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American Journal of Political Science, Vol. 40, No. 1 (Feb., 1996), 261-294.

Stable URL:
http://links.jstor.org/sici?sici=0092-5853%28199602%2940%3A1%3C261%3AMPP%3E2.0.CO%3B2-E

American Journal of Political Science is currently published by Midwest Political Science Association.

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*Measuring Political Preferences**

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Theory: When analysts adopt surrogates of actors' political preferences for purposes unanticipated by the inventors of those measures, they often stretch (but not explicitly assess) the range of reliability and validity.
Hypotheses: The consequences pushing measures beyond their intended purposes may significantly impact research findings, as well as the conclusions drawn from those findings.
Methods: "Methodological audit" of measures developed by Segal and Cover (1989) to represent the political preferences of justices on the United States Supreme Court. Mainly regression analysis using the Segal/Cover scores and vote data drawn from the United States Supreme Court Judicial Database.
Results: Analysts would be well advised to weigh carefully whether adequate tests have been performed before adopting others' preference measures for their own research. More specific conclusions are: 1) scholars should invoke the Segal/Cover scores in the set of circumstances indicated by their developers: aggregated individual-level decisions in civil liberties cases; and 2) students of the judicial process who seek to explore phenomena other than aggregated individual-level voting in civil liberties cases ought to give serious thought to devising new surrogates for judicial preferences.

Measuring the preferences of individuals and collectivities represents one of the most common and basic enterprises of empirical social science research. Although each of the various fields of political science has its own sets of concerns and controversies, virtually all must confront the task of characterizing the preferences of actors, be they political parties, individual legislators, or judges. This is so because much of the discipline concentrates on explaining the behavior of these and other actors. Why do particular combinations of political parties form government coalitions in parliamentary systems? Why do legislators support some bills and reject others? Why do judges vote the way they do? To address these and a multitude of other questions, we must have measures of the actors' preferences—measures that we deem to be reliable and valid.

*We gratefully acknowledge support provided by the National Science Foundation (SES-9024640, SBR-9320284; SBR-9320666) and the Business, Law and Economics Center at Washington University. Lawrence Baum, Robert Durr, Kenneth J. Meier, John Sprague, and the anonymous reviewers of the *American Journal of Political Science* provided extremely useful comments on an earlier version of this article. Finally, we are indebted to Jeffrey A. Segal for his guidance, patience, and support.

American Journal of Political Science, Vol. 40, No. 1, February 1996, Pp. 261–294

Yet developing such measures is a formidable task. Perhaps the most fundamental challenge is to locate sources of data that are independent of the behavior of the political actors under scrutiny. To measure the political preferences of legislators by their votes at year 1 and, then, to use those very votes to explain their behavior at year 1 is to argue that legislators vote the way they do because they vote the way they do. Responding to this challenge, scholars have turned to a vast array of independent sources, from the judgments of experts (e.g., Browne and Dreijmanis 1982; De Swaan 1973; Dodd 1974, 1976; Taylor and Laver 1973) to content analyses of policy platforms (or manifestos) issued by political parties in electoral campaigns (Budge, Robertson, and Hearl 1987).

Given the difficulty and complexity of devising such independent measures, it is not surprising to note that scholars have a tendency to borrow them without questioning their suitability for a particular research problem. After all, once an analyst has gone to the trouble of, say, coding the manifestos of a large number of European political parties, it is easier for other analysts to use that set of measures than to construct another. Reliability and validity are assumed and the research proceeds.

We argue that scholars should exercise great caution in using others' measures for their research. In particular, we suggest that they conduct "methodological audits" (Norpoth and Segal 1994, 711) before proceeding to adopt a particular set of preference scores. The balance of the article advances an approach for conducting such an audit, one that is designed to assess systematically the extent to which a given set of indicators provides a useful and appropriate means of estimating preferences. To illustrate our approach, we investigate measures developed by Segal and Cover (1989) to represent the political preferences of justices on the Supreme Court of the United States. We choose to focus on these scores because they have been widely adopted by judicial specialists but for purposes that Segal and Cover did not envision. That is, some scholars of the Court have attempted to push the Segal/Cover scores beyond their intended limits, along the way stretching (and, perhaps, surpassing) the range of the reliability and validity of the measures.

This noted, we cannot help but think that our general approach will appeal to scholars working in other areas. Judicial specialists may confront daunting measurement problems when they set out to study decision making. Surely, however, it is not only in the judicial field that analysts adopt surrogates of actors' preferences for purposes unanticipated by the inventors of the measures; and it is not only in the judicial field that "bad science" can result when borrowed measures are imported into inappropriate research contexts. We thus hope that scholars of all stripes will find our

methodological audit a useful starting point as they seek to determine the reliability and validity of the measures they adopt.

Measuring Elite Political Preferences: A View from the Judicial Literature

Although all analysts studying elite behavior confront measurement problems, it is perhaps scholars of judicial politics who face the greatest obstacles of all. Virtually all approaches to the study of legal decision making—including the attitudinal model,[1] which is, perhaps, the predominant one—ideally require the analyst to identify the preferences (or attitudes) of jurists from sources independent of the votes they cast (see Segal and Spaeth 1993, 361–2). Yet, because judges and justices—unlike other political actors—are not disposed to uncovering their attitudes and values, scholars have used their votes as surrogates for those beliefs. Here lies the crux of the problem: the measures for the independent and dependent variables are identical. As Segal and Spaeth (1993, 361) explain: "The most damning criticism of the attitudinal model in earlier days was that it employed circular reasoning inasmuch as the attitudes used to explain the justices' votes are based on these self same votes . . . [E]arly attitudinal theorists used the results of their cumulative scales and factor analyses to explain the justices' votes, with the scales and factors used to evidence the explanatory power of the attitudinal model."

Measurement Strategies

Because the judicial process literature assumes that political preferences are the most important determinants of the vote (see note 1), scholars have viewed the circularity problem as a severe impediment to their work

[1]The attitudinal model posits that the "facts of a case vis-à-vis the ideological attitudes and values" of individual justices provide the single best explanation for whether they vote to affirm or reverse a particular judgment, and that those attitudes are relatively impervious to internal or external pressure (Segal and Spaeth 1993, 65). The most important challenge to the attitudinal model is presented by positive political theorists (see Segal and Spaeth 1994), who argue that justices' votes may reflect sophisticated and strategic choices, rather than naive behavior (e.g., Eskridge 1991).

It is the similarity between these approaches, not their differences, that is most important for this article: both argue that votes are largely the product of political preferences. What is more, scholars in both schools have used the Segal/Cover scores (discussed above) to measure judicial preferences (e.g., Eskridge 1991; Segal and Spaeth 1993). For the purposes of this paper, then, we assume that preferences are important determinants of the vote. Accordingly, a critical quality of a "good" measure of judicial preferences is its ability to account for the vote (i.e., a strong statistical relationship should exist between the measure and the vote). Segal and Cover, themselves, assessed their measure on these terms.

and have offered three approaches to overcome it. The first uses the *past* votes of the justices (in particular areas of the law) to align them on left-right scales. This approach has its benefits. In particular, as researchers have demonstrated (Epstein, Walker, and Dixon 1989; Segal and Spaeth 1993, Ch. 6), past votes are an excellent predictor of current behavior. Some scholars argue, however, that they are not free of problems, for they may fall short of evaluating attitudes from sources independent of the vote (e.g., Baum 1994, 4; but see Spaeth 1995).

The two other approaches, both of which have analogs outside the judicial field, were designed to overcome this shortcoming. One was offered by David J. Danelski. In a 1966 article, he examined pre-nomination speeches delivered by two Supreme Court justices (Brandeis and Butler) to extract their values in much the same way that scholars in other areas of political science now content-analyze elite-produced documents, such as political party manifestos (see Budge, Robertson, and Hearl 1987), to locate actors' preferences.[2] Danelski's results were quite robust: a comparison of the justices' pre- and post-confirmation positions on economic issues revealed a strong relationship between the two.

The other was introduced more recently by Jeffrey A. Segal and Albert D. Cover (1989). They sought to derive independent measures of ideology by content-analyzing newspaper editorials written between the time of justices' nomination to the Court and their confirmation. Segal and Cover, in a sense, viewed editorial writers as "experts" on whose judgments they could rely for representations of judicial candidates' preferences. Seen in this way, the Segal/Cover approach resembles strategies invoked by those comparativists who have asked experts to locate the preferences of political parties in policy space (see, e.g., Browne and Dreijmanis 1982) or who have aggregated the responses of party sympathizers in mass sample surveys to capture parties' positions (e.g., Inglehart 1984; Sani and Sartori 1983). As their counterparts in the comparative study of political parties have often done, Segal and Cover translated their "experts' judgments" (i.e., newspaper editors' assessments) into ideological values or scores. In Segal and Cover's case, the scores of the justices ranged from -1 (unanimously conservative) to 0 (moderate) to $+1$ (unanimously liberal). They then tested these independent measures against votes cast by the justices, with the results indicating a high correlation between the two (see Segal and Cover 1989).

[2]More specifically, the research team headed by Budge, Robertson, and Hearl (1987) content-analyzed election manifestos issued by parties in nineteen democracies over the post-war period. Laver and Budge (1992) and their collaborators have updated the manifesto data and have also content-analyzed authoritative policy documents issued by governments.

Accordingly, Segal and Cover took their measure to provide a reliable and valid indicator of judicial preferences, but they issued important caveats. The first is that editorials do not necessarily convey accurate perceptions of the political preferences of the justices (561). The second and, for our purposes, more significant qualification is that Segal and Cover explicitly recognized that their measures are not broad-gauged surrogates for all attitudes but supply a reasonable evaluation of preferences over civil liberties and rights issues. As they wrote (561), "Because the statements in newspaper editorials deal almost exclusively with support by the justices for civil liberties and civil rights, we use as our dependent variable the votes of . . . justices in all formally decided civil liberties cases . . ."

Adoption of the Segal/Cover Scores

In some fields of political science, the kind of approach Danelski advocated—content analyses of materials produced by the actors under investigation—is now the dominant measurement strategy.[3] For several reasons, however, in the judicial realm, the Segal/Cover approach has received far more attention than that advanced by Danelski. For one thing, Segal and Cover furnished scores for each of the justices serving between 1952 and 1988, while Danelski's analysis was limited to two justices. For another, the newer approach *appears* far easier to replicate and update than Danelski's (e.g., Bowen 1992; but see note 7).[4] Accordingly, and despite the relative recency of the research, authoritative textbook accounts of judicial decision making have cited the Segal/Cover approach with approval (e.g., Baum 1992). Many scholars have integrated the scores into their considerations of decision making, publishing their results in scholarly volumes (e.g., Segal and Spaeth 1993) and in the *American Journal of Political Science* and the *American Political Science Review,* among other important disciplinary journals (e.g., Kearney and Sheehan 1992; Mishler and Sheehan 1993; Segal, Cameron, and Cover 1992; Sheehan, Mishler, and Songer 1992). Indeed, it would hardly be an exaggeration to write that almost every recent study of Court decision making has—in one way or another—invoked these scores.

Some of these studies, though, have ignored the admonitions offered by Segal and Cover. Kearney and Sheehan (1992), for example, borrowed the scores to study decision making in disputes outside the civil liberties

[3]For example, the manifesto data have largely, though not fully (see Laver and Hunt 1992), supplanted the "expert judgment" approach as the dominant strategy in contemporary comparative research on political parties. Later we explain the reasons for this change.

[4]Indeed, Segal and his colleagues have already updated and backdated their scores (Segal et al. 1995).

realm (e.g., economics and federalism). Others have not disregarded Segal and Cover's explicit caveats but instead have tried to push the measures in ways not anticipated by their developers: to array justices on policy scales, even though Segal and Cover never compared justices in this way; to study Court-level decision making, even though Segal and Cover created their scores to tap the ideological values of individual justices; and to track decision making over time, even though Segal and Cover tested their scores against the cumulated voting records of justices.

Again, Segal and Cover did not design the scores for these purposes. Yet when judicial scholars used the scores in ways not contemplated by their inventors, they did what political scientists typically do with measures of preferences (and many other kinds of indices): push the extant measures to see just how far they can go and how widely they can be applied, stretching (but not explicitly assessing) the range of their reliability and validity. The question raised by this practice is one of consequences: What are the consequences of pushing measures beyond their intended purposes? To answer this question, we conduct a methodological audit of the Segal/Cover scores. More specifically, we explore the implications of using those scores in the ways that other scholars have, that is, for four general types of research. First, we consider whether or not they help to predict votes in a range of issue areas. Second, we probe their ability to array the justices on ordinal policy scales in ways that would prove useful to judicial specialists. Third, we investigate the extent to which the Segal/Cover scores can be used to study Court-level decision making. Finally, we weigh their ability to account for micro- and macro-level decision making over time.

Individual Decisions and Issue Areas

During the course of the average United States Supreme Court term, the justices deal with a wide range of legal issues.[5] Many of these are of

[5]The following depicts the kinds of cases orally argued before the United States Supreme Court during the 1991 term. As noted, civil liberties cases (criminal procedure, civil rights, First Amendment, due process, privacy, and attorneys) comprise only slightly more than half (51.3%).

Issue Area	*Number of Cases*	*Percent of Total Cases*
Criminal Procedure	21	19.3
Civil Rights	18	16.5
First Amendment	8	7.3
Due Process	5	4.6
Privacy	2	1.8
Attorneys	2	*1.8*
Unions	1	.9
Economics	27	24.8
Judicial Power	16	14.7

little or no interest to the average American or to the media: citizens and the press, as Franklin and Kosaki (1991) indicate, focus on the splashier part of the Court's docket. Today we tend to associate "splashy" with issues of civil liberties such as abortion, capital punishment, and hate speech. It seems reasonable to assume that editors seeking to influence public opinion would address those issues most salient to their readers. Accordingly, we might expect that editorial writers, on whose judgments Segal and Cover relied to create their scores, would be inclined to evaluate a judicial candidate's ideological inclination on the basis of a few key civil liberties issues, rather than on the range of issues potentially facing the new justice.

As noted earlier, Segal and Cover recognized that their measures are not broad-gauged surrogates for all attitudes but provide a reasonable evaluation of preferences over highly salient issues. In fact, in comparing their scores with the actual votes of the justices, they used only civil liberties cases. Because Segal and Cover did not assess their measures against votes in other areas of the law, however, some researchers were apparently left with the impression that they had meaning—and predictive power—for votes in other cases. Bowen (1992) used the scores to evaluate environmental disputes; Kearney and Sheehan (1992) invoked the Segal/Cover scores to study the success of state and local governments in a variety of legal disputes (e.g., First Amendment, federalism, and economics); and Sheehan, Mishler, and Songer (1992) adopted the scores to explore the success of parties in *all* cases decided with a full opinion between 1953 and 1988.[6]

Is it reasonable to assume, as some authors do, that a set of measures designed around civil liberties issues might aptly explain outcomes in other or even all areas of the Court's docket? Bowen's (1992) work gives us pause, as he found virtually no relationship between the justices' votes in environmental, energy, and technology cases and the Segal/Cover scores.

Federalism	5	4.6
Interstate Relations	0	0
Federal Taxation	4	3.7
Total	109	100.0

Source: United States Supreme Court Judicial Database, with orally argued citation as the unit of analysis.

[6]Similarly, statements in authoritative accounts of the Court convey the message that the Segal/Cover scores are as applicable to First Amendment cases as they are to, say, economic ones. As Baum (1992, 145) remarks: "Perceptions of Court nominees' ideological stances correlate well with the *records* that they later develop on the Court, strongly suggesting that their preferences structure their approaches to *issues* before the Court" (emphasis added; see also Gibson 1991, 259).

While suggestive, his research suffered from two problems: a relatively small number of votes and an inexact replication of the Segal/Cover scores for the newest justices.[7]

To consider more fully and precisely the relationship between the Segal/Cover scores and diverse issue areas, we relied on an expanded version of the Segal/Cover scores compiled by Segal et al. (1995). As Table 1 indicates, this new list is identical to the one developed by Segal and Cover, except that it includes the most recent appointments (Souter, Thomas, Ginsburg, and Breyer), and it covers all justices who served during the Warren, Burger, and Rehnquist Court eras.[8] We used Harold J. Spaeth's United States Supreme Court Judicial Database (ICPSR #9422) to calculate the percentage of cases in six general issue areas (with one—civil liberties—broken down into five component parts) in which justices took a liberal position over their career (i.e., aggregated liberal percentages).[9] Appendix A reports the relevant decision making data.[10]

Following Segal and Cover's approach, we used OLS to estimate the following equation:

$$V_{i_j} = a + bS_i + e_i \qquad [1]$$

where V_{i_j} is the vote of justice i (as measured by the percent liberal votes cast) in issue area j and S_i is the score assigned to justice i by Segal and Cover. Table 2 reports the results of this analysis, including the estimated coefficients and summary statistics for each issue area. Several interesting findings emerge. First, the analyses put to rest the notion apparently held by some scholars that the newspaper editorial scores are universally appli-

[7]Bowen's scores were −.13 for Souter and −.36 for Thomas; the Segal et al. (1995) updating shows the scores to be −.34 for Souter and −.68 for Thomas.

[8]Readers will note a change in our Table 1 from the Segal/Cover article and Segal et al. (1995). To each justice's score we added one and divided by two to remove the negative signs. This was the same approach taken in Cameron, Cover and Segal (1990) but in Table 2 of their piece, they incorrectly computed O'Connor's score. (In correspondence with us, Jeffrey A. Segal verified this mistake). Because the vote data end with the 1991 term, Ginsburg and Breyer are excluded from all analyses in this article. We list their scores for readers' information only.

[9]We omitted from the analyses that follow those issues in which the justice participated in less than ten cases.

[10]In addition, the Appendix provides Spaeth's definitions of liberalism for each of the areas under analysis (see also Segal and Spaeth 1993, Ch. 6 for more information on using these definitions to characterize Court decisions as liberal and conservative). These were the same definitions used by Segal and Cover, along with virtually all of those scholars who adopted their preference measure. It thus seems entirely appropriate that we invoke them for our audit.

Table 1. Segal/Cover Scores for Justices Serving during the Warren, Burger, and Rehnquist Court Eras

Justice	Segal/Cover Score
Black	.875
Reed	.725
Frankfurter	.665
Douglas	.730
Jackson	1.000
Burton	.280
Clark	.500
Minton	.720
Warren	.750
Harlan	.875
Whittaker	.500
Brennan	1.000
Stewart	.750
White	.500
Goldberg	.750
Fortas	1.000
Marshall	1.000
Burger	.115
Blackmun	.115
Powell	.165
Rehnquist	.045
Stevens	.250
O'Connor	.415
Scalia	.000
Kennedy	.365
Souter	.330
Thomas	.160
Ginsburg[a]	.680
Breyer[a]	.475

Note: Readers will note a change in this list from the one reported in Segal and Cover (1989) and in Segal et al. (1995). To each justice's score we added one and divided by two to remove the negative signs. Thus, the scores reported here range from 0 (extreme conservative) to 1.00 (extreme liberal), and not −1 to 1 as reported in Segal/Cover and Segal et al.
[a]Ginsburg and Breyer are listed for readers' interest only. Because the data used throughout this article end with the 1991 term, they are not included in any of the analyses.
Source: Segal and Cover (1989), as updated by Segal et al. (1995).

Table 2. Results of Regressing Aggregated Liberal Votes on Segal/Cover Scores

Issue Area	Constant (std. error)	Slope (std. error)	Adjusted r^2	SEE
Civil Liberties (n = 27)	27.56**	43.15**	.43	15.78
	(6.0)	(9.5)		
Criminal Procedure (n = 27)	19.11**	47.46**	.44	17.12
	(6.47)	(10.32)		
Civil Rights (n = 26)	36.11**	39.87**	.38	15.84
	(6.06)	(9.96)		
First Amendment (n = 25)	31.56**	41.02**	.25	21.12
	(8.46)	(13.56)		
Due Process (n = 20)	36.39**	43.82	.57	12.03
	(5.01)	(8.54)		
Privacy (n = 13)	20.04*	39.61*	.35	17.59
	(7.78)	(14.44)		
Unions (n = 24)	45.12**	23.19**	.28	11.32
	(4.64)	(7.39)		
Economics (n = 27)	43.71**	21.42*	.18	13.78
	(5.21)	(8.31)		
Federalism (n = 24)	53.22**	9.99	.12	7.44
	(3.05)	(4.85)		
Judicial Power (n = 27)	38.40**	9.63	.04	11.37
	(4.30)	(6.86)		
Taxation (n = 24)	67.94**	3.19	.00	12.14
	(4.98)	(7.92)		

Note: The numbers after each issue area indicate how many justices were included in the analysis. Only justices participating in more than ten cases were included. See Appendix A for participation rates.

Source for Voting Data: United States Supreme Court Judicial Database, with orally argued citation plus split votes as the unit of analysis.

*$p \leq .05$.

**$p \leq .01$.

cable. As depicted, the scores have little explanatory power for most non-civil liberties areas—federalism, judicial power, and taxation. Even the results for the general category of economics are not unproblematic. Though the coefficient is statistically significant at the .05 level, the adjusted r^2 of .18 indicates that the scores explain very little about the vote; the standard error of the estimate (13.8) suggests that our predictions will not be particularly accurate. This latter point is especially troublesome since more than half of the 27 justices' support for economic liberties was in the 40 to 60% range. If we used the 50% mark as the cutoff between a supporter of economic liberties and an opponent and compared the estimates for those

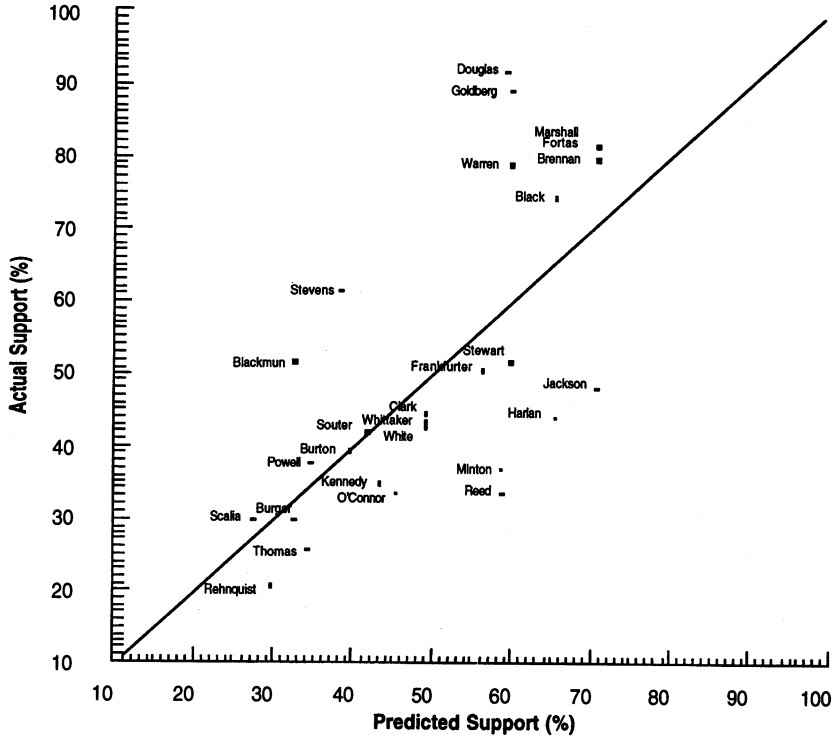
"moderate" justices with their actual voting, we would make incorrect categorizations for six. Along these lines, Reed was the worst case (with our prediction of 58.8% support more than 25 points above his actual support of 33.3).[11] Stevens presented the opposite problem: we predicted support of only 38.5 when the actual value was 61.4. It is also worth noting that the findings displayed in Table 2 did not simply result from adding new justices to the original Segal/Cover complement. Rerunning the analyses and including only their original justices had virtually no effect on the results for non-civil liberties cases: federalism, judicial power, and taxation still produced insignificant coefficients; economics improved only slightly (the adj. r^2 increased from .18 to .24 and the SEE declined from 13.8 to 11.9).

Second, even though the Segal/Cover scores do a reasonably good job of accounting for variance in civil liberties votes (and in those subsets of cases comprising civil liberties), the results call for care in interpretation. Of particular concern here is the rather large SEE (15.8), which indicates that our predictions will not be as accurate as we might like. To consider this in more detail, we plotted the actual support against the predicted values. Figure 1 displays the results. As indicated, we underestimated Douglas's liberalism by over 32 percentage points and we severely overestimated Reed's (a 25 percentage point differential). Goldberg, too, was a serious outlier, as he was in the Segal/Cover analysis, but so is a current justice, Stevens (for whom we predicted a liberalism score of 38.4% when his actual support is over 60%). Harlan and Jackson present yet another problem: our predictions suggest a degree of liberalism that neither possessed. Harlan's predicted score of 65.3% was too high by over 21 percentage points; Jackson's actual support of the liberal position was about 47%, while the predicted value was 70.7—a 23 percentage point difference.

On the more positive side, we cannot ignore the fact that using one variable (ideological values as tapped by editorials) to predict—with a high degree of accuracy—entire lifetimes of voting behavior for 27 jurists borders on the extraordinary. Even though differences among justices based on era do emerge (observe that six of the eight outliers were appointed before the onset of the Warren Court era, a point we soon consider more fully), note the excellent predictions yielded for the two most recent appointees included in our study (Souter and Thomas). For Souter the predicted value of 41.8 was almost right on the money (his actual score was 41.7); the scores overestimated Thomas's liberalism but only slightly

[11] Overall, though, Douglas was the largest residual, with our prediction of 59.1 underestimating his liberalism by nearly 32 percentage points (see Figure 1 below). Still, our prediction would have classified him as the economic liberal he was (he supported economic liberties in 91.5% of the cases).

Figure 1. Actual versus Predicted Support in Civil Liberties Cases



Note: Predicted support is based on this equation: $\hat{Y} = 27.56 + 43.15$(ideological value score). See Tables 1 and 2.

(8.9%) with no serious damage to an overall classification of conservatism (an actual score of 25.6 against a predicted value of 34.5).

In sum, the Segal/Cover measures do a good job of explaining decision making in those cases for which they were designed, civil liberties. Such a result is hardly a surprise; after all, newspaper editors are far more likely to focus on, say, capital punishment than on tax code changes. Accordingly, a recommendation of our study is that analysts would be wise to avoid editors' perceptions as surrogates for the attitudes of individual justices in litigation involving issues outside the realm of civil liberties.

Spatial Arrays of Justices

Students of the judicial process—like their counterparts in comparative and legislative politics—occasionally seek to order political actors on pol-

icy scales. For example, if judicial analysts want to test theories predicting the formation of minimal connected winning coalitions,[12] then they need to know how the justices—on a given Court—are arrayed on an underlying ideological scale (see Rohde 1972a, 1972b; Rohde and Spaeth 1976). Along similar lines, spatial arrays allow for the identification of the median justice (in one-dimensional space) or the core (in two dimensions), who should always dominate outcomes (see, in general, Black 1958 and, in particular, Segal 1986). In conducting this research, the typical procedure is to posit one- or two-dimensional policy space and devise some means to locate actors' positions in that space.

Scholars are already starting to use the Segal/Cover scores to array justices on ordinal scales (e.g., Eskridge 1991); indeed, Segal (with Spaeth 1993, 223) himself did so, lending credence to this usage. The question we raise is whether this is a reasonable adaptation of the scores. If it is, then they may prove invaluable; if not, then problems could abound, for the misplacement of justices in ideological space could lead to incorrect predictions about voting behavior, as well as to errors in inference when research seeks to test such specific hypotheses as the minimal connected winning prediction.

To assess the ability of the Segal/Cover scores to locate accurately the justices along a policy scale, we computed Spearman's r for each term. As invoked here, this statistic ranks the votes of the justices in civil liberties cases (on a nine-person Court, the values range from 1 [most conservative] to 9 [most liberal]); ranks those produced by the Segal/Cover scores (with 1 indicating most conservative and 9 indicating most liberal); and correlates the two. To establish a baseline by which to make comparisons with the Segal/Cover scores, we also computed Spearman's r for the correlation between ranks at voting in term t and those at term $t - 1$.

Table 3 displays the Spearman's r and associated Z-scores for each term. Consider, first, the correlations between the Segal/Cover and vote ranks. As the Z-scores indicate, the results are rather mixed, with no significant correlations emerging prior to 1970 and their strength tapering off after the 1985 term. Put somewhat differently, the Segal/Cover scores produce their best results for the Burger Court terms (1969–1985) and their worst ones for the Warren Court (1953–1968 terms). They attain only mod-

[12] Axelrod (1970) assumes that political actors locate themselves and others on a single, ordinal scale (for example, a left-right dimension). He predicts minimal connected winning coalitions, which join actors adjacent to each other on the policy spectrum and which would slip below a majority or stop being connected if any actor were to withdraw. Rohde (1972b) and Rohde and Spaeth (1976) adapted this reasoning to account for opinion coalition formation on the United States Supreme Court.

Table 3. Rank-Order Correlations: Civil Liberties Votes, 1953–1991 Terms

	Segal/Cover Scores		Lagged Votes	
Term	Spearman's r_s	Z	Spearman's r_s	Z
1953	.42	1.18	—	—
1954	.54	1.53	.78	2.20
1955	.51	1.44	.76	2.16
1956	.66	1.89	1.00	2.83
1957	.56	1.57	.97	2.74
1958	.31	0.88	.93	2.63
1959	.52	1.48	.71	2.00
1960	.46	1.29	.88	2.48
1961	.39	1.10	.95	2.68
1962	.07	0.21	.94	2.65
1963	.06	0.16	.96	2.72
1964	−.09	−0.24	.95	2.68
1965	.19	0.53	.95	2.69
1966	.22	0.63	.98	2.77
1967	.28	0.80	.92	2.61
1968	.08	0.23	.76	2.16
1969	.49	1.31	.79	2.08
1970	.71	2.01	.91	2.56
1971	.87	2.45	.96	2.73
1972	.81	2.28	.93	2.64
1973	.89	2.52	.92	2.59
1974	.87	2.47	.97	2.74
1975	.94	2.66	.98	2.76
1976	.93	2.63	.98	2.77
1977	.97	2.73	.91	2.58
1978	.82	2.33	.87	2.45
1979	.75	2.12	.88	2.50
1980	.77	2.16	.95	2.69
1981	.71	2.00	.95	2.69
1982	.64	1.81	.93	2.63
1983	.70	1.97	.89	2.51
1984	.68	1.93	.97	2.74
1985	.72	2.05	.92	2.59
1986	.56	1.59	.91	2.56
1987	.58	1.63	.88	2.49
1988	.49	1.38	.85	2.39
1989	.63	1.78	.85	2.39
1990	.58	1.65	.93	2.63
1991	.37	1.04	.81	2.29

Source for Voting Data: United States Supreme Court Judicial Database, with orally argued citation plus split votes as the unit of analysis.

erate success for the Rehnquist Court (1986–1991 terms), although it may be too soon to reach any firm conclusions.

Why do the Segal/Cover scores perform so poorly, particularly for portions of the Warren Court era? One explanation centers on the degree of variation in ideology during those terms: for the most part, the justices of the Warren Court era were quite liberal and, apparently, the Segal/Cover scores are not precise enough to distinguish among shades of liberalism. More generally, the higher the variation in a given term the better the Segal/Cover scores perform.[13]

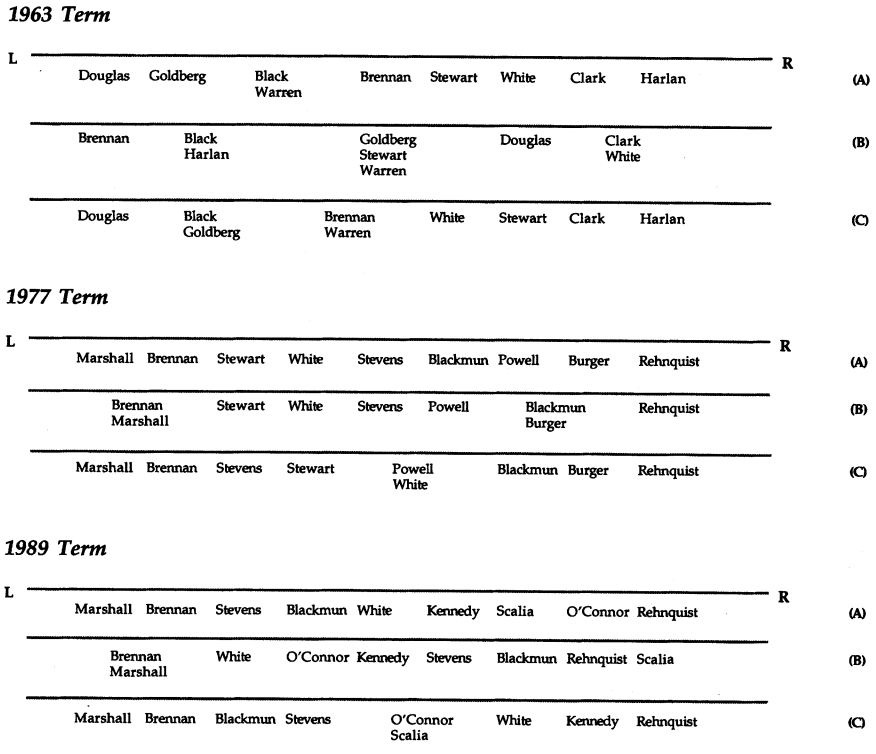
Turning to the correlations between votes and their lags, also presented in Table 3, we see a marked contrast with the Segal/Cover scores. For all but one term (1977), the lag produced better results than did the Segal/Cover scores. What is more, unlike the Segal/Cover scores, the lagged votes' predictive capacity does not depend on variation in voting.

To be sure, it may be unfair to pit the Segal/Cover scores against lagged values of past votes. Indeed, because there is virtually no area in political science where any single variable performs better than $t - 1$ in predicting t, one might argue that the lags represent a ceiling, rather than a baseline. The comparison is useful to the extent that it reinforces the message that researchers should think twice before using the Segal/Cover scores to array the justices on a unidimensional scale, especially for Courts evincing little ideological variance. Figure 2 drives home this general point by displaying spatial arrangements for three Court terms (1963, 1977, 1989). Suppose one sought to test a proposition from the game-theoretic literature, that justices are likely to form minimal connected winning opinion coalitions (see Rohde 1972b). Further assume that one wanted to do so for the 1989 term (the "A" in Figure 2 shows the continuums based on actual voting patterns). During the 1989 term, 34 minimal winning coalitions formed; from the A scale we also know that 28 of the 34 were minimal connected winning (20 were comprised of Rehnquist, O'Connor, Scalia, Kennedy, and White; the remaining 8 were formed by Marshall, Brennan, Stevens, Blackmun, and White). If we used the Segal/Cover scores to array the justices ("B" in Figure 2) and, then, to assess the formation of minimal connected winning coalitions in 1989 we would conclude that *none* formed. In contrast, the array for the 1988 term ("C" in Figure 2) would capture 20 of the 28. Thus, the Segal/Cover scores would lead us to make incorrect inferences, at least about the 1989 term of the Court.

Figure 2 alludes to another problem with the Segal/Cover scores: an

[13]To determine this, we took the standard deviation of voting per term and correlated it with the Spearman's r depicted in Table 3. The correlation for the Segal/Cover scores was significant; that obtained for the lagged votes was not.

Figure 2. Justices Arrayed on Ordinal Scales: Civil Liberties Votes, 1963, 1977, 1989 Terms



Note: Scales are from left (L) to right (R). (A) is the scale based on actual voting for the term; (B) is the scale based on the Segal/Cover scores; and (C) is that based on voting in the previous term.

Source for Voting Data: United States Supreme Court Judicial Database, with orally argued citation plus split votes as the unit of analysis.

inability to identify the median justice. Of the three terms shown in the figure, only for 1977 did the Segal/Cover scores correctly display the median justice (Stevens). More broadly, out of the 39 terms included for analysis, the Segal/Cover scores correctly identified the median in only about a quarter. And, again, the vast majority of correct predictions came during the Burger Court era, the period for which the scores yield their best results. This finding raises questions about the performance of the Segal/Cover scores over time—an issue to which we turn shortly. Suffice it to note for now that the Segal/Cover scores are not particularly useful for arraying

justices in policy space—even if that policy space concerns civil liberties, the area for which the scores evince their greatest predictive power. Judicial scholars who use them for such purposes run the risk of rejecting their hypotheses out of hand.

Court Decisions

Segal and Cover devised their measures to tap the ideological values of individual justices, but other scholars have aggregated the scores by various means to study Court-level decision making. Sheehan, Mishler, and Songer (1992, 465), for example, created a "composite measure of the ideology of the Court" by summing the Segal/Cover scores "across the nine justices who comprise the Court in any given year" (see also Mishler and Sheehan 1993).

To consider the ability of the Segal/Cover scores to account for the collective decision, we used the Supreme Court Database to calculate the percentage of cases (in the same issue areas depicted in Table 2) in which the Court took the liberal position for each term between 1953 and 1991 (Appendix B contains the relevant data).[14] For each term, we computed the mean of the Segal/Cover measures based on the scores of those justices sitting on the Court at that time.[15] Then, for each issue area, we used OLS to estimate the following equation:

$$CV_{t_j} = a + bS_t + e_{t_j} \qquad [2]$$

where CV_{t_j} is Court vote at term t (as measured by the percent of liberal decisions) in issue area j and S_t is Segal/Cover scores' mean for the Court at term t. For purposes of comparison (though again we realize it may be an unfair one), we also regressed Court vote on its lagged value, thus estimating the equation:

$$CV_{t_j} = a + bCV_{t-1_j} + e_{t_j} \qquad [3]$$

where CV_{t-1_j} is Court vote in issue j at term $t - 1$.[16]

[14] We excluded terms during which the Court decided less than five cases in the given issue area. Due Process and Privacy were completely eliminated for insufficient cases.

[15] We also computed the medians and sums (for 1969, when only eight justices sat on the Court, we added the mean to the sum so that the number of justices would be consistent across terms). The results did not differ much from those displayed in Table 4.

[16] We estimated slopes for Equation [3] only when data existed for the whole series, with the exception of the First Amendment category for which sufficient cases existed for all but the first term, 1953. In estimating the lag coefficient, we dropped the 1953 term.

Table 4. Results of Regressing Liberal Court Voting on Mean of Segal/Cover Scores and on Mean of Lagged Votes, 1953–1991

	Segal/Cover Scores			Lagged Votes		
	Slope (s.e.)	Adj. r^2	SEE	Slope (s.e.)	Adj. r^2	SEE
Civil Liberties (n = 39)	69.19** (8.24)	.65	8.54	.77** (.11)	.58	9.48
Criminal Procedure (n = 39)	68.73** (10.72)	.51	11.11	.62** (.13)	.37	12.80
Civil Rights (n = 39)	61.36** (11.80)	.41	12.23	.57** (.14)	.31	13.36
First Amendment (n = 38)	61.06** (16.60)	.25	17.20	.29 (.16)	.06	19.47
Economics (n = 39)	66.73** (9.45)	.56	9.80	.63** (.13)	.38	11.74
Unions (n = 32)	41.66 (23.59)	.06	20.76			
Federalism (n = 33)	13.34 (21.70)	.00	20.95			
Judicial Power (n = 39)	13.90 (12.72)	.01	13.18	.42** (.15)	.15	12.35
Tax (n = 17)	1.97 (30.80)	.00	16.17			

Note: For information on the issue areas, see Appendix A. We did not include areas (Privacy, Due Process) or terms that had fewer than 5 cases per term. The numbers in parentheses indicate the number of terms included in the analysis. Civil Liberties includes the subsets listed (criminal, civil rights, First Amendment, as well as Due Process and Privacy). Appendix B contains the relevant raw data.

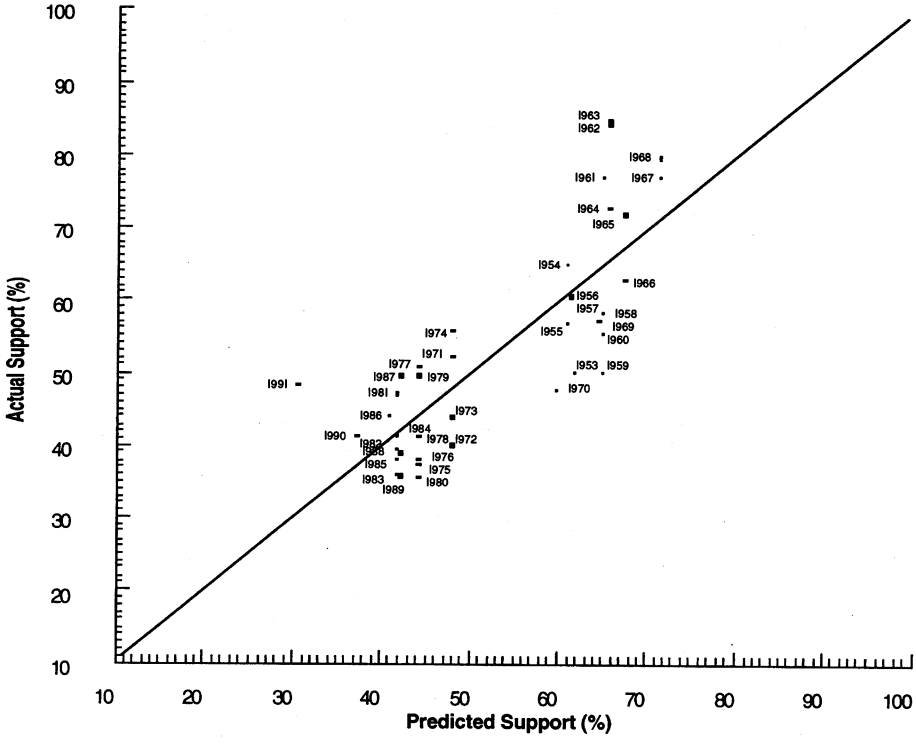
Source for Voting Data: United States Supreme Court Judicial Database, with orally argued citation as the unit of analysis.

*$p \leq .05$.

**$p \leq .01$.

Table 4 reports the coefficients, standard errors and adjusted r^2s resulting from these computations. In general, the results parallel our micro-level examination. With the notable exception of economics, the Segal/Cover scores hold virtually no explanatory power for issues other than civil liberties. The coefficients for unions, federalism, and taxation fail to attain acceptable levels of significance. The inability of the Segal/Cover scores to predict voting in judicial power cases (in contrast to the lagged variable, which performs reasonably well for this set of cases though more poorly for civil liberties) is especially discouraging since these represent over 10% of the Court's plenary docket. More promising are the results from the analysis of economic liberties issues: while the Segal/Cover scores did not

Figure 3. Actual versus Predicted Support in Civil Liberties Cases, 1953–1991 Terms

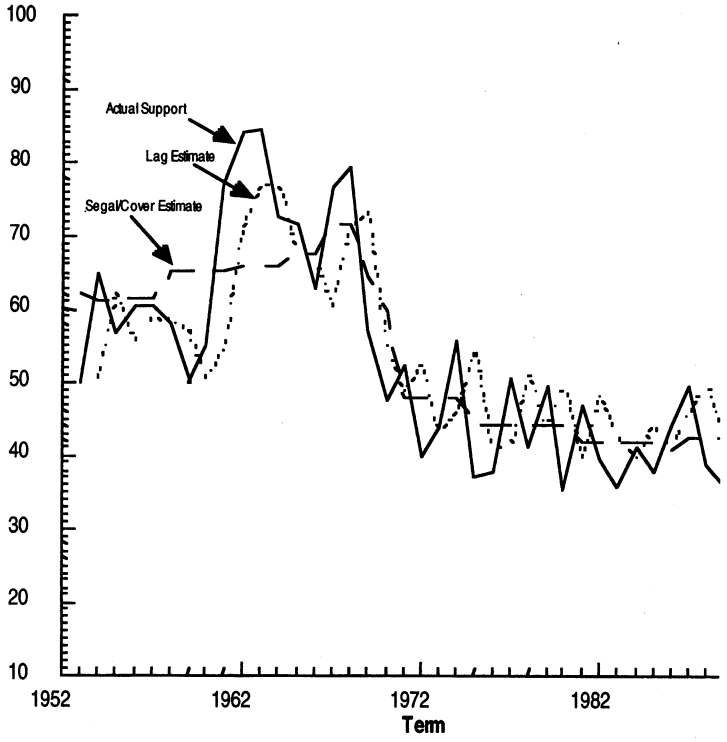


Note: Predicted support is based on this equation: $\hat{Y} = 14.02 + 69.19$(ideological value score). See Table 4.

do a particularly good job in predicting individual-level voting in such cases, they perform quite well at the macro level—even somewhat better than the lagged votes.

In the civil liberties realm, the Segal/Cover measures yield adequate results, but two factors dampen our enthusiasm. First, consider Figure 3, which plots the residuals. The largest ones belong to the Warren Court: of the 16 terms included in that era, 7 produced residuals greater than 10. Estimates for the 1962 and 1963 terms were particularly poor (around 65% for both terms, with actual support nearing 85%). A proponent of the Segal/Cover approach might argue that a variable yielding a tight fit for 9 of 16 years is not troublesome. We do not take issue with this claim except to observe that it is hard to imagine achieving credible results in, say, a time-

Figure 4. Comparison of Estimated Values from Segal/Cover Scores and Lagged Votes Against Actual Votes, 1953–1991 Terms



Note: Table 4 provides the summary statistics. For the Segal/Cover scores, r^2 = .65 and SEE = 8.54; for the lagged values, the r^2 = .58 and SEE = 9.48.

series analysis of 16 data points, more than a third of which missed their mark by over 10 percentage points. On a more positive note, fits were satisfactory for the Burger and Rehnquist Court years, with only two terms producing large residuals (1970 and 1991).

Second, compare the Segal/Cover scores to the more direct (though non-independent) measures of court voting, the lagged values. For almost all subsets of civil liberties, the lagged values provide predictors of the vote that are nearly as good as the scores. This is hardly surprising given previous research (e.g., Epstein, Walker, and Dixon 1989) on the Court. It does seem that, for civil liberties generally, the lagged value furnishes a more sensitive indicator, as Figure 4 reveals. There, we display the actual support the Court gave to civil liberties cases decided between 1953 and 1991, the support predicted

by the Segal/Cover scores (i.e., those predictions generated from Equation 2), and the support predicted by Equation 3. Note that while both the lagged values and the Segal/Cover scores picked up the dramatic change ushered in by the Burger Court, the scores did not adequately capture the liberalism of the Warren Court or the status quo posture of the Rehnquist Court (but again judgments about the Rehnquist Court may be premature).

Even though the results are mixed, we can draw the following conclusion: for areas outside civil liberties, the lagged votes provide better predictors of the current vote (and, thus, better surrogates of ideological values—see Segal and Spaeth 1993) than do the Segal/Cover scores. Even for civil liberties cases, where there are negligible differences between the Segal/Cover scores and the lagged votes, the question becomes one of balancing different disadvantages (that is, weighing the effort of collecting the data needed to animate the Segal/Cover scores against the problem of using votes to predict votes). For those negatively disposed toward the lag, we reiterate our conclusion from the micro-analytic portion of the article: analysts should not use the Segal/Cover measures for areas outside the realm of civil liberties. We must, however, amend our prior recommendation and acknowledge that the Segal/Cover scores performed quite well for economic liberties cases at the macro level.

Decision Making and Temporal Issues

In addition to using the Segal/Cover measures to study individual- and Court-level decision making at discrete points in time, scholars have applied the method to track decision making longitudinally. In so doing, analysts assume that judicial preferences remain stable over time, and that macro-level decisions can change only as the result of membership alterations. For, once computed, the Segal/Cover scores cannot vary over the course of an individual's career; they can only change for the Court as a result of the departure of old members and the entry of new ones.

Because the Segal/Cover scores do not vary, we cannot directly determine whether they furnish reasonable proxies of judicial preferences over time. An alternative, albeit indirect, strategy is to regress (for each justice) the percentage of liberal civil liberties votes cast during each term of service on a counter representing each term. Put differently, we can estimate the following equation:

$$V_{i_{cl_t}} = a + bC_t + e \qquad [4]$$

where $V_{i_{cl_t}}$ is justice i's vote at term t in civil liberties cases (as represented by the percentage of liberal vote) and C_t is a counter that increases for

each term of service. If the justice evinces stable voting patterns, then the counter should produce an insignificant coefficient. This would provide some indication that those who have used the Segal/Cover scores to study decision making over time were operating under a plausible premise.

We conducted this analysis for the 15 justices serving more than ten terms between 1953 and 1991. Our results indicate that one-third of them (Black, Blackmun, Stevens, Warren, and White) evinced a significant linear change in voting in civil liberties cases over the course of their career.[17] It is worth noting, however, that the Segal/Cover scores do a satisfactory job of predicting justices' first-year voting records in civil liberties cases (r^2 = .68; SEE = 12.55).[18] This is an interesting finding and one that comports nicely with the method Segal and Cover used to establish their scores. After all, editorial writers base their evaluations on the candidate's ideologies *at the time of nomination;* these writers should not be faulted—nor for that matter, should Segal and Cover—for failing to anticipate changes in ideological predilection.

Still, we cannot ignore the central finding: what some scholars take as an underlying premise of the Segal/Cover scores—that justices evince stable voting behavior over time—proves troublesome for a good portion of the Court's members.[19] In contrast are the results of regressing votes in civil liberties cases on their lagged values. While the Segal/Cover scores cannot account for variation over time in individual decision making, the lagged values do a reasonably satisfactory job. We found that for seven of the 14 justices with more than ten data points (O'Connor drops out here because of only nine lagged values) knowledge of their past voting record provided a satisfactory predictor of their future behavior.

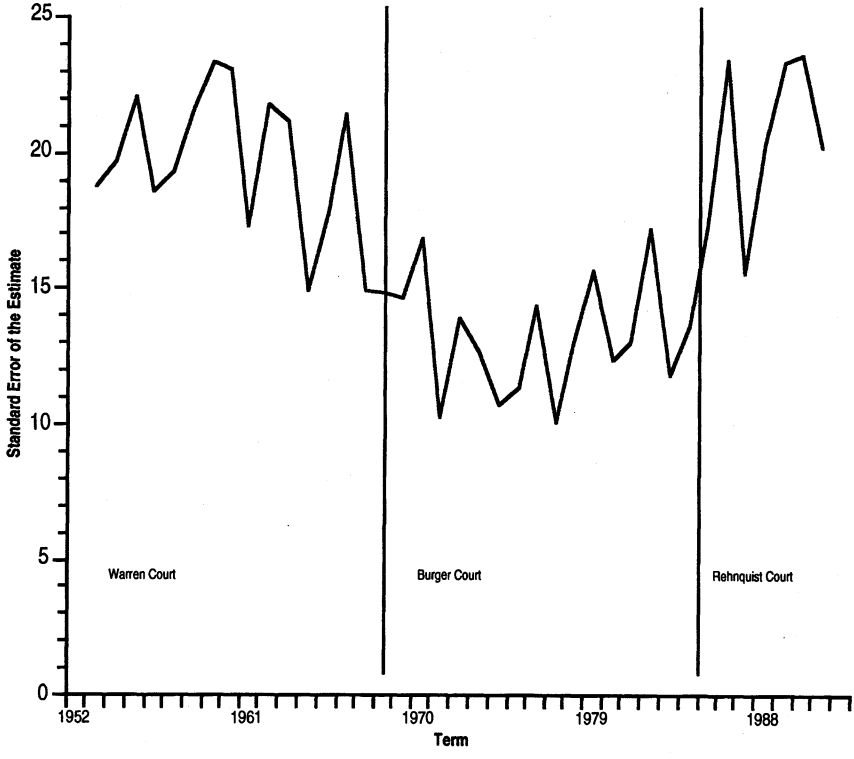
Because the membership of the Court changes periodically, we can

[17]The counter coefficient for Warren was significant at the .07 level; all others reached .01. The general finding, namely that the political preferences of some justices change over time, is explored in more detail in Epstein et al. 1995.

[18]In computing this, we used only the justices (included in our analysis) for whom the United States Supreme Court Judicial Database supplies data on their freshman year on the Court (i.e., Warren through Thomas). The data are available in Epstein et al. 1994, Table 6.2.

[19]Why the preferences of some justices change over time is an intriguing question for future work. In conducting such studies, scholars may find contextual theories of politics (see, generally, Huckfeldt and Sprague 1995) quite useful. They may also need to take into account changes in the issues coming before the Court (see Atkins and Sloope 1986; Baum 1988).

Figure 5. Standard Errors of Estimates from Term by Term Regressions



perform a second, more direct test of the Segal/Cover scores' ability to account for change over time. We already have indicated that the scores furnish a better predictor for some Court eras than for others. Another way to consider this is to estimate the following equation for *each term:*

$$V_{i_{cl}} = a + bS_i \qquad [5]$$

where $V_{i_{cl}}$ is vote of justice i (as measured by the percent liberal) in civil liberties cases and S_i is Segal/Cover score for justice i.

Rather than detailing all of the coefficients and summary statistics for each of the analyses,[20] consider Figure 5, which displays the standard

[20]These can easily be computed from the United States Supreme Court Judicial Database or from Epstein et al. 1994, Table 6.2.

errors of the estimates for each term. Note the estimation problems resulting for the Warren Court era (1953–1968 terms) and for the last two terms of the Rehnquist Court (1990 and 1991). Of the 16 terms contained in the Warren Court era only one (1956) produced an estimate statistically significant at the .05 level, whereas almost every term of the Burger Court era was predicted with a high degree of accuracy (the one exception was 1969).

Given that this general pattern has emerged throughout our study, we can now give it a name: time dependence. The Segal/Cover scores work quite well—at least for civil liberties cases—during one Court era (1969–1985), perform poorly for another (1953–1986), and produce mixed results for yet a third (1986–1991). Why this pattern emerges is and ought to be a matter of speculation. The larger point is that analysts should exercise caution when using the Segal/Cover scores to study micro- and macro-level decision making over time.

Discussion

Our results hold several lessons for social scientists seeking to measure the preferences of political actors, though, of course, they pertain most directly to scholars whose work requires them to characterize the attitudes of United States Supreme Court justices. First, such analysts should invoke the Segal/Cover scores in the set of circumstances indicated by their developers: aggregated individual-level decisions in civil liberties cases. With only the limited exceptions identified above, efforts to incorporate these measures into studies of other issue areas, of court-level decision making, or of longitudinal trends will find them less than fruitful. Second, simply because we used past voting as a baseline against which to compare the Segal/Cover scores does not mean that we advocate the use of lagged votes in other, more substantive contexts. At the start of this article, we discussed the problems with invoking such measures as surrogates for preferences; and nothing we have written since that point causes those problems to evaporate. To be sure, they provide excellent predictors of the vote. Yet they are not—unlike the Segal/Cover scores—drawn from sources independent of the things they predict (e.g., Baum 1994; but, again, see Spaeth 1995).

Taken together, these two lessons imply a third: students of the judicial process who seek to explore phenomena other than aggregated individual-level voting in civil liberties cases should give serious thought to devising new surrogates for judicial preferences. What alternatives might scholars consider? To begin to answer this question, we redirect readers' attention to the comparative study of political parties. As noted earlier, comparativ-

ists—much like current judicial scholars—long relied on the judgments of experts (e.g., De Swaan 1973; Dodd 1974, 1976; Taylor and Laver 1973) to tap the preferences of the actors they examined. A novel measurement strategy emerged in the 1980s, however: a research team headed by Budge, Robertson, and Hearl (1987; see also note 2) content-analyzed party documents—election manifestos—from nineteen democracies over the postwar period. The manifesto data have been widely used and a consensus has developed that these data offer distinct advantages over measurements based on expert judgments (for a review, see Laver and Schofield 1990).

Indeed, in each of the four areas addressed in our battery of tests, the strengths of the manifesto data are clear. With regard to issues, for example, Budge and his colleagues developed a coding frame to classify manifestos that was standard across nations and yet also sensitive to the particular issues under political debate in each country. They subjected the data generated by their coding scheme to factor analysis, which revealed a number of dimensions (that is, clusters of related issue areas) in party competition that varied across countries. These procedures allowed the researchers to isolate a distinct position on each separate policy dimension for each major party in each country studied. This mapping of actors' locations on separate issue dimensions is just what is lacking in the judicial research we have reviewed. The data set also deals readily with temporal concerns, for it features repeated measurements of party positions with each national electoral campaign. This permits such comparisons as a tracking of party movements from the late 1940s to the early 1980s in four countries (Robertson 1987). In contrast, expert judgments are typically anchored in a single time frame.

It is thus not too difficult to see why many comparativists now rely on the party manifesto data instead of measures derived from expert judgments. Equally evident are the parallels between these data and Danelski's (1966) work on judicial preferences. Although Danelski's project was less systematic and far narrower in scope than the research by Budge, Robertson, and Hearl, both show the importance of distinguishing among issue dimensions and of confronting temporal concerns.

These observations, coupled with the results of our analyses, lead us to offer the following recommendation to judicial specialists: reconsider Danelski's basic approach and investigate the party manifesto project.[21]

[21] We should note that reliance on the party manifesto data is not completely unproblematic. Scholars who use these data accept parties' statements of intention at election time as

To be sure, it will be a painstaking task to analyze the justices' writings, papers, speeches, lower court decisions, and so on. Effort will be required, moreover, to establish whether pre-nomination speeches (Danelski's focus) are or are not the single most useful subset of documents (analogous to party manifestos) that can be isolated for coding. But, in the end, the coding of justices' documents may prove the most satisfactory way for judicial scholars to come to grips with the measurement problems that have plagued their work. Of course, we urge analysts who undertake this or any other strategy to run their measures through a battery of tests of the sort presented here or through a methodological audit of their own invention.

Certainly these recommendations pertain most directly to scholars of the Court. Yet perhaps our examination will also invite reflection on the part of scholars outside the judicial field. Those analysts, too, would be well advised to weigh carefully whether adequate tests have been performed to assess the degree to which their measures of actors' preferences may be extended to cover many issue areas or are best confined to a few; the degree to which their measures yield apt ordinal rankings of the actors' preferences along one or more policy spectra; the degree to which individual-level measures may be usefully aggregated into composite, collective measures; and the degree to which measures are time-bound or instead facilitate intertemporal comparisons.

Manuscript submitted 15 August 1994.
Final manuscript received 31 March 1995.

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true preferences. In other words, they assume that political parties do not act strategically in presenting policy packages to the electorate; that parties do not modulate, moderate, or misrepresent their statements of policy intention in order to win votes or enter government. We cannot fail to note that distinguished work on political parties (Downs 1957) contests this assumption.

Even though the same concerns would apply to measures derived from the speeches, writings, and so forth of the justices (for example, especially since Robert Bork's failed nomination, one could imagine wanna-be justices strategically misrepresenting preferences in speeches and writings), they do not discredit their use entirely. To alleviate such concerns, scholars of the judicial process might consider content-analyzing concurring and dissenting opinions. Although these are not independent sources, justices *may be* less inclined to misrepresent their preferences in these opinions. Hence, they could provide useful vehicles for preference measurement, especially since they would allow for consideration of policy issues unaddressed in other sources.

APPENDIX A

Aggregated Liberal Voting of the Justices, 1953 to 1991 Terms

	Issue Areas										
		(Subsets of Civil Liberties)									
Justice	Civil Liberties	Criminal Procedure	Civil Rights	First Amendment	Due Process	Privacy	Unions	Economics	Taxation	Federalism	Judicial Power
Black	74.2	68.9	72.2	87.0	81.0	00.0	76.3	84.2	84.9	64.9	53.0
	925	399	277	192	42	3	135	467	126	111	362
Reed	33.3	28.3	35.5	20.0	75.0	—	81.8	52.5	75.0	52.6	55.6
	108	53	31	15	8	0	22	99	24	19	45
Frankfurter	50.4	46.0	55.7	49.4	63.6	00.0	53.1	38.6	75.7	60.4	30.9
	369	174	88	79	22	1	64	241	70	53	152
Douglas	91.5	89.3	92.7	96.8	85.9	81.8	64.9	86.3	38.6	69.0	68.7
	1,265	522	397	249	71	11	168	568	140	129	451
Jackson	48.1	35.7	62.5	50.0	100.0	—	66.7	42.9	33.3	75.0	12.5
	27	14	8	4	1	0	6	28	3	4	8
Burton	39.2	33.3	42.1	32.4	68.8	00.0	65.7	50.7	76.3	45.2	42.0
	212	96	57	37	16	1	35	146	38	31	88
Clark	44.4	35.9	57.3	34.6	78.8	100.0	69.3	74.6	78.0	54.5	47.1
	649	287	185	136	33	1	114	389	109	88	274
Minton	36.8	35.6	37.9	14.3	83.8	—	60.0	66.3	85.7	58.8	45.9
	95	45	29	14	6	0	20	86	21	17	37
Warren	78.5	75.1	82.8	79.9	78.8	50.0	72.0	81.9	80.0	69.1	54.8
	771	346	221	159	33	2	125	443	115	94	323
Harlan	43.7	39.4	44.8	44.1	69.2	33.3	55.4	37.7	72.1	52.9	37.3
	870	371	259	186	39	3	130	403	111	102	343
Whittaker	43.3	43.5	46.3	37.0	60.0	00.0	142.5	32.5	57.1	66.7	43.2
	240	115	54	54	15	1	40	151	42	33	95
Brennan	79.5	76.2	83.6	83.3	74.5	59.2	66.2	71.6	70.6	64.7	50.1
	2,417	944	769	408	192	49	234	867	180	238	733

APPENDIX A (*Continued*)

	Issue Areas										
		(Subsets of Civil Liberties)									
Justice	Civil Liberties	Criminal Procedure	Civil Rights	First Amendment	Due Process	Privacy	Unions	Economics	Taxation	Federalism	Judicial Power
Stewart	51.3	46.1	49.9	63.9	54.8	41.4	56.9	45.0	65.9	57.8	38.8
	1,551	607	519	274	104	29	167	564	123	128	487
White	42.4	33.2	56.1	38.3	48.3	13.2	62.7	58.3	84.8	65.6	40.9
	2,253	865	743	355	180	53	193	732	132	212	650
Goldberg	88.9	80.0	98.3	89.3	80.0	100.0	65.0	65.5	78.3	55.0	55.1
	153	60	58	28	5	1	20	84	23	20	69
Fortas	81.0	80.2	83.6	77.3	100.0	—	60.0	69.3	50.0	64.7	50.6
	205	91	61	44	5	0	20	75	16	17	79
Marshall	81.4	80.2	85.2	82.9	75.8	60.4	68.1	65.3	74.2	66.7	49.7
	1,889	722	620	287	165	48	138	556	93	156	487
Burger	29.6	19.8	37.2	31.9	38.0	12.5	43.9	42.6	72.1	63.1	26.3
	1,430	515	500	213	129	40	107	423	68	111	354
Blackmun	51.5	40.2	61.3	55.8	51.2	47.1	61.8	53.5	73.5	64.8	39.1
	1,791	672	582	269	168	51	131	546	83	159	453
Powell	37.4	28.8	41.0	47.8	44.3	32.4	51.1	44.7	56.1	61.0	32.0
	1,285	482	432	182	122	37	94	356	57	105	309
Rehnquist	20.4	15.3	25.6	18.7	28.0	8.2	43.7	42.2	66.3	38.7	28.5
	1,665	633	536	241	157	49	95	413	63	155	428
Stevens	61.4	61.9	61.1	64.6	57.8	45.2	61.1	56.7	55.6	53.0	47.1
	1,319	507	416	181	128	42	95	413	63	134	323
O'Connor	33.4	22.2	45.3	40.3	38.4	25.0	43.3	43.2	55.8	46.5	43.9
	886	352	258	129	86	24	60	266	43	99	228
Scalia	29.7	25.1	36.0	32.8	29.3	30.0	36.7	45.7	70.8	58.2	32.2
	438	187	114	67	41	10	30	140	24	55	118

Kennedy	34.7	24.1	45.3	45.8	34.8	20.0	25.0	49.1	75.0	54.3	45.9
	303	133	75	48	23	10	20	106	16	35	85
Souter	41.7	27.1	60.6	71.4	16.7	33.3	33.3	50.0	50.0	57.1	55.9
	108	48	33	14	6	3	9	48	6	7	34
Thomas	25.6	16.7	42.9	16.7	33.3	00.0	00.0	41.7	50.0	80.0	50.0
	43	18	14	6	3	1	1	24	2	5	16

Note: The numbers listed first are the percentage of cases in which the justice took the liberal position. The numbers below are the total number of cases in that issue area in which the justice participated since 1953.

Note on Issues: The issue areas are defined as follows. **Civil Liberties:** combines Criminal Procedure, Civil Rights, First Amendment, Due Process, Privacy, as well as cases involving attorney issues (attorneys' fees, commercial speech, admission to and removal from the bar, and disciplinary matters). **Criminal Procedure:** the rights of persons accused of crime except for the due process rights of prisoners. **Civil Rights:** non-First Amendment freedom cases that pertain to classifications based on race, Native Americans, age, indigence, voting, residence, military or handicapped status, sex, or alienage. **First Amendment:** guarantees contained in this constitutional provision. **Due Process:** non-criminal procedural guarantees, plus court jurisdiction over non-resident litigants and the takings clause of the Fifth Amendment. **Privacy:** abortion, contraception, the Freedom of Information Act and related federal statutes. **Unions:** labor union activity. **Economics:** commercial business activity, litigation involving injured persons or things, employee actions vis-à-vis employers, zoning regulations, and governmental regulation of corruption other than that involving campaign spending. **(Federal) Taxation:** the Internal Revenue Code and related statutes. **Federalism:** conflicts between the federal and state governments, excluding those between state and federal courts, and those involving the priority of federal fiscal claims. **Judicial Power:** the exercise of the judiciary's own power and authority.

We count the primary issue in each case only, except where cases contain both substantive and procedural issues, i.e., those involving federalism or judicial power, where both are counted.

Note on Ideology: We use the term "liberal" to represent the voting direction of the justices across the various issue areas. It is most appropriate in the areas of Civil Liberties (Criminal Procedure, Civil Rights, First Amendment, Due Process, and Privacy), where it signifies pro-defendant votes in criminal procedure cases, pro-women or -minorities in civil rights cases, pro-individual against the government in First Amendment, due process, and privacy cases and pro-attorney in attorneys' fees and bar membership cases. In takings clause cases, however, a pro-government/anti-owner vote is considered liberal. The use of the term is perhaps less appropriate in union cases, where it represents pro-union votes against both individuals and the government, and in economic cases, where it represents pro-government votes against challenges to its regulatory authority and pro-competition, anti-business, pro-liability, pro-injured person, and pro-bankruptcy votes. In federalism and federal taxation, we use liberal to indicate pro-national government positions; in judicial power cases, we use the term to represent pro-judiciary positions.

Sources: United States Supreme Court Judicial Database, with orally argued citation plus split votes as the unit of analysis. The notes on issues and ideology are adopted from Epstein et al. 1994, 429–30.

APPENDIX B

Aggregated Liberal Voting of the Court, 1953 to 1991 Terms

	Issue Areas								
		(Subsets of Civil Liberties)							
Term	Civil Liberties	Criminal Procedure	Civil Rights	First Amendment	Unions	Economics	Taxation	Federalism	Judicial Power
1953	50.0	40.0	62.5	50.0	66.7	48.5	100.0	66.7	44.4
	28	15	8	4	6	33	3	6	9
1954	64.9	50.0	70.0	80.0	100.0	78.3	90.9	99.7	30.8
	37	18	10	5	4	23	11	6	13
1955	56.7	41.7	63.6	80.0	80.0	82.8	71.4	90.0	53.3
	30	12	11	5	10	29	7	5	15
1956	60.4	65.0	33.3	55.6	72.7	87.5	75.0	80.0	47.4
	48	20	12	9	11	32	8	5	19
1957	60.3	43.8	71.4	77.8	50.0	73.3	55.6	40.0	56.3
	63	32	14	9	2	30	9	10	32
1958	58.1	46.2	66.7	81.8	80.0	63.6	100.0	100.0	40.0
	43	26	6	11	5	44	6	5	15
1959	50.0	43.8	70.0	42.9	64.3	78.6	57.1	50.0	50.0
	36	16	10	7	14	28	14	8	22
1960	55.2	66.7	57.1	36.4	90.9	51.9	90.0	50.0	28.6
	67	30	14	22	11	27	10	6	14
1961	76.9	70.6	88.9	87.5	88.9	65.5	85.7	80.0	54.5
	39	17	9	8	9	35	8	12	22
1962	84.0	71.4	94.1	100.0	70.0	80.0	100.0	66.7	69.6
	50	21	17	8	10	35	8	12	23
1963	84.5	72.7	92.6	88.9	66.7	80.0	50.0	77.8	43.5
	59	22	27	9	9	30	6	9	23
1964	72.7	56.3	85.7	72.7	54.5	72.2	77.8	100.0	46.2
	44	16	14	11	11	18	9	3	26

1965	71.7	56.3	85.7	69.2	33.3	80.0	62.5	0.0	47.8
	46	16	14	13	3	25	8	6	23
1966	62.7	64.3	68.4	54.5	71.4	78.3	50.0	80.0	53.6
	59	28	19	11	7	23	2	5	28
1967	76.8	75.0	81.3	69.2	44.4	72.0	100.0	100.0	50.0
	69	36	16	13	9	25	3	6	20
1968	79.4	63.0	87.0	100.0	75.0	76.5	100.0	50.0	51.6
	63	27	23	10	4	17	4	6	31
1969	56.9	44.4	69.2	60.0	33.3	70.0	85.7	50.0	34.8
	65	27	26	10	6	10	7	4	23
1970	47.5	29.2	50.0	54.5	66.7	56.3	75.0	40.0	15.8
	81	24	28	22	9	16	4	5	19
1971	52.2	41.2	64.3	43.8	28.6	53.1	50.0	50.0	28.0
	90	34	28	16	7	32	4	2	25
1972	40.0	31.4	54.5	22.2	50.0	69.0	66.7	75.0	15.0
	95	35	33	18	6	29	3	4	20
1973	43.8	33.3	47.2	64.3	37.5	52.4	83.3	50.0	48.1
	89	30	36	14	8	21	6	6	27
1974	55.6	54.2	58.6	62.5	66.7	50.0	100.0	87.5	22.6
	72	24	29	8	6	28	4	8	31
1975	37.1	18.2	53.6	53.8	40.0	42.9	100.0	80.0	29.2
	89	33	28	13	5	28	1	5	24
1976	37.8	36.7	42.9	33.3	71.4	36.8	75.0	57.1	16.7
	90	30	35	12	7	19	4	7	18
1977	50.7	58.6	38.5	62.5	60.0	66.7	57.1	50.0	26.7
	73	29	26	8	5	30	7	6	15
1978	41.0	37.9	44.1	25.0	60.0	44.4	50.0	50.0	33.3
	83	29	34	8	5	27	4	4	15
1979	49.4	37.5	59.1	70.0	33.3	54.8	—	100.0	41.2
	85	32	22	10	6	31	0	2	17
1980	35.3	40.0	33.3	44.4	75.0	56.0	25.0	66.7	25.0
	68	25	27	9	8	25	4	6	20
1981	46.9	26.3	55.9	75.0	88.9	56.0	66.7	85.7	30.8
	81	19	34	12	9	25	3	7	26

APPENDIX B (*Continued*)

	Issue Areas								
		(Subsets of Civil Liberties)							
Term	Civil Liberties	Criminal Procedure	Civil Rights	First Amendment	Unions	Economics	Taxation	Federalism	Judicial Power
1982	39.5	26.7	55.6	45.5	57.1	54.5	75.0	57.1	47.1
	81	30	27	11	7	33	4	14	17
1983	35.8	20.0	58.6	33.3	66.7	37.0	75.0	60.0	36.8
	95	40	29	12	6	27	4	10	19
1984	41.4	30.3	60.0	45.5	60.0	43.3	100.0	55.6	53.8
	87	33	25	11	5	30	4	9	13
1985	37.8	27.9	41.4	42.9	66.7	37.5	80.0	54.5	45.2
	98	43	29	14	3	16	5	11	31
1986	44.1	34.1	58.3	50.0	80.0	39.1	75.0	62.5	45.8
	93	41	24	12	5	23	4	16	24
1987	49.4	45.2	47.6	50.0	14.3	61.5	66.7	63.6	41.7
	77	31	21	12	7	26	3	11	24
1988	38.8	23.3	50.0	46.7	00.0	52.9	75.0	50.0	47.6
	85	30	26	15	5	17	4	10	21
1989	35.7	25.7	55.6	35.7	75.0	50.0	85.7	55.6	52.4
	70	35	9	14	4	22	7	9	21
1990	41.1	31.0	68.8	40.0	50.0	45.8	00.0	75.0	62.5
	56	29	16	5	8	24	2	4	16
1991	48.2	42.9	55.6	75.0	00.0	48.1	75.0	66.7	57.9
	56	21	18	8	1	27	4	6	19

Note: The numbers listed first are the percentage of cases in which the Court took the liberal position. The numbers below are the total number of cases in that issue area decided during the term. Civil Liberties includes cases involving: criminal procedure, civil rights, First Amendment, due process, privacy, and attorneys. For more information on issue areas and ideology, see notes to Appendix A.

Source: United States Supreme Court Judicial Database, with orally argued citation as the unit of analysis.

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