The Policy Effects of the Partisan Composition of State Government

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July 19, 2015

Abstract

How much does it matter which party controls the government? On one hand, campaign positions and roll-call records suggest that contemporary American parties are very ideologically polarized. On the other hand, the existing evidence that electing Democrats into office causes the adoption of more liberal policies is surprisingly weak. We bring clarity to this debate with the aid of a new measure of the policy liberalism of each state in each year 1936–2014, using regression-discontinuity and dynamic panel analyses to estimate the policy effects of the partisan composition of state legislatures and governorships. We find that until the 1980s, partisan control of state government had negligible effects on the liberalism of state policies, but that since then partisan effects have grown markedly. Even today, however, the policy effects of partisan composition remain small relative to differences between states—less than one-tenth of the cross-sectional standard deviation of state policy liberalism. This suggests that campaign positions and roll-call records may overstate the policy effects of partisan selection relative to other factors, such as public opinion.

We thank participants at the 2014 MPSA Conference and seminar participants at MIT, Rochester, and Duke for feedback on previous versions of this manuscript. We are grateful for feedback on earlier drafts of this manuscript from Thad Kousser, Jens Hainmueller, Andy Hall, Danny Hidalgo, and Eric Schickler. We appreciate the research assistance of Melissa Meek, Kelly Alexander, Aneesh Anand, Tiffany Chung, Emma Frank, Joseff Kolman, Mathew Peterson, Charlotte Swasey, Lauren Ullmann, and Amy Wickett. We also appreciate the willingness of Frederick Boehmke and Carl Klarner to generously share data. We are grateful for support from the School of Humanities, Arts, and Social Sciences at MIT. All mistakes, however, are our own.

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In November 2012, Republican Pat McCrory was elected governor of North Carolina, completing his party's capture of state government two years after its takeover of the state legislature. The North Carolina GOP took advantage of its newfound control of the state by passing a flood of conservative legislation. Together, Governor McCrory and the Republican legislature cut unemployment insurance, repealed the state's estate tax, "flattened" the income tax, relaxed gun laws, tightened restrictions on abortion, and enacted a variety of other changes that collectively moved state policies sharply to the right (Fausset 2014; Davey 2014).

Six decades earlier, in 1948, the Ohio Democratic Party had experienced a similar electoral triumph. With the popular Frank Lausche at the top of their ticket, the Democrats not only defeated the incumbent Republican governor but captured both houses of the legislature as well. In contrast to North Carolina, however, this switch in party control had minimal policy consequences. During their two years of unified control, Ohio Democrats failed to pass any major new liberal policies. Governor Lausche, a fiscal conservative, actually proposed a budget that reduced state expenditures from their level under his Republican predecessor. Even liberal initiatives with the governor's support, such as a bill banning racial discrimination in employment, failed to make it through both houses of the Democratic legislature (*Time* 1956; Usher 1994; Chen 2009, 165, 273).

Which of these cases, North Carolina in 2012 or Ohio in 1948, better exemplifies the policy consequences of the partisan composition of state government? That is, does electing Democrats rather than Republicans to the governorship and legislature result in markedly more liberal policies, or are the policy effects small or even nonexistent? The existing literature exhibits little consensus regarding the policy effects of partisan control of state government. Many classic studies of state politics emphasize the exceedingly weak correlations between party control of state government and state policy outcomes (e.g., Hofferbert 1966; Dye 1984; Garand 1988; Erikson, Wright, and McIver 1989). Other classic studies find weak, conditional effects of party control in a subset of states (Brown 1995; Dye 1984). More recent analyses, with stronger designs, have tended to find modest effects of partisan control of state governments on some but not all policy outcomes (Besley and Case 2003; Alt and Lowry 2000; Kousser 2002; Leigh 2008; Fredriksson, Wang, and Warren 2013). Even studies with strong identification strategies, however, tend to examine policy indicators one by one, resulting in low statistical power and multiple testing problems, which often lead to inferential errors (Westfall and Young 1993; Gelman, Hill, and Yajima 2012). Finally, none of these studies examine the possibility that party effects have changed over time as Democrats and Republicans have polarized ideologically.

To bring clarity to the ambiguous literature on party effects, we employ a research design that improves on previous studies in two major ways. First, we use a much more comprehensive policy measure, the policy liberalism scale developed by Caughey and Warshaw (Forthcoming), which is estimated from a dataset of nearly 150 policies covering each year between 1936 and 2014. Like roll-call scaling techniques such as NOMINATE (Poole and Rosenthal 2007), estimating a latent ideological dimension to policy reduces measurement error. This reduction in measurement error yields more precise estimates of party effects and allows us to examine how they change over time.

Our second advance is to use more credible identification strategies than previous state-level studies of partisan policy effects. We estimate the effects of Democratic governors and state legislatures using two designs: the electoral regression-discontinuity (RD) design, which exploits the variation in party control induced by very close elections, and dynamic panel analysis, which exploits year-specific partisan variation within states. These designs enable us to identify the causal effect of partisan control of state government on policy separately from other factors that affect state policy, such as economic circumstances or public opinion.

Our findings suggest that both cases, Ohio in 1948 and North Carolina in 2012, are typical of the period in which they occurred. In the 1930s–70s, the policy effects of the partisan composition of state government were negligible.[1] Since the 1980s, however, partisan effects on policy have grown rapidly, as has the match between the liberalism of states' policies and the partisanship of their elected officials. In other words, the policies implemented by Democrats and Republicans have diverged within states at the same time as states have sorted to bring their partisan tendencies in line with their ideological leanings.

Nevertheless, party effects on policy liberalism remain modest, at least in comparison to party effects on roll call records in Congress and state legislatures (cf. Poole and Rosenthal 2007; Shor and McCarty 2011). Even today, for example, the yearly effect of electing a Democratic rather than Republican governor is less than one-tenth the standard deviation of policy liberalism across states. Substantively, an increase in policy liberalism of this magnitude implies a half-a-point increase in the percent of policies on which a state has the liberal policy option, and an increase in welfare payments by a dollar or two per recipient.

These empirical findings sharpen our understanding of several important issues in American politics. First, they add further evidence that over the past generation the two parties have polarized ideologically—not only in the campaign positions they take or the roll-call votes they cast, but in the policies they implement once in office (cf. Ansolabehere, Snyder, and Stewart 2001; McCarty, Poole, and Rosenthal 2006). At the same time, however, our focus on policy outputs rather than legislative position-taking offers a different perspective on the substantive magnitude of partisan polarization, suggesting that even today shifts in party control do not cause extreme swings in policy, as some have feared (e.g., Bafumi and Herron 2010). Finally, by

1. Our results during this period are thus consistent with the negligible effects found by classic studies conducted using data from this period (e.g., Hofferbert 1966; Erikson, Wright, and McIver 1989)

distinguishing the effects of legislative seat share and of majority control, we contribute to the debate over majority-party power in legislatures. We find that shifts in majority control have clear policy consequences, but we do not find clear evidence of additional policy effects of legislative seat share, a pattern more consistent with partisan rather than purely preference-based theories of lawmaking (Aldrich and Rohde 2000; Cox and McCubbins 2005; contra Krehbiel 1993).[2]

The remainder of this paper is organized as follows. We first discuss the substantive and theoretical background for our inquiry. We then turn to empirics, beginning with a description of our annual measure of state policy liberalism. Next, we estimate the policy effects of Democratic governors and state legislatures using RD and dynamic panel analyses. The penultimate section offers explanations and interpretations of our empirical results, followed by a brief conclusion.

1 Substantive and Theoretical Background

In 1957, Anthony Downs offered his well-known argument that electoral competition pushes office-motivated parties to converge at the middle of the ideological spectrum. An important implication of the Downsian model is that since the parties offer identical platforms, the actual outcome of the election has no impact on policy. It is surely no coincidence that Downs's argument appeared at a time when many observers of American politics bemoaned the parties' lack of ideological distinctiveness and cohesion (e.g., American Political Science Association 1950). In subsequent years, however, as partisan polarization in the United States became increasingly apparent, scholars identified a number of theoretical mechanisms—including policy-motivated candidates, primary elections, commitment problems, and imperfect information—that could cause parties to diverge ideologically rather than converge (for a review,

2. Note, however, that our identification strategy focuses primarily on the effects of shifts in control rather than shifts in seat share. So we cannot conclusively state that legislative seat share does not affect state policy.

see Grofman 2004).

Whether convergence or divergence dominates in practice is an empirical question, and in recent years a number of studies have sought to evaluate their relative importance. This work generally concludes that congressional candidates hew closely to the policy positions of their party, moderating very little in response to the ideological preferences of their districts (Ansolabehere, Snyder, and Stewart 2001; McCarty, Poole, and Rosenthal 2009). Indeed, Lee, Moretti, and Butler (2004) find that voters do not affect candidate positions at all but merely select between the parties' (polarized) policy platforms. One implication often drawn from these findings is that polarization between the parties leads to "wide swings in policy" that "do not well represent the interests of middle-of-the-road voters" (Poole and Rosenthal 1984, 1061). Studies that measure the ideology of voters and politicians on a common scale appear to corroborate this view, suggesting that partisan polarization leads to "leapfrog representation" (Bafumi and Herron 2010).

Like national-level studies, recent research on state politics shows that there are large partisan differences in roll-call voting between legislators representing the same constituency (Shor and McCarty 2011). The relationship between state policies and the partisanship of state officials, however, is subject to a much more vigorous debate. Most classic studies find no relationship between partisan control of state government and policy.[3] Hofferbert (1966), for example, finds "no significant relationship" between "the party in power and public policy" on welfare issues. Winters (1976) finds that party control of state government makes "little or no difference" for tax burdens and spending benefits. Hanson (1984) find no significant effects of party control on the scope of Medicaid programs, while Plotnick and Winters (1985) find no effect of party control on AFDC benefits. Lax and Phillips (2011), Erikson, Wright, and McIver (1993) and Barrilleaux (1997) actually find that Democratic control is

3. Other classic studies find conditional effects of party control in a subset of states (e.g., Brown 1995; Dye 1984).

associated with more conservative policy outcomes.

These cross-sectional studies are hampered by two methodological limitations. First, they lack a credible identification strategy. As a result, their findings about the effect of party control on policy could be biased by a number of omitted variables that are correlated with partisan control of government (e.g., public opinion). Second, their findings are all based on a single slice of time. For instance, Erikson, Wright, and McIver (1993) is based on data from the 1980s, while Lax and Phillips (2011) is based on data from the early 2000s. As a result, it is hard to know whether their results are generalizable to other time periods.

A number of recent studies have used time-series cross-sectional data to examine policy effects in dynamic perspective. On the whole, these studies have found "weak and oftentimes conditional" evidence that party control affects state policies (Kousser and Phillips 2009, 70). For example, Alt and Lowry (1994) use a structural-equation model of state fiscal policy to conclude that Democrats in non-Southern states spend more than Republicans when they control state government. Besley and Case (2003) estimate a two-way fixed-effects model of four state policy indicators and find a mix of liberal, conservative, and indeterminate effects of Democratic governors and state legislatures. More recent studies that employ electoral RD designs find similarly ambiguous and contingent effects. Fredriksson, Wang, and Warren (2013) find that re-electable Democratic governors increase taxes but term-limited ones decrease taxes. Leigh (2008) examines a total of eight policy indicators and finds significant effects on just one (minumum wages), leading him to conclude that governors "behave in a fairly non-ideological manner" (256).

We are thus left with a puzzle. Studies of legislative position-taking, not only Congress but also in state legislatures, give the impression that the two parties are now cohesive, extreme, and unresponsive to constituency preferences. On this view, the moderate electorate has little influence over candidates' positions and is thus doomed to suffer swings between policy extremes. By contrast, studies of state politics present an alternative picture in which partisan effects on policy are ambiguous, contingent, or possibly non-existent. Adjudicating empirically between these two perspectives requires both better measurement and better research designs, goals we pursue in the sections that follow.

2 An Annual Measure of State Policy Liberalism

Many studies of the predictors of state policy rely on indices, factor scores, or other summary measures of the liberalism of state policies (e.g., Hofferbert 1966; Klingman and Lammers 1984; Erikson, Wright, and McIver 1993; Gray et al. 2004). Such composite measures can result in substantial reductions in measurement error (and thus gains in statistical power) if, as seems reasonable with state policies, the indicators on which they are based tap into a single latent variable (Ansolabehere, Rodden, and Snyder 2008).[4] In addition, composite measures of policy liberalism often come closer to capturing the outcome of interest, which is usually not a specific policy domain but rather the overall ideological orientation of state policies.[5] The disadvantage of the composite approach has been the difficulty of constructing time-varying measures of state policy liberalism. As a consequence, all existing analyses of the determinants of state policy liberalism employ cross-sectional designs inimical to credible causal inferences.

Conversely, studies that do exploit within-unit variation or other strong causal designs have focused on the small number of continuous policy variables available annually over many years, such as the average income tax rate, the minimum wage,

4. Some studies (e.g., Sharkansky and Hofferbert 1969; Hopkins and Weber 1976; Sorens, Muedini, and Ruger 2008) uncover additional latent dimensions beyond the main policy liberalism dimension. However, Caughey and Warshaw (Forthcoming) provide evidence that a single dimension describes the vast majority of systematic policy variation across states.

5. A measure of policy liberalism based solely on, say, income tax rates would not cover the full universe of content entailed by the concept it measures and would thus lack "content validity" (Adcock and Collier 2001, 537).

or the number of state employees (see, e.g., Leigh 2008). One obvious downside of focusing solely on continuous policies like taxes and expenditures is that categorical policies—such as the abortion restrictions enacted by North Carolina Republicans and the employment discrimination ban proposed by Ohio Democrats—are ignored. Another downside of relying on a few noisy policy indicators is a substantial loss of statistical power. The combination of multiple outcome variables and low statistical power can easily lead to inferential errors about effect magnitudes because only a few unusually large point estimates will pop out as significant (Gelman, Hill, and Yajima 2012). It is thus unsurprising that these studies have typically found significant (sometimes large) partisan effects on a few policies but null results for many others.[6] In studies like ours, where we are interested in comparing the magnitude of partisan effects across time, such concerns about statistical power are particularly salient.

In short, achieving both precise measurement and temporal variation requires a time-varying composite measure of policy liberalism. To achieve these dual goals we turn to the measurement strategy recently developed by Caughey and Warshaw (Forthcoming), who use a dataset of nearly 150 policies to estimate a policy liberalism score for each state in each year between 1936 and 2014.[7] The policy liberalism scores are estimated using a dynamic Bayesian factor-analytic model for mixed data, which allows the inclusion of both continuous and ordinal indicators of state policy (over 80% of the variables in the policy dataset are ordinal, mainly dichotomous).[8]

The policy dataset underlying the policy liberalism scores is designed to include all politically salient state policy outputs on which comparable data are available

6. Similar patterns of results have been obtained by studies of city policies (e.g., Ferreira and Gyourko 2009; Gerber and Hopkins 2011).

7. To our knowledge, the only other holistic yearly summary of state policies is that of Jacoby and Schneider (2009), which measures the particularism of state budget allocations rather than state policy liberalism.

8. The model, which extends that of Quinn (2004), is dynamic in that policy liberalism is estimated separately in each year and the policy-specific intercepts (or "difficulties") are allowed to drift over time. If, instead, the intercepts are held constant, the policies of all states are estimated to have become substantially more liberal, especially before the 1980s. Each policy's factor loading (or "discrimination"), which captures how "ideological" the policy is, is held constant over time.

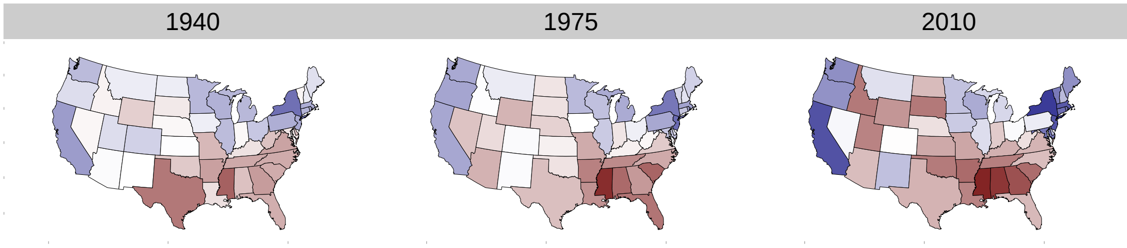


Figure 1: The geographic distribution of government policy liberalism in 1940, 1975, and 2010. Blue indicates liberalism; red, conservatism. The estimates have been centered and standardized in each year to accentuate the color contrasts.

for at least five years.[9] It covers a wide range of policy areas, including social welfare (e.g., AFDC/TANF benefit levels), taxation (e.g., income tax rates), labor (e.g., right-to-work), civil rights (e.g., fair housing laws), women's rights (e.g., jury servise for women), morals legislation (e.g., anti-sodomy laws), family planning (e.g., ban on partial birth abortion), the environment (e.g., state endangered species acts), religion (e.g., public schools allowed to post Ten Commandments), criminal justice (e.g., death penalty), and drugs (e.g., marijuana decriminalizaton). Despite the diversity of policies, there is little evidence that policy variation across states is multidimensional, and the global measure correlates highly with domain-specific indices of policy liberalism. Data on at least 43 different policies are available in every year, enough to estimate policy liberalism quite precisely.[10]

Figure 1 plots the geographic distribution of state policy liberalism in 1940, 1975, and 2010. As the figure indicates, the relative position of most states has remained quite stable across the years we consider. Throughout the period, Southern states had the most conservative policies. This holds not only on civil rights, but on taxes, welfare, and social issues. By contrast, the most liberal states have consistently been

9. Unlike many studies, the dataset explicitly excludes social outcomes (e.g., incarceration or infant-mortality rates) as well as more fundamental government institutions (e.g., legislative term limits).

10. For further details on the policy liberalism measure, see Sections A.2–A.3 of the Supplementary Appendix and Caughey and Warshaw (Forthcoming).

Table 1: Illustrative Policies of Selected States, 1950 and 2010

	Policy Lib'ism	Pct. Lib.	Women on Juries	Labor Anti-Injunction	Housing Aid	Fair Empl. Commiss.	AFDC Benefit
Year = 1950							
MS	-1.35	28%	No	No	No	No	\$460
DE	-0.94	30%	Yes	No	No	No	\$642
MT	0.05	44%	Yes	Yes	No	No	\$838
WI	0.93	56%	Yes	Yes	Yes	No	\$1028
MA	1.33	62%	Yes	Yes	Yes	Yes	\$1036

	Policy Lib'ism	Pct. Lib.	Corporal Punish. Ban	Prevailing Wage Law	Medicaid Abortion	Greenhouse Gas Cap	TANF Benefit
Year = 2010							
MS	-2.29	17%	No	No	No	No	\$253
VA	-0.89	33%	Yes	No	No	No	\$262
NV	-0.13	45%	Yes	Yes	No	No	\$304
MN	1.13	66%	Yes	Yes	Yes	No	\$323
MA	2.02	77%	Yes	Yes	Yes	Yes	\$352

in the Northeast, Pacific, and (to a lesser extent) Great Lakes regions. Nevertheless, the policy liberalism of some states have evolved considerably across time. Vermont and Idaho, for example, were once both moderate in terms of policy liberalism. Since the 1970s, however, the two states have diverged, to the point where Vermont is now one of the most liberal states and Idaho among the most conservative.

Table 1 provides a sense of how policy liberalism corresponds to substantive differences across states in 1950 and 2010. Mississippi and Massachusetts, which bookend the policy liberalism scale throughout the period, are included for both years; the other three states in each year were chosen because their policy liberalism differ from each other by about one standard deviation (in a typical year the cross-sectional SD is around 0.9).[11] The second column indicates the percentage of dichotomous policies on which the state had the liberal option.[12] (On average, a one-unit difference in policy liberalism increases a state's percentage of liberal policies by 14 points.) The next four columns provide examples of highly discriminating dichotomous policies of

11. The policy liberalism scores have zero-mean and unit-variance across state-years.
12. There are 41 dichotomous policies available in 1950 and 45 in 2010.

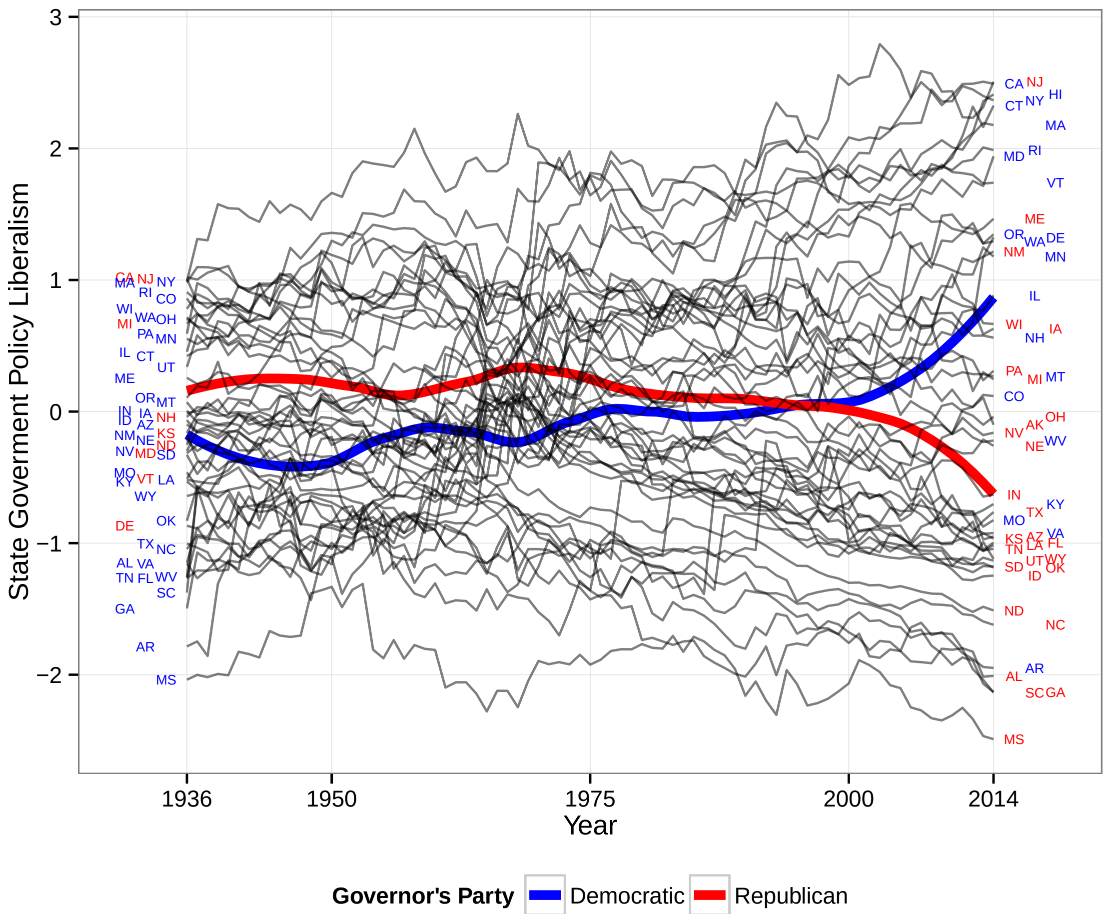


Figure 2: Yearly state policy liberalism, 1936–2014. Blue and red loess lines indicate the average policy liberalism of states with, respectively, Democratic and Republican governors.

varying "difficulty," and the final column provides an example of a continuous policy, average AFDC/TANF benefits per recipient family.[13]

Figure 2 plots the policy liberalism time series of every state between 1936 and 2014, with blue and red loess lines for states with Democratic and Republican governors, respectively. Strikingly, until the end of the 20th century states with Democratic governors actually had more conservative policies than Republican-controlled states (the patterns for state legislatures are similar). The figure thus confirms the classic

13. The welfare benefits are expressed in 2012 dollars and are adjusted for cost-of-living differences among states.

finding of a weakly negative relationship between state policy liberalism and Democratic control. Since 2000, however, party control has become aligned with state politics, and the gap in policy liberalism between Democratic- and Republican-controlled states has rapidly widened. This pattern is only partially driven by the realignment of the South; even in the non-South, Republican states were at least as liberal as Democratic ones until the late 1990s. Whether this increasing correlation is causal—and not simply the result of a better match between ideology and partisanship—is the subject of the empirical analyses in the following two sections.

3 Party Effects on Policy Liberalism

Evaluating policy divergence between the parties requires isolating the policy effects of partisan composition from other determinants of state policy; otherwise, partisan effect estimates will be biased. The public's ideological mood, for example, may affect policy not only through partisan turnover but also through the anticipatory responsiveness of incumbents (Stimson, MacKuen, and Erikson 1995), introducing spurious correlation into naive estimates of partisan effects. We use two alternative identification strategies to isolate the effects of partisan composition. The first, the RD design, exploits the exogenous variation in party control induced by narrowly decided state legislative and gubernatorial elections. Intuitively, extremely close elections may be thought of as coin flips that randomly install one party's candidate into office, independent of all other policy determinants. Our second identification strategy, dynamic panel analysis, exploits over-time variation within states, controlling for national trends and states' recent history of policy liberalism. We use the RD design to establish our basic findings and then follow up with dynamic panel analysis, whose greater statistical efficiency allows us to examine these findings with greater nuance and precision.

3.1 Regression-Discontinuity Analysis

Electoral RD designs exploit the fact that a sharp electoral threshold, 50% of the two-party vote share, determines which party controls a given office (D. S. Lee 2008; Pettersson-Lidbom 2008). The validity of the RD design hinges on the assumption that only the winning candidate—and not the distribution of units' potential outcomes—changes discontinuously at the threshold. Unlike U.S. House elections, where incumbents appear to have an advantage in very close elections (Caughey and Sekhon 2011), our analysis of state legislative and gubernatorial elections uncovers no statistically significant pre-treatment discontinuities. Following Calonico, Cattaneo, and Titiunik (2014b), we estimate both pre- and post-treatment discontinuities with local linear regression, using a bandwidth chosen to minimize mean-square-error (MSE) and adjusting confidence intervals to account for bias in the local-linear estimator. While other studies have used RD designs to estimate the effects of Democratic governors on various policy outcomes (Leigh 2008; Fredriksson, Wang, and Warren 2013), we believe we are the first to apply the design to estimate the policy effects of Democratic state legislatures.

3.1.1 RD for Governor

Consistent with Folke and Snyder (2012) and Eggers et al. (2015), we find no significant discontinuities in the partisan composition of the state government at the time of the gubernatorial election (Supplementary Appendix, Table A3). The only worrisome covariate is contemporaneous *Policy Liberalism*, which is somewhat higher where the Democrat barely won. The difference is nearly significant when the variable is residualized within state and year, but the imbalance disappears when *Policy Liberalism* is converted to a first difference.[14] In light of the better balance on first-differenced

14. The imbalance also disappears if we residualize *Policy Liberalism* using a regression with lagged dependent variables. Lee and Lemieux (2010, 331–3) suggest residualizing or differencing the dependent variable in RD designs as a way to increase statistical efficiency.

Policy Liberalism as well as for increased statistical efficiency, we estimate treatment effects on changes in policy liberalism rather than on levels.

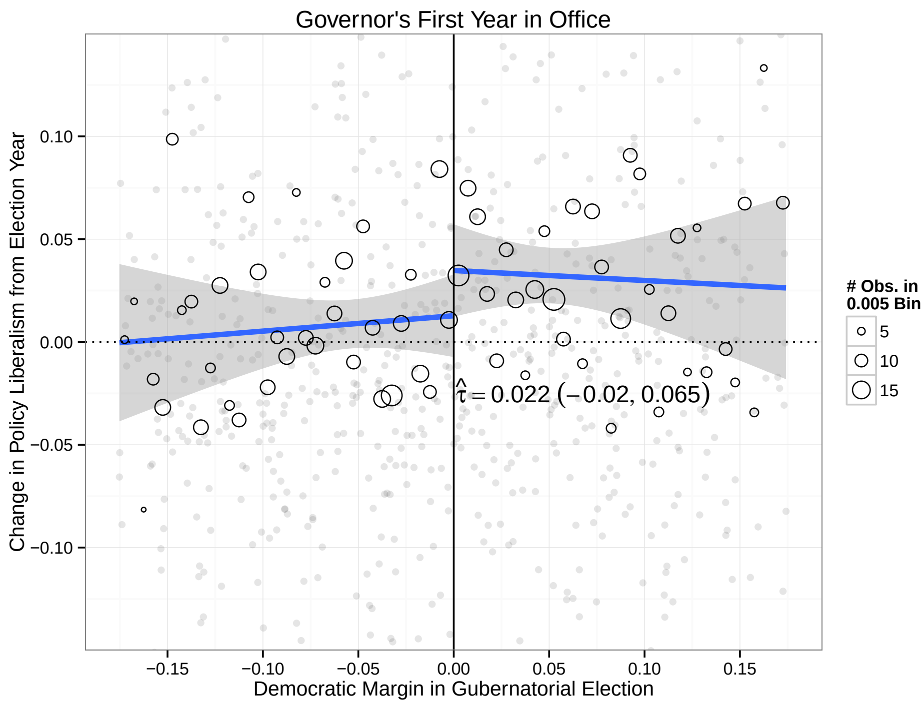
Figure 3 illustrates the estimation of the policy effects of Democratic governors using the electoral RD design. In the top panel, the dependent variable is change in policy liberalism between the year of the governor's election and the governor's first year in office (i.e., the year after the election). The bottom panel presents the same estimate for the governor's second year in office. The point estimates are based on triangular-kernel local linear regression in an MSE-optimal bandwidth, and the confidence intervals have been recentered and expanded to account for the leading term of the bias in the local-linear estimator (Calonico, Cattaneo, and Titiunik 2014a, 2014b).[15]

As the top panel shows, the RD estimate for governors' first year in office is small ($\hat{\tau}_1 = 0.022$) and indistinguishable from zero. By the second year, the point estimate is twice as large ($\hat{\tau}_2 = 0.046$) and the robust confidence interval just barely covers zero. Relative to the variation in policy liberalism across states, these effect estimates are quite small. Even the largest plausible average effect, which the confidence interval suggests is around 0.07 per year, is less than one-tenth the cross-sectional standard deviation of *Policy Liberalism*.[16] Substantively, a 0.07 increase in policy liberalism implies a one-point increase in a state's percentage of liberal policies.

These local average treatment effect (LATE) estimates, however, conceal important heterogeneity in the treatment effects. Consistent with the cross-sectional patterns in Figure 2, the policy consequences of electing a Democratic governor have grown markedly, especially in recent decades. As Figure 4 shows, before the 1990s

15. Clustering the standard errors by state generally yields slightly tighter confidence intervals.

16. The point estimates are larger if *Policy Liberalism* itself is the dependent variable, but they are statistically significant only if *Policy Liberalism* is residualized using two-way fixed-effects ($\hat{\tau}_1 = 0.11$, $\hat{\tau}_2 = 0.14$). Adding lagged dependent variables to the residualizing regression yields point estimates very close to the estimates for change in policy liberalism but a little more precisely estimated. Given this fact and the pretreatment differences in lagged policy liberalism reported in Table A3, we have the most confidence in the estimates with change in policy liberalism as the dependent variable.



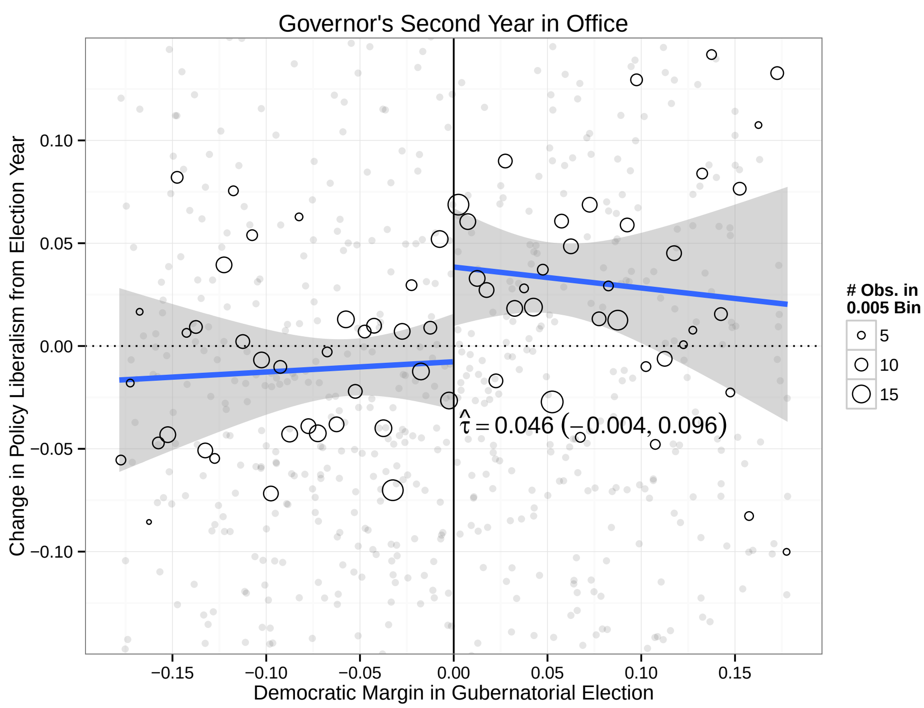


Figure 3: RD estimate of the effect of electing a Democratic governor on change in policy liberalism after the governor's first (top) and second (bottom) years in office. Estimates are based on local linear regression, with MSE-optimal bandwidths and robust confidence intervals calculated by `rdrobust`. Hollow circles are means in 0.5% bins. Shaded 95% confidence intervals are based on conventional standard errors.

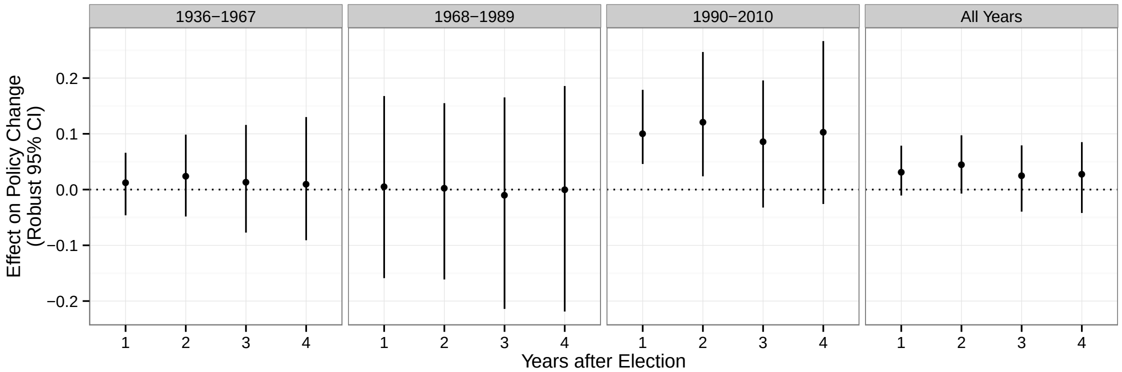


Figure 4: Growth in gubernatorial policy effects over time. Each panel reports the RD estimate of the effect of electing a Democratic governor on change in policy liberalism, one through four years after the election. The left three panels report results separately for different ranges of elections years.

electing Democratic governors did little to change policy liberalism: the RD estimates are small and statistically indistinguishable from 0. Only for governors elected since 1990 are the estimated effects clearly positive (in the first two years). There is little indication that the policy effects cumulate over time. Rather, the full policy effect seems to be accomplished by the governor's second year in office.[17]

3.1.2 RD for State House

Descriptively, the cross-sectional relationship between policy liberalism and Democratic control of the state house and senate looks very similar to the relationship Figure 2 shows for governor: negative until around 1975, then non-existent until the end of the 20th century, when a strong positive association quickly emerged. However, this growing association in recent years could be due to an increase in the effect of public opinion or other changes in the political environment. Therefore, as we did for governors, we apply an RD design to estimate the causal effects of barely electing a Democratic majority in the state house (the lower chamber of the state legislature).[18]

17. Note that some governors have two-year terms and others have four-year terms.

18. We do not examine the state senate because typically only a portion of seats are up for election in a given year.

Because majority control of the legislature is a function of many elections rather than just one, however, we must construct a more complex assignment variable than in the gubernatorial RD.

The specific approach we follow is the multidimensional RD (MRD) design described by Feigenbaum, Fouirnaies, and Hall (2015), which combines information from multiple close legislative elections.[19] The assignment variable they suggest is the Euclidean distance between a vector of district-level electoral results and the electoral results required for majority status. The first step in constructing this variable is to determine the number of seats (m) short of majority status the minority party is after a given election.[20] Then, obtain the Euclidean distance from majority status by summing the squares of the margins in the minority party's m closest losses in that election. Multiply this measure by -1 if the Democrats are in the minority. For example, if the Democrats are $m = 2$ seats short of a majority and the margins in their two closest losses are respectively 3% and 4%, then the value of the assignment variable is $-1 \times \sqrt{3^2 + 4^2} = -5$.

Using data from Klarner et al. (2013), we are able to implement the multidimensional RD design for state house elections between 1968 and 2012.[21] None of the covariates exhibit statistically significant discontinuities, though the estimates are somewhat less precise than in the gubernatorial RD (see Table A4 in the Supplementary Appendix). Figure 5 plots the RD estimates of the policy effects of narrowly elected Democratic house majorities one and two years after the legislative election. For comparability with the analysis of governors, we report the effect on change in

19. For related multidimensional approaches to RD, see Reardon and Robinson (2012), Wong, Steiner, and Cook (2013), and Folke (2014). An alternative design would be to use Democratic seat share as the assignment variable rather than a function of electoral results. We explored this design and found that it yields poor balance on important covariates, suggesting that seat share is too discrete and manipulable to be used as an RD assignment variable.

20. We estimate majority status based on the two-party seat share.

21. Since multi-member house districts cause complications for the design, state-years with multi-member districts are dropped from the analysis. We also drop Nebraska, which has a nonpartisan unicameral legislature.

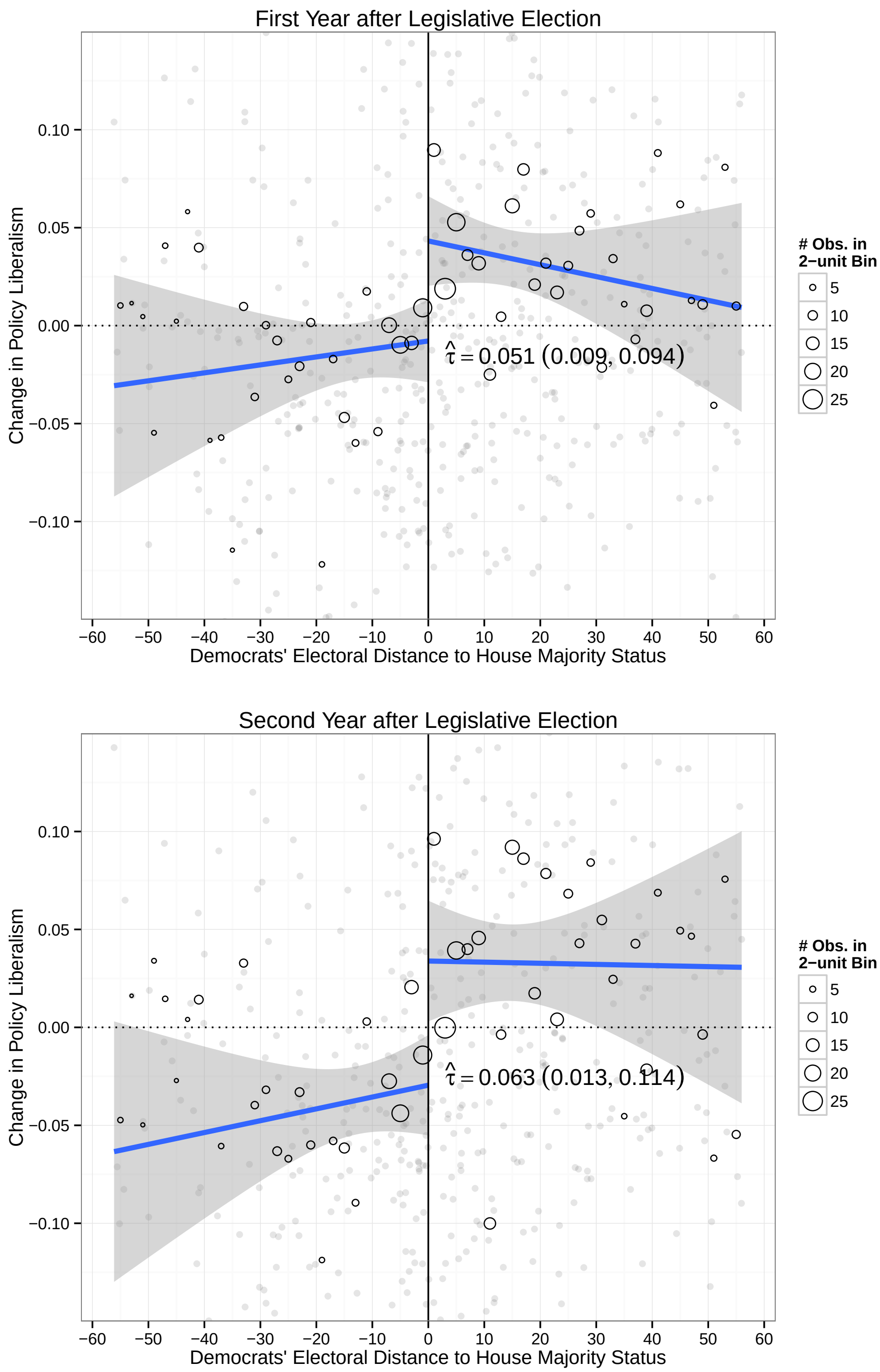


Figure 5: RD estimates of the policy effects of electing a Democratic majority in the state house. The assignment variable (horizontal axis) is the Euclidean distance to electing a Democratic majority, expressed in terms of percentage points. In the top panel the outcome is change in policy liberalism between the election year and one year after the election, and in the bottom panel it is change after two years.

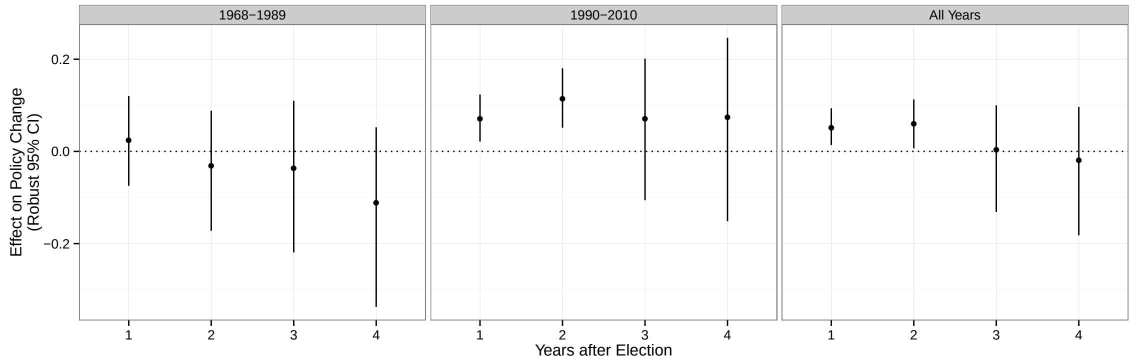


Figure 6: Growth in legislative policy effects over time. Each panel reports the RD estimate of the effect of electing a majority-Democratic legislature on change in policy liberalism, one through four years after the election. The left two panels report results separately for different ranges of elections years.

policy liberalism relative to the election year, but the results are similar for alternatives such as FE-residualized policy liberalism. The estimates are about the same magnitude as those for governor. Moreover, as for governor, only since 1990 has narrowly electing a Democratic house majority caused an increase in policy liberalism (Figure 6).

3.2 Dynamic Panel Analysis

Given its transparent and testable identifying assumptions, the RD design is an appealing mode of causal inference, but its emphasis on observations near the RD threshold restricts the effective sample size. Thus to increase statistical power we complement and extend the RD analysis reported above with an analysis that exploits within-state partisan variation in the full panel of state-years.

The crucial identifying assumption in the panel analysis is that the statistical model characterizes the counterfactual outcome each state would have exhibited under a different treatment assignment (i.e., a governor of the opposite party).[22] If unobserved confounding across states were constant across time and year-specific

22. See Section A.5 of the Supplementary Appendix for details.

shocks affected all states equally, then the effect of a Democratic governor would be identified under a two-way fixed-effect (FE) model,

$$y_{it} = \alpha_i + \xi_t + \delta Gov_{it} + \epsilon_{it}, \tag{1}$$

where Gov_{it} indicates a Democratic governor and α_i and ξ_t are, respectively, state- and year-specific intercepts. The model in (1), which is used by Besley and Case (2003) and others, assumes that the timing of shifts in party control is uncorrelated with time-varying state-specific determinants of policy liberalism (Angrist and Pischke 2009, 243–4). Unfortunately, given that ideological trends in state politics are likely to affect both partisan fortunes and policy outcomes, this assumption is unlikely to hold in this application.

We explored a variety of strategies to account for time-varying confounding, including state-specific time trends and a latent factor approach to interactive fixed effects (e.g., Bai 2009; Gaibulloev, Sandler, and Sul 2014; Xu 2015).[23] All diagnostic criteria indicate, however, that linear, quadratic, or even cubic time trends do not account for the dynamics of policy liberalism as well as lagged dependent variables (LDVs) do, and that latent factors are not necessary once LDVs are included. We therefore estimate dynamic panel models of the following form:

$$y_{it} = \alpha_i + \xi_t + \sum_{l=1}^{L} \rho_l y_{i,t-l} + \delta Gov_{it} + \epsilon_{it}, \tag{2}$$

where $y_{i,t-l}$ is state i's policy liberalism l years before t and ρ_l is the coefficient on the l-th lag. Although the FE-LDV estimator of δ in (2) is biased (Nickell 1981), it performs well in practice as long as the number of time periods T is large and greater than the number of cross-sectional units N (Beck and Katz 2011; Gaibulloev, Sandler, and Sul 2014), both of which are true in our case. Non-stationarity is also

23. See Section A.7 of the Supplementary Appendix for details.

Table 2: The Effect of a Democratic Governor on Policy Liberalism

Outcome variable	Policy liberalism				
	(1)	(2)	(3)	(4)	(5)
Democratic governor	**0.118**	**0.019**	**0.018**	**0.017**	**0.017**
	(0.038)	(0.003)	(0.004)	(0.004)	(0.004)
Policy liberalism (*t-1*)		**0.961**	**0.871**	**0.876**	**0.876**
		(0.006)	(0.017)	(0.016)	(0.016)
Policy liberalism (*t-2*)			**0.096**	**0.083**	**0.081**
			(0.017)	(0.023)	(0.023)
Policy liberalism (*t-3*)				0.008	-0.021
				(0.019)	(0.025)
Policy liberalism (*t-4*)					0.033
					(0.019)
State and year fixed effects	x	x	x	x	x
Observations	3,903	3,902	3,852	3,802	3,802
States	50	50	50	50	50
R-squared	0.851	0.986	0.987	0.987	0.987

Note: Robust standard errors clustered at the state level are in the parentheses. Coefficients statistically significant at the 5% level are in bold font type.

not a problem in our application, and all of the time-series–cross-sectional results reported in this paper are qualitatively robust to alternative estimation strategies (see Section A.6 of the Supplementary Appendix for details).

We introduce the dynamic panel analysis by examining party control of the governorship alone, after which we add the state house and senate to the model as well. Table 2 reports the gubernatorial estimates based on two-way FE models with varying numbers of lags, with all standard errors (SEs) clustered at the state level.[24] As the first column indicates, an FE specification with no LDVs estimates that Democratic governors increase state policy liberalism by 0.118 (CI = $[0.044, 0.192]$). Once LDVs are added, however, the estimate shrinks by an order of magnitude, suggesting that FEs alone do not adequately account for within-state trends in policy liberalism (see Section A.7 of the Supplementary Appendix for further evidence on this point).

24. Using heteroskedasticity- and autocorrelation-robust standard errors (Beck and Katz 1995) or bootstrapping standard errors (blocked at the state level) both yield similar results to clustering.

Columns (2)–(5) of Table 2 report the results from FE-LDV specifications with first- through fourth-order lags. The coefficient on the first-order lag in column (2) is 0.96 (CI = $[0.95, 0.97]$), indicating that the measure of policy liberalism follows a strong autoregressive process but does not have a unit root. Since only the first- and second-order lag coefficients are statistically significant, in the rest of the paper we drop higher-order lags in order to avoid over-fitting.[25] The effect estimate in column (2), $\hat{\delta} = 0.019$ (CI = $[0.013, 0.025]$), is much smaller than the FE estimate and remains extremely stable as more lags are added. This estimate, which represents the immediate policy effect of a Democratic governor, is remarkably close to the RD point estimate for the first year after the governor's election ($\hat{\tau}_1 = 0.022$). Moreover, the implied effect of the governor's first two years in office, $\hat{\delta} \times \hat{\rho}_1 + \hat{\delta} = 0.019 \times 0.961 + 0.019 = 0.037$, is quite close to the RD estimate for policy change two years after the election ($\hat{\tau}_2 = 0.046$). The similarity between the RD and dynamic panel estimates provides additional reassurance that the LDVs properly account for time-varying state-specific confounding.

Having introduced and justified our dynamic panel model, we now add the state legislature to the analysis. It is important to note that the dynamic panel analysis requires that the effect of a Democratic legislative majority be interpreted differently than in the RD analysis. In the RD design, the estimand is the LATE of electing a bare Democratic majority rather than a bare Republican majority. In the dynamic panel analysis, however, the estimand conflates the effect of chamber control *per se* with that of seat share, which is typically greater than a bare majority. Our initial analysis ignores this conflation, but in the next section we attempt to disentangle the effects of majority control and seat share.

Our baseline specification is the same FE model with second-order LDVs as in equation (2). The first column of Table 3 reports results of this model plus indicators

25. Including more lagged terms in the model does not qualitatively change our main results.

Table 3: Policy Effects of Democratic Control the Governorship, State House, and State Senate

Outcome variable	Policy liberalism			
	(1)	(2)	(3)	(4)
Democratic house majority	**0.031**	**0.029**	**0.022**	**0.043**
	(0.006)	(0.006)	(0.008)	(0.014)
Democratic senate majority	**0.022**	**0.021**	0.011	0.005
	(0.006)	(0.006)	(0.010)	(0.013)
Democratic control of both chambers			0.018	0.001
			(0.011)	(0.018)
Democratic governor		**0.012**	**0.013**	**0.016**
		(0.004)	(0.004)	(0.007)
Democratic governor * house majority				-0.037
				(0.017)
Democratic governor * senate majority				0.011
				(0.016)
Democratic governor * both chambers				0.027
				(0.022)
Two lagged terms of the outcome variable	x	x	x	x
State and year fixed effects	x	x	x	x
Observations	3,630	3,630	3,630	3,630
States	49	49	49	49
R-squared	0.987	0.987	0.987	0.987

Note: Robust standard errors clustered at the state level are in the parentheses. The state of Nebraska is dropped out of the sample. Coefficients statistically significant at the 5% level are in bold font type.

for Democratic house and senate majorities. The estimated immediate effect of a Democratic house majority is 0.03, statistically indistinguishable from the analogous RD estimate in the top panel of Figure 5, and the effect estimate for senate majority is only slightly smaller. Both are precisely estimated and are unaffected when an indicator for Democratic governor is added to the model (column 2). The estimate for governor, 0.01, is itself only slightly smaller than the estimates when governor is examined in isolation (see Figure 4 and Table 2).

One intuitive expectation is that the advantage of party control of one branch of state governor increases when the party controls other branches as well. As the third and fourth columns indicate, however, there is little evidence of positive interactions

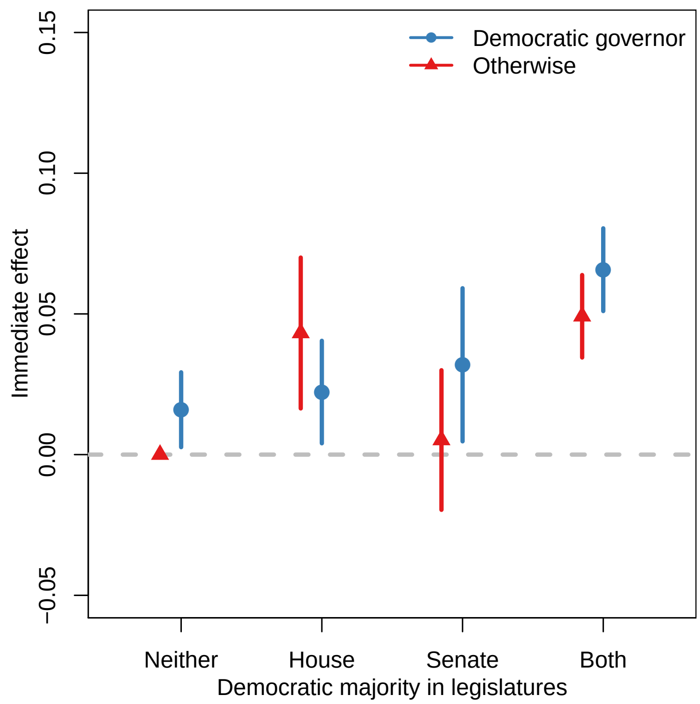


Figure 7: Predicted policy effects of different configurations of Democratic control, relative to the baseline of unified Republican control (red triangle).

between variables indicating control of different parts of the government. In fact, the only significant interaction, between governor and house, is negative, suggesting that the marginal impact of each variable diminishes if Democrats hold both offices (but not the senate). Other than this interaction, the rest of the results suggest that the effects of each office do not depend on which party controls the other offices, with the caveat that there appear to be minimal policy consequences of Democratic control of the senate only (see Figure 7). Due to the high level of collinearity between the independent variables, especially house and senate majority status, the results for the interacted specifications should be interpreted cautiously. But taking them at face value, Democratic control of all three government institutions is predicted to increase policy liberalism by 0.07 relative to unified Republican control (rightmost point in Figure 7).

The greater statistical efficiency of the dynamic panel model allows us to estimate heterogeneity over time with more precision than the RD design permits. To do so,

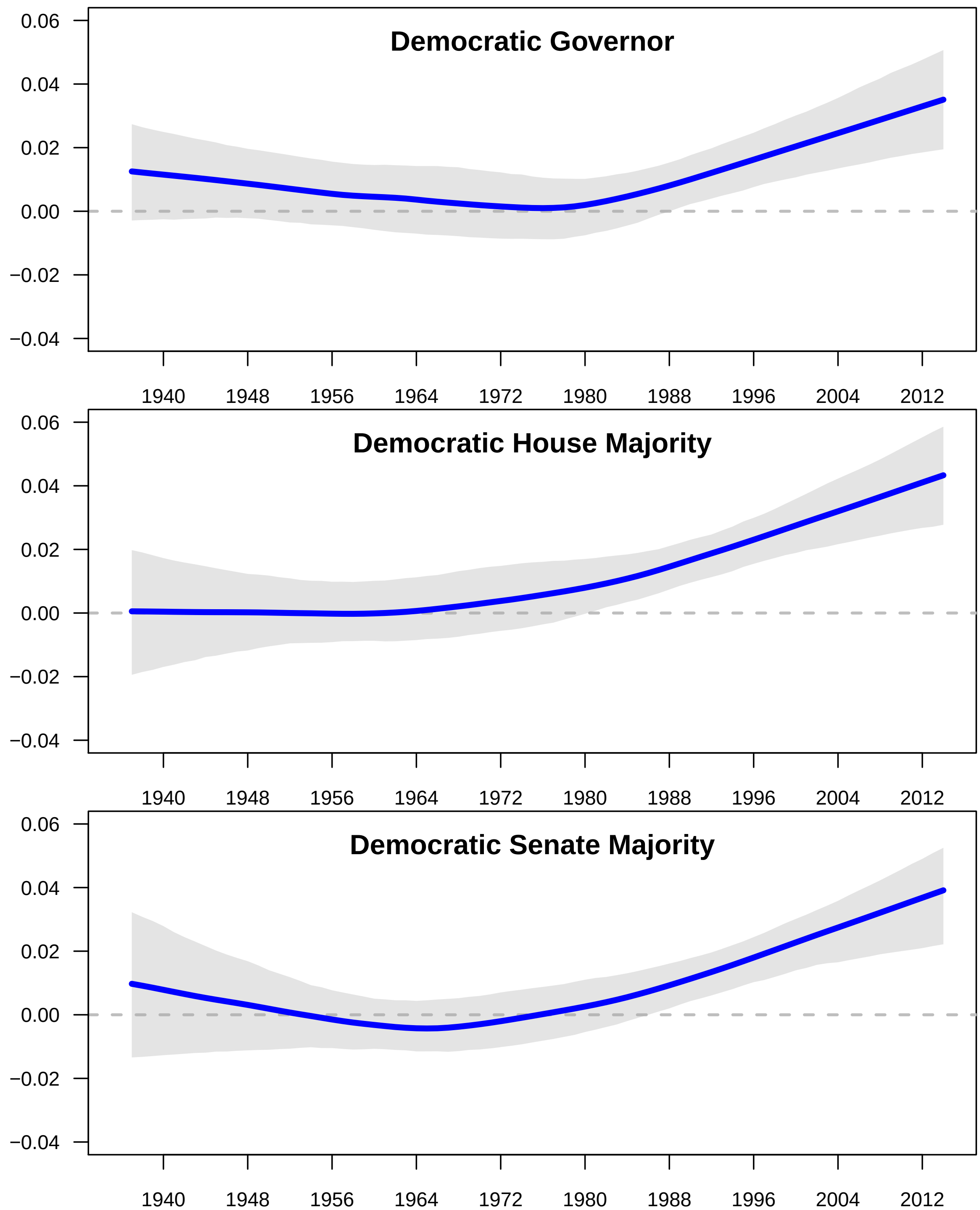


Figure 8: Evolution of the policy effects of Democratic control of the governorship (top), state house (middle), and state senate (bottom).

we estimate a modified version of the model in (2) that allows δ to vary smoothly as a function of time.[26] As Figure 8 shows, the effect of Democratic control has evolved in parallel across the three institutions. Consistent with the era-specific RD estimates in Figures 4 and 6, the dynamic panel analysis indicates that the policy effects of Democratic control of the governorship and state legislature were small and statistically insignificant through the 1970s. These findings are consistent with the null findings in the classic studies conducted using data from this time period (e.g., Hofferbert 1966; Erikson, Wright, and McIver 1993). In the 1980s, however, the effects of Democratic control took off and continued to increase through the end of the period. These findings too are consistent with the larger effect sizes in state politics studies that focus on the impact of party control in recent years (e.g., Kousser 2002). By the second decade of the 21st century, the estimates for three institutions were all around 0.04—larger than ever before, though still about one-twentieth the size of the standard deviation across states.[27]

The results reported above are unchanged if Southern states are excluded from the analysis (see Section A.9 of the Supplementary Appendix). This makes sense because both the RD and dynamic panel analyses implicitly place greater weight on

26. Specifically, we estimate models of the following form:

$$y_{it} = \alpha_i + \xi_t + \rho_1 y_{i,t-1} + \rho_2 y_{i,t-2} + k(t) \cdot Gov_{it} + Maj^H_{it} + Maj^S_{it} + \epsilon_{it}$$

where $k(\cdot)$ is a function of time t. We estimate $k(\cdot)$ using local linear regressions with default bandwidths (span = 0.75) using the `loess` package in R that control for house and senate majority statuses as well as past outcomes and fixed effect.The uncertainty estimates are obtained via block bootstrapping of 1,000 times to account for potential serial dependence in the error structure.

27. Two caveats are worth mentioning when interpreting this finding. First, there is less variation in partisan composition in the early period, especially in the southern states, which can result in imprecise estimates of the partisan effects (as suggested by the wider confidence interval on left end of each panel). Section A.8 of the Supplementary Appendix contains more details on the variation in our key independent and dependent variables in each time period. Second, when estimating $\hat{k}(.)$, we assume that the effects of senate and house majority statuses are constant over-time for the sake of simplicity. This could lead to over-estimating the upward trend since the presence of a Democratic governor is highly correlated with house and senate Democratic majority and the effects of the Democratic majority statuses may also be increasing over-time. We address this concern by incorporating the interaction terms between Maj^{H}_{it} and Maj^{S}_{it} and a full set year dummies in Equation (3), the pattern is the same though the slopes are slightly less steep. By doing this, we risk incurring post-treatment bias because partisan governors and majority statuses affect each other.

competitive states (those with closer elections and more alternation in party control) and until recently state politics in the South was dominated by the Democratic party.[28] Only in recent decades do Southern states contibute substantially to the partisan effect estimates, and during these years there appear to be minimal regional differences in partisan effects on policy.

3.3 Disentangling Seat Share and Majority Status

The models reported in the previous section do not identify the effect of Democratic majority status *per se*. In particular, it is possible that the differences between majority-Democratic and majority-Republican legislative chambers are due only to differences in the preferences of pivotal voters (Krehbiel 1998) and not to the agenda-setting or other powers of the majority party (Aldrich and Rohde 2000; Cox and McCubbins 2005). Our data do not allow us to cleanly distinguish between preference-based and party-procedural accounts. However, under the assumptions that Democratic seat share is a good proxy for the liberalism of pivotal voters and that status quos are fairly widely distributed, Krehbiel's preference-based account implies that Democratic seat share should directly increase policy liberalism. If the parties are ideologically polarized the share–policy relationship will probably be steepest when the party division is close, but it should be positive throughout the range of seat share. Party-based accounts do not rule out the independent influence of preferences, but they suggest that the effect of majority status itself should dominate that of seat share.

With these theoretical expectations in mind, consider the models summarized in Table 4, which include measures of Democratic house and senate seat shares (recentered at 0.5) in addition to the three indicators of partisan control. The coefficient estimates for the party-control variables (top three rows) are almost completely sta-

28. Moreover, in the one-party South Democratic politicians were sometimes as or even more conservative than Republicans in the same state (Key 1949).

Table 4: Disentangling Share and Control

Outcome variable	Policy liberalism			
	(1)	(2)	(3)	(4)
Democratic governor	**0.011**	**0.011**	**0.011**	**0.011**
	(0.004)	(0.004)	(0.004)	(0.004)
Democratic house majority	**0.024**	**0.027**	**0.026**	**0.025**
	(0.008)	(0.006)	(0.008)	(0.008)
Democratic senate majority	**0.019**	0.015	0.016	0.015
	(0.006)	(0.009)	(0.009)	(0.009)
Democratic house seat share	0.026		0.012	0.010
	(0.026)		(0.032)	(0.042)
Democratic senate seat share		0.027	0.018	0.059
		(0.027)	(0.033)	(0.038)
Democratic house seat share * house majority				0.008
				(0.068)
Democratic senate seat share * senate majority				-0.065
				(0.054)
Two lagged terms of the outcome variable	x	x	x	x
State and year fixed effects	x	x	x	x
Observations	3,630	3,630	3,630	3,630
States	49	49	49	49
R-squared	0.987	0.987	0.987	0.987

Note: Robust standard errors clustered at the state level are in the parentheses. The state of Nebraska is dropped out of the sample. Coefficients statistically significant at the 5% level are in bold font type.

ble across specifications. The effect of a Democratic house majority is estimated to be twice as large as that of a Democratic governor, with the senate estimate falling somewhere in between. The linear effect of seat share, however, is always indistinguishable from 0, regardless of whether share is entered separately by chamber or allowed to differ by majority status.

To evaluate the possibility of a non-linear relationship between chamber seat share and policy liberalism, we estimate the following semiparametric model for each chamber $c \in \{\text{house, senate}\}$:

$$y_{it} = f(Share_{c,it} \mid Maj_{c,it} = 0) + f'(Share_{c,it} \mid Maj_{c,it} = 1) \qquad (3)$$

$$+ \rho_1 y_{i,t-1} + \rho_2 y_{i,t-2} + \alpha_i + \xi_t + \delta Gov_{it} + \gamma Maj_{c',it} + \epsilon_{it},$$

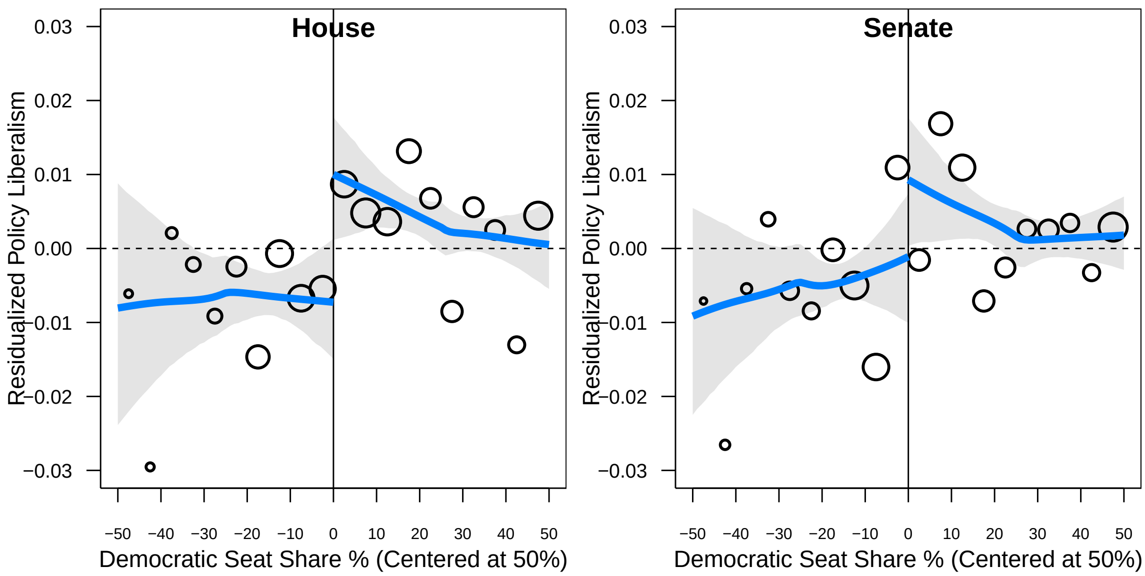


Figure 9: The policy effects of Democratic two-party seat share in the state house (left) and senate (right). The y-axes plot the residuals from regressions of policy liberalism on the parametric components of the model in (3). Blue lines indicate loess fits, and shaded regions conventional 95% confidence intervals.

where $c \neq c'$. The semi-parametric functions $f(\cdot)$ and $f'(\cdot)$ allow policy liberalism to vary non-linearly as a function of Democratic seat share in chamber c. We estimate the model in (3) using a two-step procedure. The first step is to regress y_{it} on the parametric components of the model: the LDVs, the fixed effects, and the indicators for Democratic control of the governorship and of the other legislative chamber (c'). The second step is to estimate the semi-parametric functions by applying local linear regression to the residuals from the first estimation step. Uncertainty estimates are produced using state-level block bootstraps of the entire procedure.

Figure 9 displays the results estimating the semiparametric model in the house (left panel) and senate (right panel). Although the plots in this figure look similar to an RD design, they differ in that under the identification assumptions in the FE-LDV model, the difference between any pair of points has a causal interpretation, not just the gap at the threshold itself. The results for the state house are fairly unambiguous. In line with the house RD results, the policy effect of moving from a narrow Republican house majority to a narrow Democratic one is robust and statistically significant. The relationship between policy liberalism and Democratic seat share, however, is almost completely flat, consistent with the close-to-zero coefficients on house share in Table 4.

The patterns for state senate are less clear. In particular, there is a discrepancy between the loess fits, which imply a significant positive effect of gaining majority control, and the local averages on either side of the threshold, which imply a negative effect. These discrepancies suggest that our conclusions regarding the senate should be interpreted more cautiously than those for the governor and house. Nevertheless, the results for both the senate and the house support two conclusions. First, controlling for year-specific common shocks, partisan control of other government institutions, and each state's long-term mean and recent history, policy liberalism is higher when Democratic Party control a legislative chamber than when the Republicans do. Second, except by giving Democrats majority control of the chamber, there is little affirmative evidence that Democratic seat share increases policy liberalism.

4 Interpretation and Implications

Through the 1970s, electing Democratic rather than Republican governors and legislatures had negligible effects of the liberalism of state policies. Since 1980, however, partisan effects have grown rapidly: electing Democrats now has an unambiguously positive impact on policy liberalism. In other words, the parties have increasingly diverged in the policies they implement in office. What accounts for these changes?

One potential explanation is that partisan politicians have come to place greater weight on policy achievements relative to winning elections *per se*. A key assumption of Downs's original model is that parties formulate solely policies in order to win election, not the other way around. As ideologically motivated "amateurs" have replaced office-oriented professionals as the activist bases of the parties (Wilson 1962), Democratic and Republican office-holders have faced increasing incentives to reward their core partisans with policy achievements rather than the spoils of office.[29] To the extent that politicians are drawn from their party's activist pool, candidates themselves have probably become personally more policy-motivated as well. A greater weight on policy motivations, whether induced or sincere, should increase candidates' divergence from one another.

A related potential explanation is that the candidate pools and support coalitions of each party have become more ideologically homogenous since the 1970s. As national political elites polarized, voters became better able to sort themselves into the ideologically appropriate party (Levendusky 2009). In turn, the growing ideological homogeneity of party identifiers made both parties less hospitable to moderate politicians like Frank Lausche and his Republican contemporaries Thomas Dewey and Nelson Rockefeller.[30] As the party brands have become increasingly informative, even candidates wanting to present themselves as moderate may have found it increasingly difficult to credibly signal their ideological type. The growing nationalization of state elections has similarly reduced the electoral value of appealing to the middle, raising the relative value to candidates' of simply implementing their preferred policies.

It is worth noting that the parties' policy positions (as distinct from the policies they implement) have never fully converged. Even in the 1940s–70s, the high point of candiate responsiveness to local conditions, the positions of U.S. representatives still differed greatly by party (Ansolabehere, Snyder, and Stewart 2001; for similar evidence on state party elites in the 1970s–80s, see Erikson, Wright, and McIver 1989). Thus a final possible explanation for the growth in party effects over time is

29. Of course, patronage—in the form of jobs, contracts, and other forms of highly particularized benefits—is itself a kind of policy, but not one whose variation is well-captured by our measure of policy liberalism.

30. It is worth noting that labor unions and most Democratic leaders opposed Frank Lausche's gubernatorial nomination in 1948, but Lausche was popular with a Ohio Democratic primary electorate that contained a large number of moderates and conservatives.

that parties have become better able to use control of state government to translate their policy positions into actual policy outcomes. One mechanism by which this may have occurred is suggested by Aldrich and Rohde's (2000) Conditional Party Government theory. By this logic, the policy effects of party control of the legislature have grown because the parties' increasing homogeneity has led them to delegate greater formal power to the majority leadership, further skewing legislative outcomes towards the majority median and away from the chamber median. This would be consistent with the fact that the effects of majority control have increased, while the effects of legislative seat share appear to have remained stable.[31]

Regardless of the explanation for the parties' growing divergence, the substantive magnitude of partisan effects on policy should not be overstated. In 2010, for example, Democratic governors, houses, and senates are each estimated to increase policy liberalism by around 0.04 per year (see Figure 8). As Table 1 suggests, an effect of this size would be expected to increase a state's percentage of liberal policies by a small amount, on the order of 0.5%. Or, to take an important welfare policy, it would increase average monthly TANF benefits per recipient family by a little over \$1.[32]

We can also compare the party effects with three alternative benchmarks:

1. The cross-state variation in a typical year ($\hat{\sigma}_{\text{xs}} \approx 0.9$)

2. The within-state variation across years ($\hat{\sigma}_{\text{ws}} \approx 0.3$)

3. The variation of the residuals from the FE-LDV model ($\hat{\sigma}_{\text{res}} \approx 0.1$).

The first comparison indicates that the estimated effect of a switch in unified party control is one-twentieth the size of the typical difference between states. It would

31. There is unfortunately little evidence on trends in majority-party power over time. The best available dataset, Mooney's (2013) data on state house speakers' formal powers 1981–2010, reveals "little in the way of national trend" over time, but these data do not extend far enough back in time to speak directly to our question.

32. Calculated based on the linear association between policy liberalism and TANF benefits in 2010.

thus require many decades of Republican governors and legislatures to make the policies of Massachusetts, whose policy liberalism score is around +2, as conservative as those of Mississippi, whose score is around −2.[33] Party effects loom larger when compared to within-state variation, yet they still are an order of magnitude smaller than the typical yearly fluctuation in a state's policy liberalism. Only relative to the residual variation in policy liberalism, after accounting for national trends and states' long-term mean and recent history, do party effects appear truly substantial. These comparisons suggest that the partisan composition of government accounts for a fairly small portion of variation in policy liberalism, leaving plenty of room for other factors such as shifts in public opinion.

For a final comparison, consider Figure 10, which contrasts party effects on legislative voting records and on policy outputs. The left three quantities are estimates of the effect of electing Democratic governors and legislatures on state policy liberalism (see column 2 of Table 3 above). The right three quantities are analogous counterfactual differences in roll-call ideal points between Republicans and Democrats occupying the same office.[34] For comparability, all estimates are normalized by the cross-sectional standard deviation of the dependent variable. As this figure starkly illustrates, party control has a far larger impact on roll call voting, both in Congress and in state legislatures, than it does on state policy—a difference of nearly two orders

33. This hypothetical comparison glosses over two potentially major complications. The first is that Massachusetts Republicans are less conservative than Mississippi Republicans, so party effects may differ across states (see Erikson, Wright, and McIver 1989, however, for evidence that the within-state divergence of the parties does not vary strongly with state liberalism). The second complication is that the comparison ignores any endogenous political response to changes in policy liberalism. We have both theoretical (e.g., Alesina and Rosenthal 1995) and empirical (e.g., Folke and Snyder 2012) reasons to believe that voters will respond to rightward (leftward) changes in state policy by electing more Democrats (Republicans) to state office. We thank Ben Olken for suggesting this second point.

34. The ideal point measure for the U.S. House and president is DW-NOMINATE (Poole and Rosenthal 2007). The House estimate based on an RD design (estimates based on two-way fixed effects or any other estimator are very similar); the president estimate is simply the raw difference between Democratic and Republican president-years since 1936. The figure for the state house is based on the matching estimate of intra-district partisan divergence in ideal points reported in Table 2 of Shor and McCarty (2011, 548).

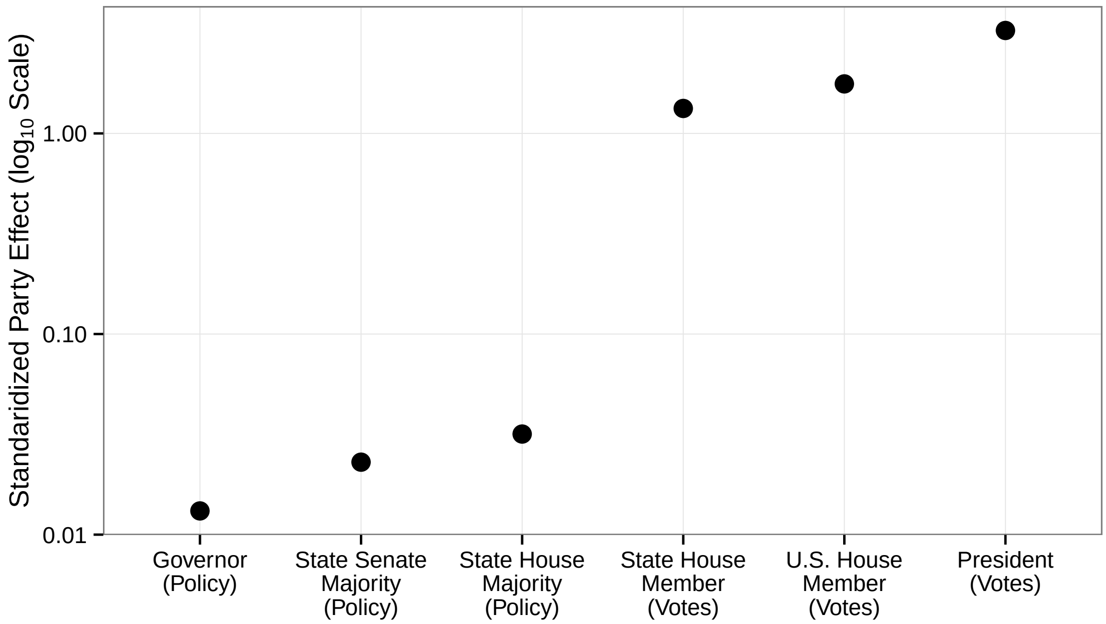


Figure 10: Comparison party effects on policy and on roll-call votes. The left three quantities are estimated effects of party control on state policy liberalism. The right three are analogous counterfactual differences in roll-call ideal points between Republicans and Democrats occupying the same office. For comparability, each of the estimates is standardized by the cross-sectional standard deviation of the dependent variable. The vertical axis is on the $\log_{10}$ scale, so each line represents an effect ten times larger than the line below it.

of magnitude (the y-axis is on the $\log_{10}$ scale).

What accounts for these differences in magnitude? One possibility is that legislators' position-taking, whether in the form of roll calls or campaign platforms, is not a sincere expression of their preferences: legislators may abandon politically advantageous positions when their vote becomes pivotal.[35] In this vein, Frances Lee (2009) argues that Congress members' tendency to oppose the other party's initiatives for tactical reasons exaggerates partisan polarization in roll-call voting. It is also possible that the gap between roll calls and policy is attributable to the difficulty of translating preferences into outcomes. For example, "fiscal federalism" may constrain the policy choices of state officials more tightly than national ones (Oates 1972), and as a result

35. An example would be congressional votes on raising the U.S. debt ceiling.

variation in policy liberalism may be driven primarily by location, financial resources, and other non-political factors. This view has a long pedigree in the state politics literature (e.g., Dye 1966; but see Erikson, Wright, and McIver 1993). One reason to discount it as a full explanation, however, is that non-fiscal items constitute the majority of our policy liberalism scale. Another barrier to policy change is the existence of multiple veto players and other biases towards the status quo. Krehbiel's (1998) theory of pivotal politics, for example, predicts the existence of a gridlock interval between the pivotal voters in different institutions in which no status quo policies can be overturned. Finally, it is possible that one or two years is simply too short a time horizon to detect policy effects; while plausible, this explanation is inconsistent with the evidence that party effects do not cumulate after two years (see Figures 4 and 6).

The caveats above notwithstanding, the contrast between party effects on ideal points and on policy cast a very different light on partisan polarization. In particular, they call into question the concern that alternation in party control leads to wild swings in policy outcomes (e.g., Poole and Rosenthal 1984; Bafumi and Herron 2010; Lax and Phillips 2011). To be clear, we cannot make direct comparisons between citizens and policies since our policy liberalism estimates are not on the same scale as citizens' preferences.[36] Nevertheless, the massive discrepancy between the ideal-point and policy effects cannot but suggest that worries about leapfrog representation—at least in terms of policy outputs—may be overblown.

5 Conclusion

Policy—what governments actually do—is arguably the ultimate metric of representation. Our focus on policy outcomes, as opposed to position-taking, thus offers a useful alternative perspective on political parties' role in American democracy. It

36. Not only are the survey data required to jointly scale citizens and state policies difficult (probably impossible) to obtain, but as Lewis and Tausanovitch (2015) note, such joint scaling requires heroic statistical assumptions that are difficult to justify.

turns out that for much of the 20th century the partisan composition of state governments had little impact on the liberalism of state policies. During these years, state governments' responsiveness to their citizens must therefore have occurred primarily through the mechanism of electoral *incentives* for candidates to anticipate voters' preferences (compare Erikson, Wright, and McIver 1993; Stimson, MacKuen, and Erikson 1995). Since the 1980s, however, as the policies implemented by the parties have increasingly diverged, partisan *selection* has become more important as a mechanism of responsiveness (compare Lee, Moretti, and Butler 2004).

The growing importance of partisan selection raises the concern that policy has become *over*-responsive to citizens' preferences, degrading other measures of representation such as proximity or congruence (Achen 1978; Matsusaka 2001). Studies of individual policies' congruence with state opinion have suggested that over-responsiveness is indeed common (Lax and Phillips 2011). Nevertheless, while our data do not permit us to evaluate over-responsiveness directly, the small magnitude of party effects on state policy at least calls into question the magnitude of over-responsiveness suggested by a focus on legislative position-taking (cf. Poole and Rosenthal 1984; Bafumi and Herron 2010). Democrats and Republicans may disagree consistently and even violently, but the policy consequences of electing one over the other pales in comparison to the policy differences among states. Moreover, given that roll-call voting is no more polarized in Congress than it is in state legislatures, this conclusion likely applies to national as well as state politics.

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A Supplementary Appendix

Appendix: Table of Contents

A.1 Policy Liberalism Data

Policy	Years	Description
Abortion Policies		
Access to Contraceptives	1974-2014	Can pharmacies dispense emergency contraception without a prescription?
Forced Counseling	1973-1991	Does the state mandate counseling before an abortion (pre-*Casey*)?
Forced Counseling	1992-2014	Does the state mandate counseling before an abortion (post-*Casey*)?
Legal Abortion Pre-Roe	1967-1973	Did the state allow abortion before Roe v. Wade?
Parental Notification/Consent Required	1976-1982	Does the state require parental notification or consent prior to a minor obtaining an abortion? (pre-*Akron*)
Parental Notification/Consent Required	1983-2014	Does the state require parental notification or consent prior to a minor obtaining an abortion? (post-*Akron*)
Partial Birth Abortion Ban	1997-2007	Does the state ban late-term or partial birth abortions?
Medicaid for Abortion	1981-2014	Does the state's Medicaid system pay for abortions?
Criminal Justice Policies:		
Age Span Provisions for Statutory Rape	1950-1998	Does a state adopt an age span provision into its statutory rape law which effectively decriminalizes sexual activity between similar-aged teens?
Death Penalty	1936-2014	Has the state abolished the death penalty?
Probation	1936-1939	Has the state established probation?
Drug & Alcohol Policies:		
Beer Keg Registration Requirement	1978-2013	Does the state require registration upon purchase of a beer keg?
Decriminalization of Marijuana Possession	1973-2014	Is marijuana possession a criminal act?
Medical Marijuana	1996-2014	Is it legal to use marijuana for medical purposes?
Minimum Legal Drinking Age 21	1936-1985	Does the state have a minimum legal drinking age of 21?
Smoking Ban - Workplaces	1995-2014	Does the state ban smoking in all workplaces?
Smoking Ban - Restaurants	1995-2014	Does the state ban smoking in restaurants?
Zero Tolerance for Underage Drinking	1983-1995	Does the state have a Zero Tolerance law for blood alcohol levels ¡0.02 for individuals under age 21?
Education Policies:		
Allow Ten Commandments in Schools	1936-2013	Does the state allow the Ten Commandments to be posted in educational institutions?
Ban on Corporal Punishment in Schools	1970-2014	Does the state ban corporal punishment in schools?
Education Spending Per Pupil	1936-2009	What is the per capita spending on public education per pupil based on daily average attendance?
Moment of Silence Required	1957-2014	Does the state have a mandatory moment of silence period at the beginning of each school day?
Per Student Spending on Higher Ed.	1988-2013	What is the per student subsidy for higher education?
Teacher Degree Required - High School	1936-1963	In what year did the state require high school teachers to hold a degree?
Teacher Degree Required - Elementary	1936-1969	In what year did the state require elementary school teachers to hold a degree?
School for Deaf	1936-1950	School for Deaf
State Library System	1980-1948	State Library System
Environmental Policies:		
Air Pollution Control Acts (Pre-CAA)	1947-1967	Does the state have an air pollution control act (Pre-Clean Air Act)?
Bottle Bill	1970-2014	Does the state require a deposit on bottles paid by the consumer and refunded when the consumer recycles?
CA Car Emissions Standard	2003-2012	Does the state adopt California's Car emissions standards (which are more stringent than the federal level)?
Electronic Waste Recycling Program	2000-2014	Does the state have a recycling program for electronic waste?
Endangered Species Act	1969-2014	Does the state have an endangered species act?
Environmental Protection Act	1969-2014	Does the state have its own version of the federal National Environmental Policy Act?
Greenhouse Gas Cap	2006-2014	Does the state have a binding cap on greenhouse gas emissions in the utility sector?
Public Benefit Fund	1996-2014	Does the state have a public benefit fund for renewable energy and energy efficiency?
Solar Tax Credit	1975-2014	Does the state have a tax credit for residential solar installations?

Description of Policies A1 Continued from previous page

Policy	Years	Description
Gambling Policies:		
Casinos Allowed	1977-2012	Does the state allow casinos?
Lottery Allowed	1964-2014	Does the state have a lottery?
Gay Rights Policies:		
Ban on Disc. Against Gays In Public Accomm.	1989-2014	Does the state ban discrimination against gays by public accomodations?
Civil Unions and Gay Marriage	2000-2012	Does the state allow civil unions or gay marriage (ordinal)?
Employment Disc. Protections for Gays	1982-2014	Does the state forbid employment discrimination on the basis of sexual orientation and/or sexual identity?
Hate Crimes Ban - Gays	1999-2014	Are hate crimes explicity illegal in the state?
Sodomy Ban	1962-2003	Does the state forbid sodomy?
Gun Control Policies:		
Assault Weapon Ban	1989-2014	Are assault weapons banned in the state?
Background check - gun purchases from dealers	1936-1993	Does the state require a background check on gun purchases from dealers?
Background check for private sales	1936-2014	Does the state require a background check on privately-sold guns?
Gun Dealer Licenses	1936-2014	Does the state have any license requirements for manufacturers or dealrs?
Gun Purchases - Waiting Period	1923-2014	Does the state have a waiting period for gun purchases?
Open Carry Law for Guns	1961-2014	Is there an open carry law for guns?
Saturday Night Special	1974-2013	”Does the state ban “Saturday Night Special”” handguns?”
Stand Your Ground	1993-2014	”Does the state have a “stand your ground”” law?”
Gun Registration	1936-2014	Does the state have a registration requirement for guns?
Immigration Policies:		
English as official language	1970-2014	Is English the state’s official language?
In-state Tuition for Immigrants	2001-2014	Does the state allow in-state tuition for illegal immigrants?
Labor Rights Policies:		
Age discrimination ban	1936-1999	Does the state ban age discrimination?
Anti-Injuncion Act	1936-1966	Does the state have an anti-injunction law?
Collective Bargaining - State Employees	1966-1996	Does the state have collective bargaining rights for state government employees?
Collective Bargaining - Teachers	1960-1996	Does the state have collective bargaining rights for local teachers?
Disability Discrimination Ban	1965-1990	Does the state ban discrimination against disabled people?
Merit System for State Employees	1936-1953	Does the state have a merit system for state employees?
Minimum Wage above Federal Level	1968-2012	Is the state’s minimum wage above the federal level?
Minimum Wage for Men	1944-1968	Does the state have a minimum wage for men?
Minimum Wage for Women	1936-1980	Does the state have a minimum wage for women?
Prevailing Wage Law	1936-2014	Does the state have prevailing wage laws?
Right to Work law	1944-2014	Is the state a right-to-work state?
State Pension System Established	1936-1960	Does the state have a pension system?
Temporary Disability Insurance	1945-2014	Does the state have a temporary disability insurance program?
Unemployment Compensation	1937-2014	What is the maximum weekly amount of unemployment benefits?
Workers Compensation	1936-1947	Has the state established workers compensation?
Child Labor (14-15)	1936-1939	Does the state require employment certificates for child labor (14 and 15)?
Labor Relations Act	1937-1966	Does the state have a Labor Relations Act?
Licensing Policies:		
Chiropractor Licensing	1936-1951	Chiropractor Licensing
Dentist Licensing	1936-1951	Dentist Licensing
Architect Licening	1936-1951	Architect Licening
Beautician Licensing	1936-1951	Beautician Licensing
Pharmacist Licensing	1936-1951	Pharmacist Licensing
Engineer Licensing	1936-1951	Engineer Licensing
Nurse Licensing	1936-1951	Nurse Licensing
Accountant Licensing	1936-1951	Accountant Licensing
Real Estate Licensing	1936-1951	Real Estate Licensing
Miscellaneous Regulatatory Policies:		
Anti-sedition laws	1936-1955	Does the state have anti-sedition laws?
Forced sterilizations	1945-1974	Does the state have a forced sterlization program?
Grandparents’ Visitation Rights	1964-1987	Does the state have a law guaranteeing grandparents’ visitation rights?
Hate Crimes Ban	1981-2014	Are hate crimes explicity illegal in the state?
Urban Housing - Enabling Federal Aid		Does the state have a law enabling federal housing aid?
Urban Housing - Direct State Aid		Does the state provide direct aid for urban housing?

Description of Policies A1 Continued from previous page

Policy	Years	Description
Living Wills	1976-1992	Does the state have a law permitting individuals control over the use of heroic medical treatment in th event of a terminal illness?
Pain and Suffering Limits in Lawsuits	1975-2012	Are there limits on damages for pain and suffering in lawsuits?
Physician-assisted Suicide		Does the state allow physician-assisted suicide?
Planning Laws Required for Local Gov.	1961-2007	Does a state have a law authorizing or requiring growth-management planning?
Protections Against Compelling Reporters to Disclose Sources	1936-2013	Does the state have a Shield Law protecting them from revealing their sources?
Rent Control Prohibition	1950-2014	Does state prohibit the passage of rent control laws in its cities or municipalities?
Religious Freedom Restoration Act	1993-2014	Did the state pass the Religious Freedom Restoration Act?
State Debt Limitation	1936-1966	State Debt Limitation
Municipal Home Rule	1936-1961	Municipal Home Rule
Lemon Laws	1970-2014	Did the state pass a law protecting consumers who purchase automobiles which fail after repeated repairs?
Utility Regulation	1936-1960	State Commission with rate-setting authority over electricity utilities
Racial Discrimination Policies:		
Requires segregation in schools	1936-1953	Did the state require segregation in public schools?
Ban on Interracial Marriage	1936-1967	Did the state have a law banning interracial marriages?
Ban discrimination in public accommodations	1936-1963	Did the state pass a law (with adminstrative enforcement) banning discrimination in public accomodations (pre-CRA)?
Ban discrimination in public accommodations	1964-2010	Did the state pass a law (with adminstrative enforcement) banning discrimination in public accomodations (post-CRA)?
Fair Employment Laws	1945-1964	Does the state have a fair employment law?
Fair Employment Laws (post-1964)	1965-2014	Does the state have a fair employment law? (post-1964)
Fair Housing - Private Housing	1959-1968	Does the state ban discrimination in private housing?
Fair Housing - Public Housing	1937-1965	Does the state ban discrimination in public housing?
Fair Housing - Urban Renewal Areas	1945-1964	Does the state have urban renewal areas?
Tax Policies:		
Cigarette Tax	1936-1946	Does the state have a cigarette tax?
Cigarette Tax Rate	1947-2014	What is the state's tax on a pack of cigarettes?
Earned Income Tax Credit	1988-2014	Does the state have an earned income tax credit?
Income Tax	1936-2014	Does the state have an income tax?
Income tax Rate - Wealthy	1977-2012	What is the state individual income tax rate for an individual that makes more than 1.5 million real dollars?
Sales Tax	1936-1945	Does the state have a sales tax?
Sales Tax Rate	1946-2014	What is the sales tax rate?
Tax Burden	1977-2010	What is the state's tax burden (per capita taxes/per capita income)?
Top Corporate Tax Rate	1941-2014	What is the top corporate tax rate?
Corporate Income Tax	1936-1940	Is there a corporate income tax?
Gasoline Tax	1936-1929	Is there a gasoline tax?
Estate Tax	2009-2014	Is there a state estate tax?
Transportation Policies:		
Controlled Access Highways	1937-1946	Did the state pass a law to create controlled-access highways?
Bicycle Helmets Required	1985-2014	Does the state require that people use helmets while on bicycles?
Mandatory Seat Belts	1984-2014	Does the state require the usage of seat belts (either primary or secondary enforcement)?
Motorcycle Helmets Required	1967-2014	Does the state require the usage of helments by people on motorcycles?
Mandatory Car Insurance	1945-1986	Does the state require drivers to obtain car insurance?
Welfare Policies:		
AFDC - Benefits for Avg Family	1936-1992	What is the average level of benefits per family under the Aid for Families with Dependent Children program?
AFDC-UP Policy	1961-1990	What is the average level of benefits under the Aid for Families with Dependent Children program?
Aid to Blind - Payments per Recip.	1936-1965	What is the average monthly payment per recipient for the permanently blind or disabled?
Aid to Disabled - Payments per Recip.	1951-1965	What is the average monthly payment per recipient for the permanently blind or disabled?
Aid to Blind - Payments per Recip.	1966-1972	What is the average monthly payment per recipient for the permanently blind or disabled? (post-1965)
Aid to Disabled - Payments per Recip.	1966-1972	What is the average monthly payment per recipient for the permanently blind or disabled? (post-1965)

Description of Policies A1 Continued from previous page

Policy	Years	Description
CHIP - Eligibility Level for Children	1988-2012	What is the CHIP eligibility level for children?
CHIP - Eligibility Level for Infants	1998-2012	What is the CHIP eligibility level for infants?
General Assistance Payments Per Case	1937-1963	What is the average monthly payment per case for general assistance (an early form of welfare)?
General Assistance Payments Per Recip.	1964-1980	What is the average monthly payment per recipient for general assistance (an early form of welfare)?
CHIP - Eligibility Level for Pregnant Women	1998-2012	What is the CHIP eligibility level for pregnant women?
Medicaid - Eligibility for Pregnant Women	1990-1997	What is the Medicaid eligibility level for pregnant women?
Old Age Assis. - Payments per Recip.	1936-1965	What is the average monthly payment per recipient per recipient for old age assistance?
Old Age Assis. - Payments per Recip.	1965-1972	What is the average monthly payment per recipient per recipient for old age assistance? (post-1965)
Senior Prescription Drugs		Does the state provide pharmaceutical coverage or assistance for seniors who do not qualify for Medicaid?
State Adoption of Medicaid	1966-1983	Does the state have a Medicaid program?
TANF - Avg Payments per Family	2006-2010	What is the average monthly level of benefits per family under the Temporary Aid for Needy Families program?
TANF - Initial Elig. Level	1996-2013	What is the initial eligibility level for benefits for a family of three under the Temporary Aid for Needy Families Program?
TANF - Max Payments	1990-2013	What is the maximum level of benefis under the Temporary Aid for Needy Families program for a family of three with no income?
Womens' Rights Policies:		
Equal Pay For Females	1936-1972	Does the state have a law providing for equal pay for women working in the same job?
Equal Right Amendment Ratified	1972-2014	Has the state ratified the Equal Rights Amendment?
Jury Service for Women	1936-1967	Can women serve on juries?
State Equal Rights Law	1971-2014	Has the state passed a state-level equivalent to the Equal Rights Amendment?
Gender Discrimination Laws	1961-1964	Does the state ban hiring discrimination on the basis of gender?
Gender Discrimination Laws (post-1964)	1965-2014	Does the state ban hiring discrimination on the basis of gender? (post-1964)
No Fault Divorce	1966-2014	Do states have a no-fault divorce policy?

A.2 Measurement Model for Policy Liberalism

Our measurement strategy treats state policies as indicators of a latent trait, government policy liberalism, which varies across states and years. Several characteristics of our policy dataset make it a poor fit for conventional latent-variable methods such as classical factor analysis. First, state policy data are irregularly available over time, so most years contain a large amount of missing data. Second, whereas factor analysis is designed for continuous indicator variables, most of our policy indicators are dichotomous or ordinal. Third, we wish to account for and take advantage of the time-series structure of the dataset by pooling some but not all parts of the model across time periods.

We address these complications using a Bayesian latent-variable model (LVM) tailored to this application (Caughey and Warshaw, Forthcoming). We model policy liberalism as a latent trait θ_{st} that varies across states and years. For each state s and year t, we observe a mix of J continuous and ordinal indicators of policy liberalism, denoted $\boldsymbol{y}_{st} = (y_{1st}, \ldots, y_{jst}, \ldots, y_{Jst})$, whose distribution is governed by a corresponding vector of latent variables $\boldsymbol{y}^*_{st}$. We model y^*_{jst} as a function of θ_{st} and item-specific parameters α_{jt} and β_j:

$$y^*_{jst} \sim \mathrm{N}(\beta_j \theta_{st} - \alpha_{jt},\ \psi_j^2). \tag{4}$$

The discrimination parameter β_j indicates how "ideological" policy j is, and the difficulty parameter α_{jt} captures the baseline liberalism of policy j in year t.

We accommodate data of mixed type by changing the link function between latent and observed variables (Quinn 2004). If policy indicator j is continuous, we assume y^*_{jst} is directly observed (i.e., $y_{jst} = y^*_{jst}$), just as in the conventional factor analysis model. If policy indicator j is ordinal, we treat the observed y_{jst} as a coarsened realization of y^*_{jst} whose distribution across $K_j > 1$ ordered categories is determined by a set of $K_j + 1$ thresholds $\boldsymbol{\tau}_j = (\tau_{j0}, \ldots, \tau_{jk}, \ldots, \tau_{j,K_j})$. As in an ordered probit model, the probability that y^*_{jst} is observed as $y_{jst} = k$ is

$$\Pr(\tau_{j,k-1} < y^*_{jst} \leq \tau_{jk} \mid \beta_j\theta_{st} - \alpha_{jt}) = \Phi(\tau_{jk} - [\beta_j\theta_{st} - \alpha_{jt}]) - \Phi(\tau_{j,k-1} - [\beta_j\theta_{st} - \alpha_{jt}]), \tag{5}$$

where Φ is the standard normal CDF. Dichotomous variables are a special case of ordinal variables with $K_j = 2$ categories ("0" and "1"). The conditional probability that dichotomous y_{jst} falls in the second category (i.e., "1") is

$$\Pr(\tau_{j1} < y^*_{jst} \leq \tau_{j2} \mid \beta_j\theta_{st} - \alpha_{jt}] = \Phi(\beta_j\theta_{st} - \alpha_{jt}), \tag{6}$$

which is identical to the usual probit item-response model (Quinn 2004, 341).

Another feature of our measurement model is that it bridges the estimates over time so that the liberalism of a state in one year can be directly compared to its liberalism in another year. In order to do this, we model the evolution of the item parameters using a dynamic linear model (Martin and Quinn 2002). We use a local-level model to model the evolution of the difficulty parameter, α_{jt} using a "random walk" prior: $\alpha_{jt} \sim \mathrm{N}(\alpha_{j,t-1},\ \sigma^2_\alpha)$. If there are no new data for an item in period t, then this transition model acts as a predictive model, imputing a value for α_{jt}. The transition variance σ^2_α controls the degree of smoothing over time. Setting $\sigma^2_\alpha = \infty$ is equivalent to estimating α_{jt} separately each year, and $\sigma^2_\alpha = 0$ is the same as assuming no change over time. We take the more agnostic approach of estimating σ^2_α from the data, while also allowing it to differ between continuous and ordinal variables.

A.3 Validation: Govt. Policy Liberalism

In this appendix, we provide more systematic evidence for the validity of our measure of state government policy liberalism based on the analysis in Caughey and Warshaw (Forthcoming). We do so by documenting our estimates' empirical relationship with alternative measures of policy liberalism, what Adcock and Collier (2001) refer to as "convergent" validation. Then we examine their association with other, theoretically related concepts ("construct" validation, in their terminology). Finally, we provide evidence that a one-dimensional model adequately captures the systematic variation in states' policies. Overall, we find strong evidence that our estimates are valid measures of state policy liberalism.

Convergent Validation

If our estimates provide a valid measure of policy liberalism, they should be strongly related to other (valid) measures of the same concept. Since ours is the first time-varying measure of state policy liberalism, we must content ourselves with examining the cross-sectional relationship between our measure and ones developed by other scholars at various points in time. Figure A1 plots the cross-sectional relationships between our measure of policy liberalism and six existing measures:

- "liberalness"/"welfare orientation" rank *circa* 1957 (Hofferbert 1966)[37]
- welfare-education liberalism in 1962 (Sharkansky and Hofferbert 1969)[38]
- policy liberalism *circa* 1973 (Klingman and Lammers 1984)[39]

37. This index is based on mean per-recipient expenditures for 1952–61 for aid to the blind, old age assistance, unemployment compensation, expenditure for elementary and secondary education, and aid to dependent children. We compare Hofferbert's (1966) scale with our measure of state policy liberalism in 1957 since this is the midpoint of the years he includes in his index.

38. This index is based on about twenty education and welfare policies. Note, however, that this index also includes several social outcomes, such as school graduation rates.

39. This index is based on data measured at a variety of points between 1961 and 1980 on state innovativeness, anti-discrimination policies, monthly payments for Aid to Families with Dependent Children (AFDC), the number of years since ratification of the Equal Rights Amendment for Women, the number of consumer-oriented provisions, and the percentage of federal allotment to the state for

- policy liberalism *circa* 1980 (Wright, Erikson, and McIver 1987)[40]
- policy liberalism in 2000 (Gray et al. 2004)[41]
- policy liberalism in 2006 (Sorens, Muedini, and Ruger 2008)[42]

Each panel plots the relationship between our policy liberalism estimates (horizontal axis) and one of the six existing measures listed above. A loess curve summarizes each relationship, and the bivariate correlation is given on the left side of each panel.

Notwithstanding measurement error and differences in data sources, our estimates are highly predictive of other measures of policy liberalism. The weakest correlation, 0.76 for Hofferbert (1966), is primarily the result of a few puzzling outliers (Washington, for example, is the seventh-most conservative state on Hofferbert's measure, whereas Wyoming is the ninth-most liberal). In addition, all the relationships are highly linear. The only partial exception is for Sorens, Muedini, and Ruger (2008), whose measure of policy liberalism does not discriminate as much between Southern states as our measure, resulting in a flat relationship at the conservative end of our scale.

In short, the very strong empirical relationships between our policy liberalism scale and existing measures of the same concept provide compelling evidence for the validity of our measure. It is worth noting that most of the existing scales were constructed explicitly with the goal of differentiating between liberal and conservative

Title XX social services programs actually spent by the state. We compare Klingman and Lammers's (1984) scale with our measure of state policy liberalism in 1973 since this is the midpoint of the years they include in their index.

40. This measure is based on state education spending, the scope of state Medicaid programs, consumer protection laws, criminal justice provisions, whether states allowed legalized gambling, the number of years since ratification of the Equal Rights Amendment for Women, and the progressivity of state tax systems. We compare Wright, Erikson, and McIver's (1987) scale with our measure of state policy liberalism in 1980 since this is roughly the midpoint of the years they include in their index.

41. This index is based on state firearms laws, state abortion laws, welfare stringency, state right-to-work laws, and the progressively of state tax systems.

42. This is the first principal component uncovered by Sorens, Muedini, and Ruger's (2008) analysis of over 100 state policies. They label this dimension "policy liberalism" and give the label "policy urbanism" to the second principal component.

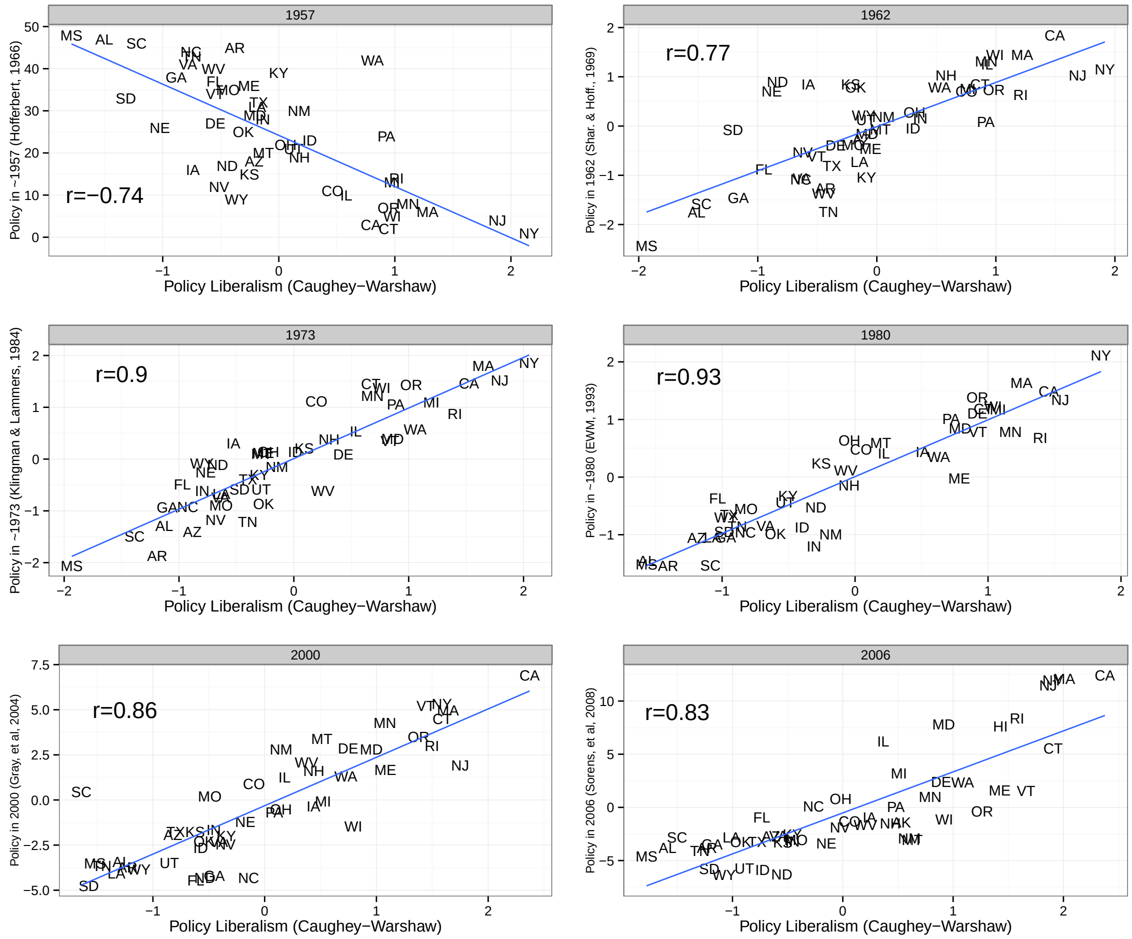


Figure A1: Validation of our Policy Measure: Correlation with Previous Policy Indices

states. Thus their tight relationship with our measure, which is based on a much more comprehensive policy dataset and was estimated without regard to the ideological content of the policy indicators,[43] suggests in particular that we are on firm ground in calling our latent dimension "policy liberalism."

Construct Validation

We provide further evidence for the validity of our measure by demonstrating its association with measures of concepts theoretically related to policy liberalism, a procedure Adcock and Collier (2001) refer to as "construct validation." First, we examine the relationship between mass political attitudes and state policy liberalism.

43. This is true except for the hard coding required to identify the latent scale.

Previous work shows that the liberalism of state publics have a strong cross-sectional association with state policy liberalism (Wright, Erikson, and McIver 1987; Erikson, Wright, and McIver 1993; Lax and Phillips 2011). Unfortunately, there is no extant survey-based measure of state ideology that extends back to 1936, so we instead use Democratic presidential vote share to proxy for mass liberalism (see, e.g., Ansolabehere, Snyder, and Stewart 2001; Canes-Wrone, Brady, and Cogan 2002). Consistent with past work, we focus on the Democratic presidential vote share in non-southern states.

Figure A2 shows the correlation of our dynamic measure of policy liberalism with the the Democratic candidate's state-level vote share in every presidential election year from 1936 to 2014. As expected, the two measures are highly correlated across the entire time period. Moreover, the relationship between public opinion and policy liberalism increases in strength over time, mirroring the growing alignment of policy preferences with partisanship and presidential voting at the individual level (Fiorina and Abrams 2008, 577–82).

Dimensionality

Our one-dimensional model of state policies implies that a single latent trait captures systematic policy variation across states. This is not to say that it captures *all* policy differences, but it does imply that once policies' characteristics and states' policy liberalism are accounted for, any additional variation in state policies is essentially random. This assumption would be violated if there were instead multiple dimensions of state policy, as some scholars have claimed. Given that roll-call alignments in the U.S. Congress were substantially two-dimensional for much of the 20th century (Poole and Rosenthal 2007), it is not unreasonable to suspect that state policies might be as well. As we demonstrate, however, a one-dimensional model captures state policy variation surprisingly well, and there is little value to increasing the complexity of

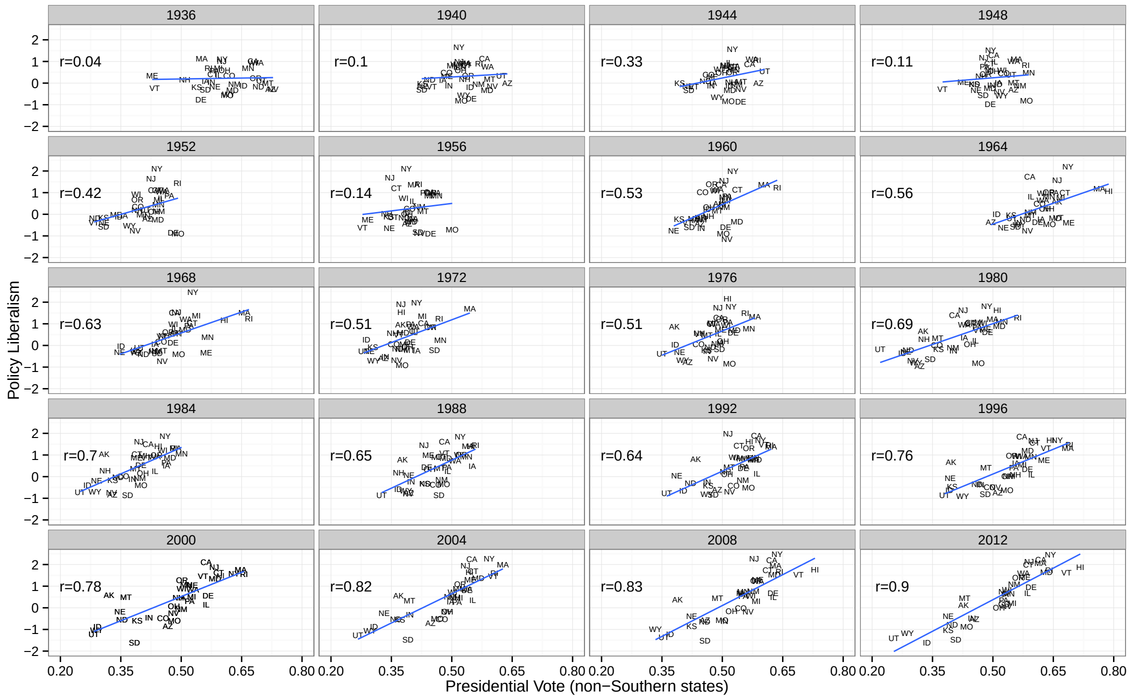


Figure A2: Relationship between State Policy Liberalism and Democratic Presidential Vote Share in the Non-South.

the model by adding further dimensions.

We can explore this question at a higher level of generality by scaling state policies within each of three broad issue domains: economic, social, and racial.[44] Policy cleavages in the mass public and in the U.S. Congress are often considered to differ across these domains, especially earlier in the 1936–2014 period (e.g., Poole and Rosenthal 2007). As the first column of the correlation matrix in Table A2 shows, however, each domain-specific scale is strongly related to the policy liberalism scale based on all policies. The domain-specific scales are also highly correlated with each other, with the correlation being weakest for racial and social policies (estimated for 1950–70 only). On the whole, Table A2 provides strong evidence that variation in state policies is one-dimensional and does not vary importantly across issue domains.

As a further piece of evidence, we show that allowing for multiple latent dimensions does not substantially improve our ability to predict policy differences between states.

44. Because cross-state variation in civil rights policies is concentrated in the 1950–70 period, we estimate the racial policy dimension for these two decades only.

Table A2: Correlations between policy liberalism scales estimated using economic, social, racial, and all policies. The unit of analysis is the state-year. The racial policy scale is estimated for the 1950–70 period only.

	All	Economic	Social
Economic	0.92		
Social	0.84	0.69	
Racial	0.86	0.68	0.55

As our measure of model fit we use percentage correctly predicted (PCP), which for binary variables is the percentage of cases for which the observed value corresponds to its model-based predicted value (0 or 1).[45] Based on this method, we find little evidence that adding dimensions improves our ability to account for the data. In the average year, a one-dimensional model correctly classifies 82% of all dichotomized policy observations. Adding a second dimension increases average PCP by only 1.5 percentage points. This improvement in model fit is less than the increase in fit that is used in the congressional literature as a barometer of whether roll-call voting in Congress has a one-dimensional structure (Poole and Rosenthal 2007, 33–4).

Taken as a whole, the evidence supports two conclusions. First, a single latent dimension captures the vast majority of policy variation across states across disparate policy domains. This is true even at times when national politics was multidimensional. Second, the approximately 20% of cross-sectional policy variation not captured by a one-dimensional model does not seem to have a systematic structure to it, or at least not one that can be described by additional dimensions.

45. In order to include ordinal and continuous variables in this calculation, we convert them into binary variables by dichotomizing them at a threshold randomly generated for each variable. We estimate one and two-dimensional probit IRT models separately in each year using the `R` function `ideal` (Jackman 2012), which automatically calculates PCP. We then evaluate how much the second dimension improves PCP (adding dimensions cannot decrease PCP).

A.4 Continuity of Pre-Treatment Covariates in RD Designs

A.4.1 RD for Governor

Table A3: Covariate continuity tests for the gubernatorial RD design, estimated using the default local-linear regression bandwidth (BW) and robust confidence intervals calculated by `rdrobust` (Calonico, Cattaneo, and Titiunik 2014). All are covariates measured in the year of the election. Residual Policy Liberalism is the residuals from a regression of *Policy Liberalism* on intercepts for state and year. Change in Policy Liberalism is measured relative to the year before the election.

	BW	Est	CI	Pr > $\|z\|$
Democratic Governor	0.23	−0.08	(−0.24, 0.08)	0.31
Dem. Majority in House	0.16	0.00	(−0.17, 0.18)	0.96
Dem. Seat Share in House	0.14	−0.01	(−0.08, 0.07)	0.86
Dem. Majority in Senate	0.17	−0.03	(−0.21, 0.14)	0.69
Dem. Seat Share in Senate	0.13	−0.00	(−0.08, 0.07)	0.94
Policy Liberalism (level)	0.15	0.06	(−0.23, 0.37)	0.65
Policy Liberalism (residual)	0.14	0.08	(−0.02, 0.23)	0.10
Policy Liberalism (change)	0.21	−0.02	(−0.06, 0.02)	0.29

A.4.2 RD for State House

Table A4: Covariate continuity tests for the state house RD design, estimated using the default local-linear regression bandwidth (BW) and robust confidence intervals calculated by `rdrobust` (Calonico, Cattaneo, and Titiunik 2014). All are covariates measured in the year of the election. Residual Policy Liberalism is the residuals from a regression of *Policy Liberalism* on state and year intercepts. Change in Policy Liberalism is measured relative to the year before the election.

	BW	Est	CI	Pr > $\|z\|$
Democratic Governor	52	0.07	(−0.11, 0.25)	0.44
Dem. Majority in House	31	0.12	(−0.11, 0.28)	0.39
Dem. Seat Share in House	34	0.02	(−0.02, 0.04)	0.41
Dem. Majority in Senate	55	0.05	(−0.14, 0.19)	0.74
Dem. Seat Share in Senate	69	0.03	(−0.01, 0.06)	0.17
Policy Liberalism	51	−0.06	(−0.34, 0.19)	0.57
Residual Policy Liberalism	42	0.03	(−0.06, 0.14)	0.39
Change in Policy Liberalism	72	0.02	(−0.04, 0.08)	0.55

A.5 Dynamic Effects of Partisan Composition.

The identifying assumption of the dynamic panel model we use states that in the absence of the treatment, the average outcome of treated units would have been similar to that of the control units after fixed effects and lagged dependent variables are controlled for. In other words, after conditioning on fixed effects and past outcomes (and perhaps partisan control of the legislatures), the evolution of policy liberalism in state A that elects a Democratic governor should be be indistinguishable, at least by expectation, from that of a state that elects a non-Democratic governor had not the Democrat governor been elected in state A.

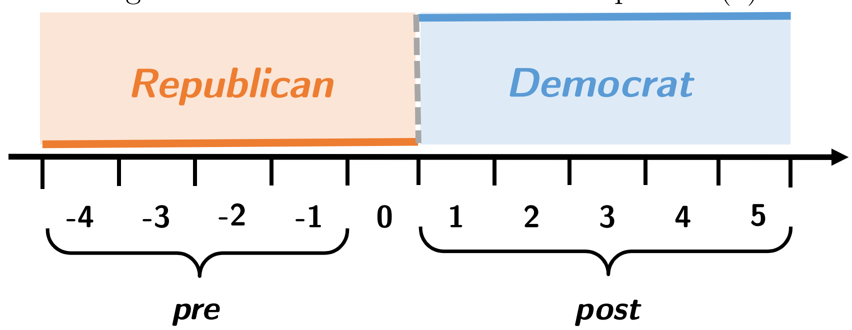
To shed some light on the validity of this assumption, we investigate the dynamic changes of the immediate effect of partisan composition on state liberalism, which partly serves as a placebo test. If, for example, we can show that the estimated coefficients of indicators of future partisan composition has no effect on the current policy measure (because the change has not happened yet), we will have more confidence in the validity of the identifying assumption stated above. Therefore, we estimate the following model:

$$\begin{aligned} y_t = & \sum_{r=1}^{4} \delta'_r GovPre_{r,it} + \sum_{s=1}^{5} \delta_s GovPost_{s,it} + \delta^0 GovRest_{it} \\ & + \sum_{u=1}^{4} \beta'_u HsPre_{u,it} + \sum_{v=1}^{5} \beta_v HsPost_{v,it} + \beta^0 HsRest_{it} \\ & + \sum_{q=1}^{4} \gamma'_q SenPre_{q,it} + \sum_{w=1}^{5} \gamma_w SenPost_{w,it} + \gamma^0 SenRest_{it} \\ & + \rho_1 y_{i,t-1} + \rho_2 y_{i,t-2} + \alpha_i + \xi_t + \epsilon_{it}. \end{aligned} \tag{7}$$

in which $GovPre_{r,it}$ is a binary indicator that equals one when year t is r year(s) before the election year in which a Democratic governor is elected and zero otherwise–for example, if 2014 is the year in which a Democrat won the governor election in state

i, $GovPre_{1,i,2013}$ would equal one because 2013 is one year before the election year; $GovPost_{s,it}$ is a binary indicator that takes value one when year t is s year(s) after the year in which a Democratic governor is elected and zero otherwise; and $GovRest_{it}$ is a dummy variable that equals one if year t is more than four years before, or more than five years after, a governor election that puts a Democrat in office. $HsPre_{u,it}$, $HsPost_{v,it}$, $HsRest_{it}$, $SenPre_{q,it}$, $SenPost_{w,it}$, and $SenRest_{it}$ are defined in a similar fashion. The definitions of the pre- and post- indicators are illustrated in Figure A4.

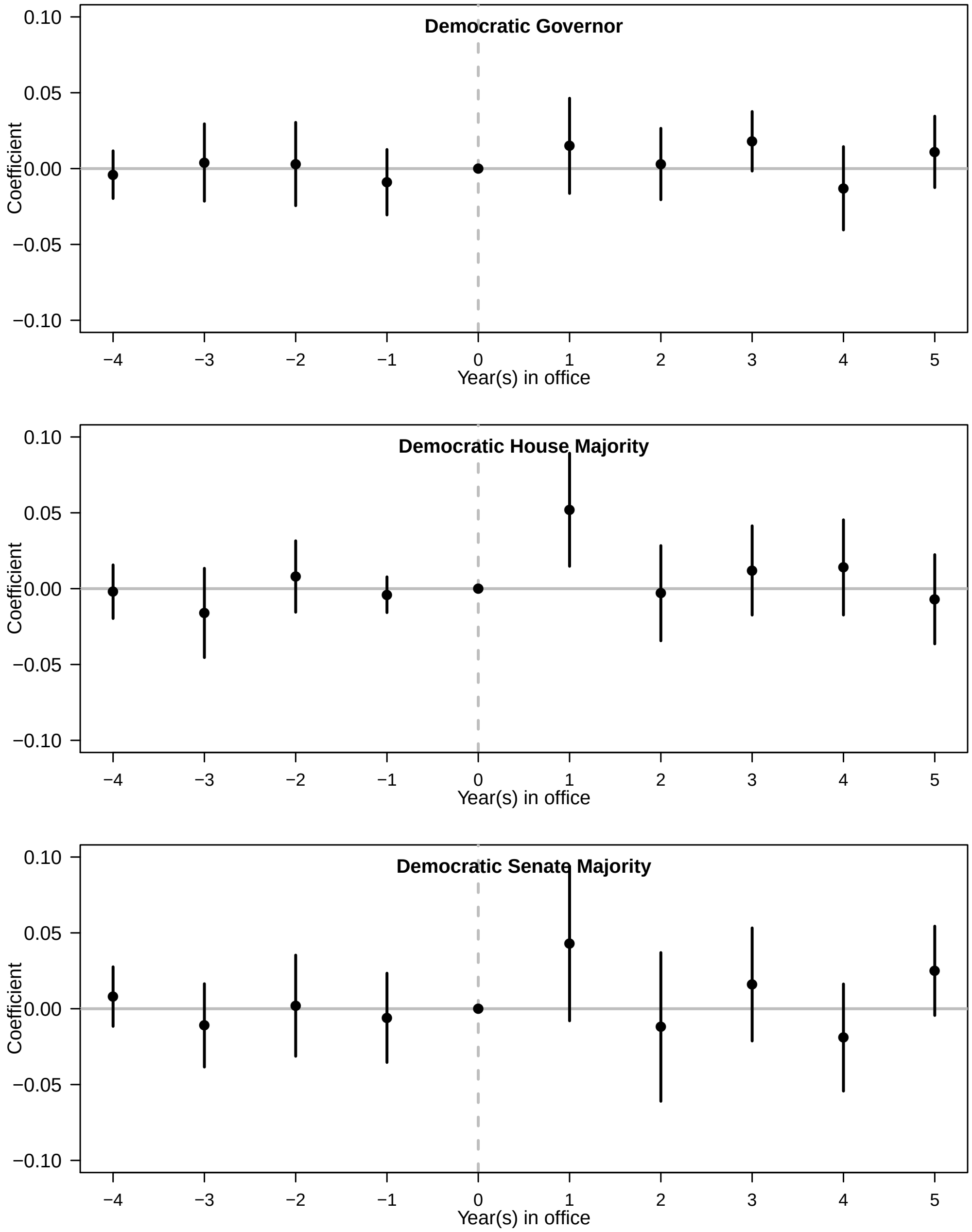
Figure A3: Indicator Definitions in Equation (7)



Again, we include only two lagged terms of the dependent variable and standard errors are clustered at the state level. Nebraska is not included as before. The results are shown in Figure A4. The y-axes in the three panels are the coefficients of immediate policy effect of a Democratic governor, a Democratic house majority status, and a Democratic senate majority status, respectively. The omitted category in each panel is the election year (e.g. the year in which a Democrat governor is elected) and is marked as "0" in the panels in Figure A4.

Figure A4 shows that, in all three panels, the coefficients of dummy variables indicating years before Democrats' taking office or controlling state legislatures are very close to zero (the trend is virtually flat). After the election year, however, we see immediate jumps for the effect of Democratic governors, house majority, as well as senate majority. The effects after the first years bump around but mostly remain positive. Consistent with previous results, the effect of Democratic house majority is

Figure A4: Dynamic Changes of the Immediate Partisan Effects



bigger than that of a Democratic governor and a house majority. The investigation of the evolution of policy effects of partisan composition lends us confidence in the identification strategy of using TSCS models with fixed effects and lagged dependent variables to estimate the effect of government partisanship on state policies.

A.6 Concerns of Unit Roots and Inconsistency

We address two potential concerns related to the TSCS models that we present in the main text. First, one might be worried that the high temporal dependence in the policy measure may indicate unit roots (i.e. the autoregressive coefficient equals 1) in the data generating process. Potential non-stationarity of the outcome variable may lead to implausible inference of the causal quantities. Second, as mentioned above, since we include both state fixed effects and past outcomes in the model, demeaned error is correlated with the past outcomes, which leads to biased estimates in finite samples (the bias goes away as T approaches infinity).

To address the first concern, we transform the outcome variable by taking a first difference and estimate the following models suggested by (Phillips and Moon 2000):

$$\Delta y_{it} = (\rho_1 - 1)y_{i,t-1} + \delta Gov_{it} + \beta Maj^{\mathrm{H}}_{it} + \gamma Maj^{\mathrm{S}}_{it} + \alpha_i + \xi_t + \epsilon_{it}, \tag{8}$$

or

$$\Delta y_{it} = (\rho_1 - 1)y_{i,t-1} + \rho_2 y_{i,t-2} + \delta Gov_{it} + \beta Maj^{\mathrm{H}}_{it} + \gamma Maj^{\mathrm{S}}_{it} + \alpha_i + \xi_t + \epsilon_{it}, \tag{9}$$

in which $\Delta y_{it} = y_{it} - y_{i,t-1}$ is the first difference of the outcome variable. Column (1) in Table A5 reports the estimation result of Equation (8) using a *within* estimator. It shows that $(1 - \hat{\rho}_1)$ is negative and statistically different from zero, a sign that a unit root does not exist, and the estimates of partisan composition coefficients are almost identical to those in Table 3.

Next, we use a generalized methods of moments (GMM) approach to address the concern of correlation between $y_{i,t-1}$ and the demeaned error term (Arellano and Bond, 1991). The basic idea of the GMM approach is to use the outcome variable in even early periods to instrument the past outcomes included in the model with the assumption of exclusion restriction that these early terms affect the current outcome only through the recent past outcomes. In column (2), for example, we use the policy measures lagged for 2 to 4 years to instrument last year's policy measure. The estimated coefficient of the partisan composition are similar to those in column (1).[46] In columns (3) and (4), we re-do the analysis by estimating Equation (9). In column (4), we use the policy measures lagged for 3 to 5 years to instrument the past outcomes in the previous two years. The main results remain qualitatively the same.

Table A5: Alternative Estimation Strategies

Outcome variable	*Δ* Policy liberalism (*t*)			
	FE (1)	GMM (2)	FE (3)	GMM (4)
Democratic governor	**0.012**	**0.019**	**0.012**	**0.018**
	(0.004)	(0.005)	(0.004)	(0.005)
Democratic house majority	**0.028**	**0.031**	**0.030**	**0.032**
	(0.006)	(0.008)	(0.006)	(0.008)
Democratic senate majority	**0.022**	**0.021**	**0.020**	**0.019**
	(0.006)	(0.008)	(0.006)	(0.009)
Policy liberalism (*t-1*)	**-0.051**	**-0.076**	**-0.142**	**-0.154**
	(0.007)	(0.014)	(0.016)	(0.048)
Policy liberalism (*t-2*)			**0.097**	**0.089**
			(0.016)	(0.043)
State and year fixed effects	x	x	x	x
Observations	3,632	3,632	3,586	3,586
States	49	49	49	49

Note: Robust standard errors clustered at the state level are in the parentheses. The state of Nebraska is dropped out of the sample. The outcome variable is the first difference of the policy measure. In column (2), the outcome variable lagged for 2 to 5 periods are used as instrements for the lagged outcome variable. In column (3), the instruments are the outcome variable lagged for 3 to 6 periods. Partisan composition of the state government and year and state dummies are treated as exogeneous. Coefficients statistically significant at the 5% level are in bold font type.

46. We use the one-step approach to avoid under-estimation of the standard errors. We do not use all available past outcomes to avoid problems caused by too many instruments. The instruments are used in both the level and first-difference equations. Our results hold for various specifications (e.g., the choice of instruments) and GMM options.

A.7 Adding State-specific Time Trends

In this subsection, we add unit-specific time trends to a conventional two-way fixed-effect model to explore alternative model specifications. We find that, even when we control for a cubic time trend for each state, the coefficients of partisan governors and state legislatures are still all positive and broadly consistent with the estimates reported in the main text (e.g. table 3 column 4). However, the standard errors are much larger than those in Table 3, indicating improper model specifications that causes inefficiency, and potentially inconsistency.

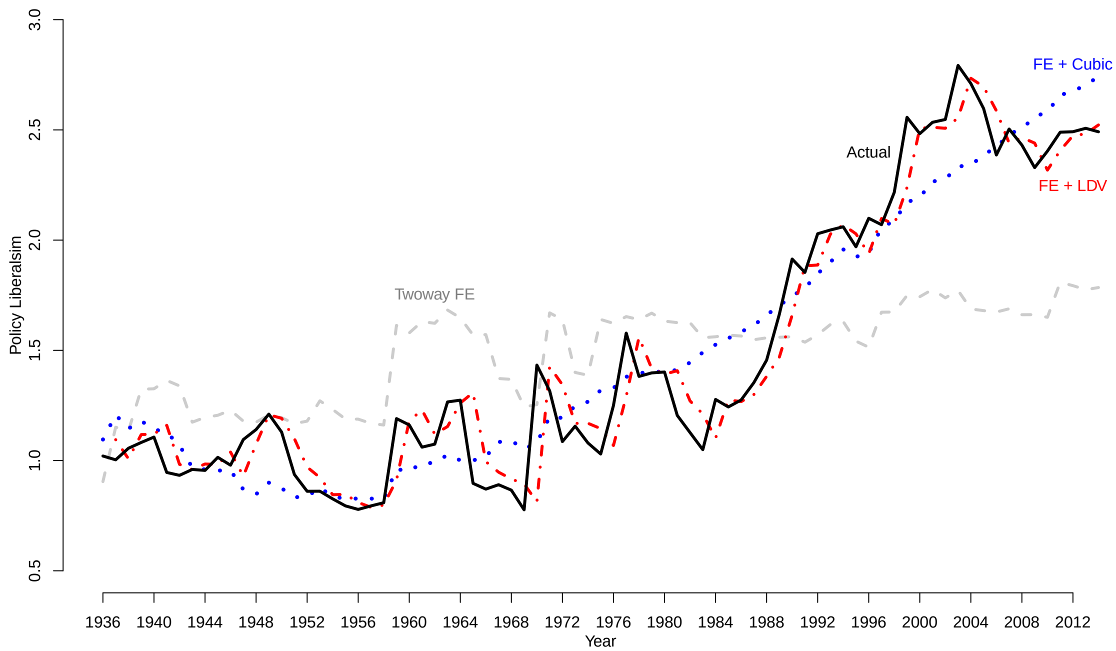
Table A6: Two-way Fixed-effect Models with Time Trends

Outcome variable	Policy liberalism			
	(1)	(2)	(3)	(4)
Democratic governor	**0.065**	0.005	0.010	0.018
	(0.032)	(0.016)	(0.013)	(0.012)
Democratic house majority	**0.166**	**0.084**	**0.083**	**0.082**
	(0.052)	(0.023)	(0.023)	(0.020)
Democratic senate majority	**0.269**	0.038	0.017	0.001
	(0.057)	(0.032)	(0.033)	(0.033)
State and year fixed effects	x	x	x	x
State-specific linear time trends		x		
State-specific quadratic time trends			x	
State-specific cubic time trends				x
Observations	3,903	3,903	3,903	3,902
States	50	50	50	50
R-squared	0.851	0.952	0.965	0.986

Note: Robust standard errors clustered at the state level are in the parentheses. Coefficients statistically significant at the 5% level are in bold font type.

This specification problem is further illustrated in Figure A5, in which several model fits are drawn for political liberalism in California (estimations are based on all available data, not just California). The three models include a conventional two-way fixed-effect model (`Twoway FE`), a model of two-way fixed-effect plus unit-specific cubic time trends (`FE + cubic`), and a model of two-way fixed-effect plus two lagged dependent variables (`FE + LDV`, our main specification). All models include three dummy variables indicating a democratic governor, a democratic state house majority, and a democratic state senate majority. It is quite clear from Figure A5 that fixed-effect models without incorporating LDVs (even when flexible time trends are added) provide much worse fits than a model that controls for LDVs.

Figure A5: Model Fits: The Example of California



A.8 Variations in Partisan Compositions

Table A7 calculates the variations in the key independent variables–Democratic control of the governorship, state house, and state senate–in the full sample, in the samples of non-Southern and Southern states, and across different time periods. The variance of a variable is decomposed in to *within* variance, variance within a state over time, and *between* variance, variance (of the each state's variable mean) between states. Because we control for state fixed effects in all regressions, our dynamic panel analyses exploit variations within states.

Table A7 shows that (1) in the full sample, the within variation in the Democratic control of the governorship remains relatively stable over time, while the within variations in the Democratic control of the state house and state senate increase after the 1990's; (2) the within variations in all three variables remain stable in non-Southern states over time; (3) since Democrats controlled state legislatures in the South before the 1990's, there are no variations in the two variables during this period. (2) and (3) indicate that the increased variations in the Democratic control of the house and senate almost entirely come from the 11 Southern states.

Hence, the main variations our identification strategies rely upon mostly come from the non-Southern states. We shown in Table A8 that dropping observations of the 11 Southern states does not affect our main results. Moreover, apparently the fact that we find almost zero partisan effects on policy in the early period is not due to lack of variations in the independent variables in that period.

Table A7: Variations in Partison Compostions

	All States			*Non-south*			*South*		
	Governor	House	Senate	Governor	House	Senate	Governor	House	Senate
1936-1967									
Mean	0.596	0.581	0.537	0.480	0.453	0.395	0.994	1.000	1.000
Within variance	0.158	0.093	0.086	0.202	0.122	0.113	0.005	0.000	0.000
Between variance	0.084	0.150	0.164	0.050	0.130	0.133	0.000	0.000	0.000
Within %	**65.4**	**38.3**	**34.5**	**80.1**	**48.3**	**45.9**	**97.4**	**NA**	**NA**
1968-1990									
Mean	0.603	0.689	0.661	0.570	0.598	0.560	0.723	1.000	1.000
Within variance	0.144	0.078	0.081	0.185	0.102	0.106	0.170	0.000	0.000
Between variance	0.098	0.139	0.146	0.053	0.142	0.144	0.033	0.000	0.000
Within %	**59.6**	**36.0**	**35.8**	**77.6**	**41.7**	**42.3**	**83.6**	**NA**	**NA**
1991-2014									
Mean	0.452	0.547	0.520	0.467	0.527	0.493	0.397	0.616	0.615
Within variance	0.143	0.118	0.114	0.182	0.102	0.100	0.202	0.173	0.161
Between variance	0.105	0.132	0.138	0.068	0.151	0.153	0.042	0.070	0.085
Within %	**57.8**	**47.0**	**45.1**	**72.8**	**40.3**	**39.5**	**82.6**	**71.1**	**65.5**
All Years									
Mean	0.554	0.602	0.568	0.502	0.519	0.474	0.734	0.883	0.885
Within variance	0.220	0.144	0.143	0.229	0.158	0.158	0.191	0.097	0.095
Between variance	0.027	0.098	0.104	0.022	0.096	0.097	0.004	0.006	0.007
Within %	**89.2**	**59.5**	**57.8**	**91.4**	**62.2**	**61.8**	**97.8**	**93.8**	**92.9**

A.9 Analysis of Non-Southern States

Finally, we show that our main results are robust when we exclude 11 southern states from the sample. All three regressions control for two lagged terms of the dependent variable and state and year fixed effects. The estimated coefficients of partisan composition are very similar to those in Table 4.

Table A8: Policy Effects of Partisan Composition: Non-southern States

Outcome variable	Policy liberalism		
	(1)	(2)	(3)
Democratic governor	**0.011**	**0.010**	**0.010**
	(0.004)	(0.004)	(0.004)
Democratic house majority	**0.032**	**0.029**	**0.029**
	(0.007)	(0.009)	(0.009)
Democratic senate majority	**0.022**	0.018	0.018
	(0.007)	(0.011)	(0.011)
Democratic house seat share		0.012	0.008
		(0.037)	(0.044)
Democratic senate seat share			0.003
			(0.075)
Democratic house seat share * house majority		0.017	0.048
		(0.036)	(0.038)
Democratic senate seat share * senate majority			-0.056
			(0.058)
Two lagged terms of the outcome variable	x	x	x
State and year fixed effects	x	x	x
Observations	2,782	2,782	2,782
States	38	38	38
R-squared	0.982	0.982	0.982

Note: Robust standard errors clustered at the state level are in the parentheses. 11 southern states, including Alabama, Arizona, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia, plus Nebraska, are not included in the sample. Coefficients statistically significant at the 5% level are in bold font type.

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