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ABSTRACT
Empirical studies have examined the effects of law and politics on judicial decision making, but many legal scholars are dissatisfied with how these studies account for law. This paper provides a novel survey technique for measuring law. I demonstrate this technique by examining judicial decision making in cases involving the single-subject rule. The rule limits ballot propositions to one “subject,” a standard that vests judges with some discretion. Measures of law developed with the surveys strongly predict judges’ votes in single-subject cases. Moving from the proposition in the sample with the lowest subject count to the one with the highest is associated with a 78-percentage-point increase in the likelihood of a judge finding a violation of the rule. Measures of ideology also predict judges’ votes, especially when propositions are politically salient and when the law is indeterminate.

1. INTRODUCTION
What motivates judicial decision making? The legal model posits that law determines case outcomes, while the political model posits that judges’ ideologies drive their decisions. Empirically testing these models presents a challenge, as law is difficult to measure. This hampers efforts...
to test the legal model. It can also raise doubts about tests of the political model, as law, which is excluded from such tests, may correlate with ideology variables, confounding the relationship between ideology and judges’ votes. For these reasons, we do not know the extent to which law and politics influence judges’ decisions.

Judicial review of direct democracy epitomizes the stakes of this debate. Americans have voted on hundreds of ballot propositions in recent decades, including measures on affirmative action, eminent domain, and same-sex marriage. Most citizens support the use of propositions (see Public Policy Institute of California 2008), and special interests spend millions of dollars promoting and opposing them (Bauer 2010; Garrett 2010). If judges objectively interpret the law, they can check direct democracy, which operates without much oversight from other branches of government. If judges vote ideologically, they can undermine citizens’ will.

The single-subject rule presents judges with a thorny responsibility in their review of propositions. Widespread in state constitutions, the rule limits propositions to one “subject.” It aims to prevent logrolling and riding, techniques for enacting policies that lack majority support on their own merits (Gilbert 2006; Cooter and Gilbert 2010). “Almost any two . . . measures may be considered part of the same subject if that subject is defined with sufficient abstraction” (Manduley v. Superior Court, 41 P.3d 3, 37 [Cal. 2002]), which makes the rule difficult to apply. To illustrate, a proposition banning same-sex marriage and same-sex civil unions may have two subjects (marriage and civil unions) or just one (same-sex relationships) depending on how abstractly one frames it.

Critics claim that judges exploit the single-subject rule’s indeterminacy and make ideological decisions in these cases (for example, Hasen 2006; Lamm 2006). Some have called for lenient enforcement (Lowenstein 1983, 2002) or elimination (Hasen 2006) of the rule. Meanwhile, defenders have called for stricter enforcement (Kastorf 2005; Figinski 1998) and for extension of the rule to Congress (Denning and Smith 1999). This debate has unfolded without systematic study of judges’ decisions in these cases.¹

This paper examines the determinants of judicial decision making in

¹. For a valuable paper on judicial decision making in single-subject disputes, see Matsusaka and Hasen (2010), whose results of a test of the political model are consistent with mine. They do not test the legal model. I discuss their work below.
single-subject disputes. Unlike prior work, I develop a technique for measuring the “correct” legal outcome for the cases in the study, by which I mean the outcome generated by sophisticated observers who objectively apply the rule to facts. This permits a direct test of the legal model and, because I can control for law, a more convincing test of the political model.

I develop two formulations of the legal model in this context: the categorization theory and the democratic process theory. The categorization theory represents the plain-language approach to the single-subject rule. It posits that judges examine provisions of a proposition and use semantics, analogies, and common sense to determine if they relate closely enough to be categorized under one subject. The democratic process theory posits that judges examine provisions of a proposition and determine if voters can make independent judgments about them. If they can, then the proposition violates the rule. As Cooter and Gilbert (2010) show, applying the democratic process theory would achieve the single-subject rule’s purposes, making it a purposive method of interpretation.

I conducted surveys to quantify these conceptions of law. For the categorization theory I surveyed law students. The survey instructed how to count subjects using the tools that judges profess to use and provided sample precedents. The survey then asked the students to apply the rule to propositions in the sample. Critically, the students did not know that finding multiple subjects implied that a proposition was invalid. Obfuscating the relationship between adjudication and outcomes should have disengaged the students’ political preferences and yielded relatively objective determinations.

For the democratic process theory I surveyed undergraduates. The survey provided the propositions and asked students how much their support for each provision in a proposition depended on whether the other provisions became law. The students were not given any information on the single-subject rule.

Regression analysis shows statistically significant associations between both measures of law and judges’ votes. A move from the proposition with the smallest number of subjects under the categorization approach to the proposition with the largest number is associated with a 78-percentage-point increase in the likelihood of a judge finding a violation of the rule. This provides strong support for the legal model. I also find a statistically significant association between ideology and judges’ votes. This relationship is strongest when propositions are po-
litical salient and when the rule as applied to a particular proposition is indeterminate.

The paper proceeds as follows. Section 2 reviews the judicial politics literature. Section 3 discusses the single-subject rule and the categorization and democratic process theories. Section 4 describes the research design and empirical results. Section 5 concludes.

2. LAW AND POLITICS IN JUDICIAL DECISION MAKING

Observers have long debated the determinants of judicial decision making. Most debates involve the legal and political models. Under the legal model, judges apply rules, precedents, and logical operations to facts. This generates determinate, legally correct decisions. Under the political model, judges decide cases on the basis of their ideological preferences.2

Social scientists have spent decades testing the political model (see Maveety 2003). They use a variety of techniques to measure judges' ideology (see, for example, Nagel 1961; Tate 1981; Martin and Quinn 2002; Segal and Cover 1989; Giles, Hettinger, and Peppers 2001; Brace, Langer, and Hall 2000). They then compare these measures to judges' votes. When, for example, conservative judges vote to weaken civil liberties and liberal judges vote to strengthen them, scholars conclude that ideology drives judges' decisions (Segal and Cover 1989). Studies of the U.S. Supreme Court (for example, Segal and Spaeth 2002), federal courts of appeals (for example, Sunstein et al. 2006; George 1998; Revesz 1997), district courts (for example, Carp and Rowland 1983), and state courts (for example, Langer 2003) support the political model.3

Matsusaka and Hasen's (2010) study of the single-subject rule falls in this tradition. They find correlations between judges' ideological support for propositions and their votes to uphold them. This is consistent with my results. They also identify differences in ideological voting in

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2. The political model comes in at least two varieties, attitudinal (see, for example, Segal and Spaeth 2002) and strategic (see, for example, Gely and Spiller 1990; Eskridge 1991; Epstein and Knight 1998). When override is difficult, as in U.S. constitutional interpretation, and plausibly in single-subject adjudication, the models collapse.

3. Ashenfelter, Eisenberg, and Schwab (1995) provide an important exception, finding no relationship between ideology and votes among district court judges. For fuller reviews of the literature, see, for example, Fischman and Law (2009), Friedman (2005), Gillman (2001), and Cross (1997).
states with “aggressive” as opposed to “restrained” enforcement of the rule. However, they do not account for law.\textsuperscript{4}

Many institutions, both public and private, presuppose that the legal model has merit, and scholars have an interest in testing it. Moreover, law and politics may correlate, and this may cast doubt on some studies of the political model. What appears to be an improper association between ideology and judges’ votes may reflect a proper association between law and votes (see Fischman and Law 2009, pp. 145–50).

To make progress on these issues, scholars have attempted to develop measures of law. Many studies examine whether presence of certain facts correlates with case outcomes in the expected manner (for example, Songer, Segal, and Cameron 1994; George and Epstein 1992; Songer and Haire 1992; Segal 1984).\textsuperscript{5} In a similar vein, scholars examine whether changes in “jurisprudential regimes” affect judges’ decisions (Richards and Kritzer 2002; Kritzer and Richards 2003, 2005; Collins 2007). Several studies examine the relationship between precedent and judges’ decisions (for example, Hansford and Spriggs 2006; Brenner and Spaeth 1995; Spaeth and Segal 1999; Segal and Spaeth 1996; Brenner and Stier 1996; Songer and Lindquist 1996; Cross 2005; Songer, Segal, and Cameron 1994; Sisk, Heise, and Morriss 1998). Still other studies take additional paths to separate law from politics (for example, Fischman and Schanzenbach 2011; Cross 2007; Klein 2002; Bartels 2009).

These studies and others have made important contributions to the judicial politics literature. They also have a commonality: none of them, nor any study I am aware of, purports to identify correct legal outcomes for a sample of cases.

The reasons for this may be obvious. Legal texts and legislative intent can be ambiguous, and precedent may cut different ways. For some cases there may be no correct legal outcome. Even when there is, anyone who attempts to identify it will be subject to biases, making it difficult to develop an objective legal baseline.\textsuperscript{6}

This presents a dilemma. The clearest test of the legal model may

\textsuperscript{4} They control for the word count of propositions, but this is only a rough proxy for subject count, as long propositions can embrace one subject and short propositions can embrace more than one.

\textsuperscript{5} For criticism of this approach, see Friedman (2006, pp. 268–70).

\textsuperscript{6} A valuable study, Ruger et al. (2004), compares Supreme Court decisions during the 2002 term to ex ante predictions by legal experts. The experts’ ideologies may have influenced their predictions, making the baseline unreliable. Johnson (1987) faces a similar difficulty.
require a measure of correct legal outcomes. A convincing test of the political model may require a control for law, and the best control for law may be a measure of correct legal outcomes. But determining such outcomes is difficult.

3. THE SINGLE-SUBJECT RULE

In 1695, the Committee of the Privy Council complained that diverse acts in Massachusetts were “joined together under ye same title,” making it impossible to vacate unpopular provisions without also invalidating favorable ones (Luce 1922, p. 548). In 1702, Queen Anne responded, instructing Lord Cornbury of New Jersey to avoid “intermixing in one and the same [Act] such things as have no proper relation to one another” (Williams 1990, p. 75). New Jersey codified this language in its 1844 single-subject rule, which limited ordinary legislation to one subject (N.J. Const., art. 4, sec. 7, p. 4). Forty-two states followed suit (Gilbert 2006).

Throughout the twentieth century, 18 states extended their legislative single-subject rules to ballot propositions (Downey, Hargrove, and Locklin 2004). All such rules resemble the one in California’s constitution: “An initiative measure embracing more than one subject may not be submitted to the electors or have any effect” (Cal. Const., art. 2, sec. 8[d]).

The rule aims to prevent logrolling and riding. Logrolling occurs when two separable proposals, neither of which would pass on its own, are combined into one measure that commands majority support. Riding occurs when two separable proposals, one of which would pass on its own and the other of which would not, are combined into one measure that commands majority support. Both practices allow a provision to become law with only minority support. These practices have been described as “perversion[s] of majority rule” (Ruud 1958, p. 399) akin to “stealth and fraud” in lawmaking (State ex rel. Dix v. Celeste, 464 N.E.2d 153, 157 [Ohio 1984]).

In recent years, courts have used the single-subject rule to strike down anti-immigration measures, reapportionment schemes, and a tax on oil companies’ profits. They have upheld against single-subject challenges propositions that would restrict abortion, permit stem cell research, and stiffen criminal penalties.

7. Cooter and Gilbert (2010) discuss these purposes and rigorously define logrolling and riding. This paper does not address a third purpose of the rule, preventing voter confusion.
How do judges decide if a proposition violates the rule? They profess to take a plain-language approach called the categorization theory of interpretation. They examine the provisions of a proposition and ask if they are “reasonably germane” to one another (California Assoc. of Retail Tobacconists v. State, 109 Cal. App. 4th 792, 809 [2003]) or if they embrace “disconnected and incongruous measures . . . that have no necessary or proper connection” (Jones v. Polhill, 46 P.3d 438, 440 [Colo. 2002]). To make this determination judges parse text, draw analogies, and use common sense. Precedent rarely matters for categorization.

When challenged provisions are incongruous, categorization yields determinate outcomes. A proposition on spotted owls and the death penalty would clearly violate the rule. But when challenged provisions are similar, categorization seems to be indeterminate. To illustrate, judges disagree on whether a proposition banning same-sex marriage and same-sex civil unions has two subjects (marriage and civil unions) or just one (same-sex relationships) (Forum for Equality PAC v. McKeithen, 893 So. 2d 715 [La. 2005]). Likewise, they split on whether a measure addressing gang-related crimes, California’s three-strikes law, and the juvenile justice system embraces three subjects (gangs, repeat offenders, and juvenile justice) or just one (“a safer California”) (Manduley, 41 P.3d 8, 27–29).

Judges understand this dilemma. Justice Gerald Kogan of Florida’s Supreme Court stated, “What may be ‘oneness’ [of subject matter] to one person might seem a crazy quilt of disparate topics to another. ‘Oneness,’ like beauty, is in the eye of the beholder; and our conception of [it] thus has changed every time new members have come onto this Court” (In re Ltd. Political Terms in Certain Elective Offices, 592 So. 2d 225, 231 [Fla. 1991]). Colorado Supreme Court Justice Nathan Coats wrote that his colleagues “[understand] the term ‘subject’ to be so elastic as to give this court unfettered discretion to . . . approve or disapprove . . . any . . . ballot measure at will” (In re Title and Ballot Title and Submission Clause for 2005–2006 #55, 138 P.3d 273, 283 [Colo. 2006]).

Cooter and Gilbert (2010) develop an alternative to categorization called the democratic process theory. Rather than focus on logical conceptions of subject matter, this theory focuses on the ability of voters to make independent judgments about the elements of a proposition. If most voters can decide whether to support element A without knowing whether element B will become law and vice versa, then most voters can make independent judgments about A and B. Under the democratic pro-
cess theory, combining A and B in one proposition would violate the single-subject rule. Conversely, if most voters cannot decide whether to support A without knowing whether B will become law, then most voters cannot make independent judgments, and combining A and B would not violate the rule.

Cooter and Gilbert (2010) prove that application of the democratic process theory would prevent logrolling and riding, making it a purposive theory of interpretation. They also show that the theory is consistent with the language that courts use to describe their single-subject tests. This paper treats the democratic process theory as a positive account, supplemental to categorization, of how judges apply the single-subject rule.

4. LAW OR POLITICS? JUDICIAL DECISION MAKING IN SINGLE-SUBJECT DISPUTES

This section tests whether the legal model—as conceptualized under the categorization and democratic process theories—and the political model explain judges' votes in single-subject cases.

4.1. Data Overview

Cases included in this study were litigated in state appellate and supreme courts in California, Colorado, Florida, and Oklahoma between January 1980 and June 2007. Collectively, these states account for about 80 percent of the single-subject challenges to propositions nationwide during this period (Gilbert 2008). All relevant judges in these states were appointed by governors and subject to retention elections. The single-subject rules in each state remained essentially unchanged during the period of the study.

Table 1 indicates the number of propositions challenged on single-subject grounds in each state. Some propositions were challenged multiple times, and some cases reviewed multiple propositions. To simplify, the table reports the total number of judgments issued with respect to the challenged propositions. In Colorado, Florida, and Oklahoma, the state supreme courts had near-exclusive jurisdiction over single-subject

8. Appendix A explains in detail the criteria for including cases.

9. Colorado adopted the single-subject rule for initiatives in 1994, so all Colorado cases were litigated thereafter. In 1994, Florida exempted from the single-subject rule initiatives addressing “the power of government to raise revenues” (Fla. Const., art. 11, sec. 3). Courts in these states used the same tests throughout the sample period for identifying single-subject violations.
Table 1. Single-Subject Challenges to Propositions

<table>
<thead>
<tr>
<th>State</th>
<th>Propositions</th>
<th>Judgments</th>
<th>Violations</th>
<th>Success Rate (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Appellate Court</td>
<td>Supreme Court</td>
<td>Appellate Court</td>
</tr>
<tr>
<td>California</td>
<td>18</td>
<td>24</td>
<td>7</td>
<td>2</td>
</tr>
<tr>
<td>Colorado</td>
<td>72</td>
<td>0</td>
<td>72</td>
<td>N.A.</td>
</tr>
<tr>
<td>Florida</td>
<td>55</td>
<td>1</td>
<td>55</td>
<td>0</td>
</tr>
<tr>
<td>Oklahoma</td>
<td>10</td>
<td>0</td>
<td>10</td>
<td>N.A.</td>
</tr>
<tr>
<td>Total</td>
<td>155</td>
<td>25</td>
<td>144</td>
<td>2</td>
</tr>
</tbody>
</table>

Note. N.A. = not applicable.

challenges during the period of the study. Consequently, most litigation took place in state supreme courts. Success rates are defined as the percentage of judgments in which a majority of judges found a violation of the rule.

Between two and nine judges voted on each judgment. These votes are the dependent variable in this study. Each vote is coded one if the judge found a single-subject violation and zero otherwise. The data set includes 1,067 votes, 411 of which implied a single-subject violation. One hundred twenty-three judges cast those votes.

4.2. Measuring Ideology

Testing the political model requires a measure of those 123 judges’ ideologies. I use party-adjusted judge ideology (PAJID) scores developed by Brace, Langer, and Hall (2000), which assume that the ideology of the governor or legislators who appointed a judge, or alternatively the ideology of the citizens who elected a judge, provides information on the judge’s ideology. Measures of elite (governor or legislator) and citizen ideology come from Berry et al. (1998). The PAJID scores combine information on elite and citizen ideology with judges’ partisan affiliations.

Testing the political model also requires a measure of the political orientation of the propositions. I am not aware of any systematic data


11. Party-adjusted judge ideology scores were available for all supreme court justices in the study but not for the appellate court judges, so I calculated those scores.
on this. To fill this gap, five public policy graduate students from the
University of California, Berkeley, independently assigned each proposi-
tion in the study an ideology score on a scale in which high scores
indicate liberal propositions and low scores indicate conservative prop-
ositions. The students were unfamiliar with the details of this research.
The average of their scores serves as a proxy for the political orientation
of the propositions.\textsuperscript{12} I refer to these scores as proposition politics scores.

Scaling the PAJID and proposition politics scores from 0 to 1 and
calculating the absolute value of the difference yields a variable labeled
Ideology. Ideology is a proxy for the extent to which a judge is ideo-
logically aligned with the proposition he or she is reviewing. Low values
imply that a judge favors a proposition, while high values imply that a
judge opposes it. Including Ideology in the regressions tests the political
model.

4.3. Measuring Law

Testing the legal model requires an assessment of how many subjects an
objective judge using the categorization and democratic process theories
would find in the propositions in the sample. Surveys were used to gather
this information.

For the categorization theory, 255 students at the University of Cal-
ifornia, Berkeley, School of Law were surveyed. Each survey randomly
placed the student respondent in the position of a judge in one of the
four states included in the study. The survey described how to count
subjects using the tests that judges profess to employ in that state. Each
survey provided two sample precedents from that state, one finding one
subject in a challenged proposition and the other finding multiple sub-
jects in a different challenged proposition.\textsuperscript{13} Finally, each survey provided
summaries of four propositions from that state that are included in the
sample.\textsuperscript{14} Respondents applied the rule and indicated how many subjects
each of the four propositions contained. Respondents provided this in-

\textsuperscript{12} Appendix B shows correlations among the coders’ scores.

\textsuperscript{13} All propositions were assigned to a topical category developed by Miller (2002):
crime; economic regulation; education; environment; health, welfare, and morals; political
or government reform; or taxes. The sample precedents always came from different cate-
gories. Within these limitations the precedents were selected at random from each state’s
list of all propositions challenged on single-subject grounds during the period of the study.
The precedents varied randomly by survey.

\textsuperscript{14} In a given survey no more than two of the summarized propositions came from the
same topical category. Otherwise, propositions summarized in each survey were selected
at random from all propositions challenged in that state.
formation by indicating how closely the provisions in each proposition related to one another on a 6-point scale. This format ensured responses that were comparable and variable across propositions from different states with different lengths and topics.

At least four respondents coded each proposition. Averages of their scores serve as proxies for the correct number of subjects in each proposition under the categorization approach.

Recall the problem identified above: persons attempting to discern correct legal outcomes will suffer from biases, making their assessments an unreliable baseline against which to compare judges’ votes. To mitigate this problem the surveys did not disclose that finding more than one subject implied unconstitutionality. I hypothesize that obscuring the connection between adjudication and outcomes disengaged the students’ political preferences and generated relatively objective determinations.

To test this hypothesis I conducted a separate experiment with an additional 157 Berkeley law students. Most identified themselves as political liberals. All were placed in the position of a judge evaluating the Florida Marriage Protection Amendment, which would ban same-sex marriage and same-sex civil unions. All were instructed to count subjects using the same tests that Florida judges profess to employ, and all received the same two sample precedents. Half of the students, chosen at random, were informed that finding more than one subject in the measure implied invalidity. Seventy-seven percent of informed students, but only 64 percent of uninformed students, found more than one subject. This difference is statistically significant. This supports the hypothesis that obscuring the implications of adjudication leads to more objective determinations.

For the democratic process theory, 247 undergraduates at Berkeley were surveyed. Each survey presented summaries of four propositions

15. A postsurvey questionnaire showed that of the 255 students who completed a survey, only eight understood that finding multiple subjects implied unconstitutionality. This is not surprising, since the single-subject rule is rarely taught in law school. Those eight responses were discarded.
16. On a 7-point scale, with 1 signifying “very conservative” and 7 signifying “very liberal,” respondents self-reported a mean score of 5.2 with a standard deviation of 1.2.
17. Florida voters approved this proposition in November 2008. The Florida Supreme Court held that it contained one subject (Advisory Opinion to the Attorney General Re Florida Marriage Protection Amendment, 926 So. 2d 1229 [Fla. 2006]).
18. A t-test rejects the hypothesis that the mean subject count of the two groups is the same ($p = .049$).
drawn at random from the sample. The summaries were identical to the summaries provided to the law students. Survey respondents used a 6-point scale to describe the ease with which they could make independent judgments about the elements of each proposition. Higher numbers indicated that students found it relatively easy to make independent judgments. Between four and eight students coded each proposition. The average of their scores serves as a proxy for the number of subjects in each proposition under the democratic process approach.

The surveys did not disclose their purpose or provide information on the single-subject rule. Consequently, this measure is independent of the categorization measure.

In addition to the propositions in the sample, all of which were challenged on single-subject grounds, surveys were used to determine the number of subjects under the categorization and democratic process theories for some propositions that were not challenged on single-subject grounds. My aim was to examine whether propositions with more subjects were more likely to be challenged in court. I collected data on a random subset of unchallenged propositions in California and on every unchallenged proposition in Colorado and Oklahoma during the period of this study. In Florida, the state supreme court reviews every proposition that qualifies for the ballot for single-subject compliance (Fla. Const., art. 4, sec. 10). Consequently, there were no unchallenged propositions in Florida.

Before proceeding, a methodological limitation merits attention. The surveys can be used to test an important version of the legal model: neutral application of rules to facts. However, the surveys may not capture all legal considerations. To illustrate, a judge who believes direct democracy threatens constitutional values may find more single-subject violations than other judges. By some accounts, that judge may be acting

19. Specifically, the surveys defined separable and nonseparable preferences and asked respondents to indicate on the scale which kind of preferences they had for the elements of each proposition, where low numbers indicate strong nonseparable preferences and high numbers indicate separable preferences.

20. For California I randomly selected 34 unchallenged propositions that qualified for the ballot during the period of the study. For Colorado I included all 32 unchallenged propositions that qualified for the ballot after 1994, the year the single-subject rule was adopted there. For Oklahoma I included all 14 unchallenged propositions that qualified for the ballot during the period of the study.

21. Single-subject review became mandatory in Florida in 1986. Between 1980 and 1986, only one proposition appeared on the Florida ballot, the Homestead Exemption, voted on in March 1980. This proposition was not challenged on single-subject grounds. I exclude it from the study.
legalistically. The legal variables in this study do not account for any such behavior. If such behavior correlates with judges’ PAJID scores or proposition politics, then the ideology variables capture some of it. In short, the surveys cannot fully disentangle the law and politics knot, but they can separate an important legal strand.

4.4. Basic Results

As an initial test of the legal model, I determine the average categorization and democratic process subject counts for propositions that were not challenged on single-subject grounds, for propositions that were challenged but that courts held did not violate the rule, and for propositions that were challenged and that courts held did violate the rule. I do this for California, Colorado, and Oklahoma but not for Florida, as Florida did not have any propositions that were not challenged. If law matters, then the first set of propositions should have lower average subject counts than the second, and the second should have lower average subject counts than the third.

The data in Table 2 support these hypotheses. The differences are statistically significant. This suggests that litigants consider the strength of their legal argument when deciding whether to file a case and that judges consider the strength of the argument when determining if a proposition violates the rule.

The data from Florida are consistent with the pattern. For propositions that did not violate the rule, the average numbers of subjects under the categorization and democratic process theories were 2.5 and 2.4, respectively. For propositions that did violate the rule, the average subject counts were 3.1 and 2.9.

To test the legal model more rigorously, and to test the political model, I run regressions. Table 3 identifies and describes the variables. Note that the categorization and democratic process subject counts, as well as the ideology variable, have been rescaled from 0 to 1.

As an estimation technique I use linear probability models. Such mod-

22. The following hypotheses are rejected by t-tests: (1) the mean categorization subject count for unchallenged propositions and for challenged propositions with no violation is the same ($p = .006$), (2) the mean categorization subject count for challenged propositions with no violation and for challenged propositions with a violation is the same ($p = .000$), (3) the mean democratic process subject count for unchallenged propositions and for challenged propositions with no violation is the same ($p = .041$), and (4) the mean democratic process subject count for challenged propositions with no violation and for challenged propositions with a violation is the same ($p = .007$).
Table 2. Average Number of Subjects in Propositions (Excluding Florida)

<table>
<thead>
<tr>
<th></th>
<th>Unchallenged Propositions</th>
<th>Challenged Propositions</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>No Violation</td>
<td>Violation</td>
</tr>
<tr>
<td>Categorization Subject Count</td>
<td>2.4</td>
<td>2.8</td>
</tr>
<tr>
<td>Democratic Process Subject Count</td>
<td>2.4</td>
<td>2.7</td>
</tr>
</tbody>
</table>

Elements can generate fitted values outside the unit interval, making the linearity restriction unrealistic for binary outcomes. However, they perform reasonably well for estimating marginal effects (Angrist and Pischke 2009) and are easier to interpret, particularly with respect to interaction terms (Ai and Norton 2003). Appendix C reports that logit and probit models yield similar results as the linear probability model. Appendix D reports robustness checks for selection bias and for attenuation bias. Errors are clustered by proposition.23

Specification (1) in Table 4 supports the legal and political models. The categorization subject count is positively associated with judges’ votes at a statistically significant (.1 percent) level. The democratic process subject count is also positively associated with judges’ votes at a statistically significant (1 percent) level. These findings support the legal model: as the number of subjects in a proposition increases, under the categorization theory or under the democratic process theory, judges are more likely to find a single-subject violation. Ideology is positively associated with violations at a statistically significant (5 percent) level. This supports the political model: as the ideological distance between a judge and the proposition she reviews increases, the judge is more likely to find a single-subject violation.

The data from California include many votes by court of appeals judges. This does not affect the results. Excluding the court of appeals judges, and excluding the California data entirely, yields results similar to those in specification (1). Excluding the California and Oklahoma data also yields results similar to those in specification (1).

Specifications (2) and (3) each remove one legal variable, on the theory that the legal variables might be highly correlated,24 and yield

23. Following Cameron, Gelbach, and Miller (2006), I double clustered by proposition and by judge and triple clustered by proposition, by judge, and by case. Neither approach affected the results.

24. The correlation between the categorization and democratic process subject counts for challenged propositions is .08.
<table>
<thead>
<tr>
<th>Variable</th>
<th>Description</th>
<th>Mean</th>
<th>SD</th>
</tr>
</thead>
<tbody>
<tr>
<td>Dependent variable: Judge’s Vote</td>
<td>Equals one if judge found a violation, zero otherwise</td>
<td>.39</td>
<td></td>
</tr>
<tr>
<td>Independent variables:</td>
<td>Categorization Subject Count</td>
<td>Subject count under the categorization theory, scaled from 0 to 1</td>
<td>.44</td>
</tr>
<tr>
<td>Democratic Process Subject Count</td>
<td>Subject count under the democratic process theory, scaled from 0 to 1</td>
<td>.50</td>
<td>.20</td>
</tr>
<tr>
<td>PAJID Scores</td>
<td>Judge’s party-adjusted judge ideology (PAJID) score, scaled from 0 to 1; higher numbers imply more liberal judges</td>
<td>.41</td>
<td>.19</td>
</tr>
<tr>
<td>Proposition Politics</td>
<td>Proposition politics score, scaled from 0 to 1; higher numbers imply more liberal propositions</td>
<td>.42</td>
<td>.27</td>
</tr>
<tr>
<td>Ideology</td>
<td>Absolute difference between PAJID scores and proposition politics, scaled from 0 to 1</td>
<td>.30</td>
<td>.21</td>
</tr>
<tr>
<td>Panel Effects 1</td>
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<td>.04</td>
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</tr>
<tr>
<td>Panel Effects 2</td>
<td>Equals one if judge ideology score &gt; .5 and majority of judges on panel have ideology scores ≤ .5, zero otherwise</td>
<td>.09</td>
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<tr>
<td>Proposition Salience</td>
<td>Proposition salience score; higher numbers imply more salient propositions</td>
<td>2.00</td>
<td>.53</td>
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<tr>
<td>Preelection</td>
<td>Equals one if proposition reviewed before vote, zero otherwise</td>
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<td>.93</td>
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<td>Equals one if judge on California court, zero otherwise</td>
<td>.11</td>
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<tr>
<td>CO</td>
<td>Equals one if judge on Colorado court, zero otherwise</td>
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<tr>
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<td>OK</td>
<td>Equals one if judge on Oklahoma court, zero otherwise</td>
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<td>Year judgment issued</td>
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Table 4. Law and Politics in Single-Subject Adjudication

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<td></td>
<td>(.096)</td>
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<tr>
<td>Proposition Salience</td>
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<td>Adjusted $R^2$</td>
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<td>.460</td>
<td>.399</td>
<td>.480</td>
<td>.482</td>
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</tbody>
</table>

Note. Models are estimated using ordinary least squares. Errors are clustered by proposition. Standard errors appear in parentheses. Each model controls for party-adjusted judge ideology scores, proposition politics, preelection review, supreme court review, state, and year fixed effects. $N = 1,067$.

*p < .10.

**p < .05.

***p < .01.

Panel Effects 1 is a dummy that takes the value of one if the judge’s ideology score is less than .5 but a majority of the judges on the panel have an ideology score that is greater than or equal to .5. This aims to identify judges who favor a proposition but whose colleagues oppose it.

Panel Effects 2 is a dummy that takes the value of one if the judge’s results similar to those for specification (1). Specification (4) attempts to control for panel effects. “Panel effects” refers to the tendency of conservative judges to vote in a liberal direction when empaneled with a majority of liberal judges and the tendency of liberal judges to vote in a conservative direction when empaneled with a majority of conservative judges (Fischman 2010; Cox and Miles 2008; Miles and Sunstein 2006; Sunstein et al. 2006; Cross and Tiller 1998; Revesz 1997). Panel effects have been studied in the context of three-judge federal appellate court panels, not in the context of the seven- and nine-member state supreme courts that account for most data in this study. Panel effects may operate differently in this context, as supreme court justices do not sit on rotating panels but instead sit with the same colleagues, often for many years. Nevertheless, I attempt to account for them.
ideology score is greater than or equal to .5 but a majority of the judges on the panel have an ideology score that is less than .5. This aims to identify judges who oppose a proposition but whose colleagues favor it. The signs on the coefficients of these variables are consistent with panel effects: judges who favor a proposition are more likely to find a single-subject violation when empaneled with judges who oppose it (Panel Effects 1), and judges who oppose a proposition are less likely to find a violation when empaneled with judges who favor it (Panel Effects 2). Neither variable is statistically significant. Their inclusion does not affect the coefficients for the legal variables. Their inclusion does, however, increase the size of the Ideology coefficient.

Specification (5) attempts to control for the salience of the propositions. Salience may correlate with judges’ votes—if, for example, judges subject to retention elections are disinclined to invalidate salient propositions—and with judges’ ideologies, with the number of subjects in a proposition, or both. Failure to include salience could therefore lead to omitted variable bias. I am not aware of any data on proposition salience. To fill this gap, three University of Virginia law students independently assigned each proposition in the study a score on a 5-point scale, where high scores indicate high-salience propositions and vice versa. The average of their scores is a proxy for salience. It is not statistically significant and does not disturb the results.

To develop a more objective measure of salience, each proposition in the sample was compared to the Harris Poll conducted closest in time to the date the court passed judgment on that proposition. The Harris Poll is a national public opinion poll that asks respondents to name the two most important issues for the government to address. Each proposition was assigned a salience score equal to the percentage of respondents who indicated that the general topic of the proposition (for example, taxes, abortion, term limits) was an important issue. Running specification (5) with this measure of salience did not yield statistically significant results, nor did it affect the other variables of interest.

All specifications support the legal and political models, which implies that both law and ideology matter in single-subject adjudication. Which matters more? This question raises conceptual and normative difficulties: how many judges must deviate from the law, how far must they deviate,

25. Appendix B shows correlations among the coders’ scores.
26. Harris Poll data are available from 1993 onward. This limited the data set to 888 observations. The correlation between this measure of salience and the measure provided by students is .32.
and how often must they do so before politics seems to matter more than law? My data cannot answer these questions. The proxy for ideology is rough, and the model does not fit the data perfectly. Finally, the law and Ideology variables are incommensurable. The former measure relatedness, and the latter measures policy agreement between a judge and a proposition.

With these caveats in mind, I provide tentative insight into the relative importance of law and ideology by considering the value of information on each to lawyers. Suppose a lawyer worked in a state in the study during the period of the study. Suppose that lawyer had to assess single-subject claims involving propositions in the study. On the basis of specification (4), where the value of the Ideology coefficient is greatest, a move from the proposition with the lowest categorization subject count in the sample to the proposition with the highest is associated with a 78-percentage-point increase in the likelihood of a judge finding a violation. The equivalent increase in the democratic process subject count is associated with a 43-percentage-point increase in the likelihood of a judge finding a violation. With respect to Ideology, a move from the judge/proposition combination with the greatest ideological alignment to the combination with the least ideological alignment is associated with a 30-percentage-point increase in the likelihood of a judge finding a violation. Swings in the legal variables are associated with greater changes in the likelihood of a violation than swings in Ideology. For the lawyer, information on law probably would have been more valuable than information on ideology.

4.5. Case Salience and Ideology

The relationships among law, politics, and judges’ votes probably are not consistent across cases, either in the single-subject context or elsewhere. Many observers hypothesize that judges’ ideologies matter most when the legal questions they face implicate fundamental values. Some propositions in the study—on criminal penalties, same-sex marriage, stem cell research—implicate such values. Other propositions—on budgeting, franchise agreements, swine-feeding operations—do not. I do not have a direct measure of the extent to which the propositions im-

27. The hypothesis that the categorization coefficient equals the Ideology coefficient can be rejected ($p = .04$). The hypothesis that the categorization coefficient equals the Democratic process coefficient cannot quite be rejected ($p = .12$). The hypothesis that the Democratic process coefficient equals the Ideology coefficient cannot be rejected ($p = .55$).
**Table 5.** Salience, Ideology, and Law

<table>
<thead>
<tr>
<th>Categorization Subject Count</th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Salience 0–33 percent</td>
<td>.70***</td>
<td>.76*</td>
</tr>
<tr>
<td></td>
<td>(.16)</td>
<td>(.30)</td>
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<tr>
<td>Salience 33–66 percent</td>
<td>.67*</td>
<td>.34*</td>
</tr>
<tr>
<td></td>
<td>(.31)</td>
<td>(.15)</td>
</tr>
<tr>
<td>Salience 66–100 percent</td>
<td>.74*</td>
<td>.44**</td>
</tr>
<tr>
<td></td>
<td>(.29)</td>
<td>(.16)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Democratic Process Subject Count</th>
<th>.35*</th>
<th>.44**</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(.14)</td>
<td>(.16)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Ideology</th>
<th>.34*</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(.15)</td>
</tr>
</tbody>
</table>

| Salience 0–33 percent           | .33  |
|                                 | (.22)|
| Salience 33–66 percent          | .28* |
|                                 | (.15)|
| Salience 66–100 percent         | .43* |
|                                 | (.21)|

| Adjusted R²                     | .507 | .483  |

**Note.** Models are estimated using ordinary least squares and include salience dummies. Errors are clustered by proposition. Standard errors appear in parentheses. Models control for party-adjusted judge ideology (PAJID) scores, proposition politics, panel effects 1 and 2, preelection review, supreme court review, state, and year fixed effects. Model 1 includes interactions between salience dummies and both PAJID scores and proposition politics. N = 1,067.

*p < .10.

*p < .05.

***p < .001.

I do, however, have a measure of the salience of the propositions, which I use as a proxy.

Specification (1) in Table 5 identifies associations between judges’ votes and ideology when the propositions have salience scores in the bottom third, middle third, and top third of the sample. These ranges are arbitrary, but other ranges yield similar results. While all three variables have the expected sign, Ideology is not statistically significant for the set of propositions that are least salient. Ideology is statistically significant for moderately salient (10 percent) and highly salient (5 percent) propositions.28 This is consistent with the hypothesis that ideology matters most when cases implicate fundamental values.

28. All four states have significant numbers of propositions in each of the three salience categories. The hypothesis that the Ideology coefficients for moderately salient and for highly salient propositions are equal cannot be rejected (p = .46).
Specification (1) uses the salience scores provided by students. Using the salience scores obtained by comparing the propositions to the Harris polls yields the same result.

Specification (2) examines associations between judges’ votes and the categorization subject count when the propositions are more and less salient. All categorization variables are statistically significant (5 percent) and have similar coefficients. While the association between ideology and votes appears to vary with salience, the association between law and votes does not.

4.6. Determinacy and Ideology

Many observers hypothesize that law predominates when rules are determinate and ideology predominates when rules are indeterminate.\textsuperscript{29} Testing this hypothesis requires information on the determinacy of the law with respect to the propositions in the sample. To illustrate, if no one challenges propositions that unequivocally have one subject, and if no one drafts propositions that unequivocally have two, then every case in the sample will be somewhat indeterminate. In that event, a finding that ideology associates with votes evenly across the sample would not be inconsistent with the hypothesis. Conversely, if some cases involve propositions that clearly have one subject or that clearly have two, and if other cases are indeterminate, then a finding that ideology correlates evenly with votes across the sample would be inconsistent with the hypothesis.

I do not have a direct measure of the determinacy of the law, but I do have a proxy. In addition to providing the categorization subject count on a 6-point scale, survey respondents provided an absolute number of subjects for each proposition. I used this information to calculate a new variable, Fraction, which indicates the percentage of respondents who found more than one subject in each proposition. A low value for Fraction implies that most or all respondents found only one subject, while a high value implies that most or all respondents found more than one subject. In either case, the law is relatively determinate: the proposition does (high Fraction value) or does not (low Fraction value) violate the rule. Intermediate values of Fraction imply that respondents dis-

\textsuperscript{29} Recent empirical studies provide some support for this hypothesis, finding changes in the relationship between judicial ideology and judges’ votes as standards of review change (Fischman and Schanzenbach 2011; Bartels 2009; Miles and Sunstein 2006).
agreed on the number of subjects and that the law is relatively indeterminate.

Figure 1 provides a histogram of Fraction. It shows that propositions differ little on this dimension. This is why I used the 6-point scale rather than Fraction in the regressions: the scale provides more variation. More important, the figure shows that some propositions have Fraction values near 0, and many have Fraction values of 1. This suggests that the sample includes some determinate cases. Therefore, I can plausibly test the determinacy hypothesis.

Specification (1) in Table 6 examines the relationship between votes and ideology in determinate and indeterminate cases. Determinate cases are those in which the Fraction variable is less than or equal to .25 or in which it equals 1. This captures almost 40 percent of the sample. All other cases are classified as indeterminate. These intervals are somewhat arbitrary, but using different intervals yields similar results.

The association between votes and ideology is statistically significant (10 percent) in indeterminate cases but not in determinate cases. Specification (2) examines the relationship between the categorization subject

30. The categorization variable used in the regressions has 38 unique values, while Fraction has only 10.
31. All four states have significant numbers of observations in the determinate and indeterminate categories.
Table 6. Determinacy, Ideology, and Law

<table>
<thead>
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<th></th>
<th>(1)</th>
<th>(2)</th>
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</thead>
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<td>.79***</td>
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<td>(.18)</td>
<td>(.18)</td>
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<td>Indeterminate</td>
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<td>.78*</td>
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<td></td>
<td>(.31)</td>
<td>(.31)</td>
</tr>
<tr>
<td>Democratic Process Subject Count</td>
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<td>.42**</td>
</tr>
<tr>
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<td>(.15)</td>
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<tr>
<td>Ideology</td>
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<td>.28*</td>
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<td>(.15)</td>
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<td>.24</td>
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<td>(.16)</td>
<td>(.16)</td>
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<tr>
<td>Indeterminate</td>
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<td>.40*</td>
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<td>(.22)</td>
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<td>Adjusted $R^2$</td>
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<td>.480</td>
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</table>

Note. Models are estimated using ordinary least squares. Errors are clustered by proposition. Standard errors appear in parentheses. Models control for party-adjusted judge ideology (PAJID) scores, proposition politics, panel effects 1 and 2, preelection review, supreme court review, state, and year fixed effects. Models include determinate case dummies, which equal 1 for observations where Fraction is less than or equal to .25 or in which it equals 1. Model 1 includes interactions between the determinate case dummy and both PAJID scores and proposition politics. $N = 1,067$.

$p < .10$.  
$p < .05$.  
$p < .01$.  
***$p < .001$.  

count and votes in these classes of cases. The coefficients on both variables are about the same. The level of statistical significance is higher in determinate cases (.1 percent) than in indeterminate cases (5 percent), but this is not necessarily meaningful.

Even with only a rough mechanism for sorting determinate from indeterminate cases, I find some support for the determinacy hypothesis. Ideology appears to matter more when rules are indeterminate.

4.7. Discussion

These results have implications for the single-subject rule. Because of the abstraction problem, observers argue that the categorization theory is indeterminate. But categorization appears to provide predictable answers in many single-subject cases. Abstraction presents greater problems in theory than in practice.

As Cooter and Gilbert (2010) explain, the categorization theory, even
when determinate, cannot achieve the single-subject rule’s purposes. They argue in favor of the democratic process theory, which can. That theory helps to explain judges’ votes, which suggests that it clarifies intuitions about the rule instead of proposing a new approach. This could facilitate acceptance of the theory.

The present paper also has implications for the broader debate over law and politics in judicial decision making. It develops a technique for measuring correct legal outcomes, by which I mean outcomes generated by neutral application of rules to facts. This technique could be used in other areas of the law.

Measuring and accounting for law brings perspective on its importance. Even in single-subject disputes, where conventional wisdom holds that the law is often indeterminate and many of the issues—abortion, immigration, same-sex marriage—are politically charged, law appears to influence judges’ decisions. The data supporting this come from cases litigated mostly in supreme courts, which are not subject to review, involving state constitutional law, which other branches of government cannot easily override. Law matters where many skeptics think it does not.

The relationship among law, ideology, and judges’ votes is not consistent. Ideology appears to matter most in those single-subject cases that implicate fundamental values. Though important, such cases constitute only a fraction of courts’ dockets. I also find some evidence that ideology matters most when the law is indeterminate, which in the single-subject context appears to be less often than observers think.

None of this means that ideology is irrelevant; in fact, this paper strongly supports a version of the political model. The categorization and democratic process approaches are the only legal theories of single-subject adjudication. Even after controlling for them, ideology has a consistent, statistically significant relationship with judges’ votes. This strongly suggests that ideology, separate and distinct from law, influences judicial decision making in this area.

I conclude on a speculative note. Scholars hypothesize that ideology matters most in the law’s “open areas” (Posner 2008, p. 9), where rules are genuinely indeterminate and judges rely on their internal values. The results in Section 4.6 support this theory. The strong and consistent associations between law and judges’ votes reported elsewhere in this paper do not undermine the theory. However, the results do suggest that the law’s open areas may be fewer and farther between than scholars suppose. If law influences judicial decision making in the single-subject
context, it plausibly does the same in many other areas where the rules are clearer and the issues less controversial.

5. CONCLUSION

The single-subject rule requires judges to apply a malleable standard to ballot propositions. This implicates the debate over whether law or ideology motivates judges’ decisions. I use a new methodology to determine the correct legal outcomes for a sample of single-subject cases. The data suggest that both law and ideology influence judicial decision making in this area.

APPENDIX A: DATA ON SINGLE-SUBJECT LITIGATION

This appendix describes the criteria used to create the data set. For each state I searched the Westlaw database containing all opinions from that state’s state-level courts. I limited the date range to the period January 1, 1980, to June 30, 2007.

I retained cases that (a) included a single-subject challenge to a (b) statewide ballot initiative, in which (c) a state appellate or state supreme court (d) issued a decision on the single-subject question. In Californians for An Open Primary v. McPherson, 134 P.3d 299 (Cal. 2006), California’s supreme court held that the state’s separate-vote rule should be construed the same as the state’s single-subject rule. That was the only California separate-vote case during the sample period and was retained.

To be clear, single-subject challenges to local propositions, referenda, and ordinary legislation were discarded, as were unresolved challenges (for example, those that were nonjusticiable because of ripeness concerns) and decisions issued by federal courts. In Florida, the single-subject rule can be used to invalidate propositions that fail to identify

32. Westlaw employees state that every single-subject opinion from state appellate and supreme courts should be stored in their databases. Any omissions are random.
33. For example, I searched the OK-CS database for Oklahoma. I used the following general search term but deleted all words in italics except for the one appearing in the single-subject rule as codified in each state’s constitution: (“single subject” or “single object” or “one subject” or “one object” +1 rule or requirement or provision or clause or limitation or doctrine) or “title-object clause” or (embrac! or relat! or refer! or contain! or confint!) IS (“single subject” or “single object” or “one subject” or “one object”).
34. Referenda, by which I mean propositions placed on the ballot by state legislatures rather than by private citizens or groups, were few in number and were excluded from the sample.
the constitutional provisions they substantially alter and that affect multiple functions of government (see, for example, *Advisory Opinion to the Attorney General re Requirement for Adequate Public Education Funding*, 703 So. 2d 446 [Fla. 1997]). Florida cases that focused on these matters and did not count subjects were discarded.

**APPENDIX B: INTERCODER RELIABILITY**

The measure of proposition politics was gathered by having five public policy graduate students independently assign ideology scores to the propositions in the sample on a scale. Table B1 reports correlations among their scores.

One measure of proposition salience was gathered by having three law students independently assign salience scores to the propositions in the sample on a scale. Table B2 reports correlations among their scores.

**Table B1. Correlation Matrix for Proposition Politics**

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<tr>
<th></th>
<th>Coder 1</th>
<th>Coder 2</th>
<th>Coder 3</th>
<th>Coder 4</th>
<th>Coder 5</th>
</tr>
</thead>
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<td></td>
<td></td>
<td></td>
<td></td>
</tr>
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<td></td>
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<tr>
<td>Coder 3</td>
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<td>.76</td>
<td>1</td>
<td></td>
<td></td>
</tr>
<tr>
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<td>.66</td>
<td>.68</td>
<td>1</td>
<td></td>
</tr>
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<td>Coder 5</td>
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<td>.68</td>
<td>.60</td>
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</table>

**Table B2. Correlation Matrix for Proposition Salience**

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<th>Coder 1</th>
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<th>Coder 3</th>
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<td>Coder 3</td>
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**APPENDIX C: COMPARING THE LINEAR PROBABILITY MODEL TO LOGIT AND PROBIT MODELS**

Table C1 compares the linear probability model to logit and probit models. For the latter two models, the table reports marginal effects. All three models reflect specification (1) from Table 4 and yield similar results.
Table C1. Comparing Linear Probability, Logit, and Probit Models

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<th>Probit</th>
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<td>(.15)</td>
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<td>Democratic Process Subject Count</td>
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<td>.50**</td>
<td>.50**</td>
</tr>
<tr>
<td></td>
<td>(.16)</td>
<td>(.17)</td>
<td>(.16)</td>
</tr>
<tr>
<td>Ideology</td>
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<td>.22*</td>
<td>.23**</td>
</tr>
<tr>
<td></td>
<td>(.10)</td>
<td>(.10)</td>
<td>(.089)</td>
</tr>
<tr>
<td>Adjusted $R^2$/pseudo-$R^2$</td>
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<td>.347</td>
<td>.346</td>
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</tbody>
</table>

Note. Average partial effects are reported for logit and probit models, with standard errors (delta method) of those effects in parentheses. Asterisks indicate the statistical significance of untransformed coefficients. Errors are clustered by proposition. Each model controls for party-adjusted judge ideology scores, proposition politics, preelection review, supreme court review, and state. Each model includes year dummies. For logit and probit models, this yields results approximately equal to a conditional logit with year fixed effects. Some year dummies correlate perfectly with the dependent variable, and consequently the logit and probit models have fewer observations than those in Table 4. The linear probability model is limited to those same observations. $N = 896$.

* $p < .05$.
** $p < .01$.
*** $p < .001$.

**APPENDIX D: ROBUSTNESS CHECKS**

**D1. Selection Bias**

Propositions challenged on single-subject grounds are not randomly selected. As Table 2 suggests, propositions likely to violate the rule are challenged more often. Because I only observe propositions that are challenged, the sample may be biased.

A standard technique for mitigating selection bias is to estimate a two-stage model using an instrument. Identifying a suitable instrument is difficult. I rely on a natural corrective. As discussed, Florida’s supreme court reviews all propositions that qualify for the ballot for single-subject compliance. Consequently, the Florida data do not suffer from the selection problem.

Table D1 reports results from running the same model as in specification (1), Table 4, with the addition of interaction terms between the Florida dummy and all independent variables. The coefficients on these interaction terms indicate the relationship between each variable and judges’ votes in Florida as compared to the other states. If these coefficients equal 0, the covariates have the same association with judges’ votes in Florida, where the sample is unbiased, as in the other states.
Table D1. Test for Selection Bias: Comparing Florida to Other States

<table>
<thead>
<tr>
<th>Variable</th>
<th>Coefficient</th>
<th>(Standard Error)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Categorization Subject Count</td>
<td>.66**</td>
<td>(.21)</td>
</tr>
<tr>
<td>Democratic Process Subject Count</td>
<td>.51*</td>
<td>(.22)</td>
</tr>
<tr>
<td>Ideology</td>
<td>.16</td>
<td>(.13)</td>
</tr>
<tr>
<td>FL × Categorization Subject Count</td>
<td>.12</td>
<td>(.33)</td>
</tr>
<tr>
<td>FL × Democratic Process Subject Count</td>
<td>-.29</td>
<td>(.31)</td>
</tr>
<tr>
<td>FL × Ideology</td>
<td>-.039</td>
<td>(.15)</td>
</tr>
<tr>
<td>Adjusted $R^2$</td>
<td>.57</td>
<td></td>
</tr>
<tr>
<td>$N$</td>
<td>1,067</td>
<td></td>
</tr>
</tbody>
</table>

Note. The model tests for selection bias by examining whether coefficients generated by data from Florida, where all propositions are reviewed for single-subject compliance, differ from coefficients generated by data from other states, where a nonrandom subset of propositions are reviewed. Model are estimated using ordinary least squares. Errors are clustered by proposition. Standard errors appear in parentheses. The model includes controls for party-adjusted judge ideology scores, proposition politics, preelection review, supreme court review, state, and year fixed effects, as well as the Florida dummy interacted with these controls.

* $p < .05$.
** $p < .01$.

Tests of the hypotheses that the coefficients on the Florida interaction terms in Table D1 equal 0 cannot be rejected. I conclude that selection bias, at least in the form I test for here, does not disturb the results.

D2. Attenuation Bias

The categorization and democratic process subject counts were gathered with student surveys. The political orientation of propositions was ascertained by having students code them. These variables may suffer from measurement problems. Each observation, $k$, of one of these variables is the sum of the true unobserved value, indicated with an asterisk, and measurement error: $x_k = x_k^* + e_k$. If the observed values correlate with the measurement error, $\text{Cov}(x_k, e_k) \neq 0$, then including the observed values in the regressions leads to attenuation bias in the coefficient estimates (see Woolridge 2002, pp. 73–75). An included regressor, $x_k$, correlates with the error term.
### Table D2. Test for Attenuation Bias

<table>
<thead>
<tr>
<th>Variable</th>
<th>Coefficient</th>
</tr>
</thead>
<tbody>
<tr>
<td>Categorization Subject Count Hat</td>
<td>.72**</td>
</tr>
<tr>
<td></td>
<td>(.14)</td>
</tr>
<tr>
<td>Democratic Process Subject Count Hat</td>
<td>.37*</td>
</tr>
<tr>
<td></td>
<td>(.13)</td>
</tr>
<tr>
<td>Ideology Hat</td>
<td>.26*</td>
</tr>
<tr>
<td></td>
<td>(.098)</td>
</tr>
<tr>
<td>Adjusted $R^2$</td>
<td>.506</td>
</tr>
<tr>
<td>N</td>
<td>1,067</td>
</tr>
</tbody>
</table>

**Note.** The model tests for attenuation bias by replacing three variables that may correlate with the error term with predicted values of those variables that do not correlate with the error term. The model is estimated using ordinary least squares. Errors are clustered by proposition. Standard errors appear in parentheses. The model controls for party-adjusted judge ideology scores, Proposition Politics Hat, pre-election review, supreme court review, state, and year fixed effects.

* $p < .01$.

** $p < .001$.

I correct for this by using the students’ responses to generate two variables and then using one as an instrument for the other. To illustrate, each observation of the variable Categorization Subject Count is the average of several students’ survey responses. I can use those responses to generate two variables, Categ1 and Categ2. The former is the average of two students’ survey responses, and the latter is the average of the remaining students’ survey responses. An OLS regression in which Categ1 is the dependent variable and Categ2 the independent variable generates a set of predicted values for Categ1 that are not correlated with the error term. I can use those predicted values in the original regression to mitigate attenuation bias.

I followed this procedure to generated predicted values for the variables Categorization Subject Count, Democratic Process Subject Count, and Proposition Politics. The new variables, all scaled from 0 to 1, have the suffix “Hat.” The variable Ideology Hat equals the absolute value of the difference between judges’ PAJID scores and Proposition Politics Hat, scaled from 0 to 1. Table D2 repeats the regression from specification (1), Table 4, using these predicted values.

All coefficients of interest are statistically significant and have values similar to those reported in Table 4. I conclude that attenuation bias does not disturb the results.
REFERENCES


Unpublished manuscript. University of California, Department of Political Science, Berkeley.


