The Role of Constitutional Features in Judicial Review

Adam R. Brown (brown@byu.edu)
Department of Political Science
Brigham Young University
Provo, UT 84602

Abstract. American state constitutions vary tremendously in their length, amendment rate, and age. These three variables—especially the first two—strongly influence the rate at which state supreme courts strike down state actions for violating the state constitution. Longer, more detailed constitutions reduce policy flexibility, increasing judicial invalidations; rarely updated constitutions may fail to address modern concerns, increasing invalidations; and recently adopted constitutions may contain fragile logrolls and similar shortcomings, also increasing invalidations. These findings add new considerations to a rich literature on judicial review in state supreme courts.

In urging ratification of the US Constitution, Alexander Hamilton wrote in Federalist 78 that judges under the new charter would have no "will but merely judgment." A rich literature on judicial behavior has since shown that judges are far more complicated than Hamilton's stark "will" versus "judgment" dichotomy allows. True, there are forces pulling judges toward neutral Hamiltonian "judgment," but research also shows that judges sometimes yield to institutional pressures or exhibit personal preferences. Much of this research on judicial behavior emerges from the American states, which provide rich institutional variance and a tremendous number of observations. This literature shows that "state supreme court justices are not the mechanical appliers of law conceptualized by normative legal theory but instead are strategic actors deciding cases within a complicated environment" (Hall MG 2014, 337). That "complicated environment" varies from state to state but may include judicial selection mechanisms, judges' personal attitudes, legislative and executive checks on the judiciary, and, of course, the actual laws and constitutions that judges must interpret.

This article adds an additional layer to this "complicated environment" by considering the specificity, amendability, and age of American state constitutions. As will be shown, state supreme courts overturn more state actions where state constitutions are lengthier, amended less often, or adopted more recently. These three variables change the jurisprudential environment in ways that influence state supreme court invalidations, regardless of a state constitution's actual content. Longer, more detailed constitutions reduce policy flexibility, increasing judicial invalidations; rarely updated constitutions may fail to sufficiently address modern concerns, increasing invalidations; and recently adopted constitutions can create uncertainty or contain fragile compromises, also increasing invalidations.

Theory and hypotheses

Research on judicial review in state supreme courts has developed four general conceptual approaches: Attitudinal, neo-institutional, separation-of-powers, and jurisprudential (Langer 2002). The attitudinal approach emphasizes that judges, like other political actors, have their own preferences, and these preferences may influence their judicial decisions. Put baldly, liberal and conservative judges rule differently, and their differences can be mapped onto ideological scales (Segal and Cover 1993; Epstein and Knight 1998; Brace, Langer, and Hall 2000; Grofman and Brazill 2002; Martin and Quinn 2002; Bonica and Woodruff 2015; Windett, Harden, and Hall 2015). Beyond political ideology, attitudinal differences may also arise from judges' sex (Boyd, Epstein, and Martin 2010) or other personal characteristics (Jaros and Canon 1971).

The neo-institutional approach observes that judges may strategically depart from their sincere preferences in response to features of the judicial branch's structure that change judges' incentives or environment (Canon and Jaros 1970; Hall and Brace 1989; Brace and Hall 1990; Hall

1992; Brace and Hall 1993; Leonard and Ross 2014). This approach pays special attention to judicial selection mechanisms (Bonneau and Hall 2009; Bonneau and Hall 2016), finding, for better or worse, that elected judges deliver rulings that more closely track citizen preferences (Brace and Hall 1997, 2001; Brace and Boyea 2008; Lewis, Wood, and Jacobsmeier 2014). Elected judges may also rule more harshly on crime (Huber and Gordon 2004; Gordon and Huber 2007), change their behavior prior to mandatory retirement (Hall MG 2014), rule in favor of campaign donors (Cann 2007), write less scholarly opinions (Goelzhauser and Cann 2014; Leonard and Ross 2016; Nelson n.d.), have more rulings overturned by the US Supreme Court (Owens et al. 2015), or respond differently to lobbying efforts via third-party amicus briefs (Kane 2017).

The balance-of-powers approach explores how court packing plans, retaliatory legislation, and other pressure mechanisms make judges vulnerable to the legislative and executive branches. The easier it is for these other branches to punish, ignore, or overrule the courts, the more judges defer to their preferences (Eskridge 1991; Epstein and Walker 1995; Epstein and Knight 1998; Brace, Hall, and Langer 2001; Langer 2002). For example, appellate courts exercise "self-restraint" when they fear court-curbing legislation (Clark 2009) or executive non-implementation (Hall MEK 2014).

In contrast, the jurisprudential approach declares that "quite simply, judges are constrained by law" (Langer 2002, 22)—that is, by the substance of the constitution, of written statutes, and of relevant precedents (Stumpf 1998), possibly receiving an assist in complex legal areas from amicus briefs (Kane 2017). Literature seeking to interpret state constitutions and constitutional conventions falls under this banner, of course (e.g. Tarr 1998; Dinan 2006; Tarr and Williams 2006; Grad and Williams 2006; but see Gardner 1992 and Kahn 1993). So does empirical work showing that case facts (Emmert 1992) and the substance of state law (Fino 1987) influence state

court decisions. For example, state constitutional guarantees of privacy rights affect how state judges rule on abortion restrictions (Brace, Hall, and Langer 1999, 2001), and legal substance influences state death penalty cases (Hall 2014). Moreover, longer (hence more detailed) state statutes limit judges' discretion within individual policy domains (Randazzo, Waterman, and Fix 2011). Of course, these four approaches need not operate in isolation; jurisprudential variables can interact with institutional rules (Brace and Hall 1990) and judges' personal attitudes (Segal and Spaeth 1993; Kane 2017).

Though this literature on judicial behavior has cast a wide net and explored diverse variables, some considerations remain overlooked. This article presents the novel argument that the length, amendment rate, and age of state constitutions directly influence the rate at which state supreme courts strike down state actions. This constitutional focus gives this article a jurisprudential feel, but with a twist: Whereas most jurisprudential studies emphasize the legal substance of court opinions or of state constitutions, this article focuses on the state constitution's specificity, amendability, and age as important variables in their own right. State constitutions vary widely in length (from 8,600 to 87,000 words), amendment rate (from 2 to 127 amendments adopted over 20 years), and age (from 29 to 235 years)—and these variations alone have predictable effects on state supreme courts' use of judicial review.[1]

First, consider constitution length. At a terse 7,600 words (4,500 without amendments), the familiar US Constitution sketches only the broadest contours of the federal government's structure and powers, leaving most policy details to Congress or to the people. With so much left unsaid,

[1] Statistics as of the end of 2015. Like the analysis below, these statistics exclude Alabama due to an unusual requirement that many county-level policies be written into the state constitution. The resulting 375,000-word document receives far more frequent amendments than any other state constitution, a clear statistical outlier. The county-level focus of most amendments renders Alabama a qualitative outlier as well; its constitution simply plays a different role than other constitutions play.

vague clauses like the Commerce Clause and the Necessary and Proper Clause create ample opportunity for federal policy to enter uncharted policy areas—and for federal judges to uphold those expansions. Though the shortest state constitutions take the same broad approach as the US Constitution, many delve into immense detail, addressing topics as mundane as "ski trails and highway routes, public holidays and motor vehicle revenues" (Tarr 1998, 2) and as lofty as labor rights, commercial rights, social rights, economic rights, or environmental rights (Dinan 2006). Others address unique local needs; the Arizona and Utah constitutions, for example, devote an entire article each to water rights. Some address public education, state lotteries, or alcohol (Dinan 2006). The lengthiest constitutions contain such minute statutory details about "highways, railroads, … corporations, mines, interest rates, lotteries and bingo, and … golf courses" that critics see "simply a frivolous people who are unable to distinguish between things that are truly important and things that are not" (Gardner 1992, 819-20).

In many states, then, constitutions address not only process but policy. Just as longer legislative statutes constrain bureaucratic discretion (Huber and Shipan 2002), longer constitutions constrain a state's policy flexibility—and because state supreme courts must enforce those constraints, longer state constitutions should cause the state supreme court to strike down more state actions.[2] Randazzo, Waterman, and Fix (2011) have already shown a related effect: Namely, that lengthier legislative statutes limit judicial discretion within specific policy realms. This article takes the next step: Judges interpreting a longer, more detailed constitution will have less wiggle room generally to uphold politically desirable but technically unconstitutional state actions. Lengthier constitutions, then, should lead to more state supreme court invalidations.

[2] There is a critical assumption here that lengthier documents contain greater specificity, an assumption that rests on foundational work by Huber and Shipan (2002) and that is supported, in the case of national constitutions across the OECD, by Tsebelis and Nardi (2016).

Second, consider amendment rates. The US Constitution receives so few amendments that it reads almost the same today as a century ago. Even if we assume their magnanimity and wisdom, the framers were nevertheless wholly unacquainted with many of the challenges modern American society faces. Failure to update the US Constitution has kept it silent on many major issues—privacy, abortion, assault weapons, transgender rights, electronic surveillance, antiterrorist interrogation techniques, campaign contributions, corporate personhood—forcing the US Supreme Court to work out how an 18th-century document relates to 21st-century problems. We can hardly wonder, then, that the US Constitution—updated far less frequently than any state constitution—inspires such vigorous debate between those who favor a living approach and those who favor an originalist or textualist approach (Brennan 2017).

By contrast, states that update their constitutions regularly ensure that their constitutions speak to modern concerns—and, thanks to widespread supermajority amendment requirements, these states also ensure that their constitutional language remains well within the mainstream of modern political discourse. Moreover, because constitutional amendments generally originate from the same legislatures that enact routine statutes, legislatures that update their constitutions regularly ensure that their constitutions accommodate their statutory interests.[3] Rarely updated constitutions therefore leave appellate courts to enforce boundaries more often; frequently updated constitutions do not.[4] As such, frequently updated constitutions should result in fewer state supreme court invalidations.

[3] Nationwide, 89% of the amendments adopted from 1994-2013 originated in state legislatures. Even with the 18 states that allow initiated amendments, 75% of adopted amendments were legislatively referred.

[4] To be clear, this discussion refers not to the total number of amendments a constitution has received since adoption, but to how regularly it receives amendments—e.g. the number of amendments per biennium.

Third and finally, consider a constitution's age. Nineteen states continue to rely on the constitutions they adopted at statehood (as amended), but the other 31 states have replaced their constitutions entirely from time to time. The average state has had 2.9 constitutions, with Louisiana's 11 marking the extreme. Constitution age has murkier theoretical effects on judicial behavior than constitution length and amendment rates. On the one hand, we can expect old constitutions to exert similar effects as low amendment rates, compelling courts to enforce antiquated boundaries on modern policy. By this logic, judicial invalidations should rise with a constitution's age.

On the other hand, important differences between piecemeal amendments and wholesale replacement suggest the opposite hypothesis. First, an entirely new constitution covers so much ground that drafters are more likely to miss errors, infelicitous phrasing, or inadvertent conflicts with existing policy, any of which can provoke litigation leading to judicial review. Similar flaws are far less likely to escape notice in piecemeal constitutional amendments. Second, an entirely new constitution will lack judicial precedents guiding legislators and judges in their application of critical clauses, likewise increasing invalidations as policymakers muddle through the new constitution's applied meaning. Third and most importantly, wholesale replacement invites logrolling in a way piecemeal amending does not. Just as an omnibus statute can bring together logrolled compromises on diverse policy matters that would not otherwise be enacted if considered separately (Brown 2012; Townsend 1985), an "omnibus" constitutional revision can bring together logrolled constitutional compromises that would not have survived consideration as separate amendments. As partisan control of the state government evolves over subsequent years, policymakers may (intentionally or otherwise) push the limits of these delicate compromises, prompting litigation leading to judicial review. For these three reasons, wholesale constitutional

replacement should have the opposite causal effect as piecemeal amending, so that younger constitutions provoke more invalidations than older ones.

Though constitution age could reasonably push invalidations in either direction, then, the latter set of mechanisms are intuitively more compelling—especially when we recall that controlling for each constitution's amendment rate may offset the former set of mechanisms. Thus, the hypothesis is that young constitutions will provoke more invalidations, though this hypothesis is stated more agnostically than those concerning constitution length and amendment rate.

To sum, this theoretical argument about a constitution's length, amendment rate, and age produces three hypotheses:

- H1: State supreme courts will invalidate more state actions if the state constitution is lengthier.
- H2: State supreme courts will invalidate more state actions if the state constitution has a low amendment rate.
- H3: State supreme courts will invalidate more state actions if the state constitution was adopted more recently.

These hypotheses derive from jurisprudential logic, yet other approaches can produce plausible competing hypotheses for some of these variables. Most notably, Langer (2002) linked invalidations to amendment rates (but not to length or age) using balance-of-power logic. Langer reasoned that an overly aggressive court could provoke the legislature into enacting retaliatory constitutional amendments, especially in states with low procedural barriers to amending the constitution; in turn, fear of retaliatory amendments could deter judges in those states from invalidating state actions. With a positive relationship in one direction (invalidations increase

amendments) but a negative one in the other (amendments decrease invalidations), Langer's logic has complicated implications that her empirical analysis did not fully address.[5] Her statistical analysis looked only for the negative effect of amendments on invalidations, finding mixed results—perhaps because of interference from a positive effect in the opposite causal direction. (By contrast, the empirical analysis below will use instrumental techniques to isolate the causal relationship flowing from amendments to invalidations without interference from any relationship flowing in the opposite direction.)

Because Langer's balance-of-power logic concurs with this article's jurisprudential logic in expecting amendments to decrease invalidations, the findings below must be interpreted carefully, with empirical results for amendment rates contrasted with results for constitution length and age. Strong evidence for H2 (amendments) but not H1 (length) or H3 (age) would be suggestive of Langer's balance-of-power logic, which makes a prediction only for amendments, not for length or age; strong evidence for multiple hypotheses would be more suggestive of jurisprudential logic, which makes a prediction for all three variables.

Attitudinal logic can also imply an indirect correlation between constitutional features and invalidations. As ideological distance between the legislature and the court grows, judges' personal political preferences may lead to increased invalidations. Responding to (or anticipating) these invalidations, legislators may amend the constitution more frequently to give their statutory preferences greater constitutional weight; they may also seek to give more policies constitutional

[5] To be clear, her hypothesis linking amendments to invalidations had a relatively low priority among the 15 hypotheses about judicial review addressed in her book. Moreover, Langer considered procedural amendment difficulty rather than actual amendment rates as a predictor of judicial review activity, though she found inconsistent results. In light of the instrumented results presented below, perhaps the reverse causation problem described here contributed to her inconsistent results for this variable.

rather than statutory heft, thereby lengthening the constitution. The analysis below will therefore include a measure of interbranch ideological distance to control for these possibilities.

The judicial review literature draws on many other attitudinal, institutional, and jurisprudential variables. When conducting a cross-sectional study of only 49 states, one must carefully choose which variables to include and which to omit. Omitting a variable will not bias a study's results unless the omitted variable plausibly relates to both the dependent variable and a critical independent variable. Although the literature suggests that, for example, the method of selecting judges, a court's total caseload, judges' professional background, and legislative professionalism may correlate with judicial invalidations, these variables have little apparent connection to constitution length, amendment rate, and age, making it unnecessary to include them in this theoretical discussion. As a precaution, however, the analysis below will include a few of the most likely confounding variables, none of which wind up changing the results given below.

To summarize, a state constitution's length (H1), amendment rate (H2), and age (H3) can influence the jurisprudential environment sufficiently to change the rate at which state supreme courts strike down state actions. Lengthy constitutions limit policy possibilities, seldom updated constitutions may insufficiently address modern policy concerns, and young constitutions may bring fragile compromises, drafting errors, and interpretive ambiguity; any of these circumstances makes it likely that the state supreme court will find itself invalidating policy actions more often. These claims add a previously unexplored angle to the literature on judicial review, suggesting additional ways that the jurisprudential environment can change state supreme court behavior.

Data and variables

Modern written constitutions first emerged in the American states, and the 50 states, if treated as separate entities, continue to represent a majority of the world's advanced constitutional democracies—that is, developed democracies that treat their constitutions as genuinely binding documents.[6] As such, the 50 states present an exceptional opportunity for comparative study of how written constitutions influence supreme appellate courts. (For reasons footnoted earlier, Alabama is omitted throughout.)

This article takes its dependent variable—the frequency with which state supreme courts strike down state actions for violating the state constitution—from the State Supreme Court Data Project, a remarkable dataset created by Paul Brace and Melinda Gann Hall. This ambitious project coded every state supreme court case heard from 1995 through 1998 on a dizzying number of dimensions (Brace and Hall 1999; Hall MG 2014). Because this article's theory and hypotheses focus on state-level variables that were relatively static from 1995 through 1998, the following analysis collapses Brace and Hall's case-level dataset into cross-sectional state-level data.[7] Brace and Hall's coding separates state actions that were struck down for violating the state constitution from actions that were struck down (in state court) for violating the federal constitution. The dependent variable is therefore the raw count of cases within each state wherein a state supreme

[6] As evidence, consider the comparative literature on constitutional amendability. Cross-national constitutional studies generally limit their analysis to consolidated democracies that take constitutionalism seriously, resulting in 22 (Lutz 1994), 24 (Rasch and Congleton 2006), 26 (Lijphart 1999), 32 (Tsebelis and Nardi 2016), or 39 (Anckar and Karvonen 2002; Lorenz 2004) cases—always fewer cases than the 50 American states.

[7] Because all variables of interest are measured at the state level, nothing is lost by collapsing the Brace and Hall data from court cases to states; the effective degrees of freedom for state-level independent variables would still be only as high as the number of states, even in a multilevel model with 28,345 court cases (cf. Snijders 2005). Moreover, because many states amend their constitutions only biennially, little is lost by collapsing the two-period biennial data into a single-period cross-section, since nearly all variance in constitution lengths and amendment rates is cross-sectional, not longitudinal, in a short two-period series.

court struck down a state action for violating the state constitution.[8] (The decision to measure the number rather than the proportion of cases striking down state actions is addressed shortly.)

Data on state constitution lengths, amendment rates, and constitution ages were compiled from various editions of the *Book of the States*, published annually by the Council of State Governments. To reflect but slightly precede the four-year 1995-98 period covered by the Brace and Hall data, each constitutional variable was gathered for each state-year from 1994 through 1997, then collapsed into cross-sectional data.[9] Due to rightward skew, this study's key independent variables—constitution length (mean word count over 1994-97), amendment rates (total amendments enacted in 1994-97), and constitution age (as of 1996)—were incremented and logged.[10] A supplemental appendix presents scatterplots of the dependent variable (total invalidations in 1995-98) on each of the three independent variables in Figures A1, A2, and A3. None of these plots raises any serious concerns about influential outliers or other threats.

For reasons discussed above, the analysis controls for the ideological distance between the judicial and legislative branches. Court ideal points are the collapsed 4-year (1995-98) mean of Bonica and Woodruff's (2015) annual common-space estimate of each state court's median justice. Legislature ideal points are the collapsed 4-year (1995-98) mean of Shor and McCarty's annual common-space estimate of each legislature's median (outside Nebraska, the annual lower and

[8] The dependent variable counts cases where a state supreme court invalidated a state action based on "a constitutional challenge to a law passed by another branch of government under the state constitution," language that can include challenges to administrative as well as legislative actions. Brace and Hall separate out cases into five categories: criminal appeals, civil private cases, civil government cases, juvenile cases, and miscellaneous cases. All five types are aggregated here.

[9] These variables change slowly within states and are relatively static within a single decade. As shown in the supplemental appendix (Table A4), the central conclusions reported here hold if these variables are measured from 1992-95, predating measurement of the dependent variables entirely.

[10] The decision to measure age in 1996 is trivial, chosen only because it comes near the beginning of the 1995-98 period on the dependent variable. Because all constitutions age at the same rate, measuring age in a different year would change only the constant, not the estimated marginal effect.

upper chamber medians are first averaged together). Both sources constrain scores between −1 (more liberal) and +1 (more conservative). The ideological distance measure used here is therefore the absolute difference between the court's ideal point and the legislature's, with a hypothetical range from 0 (no distance) to 2 (extreme distance). Actual scores range from 0.08 to 1.63, with a median of 0.52, mean of 0.58, standard deviation of 0.35, and modest rightward skew. The supplement provides a scatterplot of invalidations against this variable, showing the expected positive correlation.

The analysis also controls for three additional variables. An omitted variable cannot produce omitted variable bias unless it correlates with both the dependent variable and an independent variable; these three additional variables are included for most plausibly passing this test. First, judicial selection. The analysis includes a dichotomous indicator for states that select state supreme court judges through partisan elections, drawing on the *Book of the States*. Elected judges may have different incentives when it comes to invalidations; in turn, these political incentives may motivate legislatures to protect their interests by constitutionalizing legislation. (As it happens, this variable correlates modestly with the dependent variable—$r=0.33$, $p=0.02$—but not with any constitutional variables.[11] Table A1 in the supplement presents a correlation matrix for all variables used in the analysis.)

Second, docket control. States vary in whether an intermediate court of appeals stands between entry-level courts and the state supreme court; among states with an intermediate court, states also vary in how many cases advance to the supreme court. To capture both angles, the analysis controls for each supreme court's total caseload from 1995 through 1998 (from the Brace

[11] Other selection variables (dummies for appointment, merit selection, or nonpartisan elections) have no correlation with any variables.

and Hall dataset). Caseloads vary from 219 to 863 rulings, with a nearly normal distribution around the mean (562) and median (558).[12] Caseloads turn out not to correlate with the three constitutional variables. Moreover, caseloads do not even correlate with invalidations, which may surprise some readers; after all, we might expect the number of invalidations to rise with the number of rulings.[13] On reflection, however, it becomes clear why these two variables do not correlate. Docket control is designed to shield state supreme courts from minor cases so they can focus on major ones. Although some states create intermediate courts to handle minor cases, major cases—and cases raising credible constitutional claims are major cases—generally reach the state supreme court regardless. Thus, much of the variance in supreme court caseload reflects variance in how many minor cases make it to the highest court, not variance in how many major cases do. As such, state supreme courts that hear more total cases do not strike down more actions than state supreme courts that hear fewer ($r=0.16$, $p=0.26$).

Third, legislative professionalism. Some legislatures may be more prone to enacting careless legislation that attracts constitutional scrutiny. Though this propensity cannot be measured directly, one variable that may proxy for a legislature's carelessness is the length of its legislative session, since states with only a month or two per year to enact new legislation have less time to vet bills carefully and catch errors than states with year-round sessions. The analysis therefore includes the mean number of days per year spent in any kind of session by each state legislature from 1994-97, drawing on data from the *Book of the States*.[14]

---

[12] For perspective, the US Supreme Court issued 427 rulings during this period, placing it exactly at the 25th percentile when compared to state supreme courts (based on author's analysis of data from Spaeth et al. 2016).

[13] We might also expect the opposite; writing an opinion justifying an invalidation requires more judicial energy than upholding a state action, and overworked courts may hesitate to bear these costs. I thank an anonymous reviewer for this insight.

[14] States report session length in either calendar days (including weekends and holidays) or legislative days. Following standard practice, calendar reports are multiplied by 5/7 to estimate legislative days. Some states report more days for

Analysis

Table 1 presents results from four models. In each, the dependent variable is the total number of cases from 1995 through 1998 where a state supreme court struck down a state action for violating the state constitution. Model (1a) unwisely ignores any potential for reverse causation by presenting an ordinary least squares estimate without any instruments. If, following Langer's (2002) logic presented above, amendment rates have a negative causal effect on invalidations but invalidations have a positive causal effect on amendment rates, then we would expect these effects to cancel each other out in OLS estimation. Indeed, that is what we see in (1a): A null effect for the amendment rate.

[Table 1 here]

Where good instruments are available, instrumental regression can disentangle these sorts of reciprocal relationships, allowing a clean test of the effect of amendments on invalidations without interference from any effect of invalidations on amendments. Fortunately, this is a case where such instruments exist. Good instrumental variables must strongly predict the problematic independent variable (amendment rates) without being causally influenced by the dependent variable (invalidations). Brown (2015) identifies five variables that predict amendment rates.[15] Two of them, a constitution's length and age, enter the present analysis both as first-stage

one chamber than the other, often reflecting extra days that senates spend in confirmation sessions; when the chambers diverge, the lower chamber length is used. *Book of the States* groups data into even-odd biennia; where a state reports in odd-even biennia, the reported length was divided in half and distributed across the two years. Legislative websites and journals were investigated as needed to correct implausible records or fill in missing data, with session length inferred from session dates or from legislative voting records.

[15] The cited study conducted both cross-sectional and multidecade longitudinal analysis of state constitutional amendment rates, both of which produced similar results, although the longitudinal analysis included additional time-variant regressors. Because the present analysis is cross-sectional, it draws its instruments only from the cross-sectional analysis.

instruments and as second-stage independent variables.[16] The other three, all drawn from *Book of the States*, are used only as instruments. They are (1) whether amending the constitution by legislative referral requires legislative procedures more complex than a simple majority, such as a supermajority vote or concurrent votes in consecutive legislative sessions, with complexity reducing amendment rates[17]; (2) the total membership of the legislature, with larger legislatures enacting fewer amendments[18]; and (3) whether the state constitution limits the number of amendments the legislature may propose for voter ratification at each election.[19]

Returning to Table 1, model (1b) presents results from two-stage least squares (2SLS) instrumental regression. Unlike (1a), (1b) provides strong support for the preceding hypotheses. The raw coefficients in (1b) imply tiny marginal effects; for example, at the margin, adding a single word to a state constitution is associated with a 0.070% increase in the number of state statutes struck down from 1995-1998. Nevertheless, the independent variables have such wide ranges that these tiny marginal effects translate into large effects in practice. With other variables at their median values, moving from the 25th to 75th percentile in logged constitution length (in raw terms, a move from 14,630 to 31,550 words) is associated with an increase from 0.9 to 6.3 state statutes struck down; moving from the 25th to 75th percentile in logged amendment rate (from 2 to 8 amendments) is associated with a decrease from 6.8 to 0.7 statutes struck down; and moving from the 25th to 75th percentile in logged constitution age (from 37 to 129 years) is associated with

[16] Brown (2015) also found that interacting the constitution's age and length meaningfully improved prediction of constitutional amendment rates. Including that interaction term as an instrument in the models below does improve the first-stage model's fit in Table 1 and Table 2. However, adding the interaction term causes the model to fail the Sargan (1958) and Basmann (1960) tests for correlation between instruments and second-stage residuals (p=0.03 for both tests), indicating that it is not a valid instrument.

[17] Several state constitutions give their legislature the option of proposing an amendment either by two separate simple majority votes held in consecutive legislative sessions or by supermajority vote in a single legislative session, forcing these two amendment procedures to be merged into a single "legislative complexity" dummy (Ferejohn 1997).

[18] On the effects of increasing the number of institutional veto players generally, see Tsebelis (2002).

[19] States rarely reach their amendment limit, but the presence of these limits deters legislatures from proposing amendments.

a decrease from 6.5 to 3.5 statutes struck down. Considering that the dependent variable ranges from 0 to 16, with a mean of 4.5 and only modest skew, these are unquestionably large effects.

Given the difference between the OLS results in (1a) and the 2SLS results in (1b), let us pause to consider the appropriateness of instrumental estimation. Analysts should employ instrumental estimation when an independent variable is potentially endogenous, when available instruments are exogenous, and when instruments explain a substantial amount of the endogenous variable's variance. Diagnostic tests suggest that (1b) clearly meets the first two criteria, while reasonably meeting the third. First, the Durbin (1954) and Wu-Hausman (Wu 1974; Hausman 1978) statistics indicate that the variable we suspect of endogeneity—the total number of amendments adopted—is indeed endogenous; we reject exogeneity with $p<0.02$ under both tests. Second, the Sargan (1958) and Basmann (1960) tests find no evidence that the instrumental variables correlate with the second-stage residuals ($p>0.45$ for both tests), suggesting that the instruments are indeed exogenous and the exclusion restriction is satisfied.

The third criterion—that available instruments explain a substantial amount of the endogenous variable's variance—is less clearly defined, as it involves judging whether the first-stage regression has "good enough" fit. A soft rule of thumb has emerged that instruments are unquestionably "strong" when the first stage regression has an F statistic of roughly 10 or greater (Staiger and Stock 1997). Regressing the total number of amendments on all five first-stage regressors—constitution length, constitution age, complex procedures, legislature size, and amendment limits—produces an F statistic approaching this advice ($F=7.4$, $p<0.01$, adj $R^2=0.40$); however, the partial F statistic for the three "pure" instruments alone (complex procedures, legislature size, and amendment limits) is much lower ($F=2.4$), since constitution length and age do much of the first stage's heavy lifting.

When, as in this case, the instruments are valid but perhaps weak, limited information maximum likelihood estimation (LIML) has better properties than 2SLS (Staiger and Stock 1997). As it happens, however, the LIML estimates presented in (1c) yield the same substantive conclusions as the 2SLS estimates in (1b); if anything, LIML produces stronger estimated coefficients. Other instrumental specifications produce similar results. For example, an instrumental tobit model would account for the five observations that are left-censored at zero on the dependent variable. Whether employing conditional maximum-likelihood tobit estimates or Newey's (1987) minimum chi-squared (two-step) estimator, however, the results are essentially the same as in (1b) and (1c) and are therefore not presented here.

Given that invalidations are a count variable, the most appropriate specification appears in (1d): Instrumental Poisson regression (employing the two-step generalized method of moments estimator and robust multiplicative standard errors). To this specification, model (1e) adds controls for judicial selection, total caseload, interbranch ideological distance, and legislative session length; these controls do not appreciably change the results, though constitution age declines in statistical significance ($p=0.06$). Of all these controls, only ideological distance has a significant relationship with invalidations; this variable therefore can serve as a useful reference when interpreting the substantive impact of other variables.

Toward that end, Figure 1 plots predicted values for the dependent variable using coefficients from (1d) and (1e). The first three panels depict the constitutional variables; the fourth panel depicts ideological distance for comparison. Each independent variable is plotted from its 10^{th} to its 90^{th} percentile, with other variables held at their medians. Because this approach produces some unobserved combinations of constitutional features, some predictions fall outside

the dependent variable's observed range, particularly for the lengthiest constitutions. All four panels constrain the vertical axis to the same range to simplify visual comparisons.

[Figure 1]

Constitution lengths and amendment rates have strong effects across their observed range, far exceeding the effect of ideological distance. Amendment rates appear to have diminishing marginal effects on invalidations; other things being equal, the median state needs only a modest amendment rate—an average of only one newly adopted amendment per year, or 4 total from 1994-97—to dramatically reduce judicial invalidations. By contrast, constitution lengths have a more consistent effect across the plotted range. A constitution's age matters far less than its length or amendment rate, though age nevertheless has effects comparable to that of ideological distance.

Robustness

The preceding analysis considers how frequently courts actually strike down state actions for violating the state constitution. Of course, judges cannot take action unless some petitioner asks them to.[20] Attorneys arguing before a state supreme court have every incentive to mine their state constitutions for any clause suggesting that a state action should be struck down. As such, the arguments given above about how constitution lengths, ages, and amendment rates affect the jurisprudential environment should affect petitioners as much as judges.

[20] Petitioner claims are not strictly necessary for an invalidation; judges can independently identify constitutional violations. Such action is rare, though: Across the 228 state supreme court cases in which justices struck down a state action for violating the state constitution, only 2 did not feature prior constitutional claims by petitioners. Thus, the dependent variables from Table 1 and Table 2 correlate strongly ($r=0.77$, $p<0.0001$). On the flip side, there were hundreds of cases in which petitioners raised state constitutional claims that courts did not sustain; only 21% of constitutional claims resulted in actual invalidations.

Accordingly, Table 2 adapts the dependent variable. Rather than count invalidations, models in this table instead count cases where petitioners claimed that a state action violates the state constitution. Obviously, claims that an action violates the constitution arise far more frequently than decisions that actually strike actions down. This revised dependent variable ranges from 3 to 62, with a mean (21.5) nearly 5 times larger than the original dependent variable's mean and a standard deviation (12.6) more than 3 times larger. This measure's larger variance presents statistical advantages, especially with so few degrees of freedom. Other than this change to the dependent variable, Table 2 replicates Table 1 exactly.

[Table 2 here]

Owing to this new dependent variable's greater range, most coefficients are larger in Table 2 than in Table 1. On the whole, though, the same patterns arise: The naïve OLS specification in (2a) produces a null effect for amendment rates, but the instrumental specifications in (2b), (2c), and (2d) produce the predicted results for all variables. Petitioners are more likely to tell judges that a state action violates the state constitution when the constitution is lengthy, rarely amended, or young. These findings increase confidence in the results from Table 1.

The supplemental appendix presents several additional tables and figures. Table A1 gives pairwise correlations for all variables. Tables A2 and A3 replicate Tables 1 and 2 respectively but with control variables included in all specifications. Table A4 shifts the constitutional variables back to 1992-95 rather than 1994-97, entirely preceding the 1995-98 range for the dependent variable. The results presented here persist through all these alternatives. Moreover, Tables A5 and A6 show that this finding wanes when it ought to—that is, when we consider the number of state actions struck down in state court for violating the federal rather than the state constitution.

On top of all this, the supplement's scatterplots, referenced earlier, give little reason to worry about influential outliers. On balance, then, the models presented here and in the supplement favor the conclusion that judicial invalidations vary meaningfully in response to the length, amendment rate, and age of the state constitution.

Conclusion

Alexander Hamilton expected federal judges to have no "will but merely judgment." The extensive literature on state supreme courts shows that judges are more complicated than Hamilton's dichotomy allows. Judges have personal preferences; they react to credible legislative or executive pressures; they adapt to the judiciary's structure; yet they also take careful account of the relevant case facts, laws, and precedents. They are indeed "strategic actors deciding cases within a complicated environment" (Hall MG 2014, 337).

This article has added one additional dimension to that complicated environment, demonstrating how the length, amendment rate, and age of a state constitution affect the rate at which state supreme courts strike down state actions. The analysis employed instrumental estimation techniques that isolate the effect of amendments on invalidations without interference in the other causal direction, and these results persist through diverse specifications and after controlling for the most plausible confounding variables. Constitution age has a similar substantive effect on invalidations as the ideological distance between the legislative and judicial branches; constitution length and amendment rate have much stronger effects. These results favor this article's central claim. The content of each state's constitution certainly matters and deserves careful interpretation, but even without engaging the substance of a state's constitution, its length, amendment rate, and age influence the jurisprudential environment sufficiently to affect

invalidations. Courts strike down more actions where the state constitution is lengthy, rarely updated, or (to a lesser extent) young.

This article has drawn its dependent variable from the well-developed literature on state supreme courts—a literature, together with the broader law and courts literature, that recent reviews argue has tremendous untapped potential (Martin and Hazelton 2012; Bartels and Bonneau 2015). However, this article has drawn its independent variables from the sparser empirical literature on state constitutions. Most nations have adopted written constitutions; though some governments have only the most casual relationship with their foundational documents, many take them seriously. The American states represent a majority of the world's developed and stable constitutional democracies, creating rich research opportunities. These written constitutions erect the arenas where today's political games are played. In addition to its immediate findings, this article also suggests the importance of these state constitutions more generally.

References

Anckar, Dag, and Lauri Karvonen. 2002. Constitutional amendment methods in the democracies of the world. Delivered at the XIIIth Nordic Political Science Congress, Aalborg, Denmark, August 15-17.

Bartels, Brandon L., and Chris W. Bonneau, eds. 2015. *Making law and courts research relevant: The normative implications of empirical research*. New York: Routledge.

Basman, Robert L. 1960. On finite sample distributions of generalized classical linear identifiability test statistics. *Journal of the American Statistical Association* 55: 650-9.

Bonica, Adam, and Michael Woodruff. 2015. A common-space measure of state supreme court ideology. *Journal of Law, Economics, and Organization* 31 (August): 472-98.

Bonneau, Chris W., and Melinda Gann Hall. 2009. *In defense of judicial elections*. New York: Routledge.

Bonneau, Chris W., and Melinda Gann Hall, eds. 2016. *Judicial elections in the 21st century*. New York: Routledge.

Boyd, Christina L., Lee Epstein, and Andrew D. Martin. 2010. Untangling the causal effects of sex on judging. *American Journal of Political Science* 54 (April): 389-411.

Brace, Paul, and Brent D. Boyea. 2008. State public opinion, the death penalty, and the practice of electing judges. *American Journal of Political Science* 52 (April): 360-72.

Brace, Paul, and Melinda Gann Hall 1990. Neo-institutionalism and dissent in state supreme courts. *Journal of Politics* 52: 54-70.

Brace, Paul, and Melinda Gann Hall. 1993. Integrated models of judicial dissent. *Journal of Politics* 55: 914-935.

Brace, Paul, and Melinda Gann Hall. 1997. The interplay of preferences, case facts, context, and structure in the politics of judicial choice. *Journal of Politics* 59 (November): 1206-31.

Brace, Paul, and Melinda Gann Hall. 1999. The state supreme court data project. *Law and Courts* 9 (spring): 21-21.

Brace, Paul, and Melinda Gann Hall. 2001. 'Haves' versus 'have nots' in state supreme courts: Allocating docket space and wins in power asymmetric cases. *Law and Society Review* 35 (2): 393-413.

Brace, Paul, Melinda Gann Hall, and Laura Langer. 1999. Judicial choice and the politics of abortion: Institutions, context, and the autonomy of courts. *Albany Law Review* 62:1265-1304.

Brace, Paul, Melinda Gann Hall, and Laura Langer. 2001. Placing courts in state politics. *State Politics and Policy Quarterly* 1:81-108.

Brace, Paul, Laura Langer, and Melinda Gann Hall. 2000. Measuring the preferences of state supreme court judges. *Journal of Politics* 62 (May): 387-413.

Brennan, Timothy. 2017. Thomas Jefferson and the living Constitution. *Journal of Politics* 79 (3): 936-48.

Brown, Adam R. 2012. The item veto's sting. *State Politics and Policy Quarterly* 12 (2): 183-203.

Brown, Adam R. 2015. When do states amend their constitutions? Presented at the annual meeting of the American Political Science Association, held in San Francisco, California, September 3-6.

Cann, Damon M. 2007. Justice for sale? Campaign contributions and judicial decisionmaking. *State Politics and Policy Quarterly* 7 (fall): 281-297.

Canon, Bradley C. and Dean Jaros. 1970. External variables, institutional structure, and dissent on state supreme courts. *Polity* 3: 175-200.

Clark, Tom S. 2009. The separation of powers, court curbing, and judicial legitimacy. *American Journal of Political Science* 53 (October): 971-89.

Council of State Governments. 1990-2014. *Book of the States*. Lexington, KY: Council of State Governments.

Dinan, John J. 2006. *The American state constitutional tradition*. Lawrence, KS: University Press of Kansas.

Durbin, James. 1954. Errors in variables. *Review of the International Statistical Institute* 22: 23-32.

Emmert, Craig F. 1992. An integrated case-related model of judicial decision-making: Explaining state supreme court decisions in judicial review cases. *Journal of Politics* 54: 543-52.

Epstein, Lee, and Thomas G. Walker. 1995. The role of the Supreme Court in American society: Playing the reconstruction game. In *Contemplating Courts*, ed. Lee Epstein. Washington DC: CQ Press.

Epstein, Lee, and Jack Knight. 1998. *The choices justices make*. Washington DC: Congressional Quarterly Press.

Eskridge, William N. 1991. Overriding Supreme Court statutory interpretation decisions. *Yale Law Journal* 101:825-41.

Ferejohn, John. 1997. The politics of imperfection: The amendment of constitutions. *Law and Social Inquiry* 22 (2): 501-530.

Fino, Susan. 1987. Judicial federalism and equality guarantees in state supreme courts. *Publius* 17:51-67.

Gardner, James A. 1992. The failed discourse of state constitutionalism. *Michigan Law Review* 90 (February): 761-837.

Goelzhauser, Greg, and Damon M. Cann. 2014. Judicial independence and opinion clarity on state supreme courts. *State Politics and Policy Quarterly* 14 (2): 123-41.

Gordon, Sanford C., and Gregory A. Huber. 2007. The effect of electoral competitiveness on incumbent behavior. *Quarterly Journal of Political Science* 2 (2): 107-38.

Grad, Frank P., and Robert F. Williams. 2006. *State Constitutions for the Twenty-First Century: Drafting State Constitutions, Revisions, and Amendments*. Albany: State University of New York Press.

Grofman, Bernard, and Timothy J. Brazill. 2002. Identifying the median justice on the Supreme Court through multidimensional scaling: Analysis of "natural courts" 1953-1991. *Public Choice* 112: 55-79.

Hall, Matthew E.K. 2014. The semiconstrained court: Public opinion, the separation of powers, and the US Supreme Court's fear of nonimplementation. *American Journal of Political Science* 58 (April): 352-66.

Hall, Melinda Gann. 1992. Electoral politics and strategic voting in state supreme courts. *Journal of politics* 54: 427-446.

Hall, Melinda Gann. 2014. Representation in state supreme courts: Evidence from the terminal term. *Political Research Quarterly* 67 (2): 335-346.

Hall, Melinda Gann, and Paul Brace. 1989. Order in the courts: A neo-institutional approach to judicial consensus. *Western Political Quarterly* 42: 391-407.

Hausman, Jerry A. 1978. Specification tests in econometrics. *Econometrica* 46: 1251-71.

Huber, Gregory A., and Sanford C. Gordon. 2004. Accountability and coercion: Is justice blind when it runs for office? *American Journal of Political Science* 48 (2): 247-63.

Huber, John D., and Charles R. Shipan. 2002. *Deliberate discretion? The institutional foundations of bureaucratic autonomy*. Cambridge: Cambridge University Press.

Jaros, Dean, and Bradley C. Canon. 1971. Dissent on state supreme courts: The differential significance of characteristics of judges. *Midwest Journal of Political Science* 15: 322-346.

Kahn, Paul W. 1993. Interpretation and authority in state constitutionalism. *Harvard Law Review* 106 (March): 1147-68.

Kane, Jenna Becker. 2017. Lobbying justice(s)? Exploring the nature of amici influence in state supreme court decision making. *State Politics and Policy Quarterly* 17 (December): 251-74.

Langer, Laura. 2002. *Judicial review in state supreme courts: A comparative study*. Albany: State University of New York Press.

Leonard, Meghan E. and Joseph V. Ross. 2016. Understanding the length of state supreme court opinions. *American Politics Research* 44: 710-733.

Leonard, Meghan E. and Joseph V. Ross. 2014. Consensus and cooperation on state supreme courts. *State Politics and Policy Quarterly* 14: 3-28.

Lewis, Daniel C., Frederick S. Wood, and Matthew L. Jacobsmeier. 2014. Public opinion and judicial behavior in direct democracy systems: Gay rights in the American states. *State Politics and Policy Quarterly* 14 (4): 367-388.

Lijphart, Arend. 1999. *Patterns of Democracy: Government Forms and Performance in Thirty-Six Countries*. New Haven: Yale University press.

Lorenz, Astrid. 2004. Stabile Verfassungen? Konstitutionelle Reformen in Demokratien. *Zeitschrift für Parlamentsfragen* 35 (3): 448-68.

Lutz, Donald S. 1994. Toward a theory of constitutional amendment. *American Political Science Review* 88 (June): 335-370.

Martin, Andrew D., and Kevin M. Quinn. 2002. Dynamic ideal point estimation via Markov chain Monte Carlo for the US Supreme Court, 1953-1999. *Political Analysis* 10: 134-153.

Martin, Andrew D., and Morgan L. W. Hazelton. 2012. What political science can contribute to the study of law. *Review of Law and Economics* 8 (2): 511-29.

Nelson, Michael J. n.d. Elections and explanations: Judicial retention and the readability of judicial opinions. Unpublished manuscript.

Newey, Whitney K. 1987, Efficient estimation of limited dependent variable models with endogenous explanatory variables. *Journal of Econometrics* 36: 231-50.

Owens, Ryan J., Alexander Tahk, Patrick C. Wohlfarth, and Amanda C. Bryan. 2015. Nominating commissions, judicial retention, and forward-looking behavior on state supreme courts: An empirical examination of selection and retention methods. *State Politics and Policy Quarterly* 15 (2): 211-238.

Randazzo, Kirk A., Richard W. Waterman, and Michael P. Fix. 2011. State supreme courts and the effects of statutory constraint: A test of the model of contingent discretion. *Political Research Quarterly* 64 (4): 779-89.

Rasch, Bjorn Erik, and Rodger D. Congleton. 2006. Amendment procedures and constitutional stability. In *Democratic Constitutional Design and Public Policy: Analysis and Evidence*, eds. Rodger D. Congleton and Birgitta Swedenborg. Cambridge: MIT Press.

Sargan, John Denis. 1958. The estimation of economic relationships using instrumental variables. *Econometrica* 26: 393-415.

Segal, Jeffrey A., and Harold J. Spaeth. 1993. *The Supreme Court and the attitudinal model*. Cambridge: Cambridge University Press.

Shor, Boris, and Nolan McCarty. 2015. Aggregate state legislator Shor-McCarty ideology data, June 2015 update. doi:10.7910/DVN/K7ELHW, Harvard Dataverse, V1, UNF:6:l5O+/whNdgWGB1Vt4nEheA==

Snijders, Tom AB. 2005. Power and sample size in multilevel modeling. In *Encyclopedia of Statistics in Behavioral Science*, eds BS Everitt and DC Howell, Vol. 3. Chicester: Wiley.

Spaeth, Harold J., Lee Epstein, Andrew D. Martin, Jeffrey A. Segal, Theodore J. Ruger, and Sara C. Benesh. 2016. Supreme Court Database, Version 2015, Release 03. URL: http://Supremecourtdatabase.org

Staiger, Douglas, and James H. Stock. 1997. Instrumental variables regression with weak instruments. *Econometrica* 65 (May): 557-86.

Stumpf, Harry P. 1998. *American judicial politics*. San Diego: Harcourt Brace Jovanovich.

Tarr, G. Alan. 1998. *Understanding state constitutions*. Princeton: Princeton University Press.

Tarr, G. Alan, and Robert F. Williams, eds. 2006. *State constitutions for the twenty-first century: The politics of state constitutional reform*. Albany: State University of New York Press.

Townsend, Nancy J. 1985. Single subject restrictions as an alternative to the line-item veto. *Notre Dame Journal of Law, Ethics, and Public Policy* 1 (2): 227-58.

Tsebelis, George. 2002. *Veto players: How political institutions work*. Princeton: Princeton University Press.

Tsebelis, George, and Dominic J. Nardi. 2016. A long constitution is a (positively) bad constitution: Evidence from OECD countries. *British Journal of Political Science* 46: 457-478.

Windett, Jason H., Jeffrey J. Harden, and Matthew E.K. Hall. 2015. Estimating dynamic ideal points for state supreme courts. *Political Analysis* 23 (3): 461-9.

Wu, De-Min. 1974. Alternative tests of independence between stochastic regressors and disturbances: Finite sample results. *Econometrica* 42: 529-46.

Table 1: State Actions Struck Down by State Supreme Courts, 1995-98

	(1a)	**(1b)**	**(1c)**	**(1d)**	**(1e)**
Specification	OLS	2SLS	LIML	Poisson	Poisson
Amendments (ln), 1994-97	-0.98	-5.6*	-6.7*	-1.2*	-1.6*
	(0.84)	(2.7)	(3.3)	(0.39)	(0.40)
Constitution length (ln), 1994-97	3.5*	7.0*	7.9*	1.7*	2.2*
	(1.1)	(2.4)	(2.8)	(0.42)	(0.54)
Constitution age (ln), 1996	-1.2	-2.5*	-2.8*	-0.44*	-0.37
	(0.71)	(1.1)	(1.3)	(0.19)	(0.20)
Partisan judicial elections	--	--	--	--	0.40
					(0.34)
Total caseload, 1995-98	--	--	--	--	-0.00074
					(0.00084)
Mean ideological distance	--	--	--	--	0.99*
					(0.32)
Mean session length, 1994-97	--	--	--	--	-0.0033
					(0.0019)
Constant	-24*	-46*	-51*	-12*	-16*
	(10)	(17)	(20)	(3.7)	(4.5)
Observations	49	49	49	49	49

The dependent variable is the total number of state actions struck down by each state's supreme court for violating the state constitution from 1995 through 1998. Rounding to two significant digits. Standard errors in parentheses. *$p \leq 0.05$ (two-tailed).

Table 2: State Actions Provoking State Constitutional Claims, 1995-98

	(2a)	**(2b)**	**(2c)**	**(2d)**	**(2e)**
Specification	OLS	2SLS	LIML	Poisson	Poisson
Amendments (ln), 1994-97	-3.8 (2.7)	-23* (10)	-26* (12)	-1.1* (0.23)	-1.2* (0.30)
Constitution length (ln), 1994-97	11* (3.6)	25* (8.6)	28* (9.9)	1.3* (0.31)	1.4* (0.36)
Constitution age (ln), 1996	-6.5* (2.3)	-12* (4.1)	-13* (4.6)	-0.48* (0.12)	-0.48* (0.12)
Partisan judicial elections	--	--	--	--	0.24 (0.29)
Total caseload, 1995-98	--	--	--	--	0.00015 (0.00054)
Mean ideological distance	--	--	--	--	0.62* (0.29)
Mean session length, 1994-97	--	--	--	--	-0.0022 (0.0011)
Constant	-51 (33)	-143* (64)	-158* (72)	-6.0* (2.8)	-6.9* (3.0)
Observations	49	49	49	49	49

The dependent variable is the total number of cases heard in state supreme courts from 1995 through 1998 wherein petitioners raised claims that the state constitution had been infringed. Rounding to two significant digits. Standard errors in parentheses. *$p \leq 0.05$ (two-tailed).

Figure 1: Predicted Invalidations in the Median State