confidential informants. Consider first a justice who supports such a test. If that justice is acting attitudinally, then that factor should be just as important to him or her before the Court decision on the topic as after. Next, consider a justice who opposes such a test. That factor would not be important before the regime, but may be important after the Court announces the decision.

Empirically, Kritzer and Richards show that different variables take on significantly different values before and after 1983 or so. What they do not do is compare the results from such a model to results from a variety of different specifications of regime change, as in Segal (1985), who found that personnel change outperformed competing explanations. Thus, if the Supreme Court's actual treatment of variables were consistently changing in a single direction, creating a dummy (binary) variable for that change at almost any interval would show differences before and after that point. Moreover, in rejecting Segal's comparative approach, Kritzer states that the results from Segal's best model "just barely achieved statistical significance." Yet, the overall model was significant at $p < .001$, while the change in the chi square from the baseline model to the personnel-change model is significant at $p < .025$ (Segal 1985, p. 474), meaning that the results are in fact quite unlikely to have occurred by chance.

Moreover, the changes that Kritzer and Richards find are subject to various interpretations. Recall that what initiates the jurisprudential regime is three decisions by the Supreme Court: changing the probable cause requirements for confidential informants, creating a good faith exception to the exclusionary rule, and creating an inevitable discovery exception. Whether these changes themselves are due to legal or attitudinal questions might rightly be questioned, but it would be hard not to notice that these changes were engineered by a conservative Burger Court that had done more damage to Warren Court 4th Amendment decisions than to Warren Court decisions in any other area (Segal 1985). So we have no inherent problem in labeling the decisions that followed part of a "jurisprudential (or any other sort of) regime," so long as it is recognized that that regime might well have been attitudinally created.

The particular results by Kritzer and Richards provide no greater comfort. Specifically, they find that the lower court's decision on probable cause has a greater impact following 1983 than before; that the coefficient for searches of houses, businesses, and cars diminished following 1983; and that a search incident to an arrest that the lower court deemed unlawful had greater influence following 1983.

When we consider these findings carefully, it's clear that these results tell us little about jurisprudential regimes. Consider the lower court probable cause finding. Recall that in *Illinois v. Gates* (1983), the Supreme Court retracted a rather odd rule from the Warren Court. The *Aguillar-Spinelli* rule stated that unlike virtually all other probable-cause determinations, which were to be judged by a totality of the circumstances approach, probable-cause determinations from anonymous tipsters had to provide: (1) the tipster's basis of knowledge; and (2) either information on the tipster's veracity or the reliability of the information. Since anonymous tipsters are very unlikely to be familiar with the *Aguillar-Spinelli* rules, it would be remarkably serendipitous for them to detail not just the relevant information, but how they came upon the information plus information about their own veracity.

Be that as it may, none of this has very much to do with an increased value of the probable-cause coefficient in the search and seizure model. Increases in this coefficient can be due to the fact that the Supreme Court is: 1) more likely to uphold searches when the lower (appellate) court finds probable cause; 2) less likely to uphold searches when the lower (appellate) court does not find probable cause; or 3) some combination of 1) and 2). *Gates* has nothing to say, either implicitly or explicitly, about any of these possibilities.

Next consider the place-of-the-search variables and their respective coefficients. These coefficients represent the change in the likelihood of upholding a search compared to a baseline of a search where the defendant had no property interest. A decrease in the house variable could thus represent 1) a decrease in the protection afforded one's house, 2) an increase in the protection afforded to areas where one has no property interest, or 3) some combination of 1) and 2). We honestly do not know what a totality-of-the-circumstances approach to probable cause implies for this scenario. Even if we took the totality-of-the-circumstances test outside of its moorings in probable cause, having a property interest is one circumstance that courts would presumably consider.

Absent supporting evidence for Kritzer's inability to accept attitude theory's proposition that unanimous decisions simply lie to the left or right of all the participating justices' indifference (ideal) points, we stand by the theory. Again, we don't deny that something else *may* be going on in unanimous decisions; we do not know. But given the absence of evidence for any such "something" (*cf.* Kritzer, Pickerill, and Richards 1998), we are not prepared to discount our theory.

Spriggs

Perhaps the most interesting point about Spriggs's comments is not what he does say, but what he does not say. Two crucial issues involve rational choice theory. At various points in the book we note that rational choice theory has an extraordinary comparative advantage over other theories: equilibrium analysis. When scholars can demonstrate that a particular choice represents a best response to a best response or some alternative equilibrium concept, and then demonstrate that under those conditions such choices are in fact made, then one has a tremendously compelling argument. We note, though, that the judicial rational-choice literature, outside of the separation-of-powers games, has rarely invoked equilibrium analysis. Scholars claim that certain strategies are rational, but, without an analytic framework, we do not know that they are. We explicitly reject the notion embraced by many that rational choice theory without equilibrium analysis is an oxymoron, but nevertheless claim that informal rational choice robs the theory of its most important advantage. Not a word of contradiction from Spriggs here.

Nor does Spriggs contradict our findings on the one judicial area that has both equilibrium analysis and empirical results: the separation-of-powers model. Of course the book is not a monograph and there are many things that Spriggs could have chosen to discuss. But the fact that one of the foremost judicial rational choice scholars leaves these issues aside speaks volumes.

Alternatively, Spriggs criticizes our emphasis on the decision on the merits, as opposed to the opinion of the Court, where the Court's real policy is made. It's a shame we didn't say something along the lines of "The decision on the merits merely indicates whether the ruling of the court whose decision the Supreme Court reviewed is affirmed or reversed, and consequently, which party has won and which has lost. The opinion of the Court, by comparison, constitutes the core of the Court's policymaking process" (p. 357).

Of course one might wonder why we spend so much more time examining the relationship between the justices' ideology and their votes on the merits than we do on the relationship between the justices' ideology and their written opinions. Some of this, of course, is due to the difficulty of measuring opinions ideologically. But fortunately for us, the one person who has most carefully measured Supreme Court opinions finds that they correlate overwhelmingly with the justices votes on the merits (Wenzel 1995). This is true for other textual analyses as well (Gates and Phelps 1996).

Moreover, Spriggs's model simply does not correspond to what the decision on the merits actually means. According to Spriggs, the decision on the merits represents a choice between two outcomes, X_L and X_C . This sort of choice may be common and appropriate in choosing candidates, but it does not apply to the Court's decision on the merits.

A decision to support Bakke's admission to the Davis's medical school represents a range of possible outcomes, from prohibiting race from being used as a factor (the Stevens position) to ruling that the state has a compelling interest in using race but the Davis program is not narrowly drawn to meet that interest. Alternatively, ruling for Davis also represents a range of possible outcomes, not a single point on a scale. Since the decision on the merits only decides whether Bakke gets admitted, ruling for him means that Davis's program is to the right of a justice's indifference point, while ruling against him means that California's program is to the left of a justice's indifference point. Thus, by not coding concurrences in comparison to majority opinions, we do lose some information.

Nevertheless, cases will over time undoubtedly fall between the indifference points of those justices in the majority decision coalition as cases are decided liberally and conservatively by different votes, thus allowing an aggregate use of merits decisions to faithfully represent the policy positions of the relevant justices. And, of course, this has happened, with almost perfect predictability resulting from the Rehnquist Court's decisions.¹¹

While we admit that a focus on ideological outcome simplifies the Court's decisions, that, after all, is the purpose of a model: to simplify the real world. Whether other considerations supplant policy goals depends in large measure on the definition of such goals and whether justices are willing to shape their preferences in order to secure a majority vote and thereby make their policy one of general applicability. Such accommodation in no way undermines ideological considerations. A bird in the hand may well be worth two in the bush. Spriggs' comments about distinguishing "policy positions" from the final vote on the merits amount to nothing more than a craving to measure a qualitative matter (words) quantitatively. We wish him well.

In disposing of their cases, the justices obviously concern themselves with their explanations for their choices. Separation of such choice from their dispositive vote does not countermand the attitudinal model. It may serve to cloak it (Spaeth 1964; Spaeth and Teger 1982; Spaeth and Altfeld 1986), but so what else is new?

Spriggs' summary puzzles us. Neither we nor the attitudinal model contravenes the notion that "justices have preferences over substantive legal rules." But, given the fact that justices use the same legal rule to support contrary "case dispositions," the model clearly establishes the primacy of the former over the latter. So, if one wishes to define the final decision on the merits as including "which opinion a justice joins," one should recognize that rather ad hoc arguments will be needed to position the opinion as more or less proximate to the subject's "real" preferences. Moreover, inclusion of opinions as part of the justices' goals assumes that they all lie on the same ideological dimension and are objectively distinguishable from one another. Given the obfuscation characterizing opinions and the fact that they frequently do not speak the same subject-related language reduces such analytical efforts to a kabalistic séance.

Lindquist

We like Professor Lindquist's equation of the attitudinal model as forming the "hard core" of judicial research, and that it is protected by a "positive heuristic" – a belt of auxiliary assumptions and hypotheses – that guides research that addresses counter-examples or anomalies to the model's hard core. The best current example of such growth is the development of models of judicial strategy (e.g., Epstein and Knight 1998; Maltzman, Spriggs, and Wahlbeck 2000), which formulate and elaborate the "positive heuristic" of the basic model, thereby addressing complexities inadequately considered previously.

As mentioned, Professor Lindquist accurately separates the attitudinal model from latter-day elaborations. As she observes, "highlighting and evaluating empirical anomalies need not involve falsifying the theoretical hard core." This is especially true of strategic analyses that accept the justices' policy preferences as their starting point. Hence, in no way do we disagree that "a more productive approach is to ask whether any anomalies not well explained by attitudinal theory can be accounted for by legal considerations, not because the law trumps attitudes but because the impact of attitudes is conditional in some way." Obviously, non-legal considerations may be just as likely to account for these anomalies, as well as the further development of the attitudinal model itself. Even so, the attitudinal model seems to have parsimony on its side.

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Notes: Ideol: Justices' Ideology from Segal and Spaeth 2002, p. 322. Votes: percent liberal of justices' votes in all cases except interstate relations and miscellaneous as derived from U.S Supreme Court judicial data base. Civil Liberties Votes: percent liberal of justices' votes in all civil liberties cases, from Segal and Spaeth 2002, p. 322.

NOTES

¹ Unless otherwise noted, all page citations are to *The Supreme Court and the Attitudinal Model Revisited*.

² We do not provide direct attribution here because we believe the listserv should be a place where scholars can freely offer unfinished ideas. Nevertheless, these expressions, even if incomplete, do indicate where many who study the Court stand on this issue.

³ See particularly the exchanges in July 1998 and October 1999.

⁴ Again see the conlawprof listserv from May 2003.

⁵ Concurring in this opinion, and indeed, going further than we did, is the esteemed judge and scholar, Richard Posner (2003), who recently noted that formalism “retains to this day a strong hold on the legal imagination.”

⁶ To be fair to Gillman, we don't believe the comment to be correct, as we have a different recollection about the comment in question.

⁷ There is a crucial theoretical difference between carefully noting that our ideological measures are not based on attitudes toward, say, federalism, and therefore excluding such cases from our analysis, and stating that the justices' attitudes do not affect their votes on federalism.

⁸ We selected orally argued cases with dec_types 1, 6, or 7. We exclude only cases in the database listed as interstate relations and miscellaneous, which have no ideological coding and account for 59 out of 5,837 cases.

⁹ Though Gillman only discusses the correlation coefficient, the slope coefficient in the regression equation drops from 23.5 to 18.0.

¹⁰ Certainly, though, the attitudinal model is not going to accomplish much in areas that typically have no ideological content, such as boundary disputes between states. Nevertheless, we were distressed to see Justice Ginsburg of Brooklyn vote with the Court majority that much of Ellis Island belongs to New Jersey. Segal's grandparents, like Breyer's, did not leave the old country for Ellis Island, *New Jersey*. See *New Jersey v. New York*, 523 U.S. 767 (1998), Breyer concurring, at 812.

¹¹ This happened somewhat less for the Burger Court. See the tables in Spaeth (1995) displaying the votes of the justices in the affirmative action cases.

Table 1. Ideology, Liberalism in All Votes, Liberalism in Civil Liberties Votes.

NAME	IDEOL	VOTES	Civil Liberties Votes
WARREN	.50	76.7	78.6
HARLAN	.75	46.5	43.6
BRENNAN	1.00	73.2	79.5
WHITTAKER	.00	42.4	43.3
STEWART	.50	50.2	51.4
WHITE	.00	49.4	42.4
GOLDBERG	.50	73.5	88.9
FORTAS1	1.00	72.1	81.0
MARSHALL	1.00	74.0	81.4
BURGER	-.77	35.6	29.6
BLACKMUN	-.77	54.3	52.8
POWELL	-.67	39.9	37.4
REHNQUIST	-.91	30.7	19.8
STEVENS	-.50	60.7	64.2
O'CONNOR	-.17	39.7	35.5
SCALIA	-1.00	35.7	29.6
KENNEDY	-.27	42.2	36.9
SOUTER	-.34	57.1	59.9
THOMAS	-.68	32.5	25.7
GINSBURG	.36	59.6	64.4
BREYER	-.05	57.3	61.1

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9. Teaching and Learning in Political Science

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Conference Group on Jurisprudence and Public Law

[Panel 1 The Ethics of Cloning and Stem-Cell Research: The Embryo and Human Dignity](#)

Law and Political Process Study Group

[Panel 1 The 2002 Bipartisan Campaign Reform Act and the Constitution](#)

[Panel 2 Election Law Controversies, Past and Present](#)

Project on American Constitutionalism

[Panel 1 Defending Constitutional Republicanism: The Achievement of George W. Carey](#)

BOOKS TO WATCH FOR

HELENA SILVERSTEIN

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From its legal recognition in Hawaii in 1993, the idea and possibility of same-sex marriage has been a fuse that has ignited political controversy across the United States to the world. In *The Limits to Union: Same-Sex Marriage and the Politics of Civil Rights*, **Jonathan Goldberg-Hiller** (University of Hawaii) explores this incendiary debate and explains the political discourses and tactics that overturn decisions of state courts favorably inclined toward same-sex marriage and gay rights. The opposition of public majorities to court-mandated rights is shown to be an enduring yet postmodern manifestation of political sovereignty, one with broad implications for how we must now come to think about civil rights. Building on developments in postmodern, postcolonial, and queer legal theory, **Goldberg-Hiller** argues that the controversy over legal rights for same-sex marriages has exploded onto the American stage in response to deep-seated anxieties over the fragmented nature of community, changing social hierarchies, and economic and national security in the face of globalization. He shows that the legal fate of the same-sex marriage is more than an issue of the social and political acceptance of lesbians and gays as it rapidly becomes a central site for re-imagining the contours of political sovereignty. *The Limits to Union* is published in the Law, Meaning and Violence Series of the University of Michigan Press.

In *Rights of Inclusion: Law and Identity in the Life Stories of Americans with Disabilities*, authors **David M. Engel** (University of Buffalo Law School, SUNY) and **Frank W. Munger** (New York Law School) provide a novel perspective on how civil rights legislation affects the lives of ordinary Americans. Based on interviews with intended beneficiaries of the Americans with Disabilities Act (ADA), this book argues for a new understanding of rights—one that focuses on their role in everyday lives rather than in formal legal claims. Although all sixty interviewees had experienced discrimination, none had filed a formal protest or lawsuit. Nevertheless, civil rights played a crucial role in their lives. Rights improved their self-image, enhanced their career aspirations, and altered the perceptions and assumptions of their employers and co-workers—in effect producing more inclusive institutional arrangements. Focusing on these long-term life histories, **Engel** and **Munger** show how rights and identity affect one another over time and how that interaction ultimately determines the success of laws such as the ADA. *Rights of Inclusion* is now available from the University of Chicago Press.

In *Patriots, Settlers, and the Origins of American Social Policy*, **Laura Jensen** (University of Massachusetts, Amherst) offers an account of the pivotal role played by entitlement policies during the first hundred years of the United States' existence. Contrary to the story of developmental delay contained in the standard historiography, **Jensen** argues that national social policies not only existed in early America, but also were a major instrument by which the fledgling U.S. government built itself and the new nation. From 1776 on, Federal pensions and land entitlements figured prominently in the growth and empowerment of a unique American state, the consolidation and expansion of the country, and the political incorporation of a diverse citizenry. Available from Cambridge University Press, *Patriots, Settlers, and the Origins of American Social Policy* explains how governing institutions, public expectations, ideas about law and legality, political necessity and public policy gave shape to definitions of need, worth, and eligibility in late eighteenth and nineteenth century America.

Countering conventional wisdom about presidential inability to significantly sway judicial interpretation, **Kevin J. McMahon's** (SUNY, Fredonia) forthcoming book argues that presidents, especially "reconstructive" presidents like FDR, are powerful agents of constitutional change. In *Reconsidering Roosevelt on Race: How the Presidency Paved the Road to Brown*, **McMahon** suggests that under the influence of these presidents, Supreme Courts are predisposed to follow the command of the executive branch. The book reconsiders traditional thought on FDR that gives him scant praise for advances in civil rights. Rejecting these conclusions as overly bound by the president's legislative record, **McMahon** shows how the

Roosevelt administration made a foundational contribution to the federal protection of African American rights. Undertaking a systematic analysis of the Roosevelt administration's policy toward the judiciary, **McMahon** argues that in an effort to lead the Democratic Party away from its conservative southern base and construct the modern presidency, FDR helped forge an institutional mission on the Court that ultimately led to *Brown v. Board of Education*. FDR's conclusion that "southern democracy" was incompatible with his institutional design for twentieth-century America served as a mainspring for the Supreme Court's later commitment to federal civil rights protection. *Reconsidering Roosevelt on Race* will be released this fall from The University of Chicago Press.

Ever-more-frequent calls for the establishment of a rule of law in the developing world have been oddly paralleled by the increasing use of "exceptional" measures to deal with political crises. To untangle this apparent contradiction, *The Jurisprudence of Emergency: Colonialism and the Rule of Law* analyzes the historical uses of a range of emergency powers, such as the suspension of habeas corpus and the use of military tribunals. **Nasser Hussain** (Amherst College) focuses on the relationship between "emergency" and the law to develop a new theory of those moments in which the normative rule of law is suspended. **Hussain** examines British colonial rule in India from the late eighteenth to the early twentieth century in order to trace tensions between the ideology of liberty and government by law, which was used to justify the British presence, and the colonizing power's concurrent insistence on a regime of conquest. **Hussain** argues that the interaction of these competing ideologies exemplifies a conflict central to all Western legal systems—between the universal, rational operation of law on the one hand and the absolute sovereignty of the state on the other. Soon to be released by University of Michigan Press, *The Jurisprudence of Emergency* uses an array of historical evidence to demonstrate how questions of law and emergency shaped colonial rule, which in turn affected the development of Western legality.

Forthcoming this fall from Cambridge University Press is *Public Reaction to Supreme Court Decisions*, by **Valerie Hoekstra** (Arizona State University). In this book, **Hoekstra** looks incrementally at reactions to Supreme Court decisions in the local communities where the controversies began. Using evidence from a series of two-wave panel studies conducted prior to and following the Supreme Court's decisions, **Hoekstra** finds considerable media coverage of these cases and a highly informed local populace. While the rulings did not have a significant impact on how citizens felt about the issues in these cases, the rulings did have an important effect on how citizens felt about the Court. This book provides insights into how the public learns about Supreme Court decisions and how support for the Court is gained and lost as it announces its decisions.

"Diversity" has become a mantra within discussions of university admissions policies and many other arenas of American society. In *Wrestling with Diversity*, **Sanford Levinson** (University of Texas School of Law) wrestles with various notions of diversity. He begins by explaining why he finds the concept to be almost useless as a genuine guide to public policy. Discussing affirmative action in university admissions, including the now famous University of Michigan Law School case, **Levinson** argues both that there may be good reasons to use preferences—including race and ethnicity—and that these reasons have relatively little to do with any cogently developed theory of diversity. Each of the nine essays presented in this book, written over the past decade, develops a case study focusing on a particular aspect of public life in a richly diverse, and sometimes bitterly divided, society. Look for *Wrestling with Diversity* this fall from Duke University Press.

Law's Madness will soon be available from the University of Michigan Press. In this volume, editors **Austin Sarat** (Amherst College), **Lawrence Douglas** (Amherst College), and **Martha Umphrey** (Amherst College) bring together a collection of essays that link the categories of law and madness to see what that linkage generates. Exploring the gray area between the realms of reason and madness, *Law's Madness* suggests a relationship that is both possessive (a madness defined by legal discourse) and constitutive (a madness that resides in law). This collection of essays highlights the ways in which the law takes its definition from that which it excludes, suppresses, or excises from itself, and asks what must be forgotten in order for law to be sustained.

In almost all legal disputing, formalities are employed as a last resort for a small proportion of cases. Case attrition is a constant feature in the legal system, whether criminal or civil, since extensive pre-trial negotiations search for solutions to problems that avoid the costs, risks, and delays of trial. In *Law as Last Resort: Prosecution Decision-Making in a Regulating Agency*, **Keith Hawkins** (Oriel College, Oxford) analyzes the attrition of cases by studying decisions made about their creation, handling, disposal, and prosecution. In doing so, **Hawkins** presents a new theory of decision-making, exploring the conditions under which prosecution is employed to handle occupational health and safety problems. *Law as Last Resort* is now available from Oxford University Press.

Called a fig leaf for inaction by many at its inception, the International Criminal Tribunal for the Former Yugoslavia has grown from an unfunded U.N. Security Council resolution to an institution with more than 1,000 employees and a \$100 million annual budget. With Slobodan Milosevic on trial and more than forty fellow indictees currently detained, the success of the Hague tribunal has caught many of its former critics by surprise. *Justice in the Balkans: Prosecuting War Crimes in the Hague Tribunal* provides a firsthand look at the inner workings of the tribunal as it has moved from an initial period of irrelevance to the first truly effective international court since Nuremberg. The chief reason for its success, argues author **John Hagan** (Northwestern University), is the people who have shaped it, particularly its charismatic chief prosecutor, Louise Arbour. Scheduled for release in September 2003 from University of Chicago Press, *Justice in the Balkans* re-creates how Arbour worked with others to turn the tribunal's fortunes around—reversing its initial failure to arrest and convict significant figures and advancing the tribunal's agenda to the point at which Arbour and her colleagues, including her successor, Carla Del Ponte, were able to indict Milosevic himself.

Within hours after the collapse of the Twin Towers, the idea that the September 11 attacks had “changed everything” permeated American popular and political discussion. In the period since then, September 11 has been used to justify profound changes in U.S. public policy and foreign relations. Bringing together leading scholars of history, law, literature, and Islam, a new volume edited by **Mary L. Dudziak** (University of Southern California Law School) asks whether the attacks and their aftermath truly marked a transition in U.S. and world history or whether they are best understood as part of pre-existing historical trajectories. From a variety of perspectives, the contributors to *September 11 in History: A Watershed Moment?* scrutinize claims about September 11, both in terms of their historical validity and their consequences. Essays range from an analysis of terms like Ground Zero, Homeland, and “the Axis of Evil” to an argument that the U.S. naval base at Guantanamo Bay has become a site for acting out a repressed imperial history. Examining the effect of the attacks on Islamic self-identity, one contributor argues that Osama bin Laden enacted an interpretation of Islam on September 11 and asserts that progressive Muslims must respond to it. Other essays focus on the deployment of Orientalist tropes in categorizations of those “who look Middle Eastern,” the blurring of domestic and international law evident in a number of legal developments including the use of military tribunals to prosecute suspected terrorists, and the justifications for and consequences of American unilateralism. *September 11 in History*, scheduled for release this fall from Duke University Press, suggests that everything did not change on September 11, 2001, but that some bedrocks of democratic legitimacy have been significantly eroded by claims that it did.

Since 1787, only 27 amendments have been proposed by two-thirds majorities in Congress and ratified by three-fourths of the states. During this same time, members of Congress have introduced more than 11,500 proposals, and states have filed close to 400 additional petitions for constitutional conventions to propose amendments. In *Proposed Amendments to the U.S. Constitution: 1787-2001*, editor **John R. Vile** (Middle Tennessee State University) has collected and updated lists of proposed amendments and convention petitions that have been scattered about in a variety of governmental reports. This three-volume work, published by The Lawbook Exchange, includes texts of basic constitutional documents like the Articles of Confederation, the U.S. Constitution and its amendments, and the Confederate Constitution, as well as a comprehensive index of all amendments proposed through 2001.

A man murders his wife after she has admitted her infidelity; another man kills an openly gay teammate after receiving a massage; a third man, white, goes for a jog in a “bad” neighborhood, carrying a pistol, and shoots an African American teenager who had his hands in his pockets. When brought before the criminal justice system, all three men argue that they should be found not guilty; the first two use the defense of provocation, while the third argues he used his gun in self-

Conferences & Events

APSA (<http://www.apsanet.org/mtgs/>)

AUG 28-31

PHILADELPHIA PA

LAW AND COURTS SECTION CHAIR: KEVIN T. MCGUIRE, UNIVERSITY NO. CAROLINA

CON LAW & JURISPRUDENCE SECTION CHAIR: PRISCILLA MACHADO ZOTTI, U.S. NAVAL ACADEMY

PACIFIC NORTHWEST POLITICAL SCIENCE ASSOCIATION (<http://www.lclark.edu/~pnwpsa/>)

OCT 16-18

VANCOUVER BC

JUDICIAL POLITICS: JAMES FOSTER JAMES.FOSTER@OSUCASCADES.EDU

Northeastern Political Science Association (<http://www.cas.suffolk.edu/government/npsa/npsacfp.html>)

Nov 6-8

Philadelphia PA

Congress, Presidency, and the Courts: J. Mark Wrighton MARK.WRIGHTON@UNH.EDU

SOUTHERN POLITICAL SCIENCE ASSOCIATION (<http://www2.gasou.edu/spsa/conference.htm>)

JANUARY 8-10 2004

NEW ORLEANS, LA

JUDICIAL POLITICS: ROY B. FLEMMING, Texas A&M SPSA@GASOU.EDU