Incremental Democracy: The Policy Effects of Partisan Control of State Government

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Abstract

How much does it matter whether Democrats or Republicans control the government? Unless the two parties converge completely, election outcomes should have some impact on policy, but the existing evidence for policy effects of party control is surprisingly weak and inconsistent. We bring clarity to this question, using regression-discontinuity and dynamic panel analyses to estimate the effects of party control of state legislatures and governorships on a new annual measure of state policy liberalism. We find that throughout the 1936–2014 period, electing Democrats has led to more liberal policies, but that in recent decades the policy effects of party control have approximately doubled in magnitude. We present evidence that this increase is at least partially explained by the ideological divergence of the parties' officeholders and electoral coalitions. At the same time, we also show that party effects remain substantively modest, paling relative to policy differences across states.

Keywords: State politics, policy, parties, elections, governors [1]

1. Replication files are available in the JOP Data Archive on the Dataverse (http://thedata.harvard.edu/dvn/dv/jop). In addition, supplementary material for this article is available in the appendix in the online edition.

In November 1948, the Ohio Democratic Party gained control of state government for the first time in ten years. With the popular Frank Lausche at the top of their ticket, the Democrats defeated the incumbent Republican governor and won majorities in both houses of the legislature. During their two years of unified control, however, Ohio Democrats failed to pass any major new liberal policies. In fact, Governor Lausche, a fiscal conservative who had defeated a more liberal candidate in the Democratic primary, proposed a budget cutting state expenditures, and the liberal initiatives he did support, such as a ban on racial discrimination in employment, failed to make it through the Democratic legislature (*Time* 1956; Usher 1994; Chen 2009, 165, 273). Six decades later, in 2012, North Carolina Republicans experienced a similar triumph with the election of Governor Pat McCrory, who completed the GOP takeover of the state initiated two years earlier with its capture of the legislature. Unlike Ohio Democrats in 1948, North Carolina Republicans took advantage of their newfound control by passing a flood of conservative legislation: cutting unemployment insurance, repealing the estate tax, "flattening" the income tax, relaxing gun laws, and tightening restrictions on abortion (Fausset 2014).

These two cases, Ohio in 1948 and North Carolina in 2012, suggest very different conclusions about policy effects of party control of state government. Does electing Democrats rather than Republicans have only an incremental, or perhaps non-existent, impact on state policies, or does it result in dramatic policy shifts that leapfrog over the median voter? The scholarly literature exhibits surprisingly little consensus on this question. Many classic studies of state politics emphasize the exceedingly weak or even negative cross-sectional association between state policy liberalism and Democratic control of state offices, suggesting that electoral pressure to converge on the median voter may be so strong as to all-but-eliminate differences between Democrats and Republicans (e.g., Hofferbert 1966; Erikson, Wright, and McIver 1993). More recent studies employing panel or regression-discontinuity (RD) designs have uncovered partisan policy effects, but typically only for certain offices, on some policies, in a subset of states, or under particular conditions (e.g., Alt and Lowry

1994; Besley and Case 2003; Kousser 2002; Leigh 2008).

Combining multiple research designs, a long historical perspective, and a wealth of new data, we offer clearer answers to the question of partisan effects on policy. We improve upon existing research in three major ways. First, we use a much more comprehensive policy measure, the policy liberalism scale developed by Caughey and Warshaw (2016), which is estimated from a dataset of nearly 150 distinct policies covering each year between 1936 and 2014. Second, we use more credible identification strategies. Specifically, we estimate the effects of Democratic governors and state legislatures using two designs: an electoral regression-discontinuity (RD) design, which exploits 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 isolate the causal effects of partisan control from other time-varying determinants of state policy, such as changes in public opinion. Third, we examine whether party effects have grown over time, and whether this growth is related to partisan polarization at the mass and elite levels.

We find that partisan effects on state policy have indeed increased substantially over the past eight decades, with the growth concentrated in the last quarter century. Between the 1930s and 1980s, the partisan composition of state governments had little causal impact on the ideological orientation of state policies. Since the 1980s, however, partisan effects have grown dramatically. We find little indication that this growth differed by region or were driven by the anomalously Democratic partisanship of the formerly "solid" South. We do, however, find robust support for the hypothesis that partisan polarization has increased partisan effects on policy. Specifically, we find greater policy effects when and where Democratic and Republican identifiers diverge more in their policy views and where roll call voting in the state legislature is more polarized by party.

Notwithstanding their dramatic growth, the substantive magnitude of partisan effects on policy should not be exaggerated. Even today, for example, electing a Democratic rather than Republican governor only has an incremental effect on policy.[2] It should be expected to increase monthly welfare payments by only $1–2 per recipient, and to increase by just half a percentage point the proportion of policies on which a state has the liberal policy option. These effects are small relative to policy differences across states, and also relative to partisan differences in legislative voting records. Our findings thus partially assuage the normative concern that partisan polarization has resulted in a "leapfrog democracy" of wide policy swings and poor congruence with citizens' preferences (Bafumi and Herron 2010; see also Poole and Rosenthal 1984; Lax and Phillips 2011).

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. We also document the strong relationship between the growth in party effects on policy and partisan polarization. The final section discusses the implications of our results.

Substantive Background

Although the relationship between state policies and the partisanship of state officials is a longstanding focus of the state politics literature, there is no consensus regarding the causal effects of partisan control on state policy. Most classic studies find little association between states' policies and the partisanship of their officials.[3] After controlling for public opinion, some studies even find Democratic party control and liberal policies to be *negatively* correlated across states (e.g., Erikson, Wright, and McIver 1993; Lax and Phillips 2011).

These cross-sectional studies, however, are hampered by two important methodological

2. We are using the term "incremental" in a more general way than the incrementalism literature in public administration (e.g., Lindblom 1959). Most importantly, our explanation for why policy change is incremental is not based on the cognitive or informational limitations of decision-makers.

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. Hanson (1984) finds no significant effects of party control on Medicaid programs, while Plotnick and Winters (1985) find no effect of party control on AFDC benefits.

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 any number of omitted variables that are correlated with partisan control of government (economic conditions, mass or elite policy preferences, etc.). Second, their findings are all based on a single slice of time, and sometimes a single policy area. As a result, it is hard to know whether each study's results are generalizable to other time periods or policy areas.

A smaller literature has used panel data to examine policy effects using more credible causal identification strategies. Most studies, including those with strong designs, find that in general partisan control of the governorship does not substantially affect policy. Besley and Case (2003), for example, 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. Studies that employ electoral RD designs to examine the policy effects of governors find similarly ambiguous and contingent effects. For instance, Fredriksson, Wang, and Warren (2013) find that re-electable Democratic governors increase taxes, but term-limited ones *decrease* them. Similarly, Leigh (2008) examines a total of eight policy indicators and finds significant effects on just one, leading him to conclude that governors "behave in a fairly non-ideological manner" (256). Evidence that party control of the state legislature influences policy outcomes is more consistent, but hardly universal. Panel studies have found that control of the legislature influences some policies, such as civil rights, tax burdens, and welfare benefits (e.g., Besley and Case 2003; Reed 2006; Chen 2007), but have no effect on others (e.g., Konisky 2007). Each of these studies, however, focuses on only a handful of policies. It is thus hard to know what to make of their mixed and ambiguous results. Moreover, it is difficult to assess whether their results generalize to the larger policy agenda.

In sum, the state politics literature exhibits little agreement regarding the policy effects of partisan control of state government (see Supplementary Information A1 for a more comprehensive summary of the previous literature that further demonstrates this point). On the whole, these studies have found "weak and oftentimes conditional" evidence that party control affects state policies (Kousser and Phillips 2009, 70). In the sections that follow, we bring clarity to this debate with both new theory and evidence on the policy effects of the partisan composition of state government.

Theoretical Framework

Like many other works on state politics, our basic theoretical framework is a model of two-party competition over a one-dimensional policy space.[4] In a perfectly Downsian world, in which electorally motivated parties adopt the positions of the median voter, party control of state offices has no effect on state policies. Only if the parties diverge from the median voter do partisan policy effects—counterfactual differences in policy liberalism under Democratic versus Republican control—actually emerge.

Given that candidates cannot perfectly predict election outcomes and often care about influencing policy in addition to winning office, we should in general expect some degree of ideological divergence between the two parties (Roemer 2001, 72; Grofman 2004). In fact, as Gerring (1998) shows, national party conflict has had a strong ideological component throughout U.S. history, with the parties' current ideological orientations dating back to 1928 for the Republicans and 1952 for the Democrats. Within states, Democratic senators (Poole and Rosenthal 1984), candidates and activists (Erikson, Wright, and McIver 1989), and state legislators (Shor and McCarty 2011) take more liberal policy positions than their Republican counterparts. Within-state partisan divergence on economic issues extends back to the New Deal realignment, if not before, but even on racial issues, where the national parties took longer to sort out ideologically, Democrats have been more liberal than same-state Republicans since the 1940s (Feinstein and Schickler 2008).

Given this evidence for partisan divergence, the more interesting question is not whether partisan effects exist, but how large they are. If centripetal pressures dominate, then the

4. Caughey and Warshaw (2016) show that throughout this period, cross-state policy variation was primarily structured by a single latent dimension, and modeling state policies as a function of two or more latent dimensions does little to improve the model's predictive accuracy.

parties in each state will converge closely on the state's median voter and differ only modestly in their policy platforms. Policy effects will be further attenuated by the limitations imposed by the minority party and other constraints on the majority party's capacity to implement their preferred policies (e.g., Alesina, Londregan, and Rosenthal 1993). Governors, for example, cannot simply implement their ideal points, but rather must compromise with a legislature in which the opposing party probably has at least some influence. Such limitations on Democrats' and Republicans' desire and capacity to implement divergent policies lead us to the expectation that policy effects should generally be small relative to, say, the policy variation across states.

Nevertheless, there are also good reasons to expect partisan effects on state policy to have increased over the period we examine. At the national level, Democratic and Republican officials have become increasingly ideologically polarized, especially since the 1970s (McCarty, Poole, and Rosenthal 2006). Policy conflict between the national parties has become increasingly aligned with what is now defined as "liberalism" and "conservatism" (Noel 2014). Whether due to true polarization (Abramowitz 2010) or partisan sorting (Fiorina and Abrams 2008), the mass public has followed suit, increasing the ideological distance between the parties' electoral coalitions (Hill and Tausanovitch 2016). As formal theorists have long noted, ideological divergence between parties' primary electorates increases the electoral incentives for party nominees to diverge from the median voter (e.g., Adams and Merrill 2008). Moreover, if candidates are drawn from the set of party identifiers, their own sincere policy views should become more extreme as well (e.g., Cadigan and Janeba 2002; Thomsen 2014). Mass polarization between the parties has thus reinforced and exacerbated elite polarization (Jacobson 2012), resulting in larger policy effects of the partisan composition of government.[5] Indeed, some scholars have warned that polarization has become so

5. Other factors too have probably contributed to an increase in partisan effects on policy. For example, policy effects in state legislatures should depend on the degree to which the majority party can use its control to skew policy outcomes away from the median legislator in the chamber (e.g., Cox, Kousser, and McCubbins 2010). Over the past half century, there is a variety of evidence that the two parties in Congress have leveraged their greater homogeneity into strong formal mechanisms of party discipline and control, enhancing the majority's influence over policymaking (Aldrich and Rohde 2000). Given state legislatures

extreme that representatives now "leapfrog" over the median voter, leading to wide swings between liberal and conservative policy outcomes incongruent with the preferences of the median voter (Poole and Rosenthal 1984; Bafumi and Herron 2010; Lax and Phillips 2011).

In sum, these theoretical results and empirical trends give rise to several expectations. On one hand, the centripetal pull of electoral competition and the limitations on officials' capacity to fully implement their policy preferences lead to the expectation that policy effects will be modest, at least relative to policy differences between states. One the other hand, given the growth of partisan polarization, partisan effects on policy are likely to be larger now than in the past. To the extent that this growth has been driven by the diverging policy preferences of Democratic and Republican officials (as opposed to, say, increases in the majority party's control over state policy), we should also expect policy effects to be larger where Democratic and Republican politicians are more ideologically polarized. Finally, if elite polarization is rooted in ideological divergence between the parties' electoral coalitions, we should expect the magnitude of policy effects to be correlated with the extent of mass polarization. We assess these hypotheses below, but first we describe our strategy for measuring the dependent variable in our analysis: state policy liberalism.

An Annual Measure of State Policy Liberalism

Studies of state policy generally employ one of two measurement strategies: they either analyze one or more policy-specific indicators, or they construct composite measures intended to summarize the general orientation of state policies (Jacoby and Schneider 2014, 568). Each approach has advantages and disadvantages. An important benefit of policy-specific indicators is that they yield easily interpretable measures and causal estimates. When the

have polarized too (Shor and McCarty 2011), it is plausible that party power has increased there as well (but see Mooney 2013, who finds no evidence that the formal powers of state speakers have increased since 1981). Another contributing factor is the decline in the non-policy benefits of holding office as patronage-oriented machines have been replaced by an activist base of issue-oriented "amateurs" (Wilson 1962). Since candidates should adopt more moderate (and thus electorally appealing) policy positions to the extent that they value holding office in itself (Calvert 1985), the decline of patronage politics has probably contributed to ideological divergence as well.

concept of interest is the overall orientation of state policies, however, individual policies are often inadequate. A state's minimum wage, for example, is at best a partial indicator of the liberalism of its economic policies, let alone its policies in other domains.[6] Another downside of focusing solely on continuous policies such as taxes and expenditures is that it ignores categorical policies like the abortion restrictions enacted by North Carolina Republicans after the 2012 election. Finally, relying on a few noisy policy indicators leads to 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 studies focusing on individual policies have typically found significant (sometimes large) partisan effects on a few policies but null results for many others. For the same reasons, studies of city policies have often found similar patterns of results (e.g., Ferreira and Gyourko 2009; Gerber and Hopkins 2011).

To address these problems, many studies of state policy rely on indices, factor scores, or other holistic summaries of the liberalism of state policies (e.g., Hofferbert 1966; Erikson, Wright, and McIver 1993). Such composite measures substantially reduce measurement error and thus increase 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). 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. A major disadvantage of the composite approach, however, has been the difficulty of constructing time-varying measures of state policy liberalism. Because of this, all existing analyses of the determinants of state policy liberalism employ cross-sectional designs inimical to credible causal inferences.

In our analysis, we utilize the dynamic measure of state policy liberalism recently developed by Caughey and Warshaw (2016), who use a dataset of nearly 150 policies to estimate

6. Adcock and Collier (2001) call this a failure of content validation.

a policy liberalism score for each state in each year between 1936 and 2014. 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).[7] The policy dataset Caughey and Warshaw used to estimate these scores was designed to include all politically salient state policy outputs on which comparable data are available for at least five years.[8] The data cover a wide range of policy areas, including social welfare (e.g., AFDC/TANF benefit levels), taxation, labor (e.g., right-to-work), civil rights (e.g., fair housing laws), women's rights (e.g., jury service 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 decriminalization). Despite the diversity of policies, Caughey and Warshaw (2016) find little evidence that policy variation across states is multidimensional, and they report that 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.[9]

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.[10] The second column indicates the percentage of dichotomous policies

7. The model 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. This has the effect of dampening shifts that are common to all states. If, instead, the intercepts are held constant, the policies of all states are estimated to have become substantially more liberal, especially before the 1980s. The precise structure of the item parameters in the policy model do not significantly affect our results, however, since our estimation strategies net out shifts in policy liberalism common to all states.

8. 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).

9. For further details on the policy liberalism measure, see Sections A.2-A3 of the Supplementary Information and Caughey and Warshaw (2016).

10. The policy liberalism scores have zero-mean and unit-variance across state-years. In a typical year, the cross-sectional SD is around 0.9.

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

	Policy Liberalism	Pct. Lib.	Women on Juries	Labor Anti-Injunction	Housing Aid	Fair Empl. Commiss.	AFDC Benefit
Year = 1950							
MS	−1.35	28%	0	0	0	0	$460
DE	−0.94	30%	1	0	0	0	$642
MT	0.05	44%	1	1	0	0	$838
WI	0.93	56%	1	1	1	0	$1028
MA	1.33	62%	1	1	1	1	$1036

	Policy Liberalism	Pct. Lib.	Corporal Punish. Ban	Prevailing Wage Law	Medicaid Abortion	Greenhouse Gas Cap	TANF Benefit
Year = 2010							
MS	−2.29	17%	0	0	0	0	$253
VA	−0.89	33%	1	0	0	0	$262
NV	−0.13	45%	1	1	0	0	$304
MN	1.13	66%	1	1	1	0	$323
MA	2.02	77%	1	1	1	1	$352

on which the state had the liberal option.[11] In a typical year, a one-unit change in policy liberalism corresponds to a 14-point increase in a state's percentage of liberal policies. The next four columns provide examples of highly discriminating dichotomous policies of varying "difficulty," and the rightmost column provides an example of a continuous policy, average monthly AFDC/TANF benefits per recipient family.[12]

Figure 1 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 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. The realignment of the South is only partly responsible for this shift, for even in the non-South Republican states

11. There are 41 dichotomous policies available in 1950 and 45 in 2010.

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

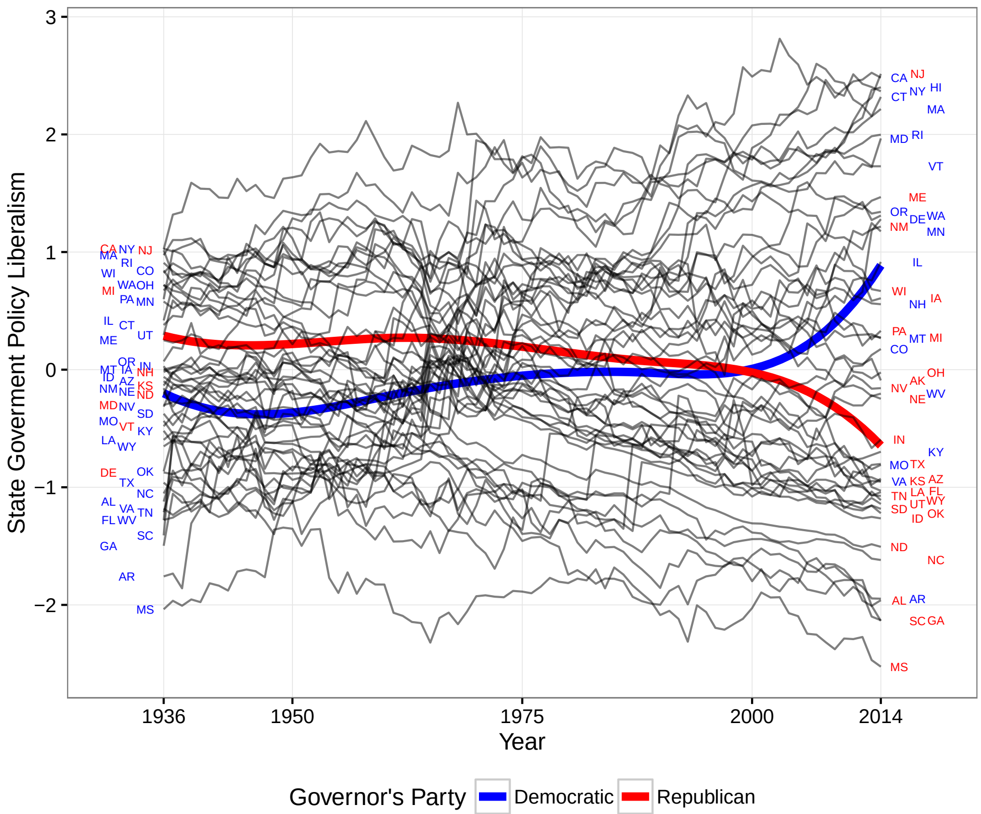

Figure 1: Yearly state policy liberalism, 1936–2014. Blue and red loess lines indicate the average policy liberalism of states with, respectively, Democratic and Republican governors.

were at least as liberal as Democratic ones until the late 1990s. Whether the increasing correlation between party control of government and policy is causal—and not simply the result of a better match between ideology and partisanship—is the subject of the empirical analyses in the next section.

Empirical Analysis of Policy Effects

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. In order to isolate the policy effects of partisan composition *per se*, we rely on two identification strategies. The first is an RD design, which exploits the exogenous variation in party control induced by narrowly decided 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 is a dynamic panel analysis, which exploits over-time variation within states while 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.

Regression-Discontinuity Analysis

Electoral regression-discontinuity (RD) designs exploit the fact that a sharp electoral threshold, 50% of the two-party vote share, determines which party controls a given office (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 (2014a, 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.

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 Information A.4, Table A3). The only worrisome covariate is contemporaneous *Policy Liberalism*, which is somewhat higher where the Democrat barely won. The imbalance disappears, however, when *Policy Liberalism* is converted to a first difference.[13] In light of the better balance on first-differenced *Policy Liberalism* as well as for increased statistical efficiency, we estimate treatment effects on changes in policy liberalism rather than on levels.

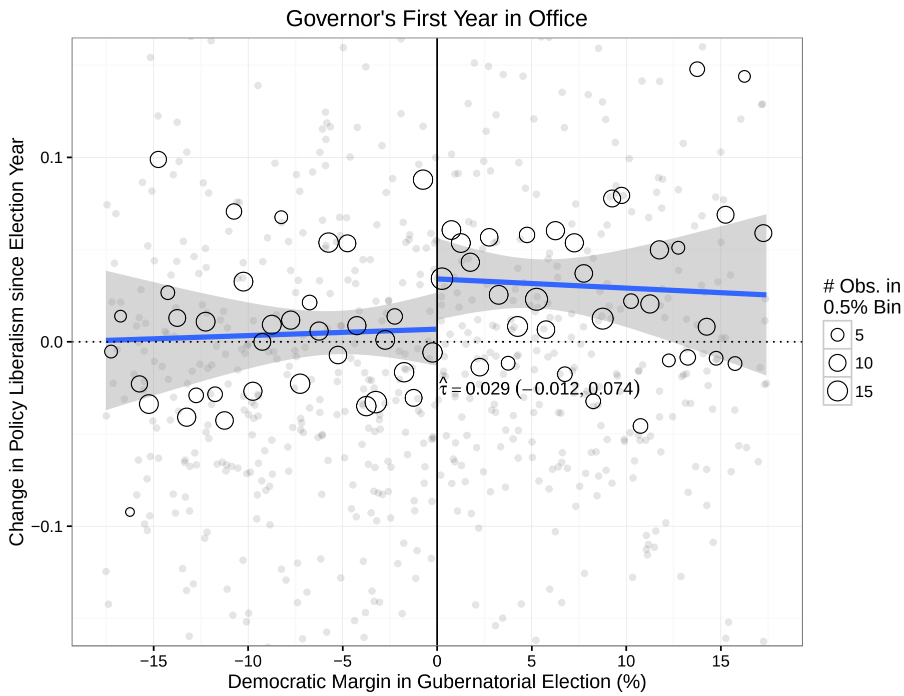

Figure 2: RD estimate of the effect of electing a Democratic governor on change in policy liberalism after the governor's first year in office. Estimates are based on triangular-kernel local linear regression, with MSE-optimal bandwidths and robust confidence intervals calculated by `rdrobust` (Calonico, Cattaneo, and Titiunik 2014a). Hollow circles are averages in 0.5% bins. Shaded 95% confidence intervals are based on conventional standard errors.

Figure 2 illustrates the estimation of the policy effects of Democratic governors (relative to Republican governors) using the electoral RD design. The dependent variable is change in policy liberalism between the year of the governor's election and the governor's first year in office. On average, barely electing a Democratic governor is estimated to increase change

13. 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.

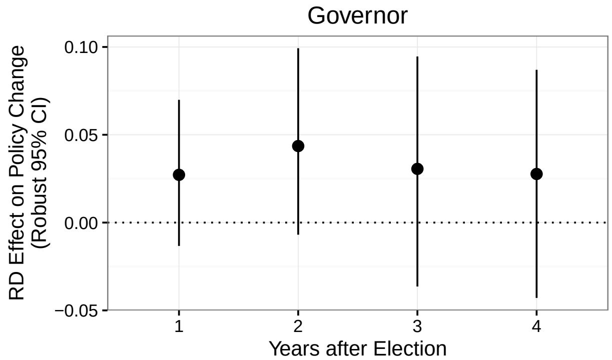

Figure 3: RD estimates of the effect of electing a Democratic governor, 1 to 4 years after the election.

in policy liberalism by about 0.03. Consistent with our expectations, this estimate is quite small relative to the variation in policy liberalism across states. Even the largest plausible one-year effect, which the confidence interval suggests is around 0.07, is less than one-tenth the cross-sectional standard deviation of *Policy Liberalism*.[14] Substantively, an effect of this size corresponds to about a one-point increase in a state's percentage of liberal policies.

Moreover, as Figure 3 indicates, there is little solid evidence that policy effects cumulate over time. The effect after two years is only a bit larger than the one-year effect, and the effects after three and four years are essentially the same magnitude as the first year, though less precisely estimated. It thus appears that any effect of electing a Democratic governor is accomplished by the governor's second year in office. One possible reason for this lack of cumulation is that winning a gubernatorial election typically causes a party to lose seats in the next state legislative election (Folke and Snyder 2012), which could in turn lead to countervailing policy shifts. Indeed, voters' desire to counterbalance gubernatorial policy effects by electing a legislature of the opposing party may be a primary mechanism for such midterm slumps (cf. Alesina, Londregan, and Rosenthal 1993).[15]

14. 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. 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.

15. Note that some governors have two-year terms and others have four-year terms. However, we see no

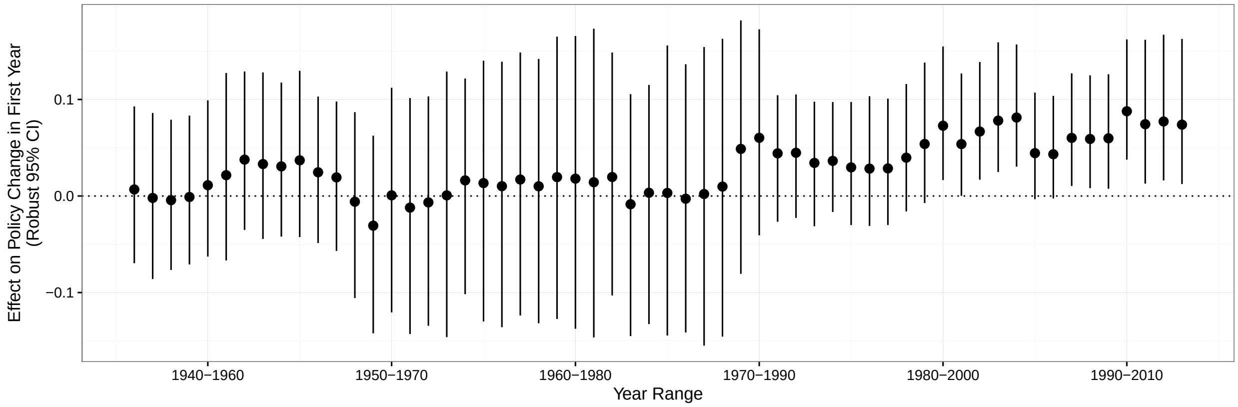

Figure 4: Changes in gubernatorial policy effects across the 1936–2014 period.

These local average treatment effect (LATE) estimates, however, conceal substantial temporal heterogeneity in the effect of partisan control. Mirroring the cross-sectional correlations plotted in Figure 1, the policy consequences of electing a Democratic governor have grown markedly, particularly in recent decades. These changes are visualized in Figure 4, which plots the evolution of gubernatorial policy effects over time. Each point and confidence interval in this plot corresponds to the gubernatorial RD estimate in a two-decade window. That is, the leftmost point is the estimated effect on one-year policy change for the period 1936–56, and the rightmost one is the same estimate for 1994–2014. This figure shows that through the 1970s, Democratic governors had essentially no estimated effect on policy liberalism. The magnitude of the estimates jumps up in the 1969–89 window, but not until a decade later do the estimates become unambiguously positive. Between 1980 and 2014, the estimates hover around 0.06—approximately double the LATE estimate for the whole 1936–2014 period.

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 what Figure 1 shows for governor: negative until around 1975, then non-existent until the end of the 20th century, when a

difference in the cumulation of policy effects across states with different term lengths.

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).[16] 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.[17] 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.[18] 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.[19] None of the covariates exhibit statistically significant discontinuities, though the estimates of imbalance are somewhat less precise than in the gubernatorial RD (Supplementary Information A.4, Table A4).

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

17. For related multidimensional approaches to RD, see Reardon and Robinson (2012) 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.

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

19. 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 legislature.

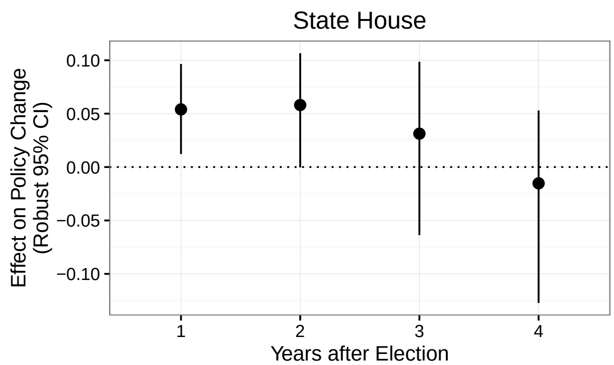

Figure 5: RD estimates of the effect of electing a Democratic state house, 1 to 4 years after the election.

Figure 5 plots the RD estimates of the policy effects of narrowly elected Democratic house majorities. Overall, the results are similar to those for governor. Narrowly electing a Democratic house majority causes a 0.05 increase in policy liberalism change after one year but no additional increase in the second year. Beyond the second year, these effects dissipate even more sharply than for governors. Indeed, the point estimate four years after the election is slightly negative, indicating that the positive effects of the first year are wiped out by the fourth year. As with governors, this could be the result of the endogenous political response to policy changes in the first two years, a possibility supported by the fact that narrowly winning a legislative majority decreases a party's probability of controlling the legislature in the future (Feigenbaum, Fouirnaies, and Hall 2015). Finally, Figure 4 shows that like gubernatorial policy effects, legislative policy effects have also grown over time. From a baseline of essentially zero, the one-year effect of electing a Democratic house has gradually climbed, reaching 0.08 by the end of the period and showing no signs of slowing.

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 analyses reported above with an analysis that exploits within-state partisan variation in

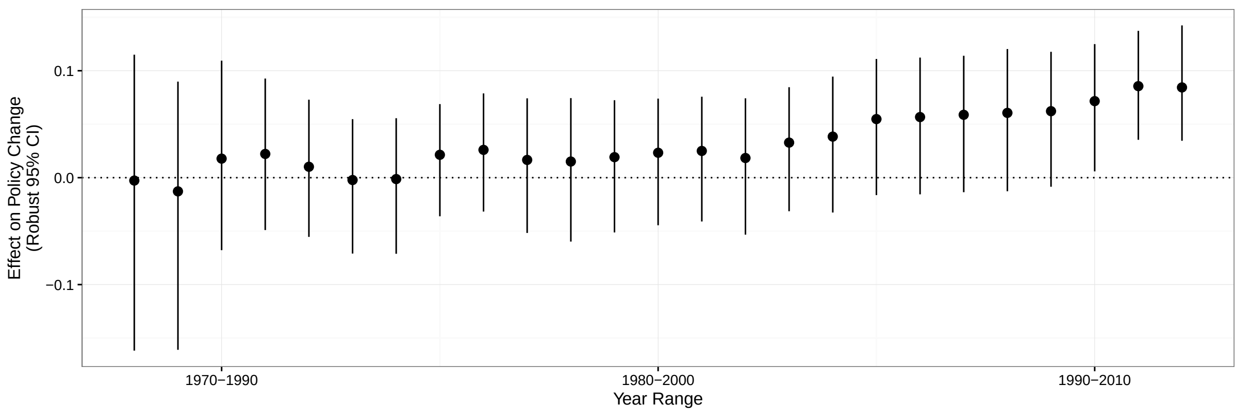

Figure 6: Changes in legislative policy effects across the 1968–2012 period.

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).[20] If unobserved confounding across states were constant across time and year-specific shocks affected all states equally, then the effect of a Democratic governor would be identified under a two-way fixed-effect (FE) model. This model, 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.[21] We therefore estimate dynamic panel models with two-way FEs and lagged values of our dependent variable (Beck and Katz 2011):

$$y_{it} = \delta Gov_{it} + \beta Maj^{H}_{it} + \gamma Maj^{S}_{it} + \sum_{l=1}^{L} \rho_l y_{i,t-l} + \alpha_i + \xi_t + \epsilon_{it}, \qquad (1)$$

where Gov_{it} indicates a Democratic governor; Maj^{H}_{it} indicates a Democratic house majority;

20. For details see Supplementary Information A.5.

21. Another concern with the two-way FE model is that lagged dependent variables (LDVs) are potential confounders. This is because state policies change incrementally, and thus are highly correlated over time; meanwhile, policy outcomes could also affect the partisan composition of state government.

Maj_{it}^S indicates a Democratic senate majority; $y_{i,t-l}$ is state i's policy liberalism l years before t; ρ_l is the coefficient on the l-th lag; and α_i and ξ_t are, respectively, state- and year-specific intercepts.[22] All of the panel results reported in this paper are qualitatively robust to alternative estimation strategies.[23]

Table 2: Policy Effects of Democratic Control the Governorship, State House, and State Senate

Outcome variable	Policy Liberalism t				
	Full sample			Non-south	South
	(1)	(2)	(3)	(4)	(5)
Dem. Governor	**0.065**	**0.012**	**0.014**	**0.010**	0.022
	(0.031)	(0.004)	(0.007)	(0.004)	(0.012)
Dem. House Majority	**0.165**	**0.030**	**0.043**	**0.032**	0.014
	(0.051)	(0.006)	(0.013)	(0.007)	(0.011)
Dem. Senate Majority	**0.271**	**0.021**	0.008	**0.021**	−0.023
	(0.058)	(0.006)	(0.012)	(0.006)	(0.011)
Dem. House Majority × Dem. Senate Majority			−0.002		
			(0.017)		
Dem. Governor × Dem. House Majority			**−0.032**		
			(0.016)		
Dem. Governor × Dem. Senate Majority			0.009		
			(0.015)		
Dem. Governor × Dem. House Majority × Dem. Senate Majority			0.025		
			(0.021)		
State & Year FEs	✓	✓	✓	✓	✓
Policy Liberalism $t-1$		✓	✓	✓	✓
Policy Liberalism $t-2$		✓	✓	✓	✓
Observations	3,678	3,586	3,586	2,749	837
States	49	49	49	38	11
R-squared	0.871	0.988	0.988	0.983	0.947

Note: Standard errors produced by block bootstraps (clustered at the state level) of 1,000 times are in the parentheses. Nebraska is not included in the sample. Coefficients statistically significant at the 5% level are in bold font type.

Table 2 shows the results from the dynamic panel analysis. We first report gubernato-

22. The FE-LDV estimator of δ in (1) is biased (Nickell 1981), but when the number of time periods is large, as it is in our case, the bias is a minor concern (Beck and Katz 2011; Gaibulloev, Sandler, and Sul 2014). Non-stationarity is also not a problem in our application (see Supplementary Information A.6).

23. We explored a variety of alternative 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). For details, see Supplementary Information A.8. 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 LDVs do, and that latent factors are not necessary once LDVs are included.

rial estimates based on the conventional two-way FE model without LDVs in column (1). These (implausible) two-way FE estimates suggest that relative to Republicans,[24] Democratic governors increase state policy liberalism by about 0.07, and that Democratic control of the state house and senate increases it by 0.17 and 0.27, respectively. The estimates shrink dramatically, however, if we control for LDVs. Column (2) reports the results from our preferred baseline specification, a FE-LDV model with two lagged terms, as specified by Equation (1) with $l = 2$.[25] Under this specification, the estimated immediate effects of a Democratic governor, Democratic control of the house, and Democratic control of the senate are 0.01, 0.03, and 0.02, respectively.[26] All three estimates remain highly statistically significant, but the point estimates are an order of magnitude smaller than the FE model. This strongly suggests that FEs alone do not adequately account for within-state trends in policy liberalism and are likely to overestimate policy effects (for further evidence on this point, see Supplementary Information A.8).

It is important to note that the effect of a Democratic legislative majority has a different interpretation in the dynamic panel analysis 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 since the party in control typically has more than a bare majority. This conceptual difference notwithstanding, the estimates for majority control barely change if we control for seat share because share has little independent association with policy liberalism (Supplementary Information A.10). Indeed, for both state house and governor, the panel estimate are somewhat smaller than (though statistically indistinguishable from) the corresponding RD estimate, suggesting that parties

24. Among the 3,630 state year observations, only 29 have independents as governors. Dropping these observations does not change our main finding at all.

25. The gubernatorial estimate remain very stable if we control for more than two LDVs; see Supplementary Information A.9.

26. In a dynamic panel model, a treatment will affect not only the contemporaneous outcome, but also outcomes in future periods through the channel of the LDVs. The effect on the contemporaneous outcome is often called the "immediate" effect.

receive little additional policy benefit if they win control by a larger-than-bare margin. Table 2 also explores the possibility that the policy effects of one institution depend on party control of other institutions. We might expect, for example, that capturing the governorship yields greater policy benefits if the same party also controls both houses of the legislature. However, there is no clear evidence of positive interaction effects between the coefficients.[27]

Next, we examine whether the results differ between the South and non-South. As column (4) of Table 2 shows, the results for the non-South are substantively similar to (and statistically indistinguishable from) those for the whole sample. This makes sense because both the RD and dynamic panel analyses implicitly place greater weight on 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. Due to the lack of partisan variation in Southern states, the estimates for the South are very imprecise, and none are distinguishable from zero.

Partisan Polarization and the Growth in Party Effects on Policy

We saw in Figures 4 and 6 that partisan effects on policy have grown markedly, especially in the last quarter century. What has driven these increases? One obvious potential culprit is polarization in the policy preferences (whether sincere or induced) between Democratic and Republican candidates and officeholders, which is well documented among members of Congress and other national politicians (e.g., Layman, Carsey, and Horowitz 2006). If, as seems likely, the policy positions of state-level politicians have also diverged by party, we should expect them to pursue increasingly distinct policies in office, thus increasing partisan effects on policy. Moreover, to the extent that government officials are responsive to their partisan subconstituencies, we should also expect elite polarization—and thus partisan effects on policy—to be larger where the policy preferences of Democrats and Republicans in the public diverge more (Clinton 2006; Adams and Merrill 2008; Jacobson 2012).

27. Supplementary Information A.7 shows a graph of these interactions.

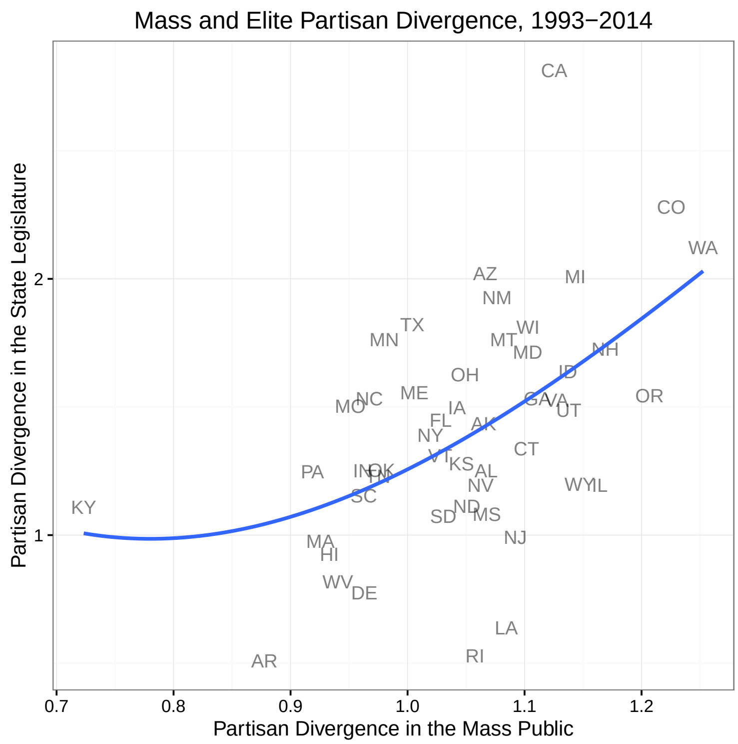

Figure 7: Relationship between mass partisan divergence (Caughey, Dunham, and Warshaw 2016) and elite partisan divergence (Shor and McCarty 2011), averaged within states across the 1993–2014 period. The fitted line is a three-knot natural spline.

Preliminary evidence for this last point is provided by Figure 7, which plots the cross-sectional relationship between elite and mass partisan divergence. We measure elite divergence (vertical axis) as the ideological distance between the median Democrat and median Republican in the state legislature, which Shor and McCarty (2011) have estimated annually since 1993. Analogously, we measure mass divergence (horizontal axis) as the ideological distance between the average Democrat and average Republican identifier in the state public, using the estimates of mass-level economic policy liberalism developed by Caughey, Dunham, and Warshaw (2016). This measure, available for each state in each year between 1946 and 2014, was derived from a dynamic group-level item-response model of over 800,000 survey respondents' preferences on economic issues (Caughey and Warshaw 2015).[28] Plotting the within-state averages of both measures over the 1993–2014 period, Figure 7 shows that al-

28. See Supplementary Information A.11 for a more comprehensive description of the measure of opinion divergence between Democrats and Republicans in each state.

though their correlation is not perfect ($r = 0.5$), states with greater mass divergence clearly tend to have more polarized state legislatures.

Next, we examine whether partisan effects on policy also tend to be larger where mass and elite divergence is greater. To simplify the analysis, we create a modified version of the panel model in Equation (1) that includes a variable indicating the proportion of government offices/chambers (i.e., governorship, state house, and state senate) controlled by the Democratic Party.[29] The first column of Table 3 reports the results of a specification that interacts this *Democratic Control* variable with indicators for three time periods: 1936–1968, 1969–1991, and 1992–2014.[30] Consistent with the RD estimates in Figures 4 and 6, the coefficient estimates indicate that the effect of *Democratic Control* was roughly constant in the first two periods but doubled in magnitude after 1991. As column 2 shows, the results are qualitatively identical if we restrict the analysis to the years for which mass partisan divergence is available (1947–2014). If we also interact *Democratic Control* with lagged *Mass Divergence*, however, the former's interaction with the post-1992 dummy is reduced to insignificance. This suggests, though hardly proves, that era indicators may simply be proxying for changes in mass divergence over time.

Ideally we would conduct the same analysis for *Elite Divergence*, but the Shor-McCarty state legislative ideal points do not extend before 1993. Nevertheless, we can still examine whether *Elite Divergence* moderates the effect of *Democratic Control* in the post-1993 period. The answer, provided by Column (4), is a clear yes. The coefficient estimate for the interaction of *Democratic Control* and lagged *Elite Divergence* indicates that the former's effect increases by 0.05 for every standard deviation increase in the latter.[31] This result persists even if *Democratic Control* is also interacted with dummies for state and year, which indicates that the moderating effect of *Elite Divergence* is not driven by national time trends

29. The linearity assumption implied by the use of this index seems reasonable in light of the roughly additive effects of different offices reported in Table 2.

30. We defined the eras in this way because they divide the years which our measure of mass partisan divergence is available into three equal parts.

31. We tested the validity of the multiplicative interaction models using diagnostic tools proposed by Hainmueller, Mummolo, and Xu (2016). Both the overlap and linearity assumptions appear to be valid.

Table 3: Moderators of Partisan Effects on Policy.

Outcome variable	Policy Liberalism t				
	(1)	(2)	(3)	(4)	(5)
Dem. Control	**0.055**	**0.063**	0.042	−0.024	−0.089
	(0.018)	(0.022)	(0.026)	(0.046)	(0.058)
Dem. Control × Era 1969–1991	−0.014	−0.020	−0.027	NA	NA
	(0.022)	(0.024)	(0.024)		
Dem. Control × Era 1992–2014	**0.066**	**0.061**	0.029	NA	NA
	(0.022)	(0.026)	(0.026)		
Mass Divergence$_{t-1}$			−0.025		−0.005
			(0.015)		(0.068)
Dem. Control × Mass Divergence$_{t-1}$			**0.015**		**0.028**
			(0.008)		(0.013)
Elite Divergence$_{t-1}$				−0.027	−0.020
				(0.018)	(0.019)
Dem. Control × Elite Divergence$_{t-1}$				**0.049**	**0.038**
				(0.014)	(0.013)
Years Covered	1936–2014	1947–2014	1947–2014	1994–2014	1994–2014
State & Year FEs	✓	✓	✓	✓	✓
State × Era FEs	✓	✓	✓	NA	NA
Policy Liberalism $t-1$	✓	✓	✓	✓	✓
Policy Liberalism $t-2$	✓	✓	✓	✓	✓
Observations	3,586	3,182	3,182	812	812
States	49	49	49	49	49
R-squared	0.989	0.989	0.989	0.995	0.995

Note: Standard errors produced by block bootstraps (clustered at the state level) of 1,000 times are in the parentheses. Nebraska is not included in the sample. Coefficients statistically significant at the 5% level are in bold font type. Measures of mass partisan divergence and elite partisan divergence are rescaled based on their standard deviations during the period of 1994–2014.

in partisan policy effects or by durable state differences in these effects. It is interesting to note that the implied effect of *Democratic Control* when the party medians in the legislature are equal is essentially 0, as one would expect if candidates converged on the same policy positions.[32] Finally, the rightmost column of Table 3 demonstrates that both *Mass Divergence* and *Elite Divergence* continue to moderate *Democratic Control* when they are included in the same model. This suggests that *Mass Divergence* may lead to or proxy for ideological

32. No state is estimated to have no elite divergence, but some get quite close. The least polarized state-year is Arkansas in 1993, whose *Elite Divergence* score is 0.4 (the average score across state-years is 3).

differences between Democratic and Republican candidates that are not fully captured by roll-call patterns in the state legislature.

Taken together, the evidence presented in this section corroborates the hypothesis that the magnitude of party effects is a function of the ideological distance between candidates of different parties. More tentatively, they also suggest that the size of policy effects may be influenced by the mass public as well, whether through electoral pressures to cater to more-or-less extreme primary electorates or some other mechanism. Given that partisan divergence has increased at both the mass and elite levels (Hill and Tausanovitch 2016; Caughey, Dunham, and Warshaw 2016), these results thus provide a potential explanation for the growth of partisan effects on state policy.

Discussion and Implications

Commenting on state politics around 1980, Erikson, Wright, and McIver observed that Democratic and Republican parties in each state "respond to state opinion—perhaps even to the point of enacting similar policies when in. . . control" (1993, 121). Based on an analysis spanning eight decades, we come to similar conclusions about statehouse democracy at that time. Before the 1990s, electing Democratic rather than Republican governors and legislatures generally had small effects on the liberalism of state policies. Since Erikson, Wright, and McIver's seminal analysis, however, partisan effects have grown rapidly, and 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, a trend that seems at least partly attributable to the growing ideological gap between the parties' candidates and electoral coalitions.

The substantive magnitude of contemporary policy effects, however, 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. As Table 1 suggests, an effect of

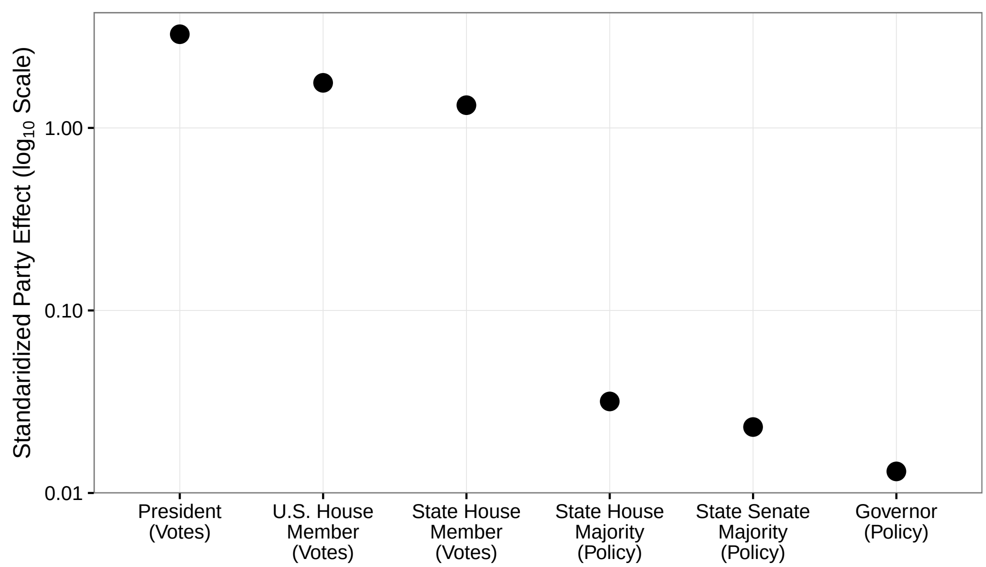

Figure 8: Position effects and policy effects. The right three quantities are counterfactual differences in roll-call ideal points between Republicans and Democrats occupying the same office. The left three are analogous estimated effects of party control on state policy liberalism. For comparability, 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.

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.[33] The substantive magnitude of partisan effects on policy also pales relative to the cross-sectional differences between states. The estimated policy effect of a switch in unified party control in recent decades is one-tenth the size of the typical difference between states, suggesting that many decades of Republican governors and legislatures would be required to make the policies of Massachusetts as conservative as those of Mississippi.[34]

As a final point of comparison, consider the focus of most research on partisan polar-

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

34. Of course, this hypothetical comparison glosses over two complications. First, Massachusetts Republicans are less conservative than Mississippi Republicans, so party effects may differ across states (see Erikson, Wright, and McIver 1993, 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.

ization: the difference between candidates' policy positions, as measured by their roll-call records, campaign platforms, or financial supporters (e.g., Poole and Rosenthal 1984; Ansolabehere, Snyder, and Stewart 2001; Bonica 2014). We can call such differences *position effects*. Numerous studies have found that party affiliation is by far the most powerful predictor of politicians' policy positions, at both the national and the state level (e.g., Shor and McCarty 2011). Figure 8 confirms this finding, showing that there is a difference of 1 to 4 standard deviations in the ideal points of otherwise similar presidents, U.S. House members, and state house members from opposing parties (left three dots).[35]

By contrast, analogously standardized policy effects are nearly two orders of magnitude smaller.[36] Of course, the two sets of quantities are not fully comparable—some are defined at the individual level, others at the level of the office or body—and standardizing the estimates does necessarily not put them on the same scale as each other, let alone the same scale as citizens. But the vast differences in magnitude between position and policy effects cannot help but cast a very different light on partisan polarization. In particular, they call into question the concern that alternation in party control leads to "wide swings in policy" that "do not well represent the interests of middle-of-the-road voters" (Poole and Rosenthal 1984, 1061). Whether due to status quo bias, the necessity of compromise, or the realities of policymaking as opposed to symbolic position taking, the effects of party control appear much less dramatic by the metric of actual policy outcomes. Even if the policy *positions* of politicians from different parties "leapfrog" over the citizens they represent (Bafumi and Herron 2010), partisan control of government has only incremental effects on policy *outcomes*. In short, Democrats and Republicans may disagree consistently and even violently, but the actual policy consequences of these disagreements are far less dramatic.

35. 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).

36. These are the estimates reported in column (2) of Table 2, divided by the standard deviation of policy liberalism across states in a typical year.

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A Supplementary Information for "The Policy Effects of the Partisan Composition of State Government"

Appendix: Table of Contents

A.1 Summary of Previous Studies on the Partisan Composition of State Government

Table A1 summarizes a sample of major previous studies on the policy effects of the partisan composition of state government. Overall, it shows that the state politics literature exhibits little agreement regarding the policy effects of partisan control of state government. Most previous studies, including those with strong designs, find that control of the governorship generally does not affect policy. For the state legislature, some previous studies conclude that party control matters for at least some policies, while others conclude that party control of the legislature has little or no effect on policy. Thus, there continues to be a vigorous debate about whether it matters for policy whether Democrats or Republicans control the governorship and state legislature.

Table A1: Sample of Major Previous Studies on the Partisan Composition of State Government

Study	Policy Outcome(s)	Time Period	Research Design	Significant Effect?
Party Control of Governor				
Garand (1988)	Spending	1945-84	Time-Series	No
Smith (1997)	Welfare	1977-92	Panel	No
Norrander and Wilcox (1999)	Abortion	1992	Cross-Sectional	No
Kousser (2002)	Medicaid	1980-93	Panel	No
Jacobs and Carmichael (2002)	Death Penalty	1971-91	Panel	No
Besley and Case (2003)	Multiple Policies	1953-99	Panel	Mixed
Klarner (2003)	Welfare	1970-96	Panel	No
Yates and Fording (2005)	Imprisonment Rates	1975-1995	Panel	Yes
Reed (2006)	Tax Burdens	1960-2000	Panel	No
Konisky (2007)	Environmental Policy	1985-2000	Panel	No
Leigh (2008)	Multiple Policies	1941-2002	RDD	Mixed
Lax and Phillips (2011)	Multiple Policies	2000s	Cross-Sectional	No
Fredriksson, Wang, and Warren (2013)	Tax Policies	1970-2007	RDD	Mixed
Party Control of State Legislature				
Hanson (1984)	Medicaid	1977	Cross-Sectional	No
Plotnick and Winters (1985)	Welfare	1971-71	Cross-Sectional	No
Garand (1988)	Spending	1945-84	Time-Series	No
Erikson, Wright, and McIver (1993)	Policy Liberalism	1̃980	Cross-Sectional	No
Smith (1997)	Welfare	1977-92	Panel	Yes
Norrander and Wilcox (1999)	Abortion	1992	Cross-Sectional	Yes
Kousser (2002)	Medicaid	1980-93	Panel	Mixed
Besley and Case (2003)	Multiple Policies	1953-99	Panel	Mixed
Klarner (2003)	Welfare	1970-96	Panel	Yes
Yates and Fording (2005)	Imprisonment Rates	1975-1995	Panel	Yes
Reed (2006)	Tax Burdens	1960-2000	Panel	Yes
Chen (2007)	Civil Rights	1968-87	Panel	Yes
Konisky (2007)	Environmental Policy	1985-2000	Panel	Mixed
Lax and Phillips (2011)	Multiple Policies	2000s	Cross-Sectional	No
Additive Index of Party Control of Governor and State Legislature				
Hofferbert (1966)	Multiple Policies	1952-62	Cross-Sectional	No
Winters (1976)	Redistribution	1965	Cross-Sectional	No
Dye (1984)	Welfare Spending	1950-80	Cross-Sectional	No
Alt and Lowry (1994)	Expenditures	1968-87	Panel	Yes
Brown (1995)	Welfare Effort	1941-64	Panel	Yes

A.2 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 less than 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 A2 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 accommodations?
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 explicitly 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 dealers?
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-Injunction 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 Licensing	1936-1951	Architect Licensing
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 Regulatory Policies:		
Anti-sedition laws	1936-1955	Does the state have anti-sedition laws?
Forced sterilizations	1945-1974	Does the state have a forced sterilization program?
Grandparents' Visitation Rights	1964-1987	Does the state have a law guaranteeing grandparents' visitation rights?
Hate Crimes Ban	1981-2014	Are hate crimes explicitly 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 A2 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 the 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 administrative enforcement) banning discrimination in public accommodations (pre-CRA)?
Ban discrimination in public accommodations	1964-2010	Did the state pass a law (with administrative enforcement) banning discrimination in public accommodations (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 helmets 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 A2 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 benefits 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.3 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 2016). 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:

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

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

$$\Pr(\tau_{j,k-1} < y^*_{jst} \leq \tau_{jk} \mid \beta_j \theta_{st} - \alpha_{jt}) = \Phi(\tau_{jk} - [\beta_j \theta_{st} - \alpha_{jt}]) - \Phi(\tau_{j,k-1} - [\beta_j \theta_{st} - \alpha_{jt}]), \quad (3)$$

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

$$\Pr(\tau_{j1} < y^*_{jst} \leq \tau_{j2} \mid \beta_j \theta_{st} - \alpha_{jt}] = \Phi(\beta_j \theta_{st} - \alpha_{jt}), \quad (4)$$

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.4 Further Details on Validity of Regression Discontinuity Design

This section presents more details about the validity of our regression discontinuity design for the policy effects of party control of government. Consistent with past work (Folke and Snyder 2012; Eggers et al. 2015), we find no significant discontinuities in the partisan composition of state government at the time of the gubernatorial or state house elections (Tables A3 and A4).

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. 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	0.17	0.01	(−0.16, 0.19)	0.86
Dem. Seat Share in House	0.20	−0.02	(−0.07, 0.05)	0.73
Dem. Majority in Senate	0.16	0.00	(−0.18, 0.19)	0.97
Dem. Seat Share in Senate	0.18	−0.01	(−0.07, 0.07)	0.92
Policy Liberalism (level)	0.14	0.06	(−0.22, 0.39)	0.60
Policy Liberalism (change)	0.20	−0.02	(−0.06, 0.02)	0.37

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	0.08	(−0.10, 0.25)	0.38
Dem. Majority in House	34	0.10	(−0.09, 0.22)	0.29
Dem. Seat Share in House	34	0.02	(−0.02, 0.04)	0.51
Dem. Majority in Senate	49	0.06	(−0.15, 0.21)	0.75
Dem. Seat Share in Senate	52	0.02	(−0.01, 0.05)	0.39
Policy Liberalism (level)	54	−0.11	(−0.39, 0.12)	0.29
Policy Liberalism (change)	62	0.00	(−0.03, 0.05)	0.88

A.5 Dynamic Effects of Partisan Composition

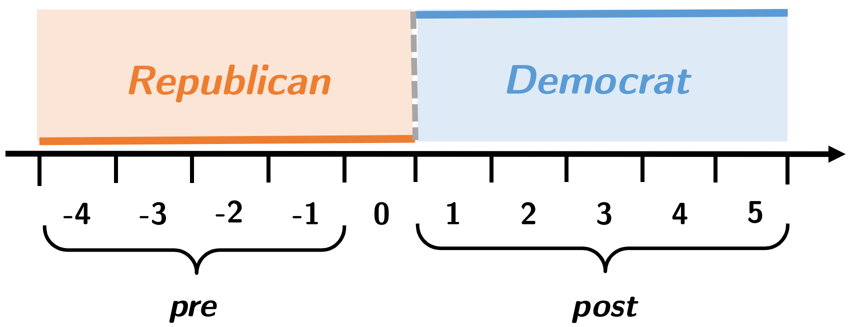
The identifying assumption of the dynamic panel model we use states that in the absence of the treatment, the average outcome of treated units would have been similar to that of the control units after fixed effects and lagged dependent variables are controlled for. In other words, after conditioning 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 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:

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

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 A2.

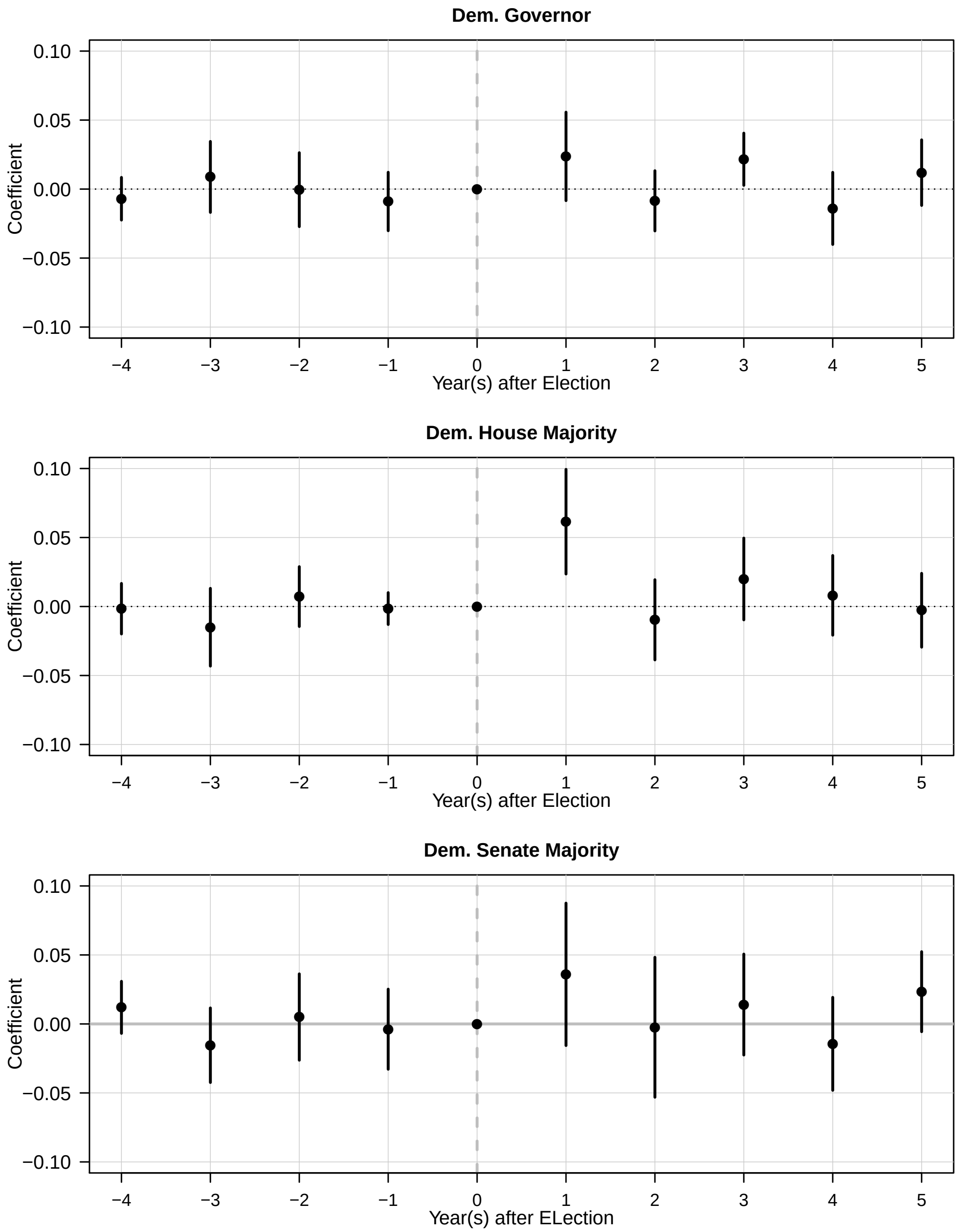
Figure A1: Indicator Definitions in Equation (5)



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 A2. 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 A2.

Figure A2 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 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.

Figure A2: Dynamic Changes of the Immediate Partisan Effects



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):

$$\Delta y_{it} = (\rho_1 - 1)y_{i,t-1} + \delta Gov_{it} + \beta Maj^{\mathrm{H}}_{it} + \gamma Maj^{\mathrm{S}}_{it} + \alpha_i + \xi_t + \epsilon_{it}, \tag{6}$$

$$\text{or} \qquad \Delta y_{it} = (\rho_1 - 1)y_{i,t-1} + \rho_2 y_{i,t-2} + \delta Gov_{it} + \beta Maj^{\mathrm{H}}_{it} + \gamma Maj^{\mathrm{S}}_{it} + \alpha_i + \xi_t + \epsilon_{it}, \tag{7}$$

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 (6) 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 2.

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).[37] In columns (3) and (4), we re-do the analysis by estimating Equation (7). 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	GMM	FE	GMM
	(1)	(2)	(3)	(4)
Dem. Governor	**0.013**	**0.019**	**0.012**	**0.018**
	(0.004)	(0.005)	(0.004)	(0.005)
Dem. House Majority	**0.028**	**0.032**	**0.030**	**0.033**
	(0.006)	(0.008)	(0.006)	(0.008)
Dem. Senate Majority	**0.023**	**0.023**	**0.021**	**0.020**
	(0.006)	(0.008)	(0.006)	(0.009)
Policy Liberalism $t-1$	**-0.051**	**-0.075**	**-0.134**	**-0.130**
	(0.007)	(0.014)	(0.016)	(0.049)
Policy Liberalism $t-2$			**0.088**	0.063
			(0.016)	(0.045)
State & Year FEs	X	X	X	X
Observations	3,632	3,632	3,586	3,586
States	49	49	49	49
R-squared	0.094	NA	0.099	NA

Note: Robust standard errors clustered at the state level are in the parentheses. Nebraska is not included in the sample. The outcome variable is the first difference of the Policy Liberalism measure. In Column (2), the outcome variable lagged for 2 to 5 periods are used as instruments for the lagged outcome variable. In Column (4), 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 exogenous. Coefficients statistically significant at the 5% level are in bold font type.

37. 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 Visualizing the Policy Effects of Party Control of Government

Table 2 explores the possibility that the policy effects of one institution depend on party control of other institutions. We might expect, for example, that capturing the governorship yields greater policy benefits if the same party also controls both houses of the legislature. As column (3) indicates, however, there is no clear evidence of positive interaction effects between the coefficients. Figure A3 presents these results visually. The x-axis lists four configurations of partisan control of the two chambers of the state legislature, and the y-axis plots the estimated policy effects of that legislative configuration under Republican (red) and Democratic (blue) governors. All the effects are relative to the baseline of unified Republican control (gray dashed line). Though the estimates are noisy due to multicollinearity and should thus be treated cautiously, the plot suggests that the marginal effect of party control is roughly additive for each institution. The estimated effect of unified Democratic relative to unified Republican control (rightmost point) is 0.07, which is approximately equal to the sum of the three main effects in column (2) of Table 2.

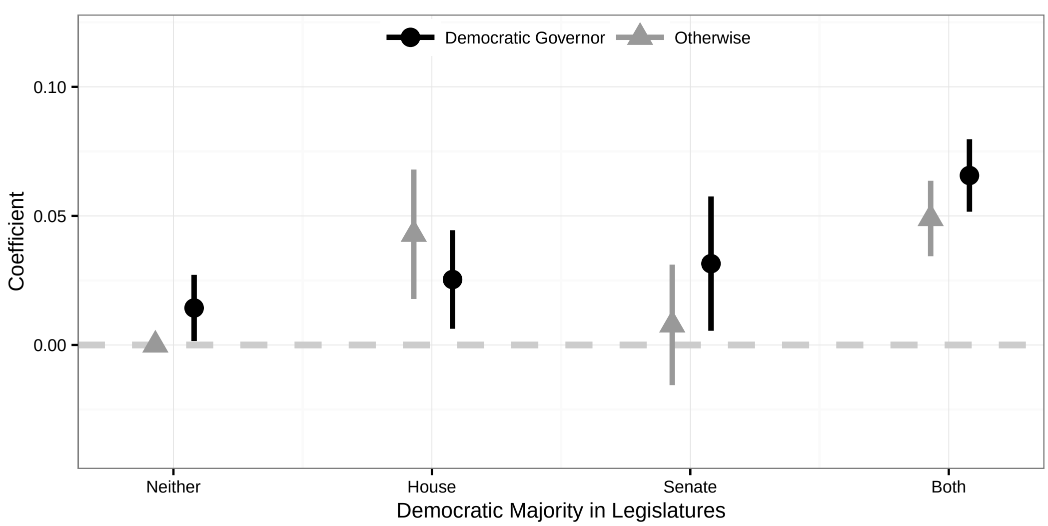


Figure A3: Predicted policy effects of different configurations of Democratic control, relative to the baseline of unified Republican control (red triangle).

A.8 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 2, column 2). However, the standard errors are much larger than those in Table 2, indicating improper model specifications that causes inefficiency, and potentially inconsistency.

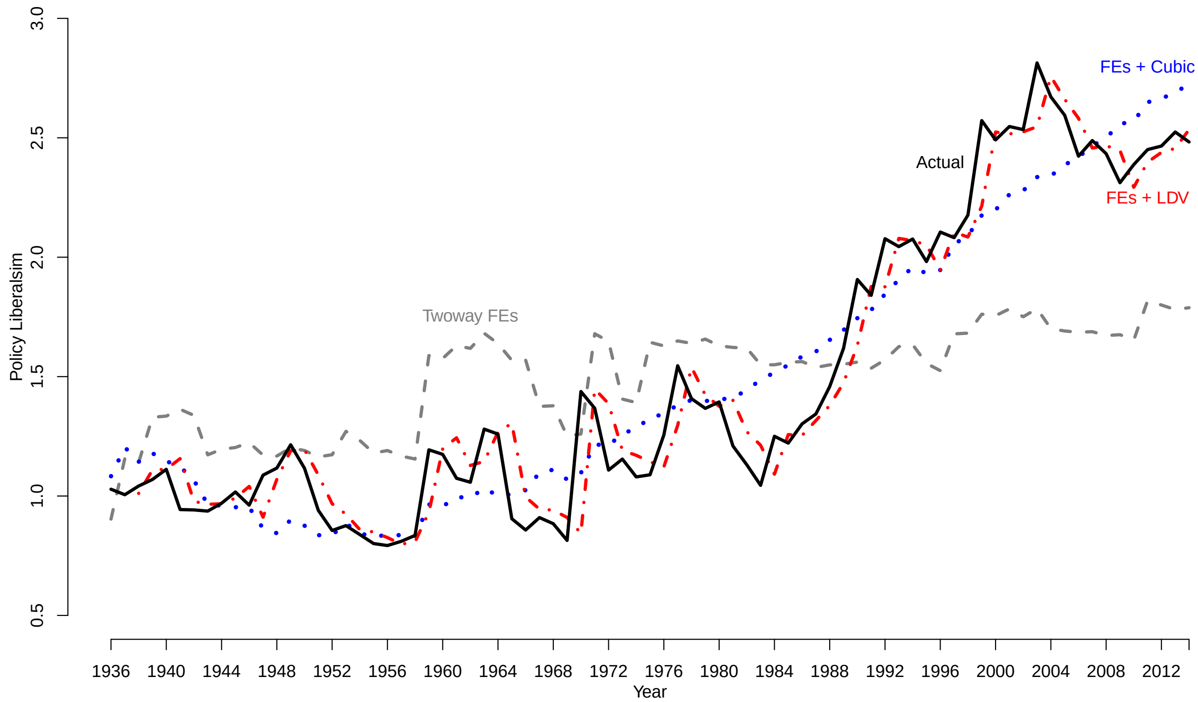
Table A6: Two-way Fixed-effect Models with Time Trends

Outcome variable	Policy Liberalism t			
	(1)	(2)	(3)	(4)
Dem. Governor	**0.065**	0.004	0.010	0.018
	(0.031)	(0.015)	(0.013)	(0.011)
Dem. House Majority	**0.165**	**0.083**	**0.083**	**0.081**
	(0.051)	(0.023)	(0.023)	(0.020)
Dem. Senator Majority	**0.271**	0.040	0.017	0.002
	(0.058)	(0.032)	(0.032)	(0.031)
State & Year FEs	X	X	X	X
State-specific Linear Time Trends		X		
State-specific Quadratic Time Trends			X	
State-specific Cubit Time Trends				X
Observations	3,678	3,678	3,678	3,678
States	49	49	49	49
R-squared	0.871	0.955	0.967	0.974

Note: Standard errors produced by block bootstraps (clustered at the state level) of 1,000 times are in the parentheses. Nebraska is not included in the sample. Coefficients statistically significant at the 5% level are in bold font type.

This specification problem is further illustrated in Figure A4, 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 A4 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 A4: Model Fits: The Example of California



A.9 The Number of Lagged Terms

In this section, we show that our main finding is robust to adding more lagged terms of the dependent variable. We report the gubernatorial estimates based on two-way FE models with varying numbers of lags. All standard errors (SEs) are clustered at the state level. In column (1) of Table A7, a two-way FE model without LDVs is employed. In columns (2)–(5), we estimate FE-LDV models with first- through fourth-order lags. We find that the estimates of the key independent variables barely change once two lagged terms are included and the third- and forth-order lags have limited predictive power of the dependent variable. Therefore, to avoid over-fitting, we use the FD-LDV model with two lagged terms as the baseline specification.

Table A7: Policy Effects of Democratic Control: Number of Lagged Terms Included

Outcome variable	Policy Liberalism t				
	(1)	(2)	(3)	(4)	(5)
Dem. Governor	**0.065**	**0.013**	**0.012**	**0.012**	**0.012**
	(0.031)	(0.003)	(0.004)	(0.004)	(0.004)
Dem. House Majority	**0.165**	**0.028**	**0.030**	**0.031**	**0.032**
	(0.051)	(0.006)	(0.006)	(0.006)	(0.006)
Dem. Senate Majority	**0.271**	**0.023**	**0.021**	**0.021**	**0.020**
	(0.058)	(0.006)	(0.006)	(0.006)	(0.006)
Policy Liberalism $t-1$		**0.949**	**0.866**	**0.865**	**0.866**
		(0.008)	(0.016)	(0.017)	(0.017)
Policy Liberalism $t-2$			**0.088**	**0.082**	**0.074**
			(0.016)	(0.022)	(0.022)
Policy Liberalism $t-3$				0.007	-0.026
				(0.019)	(0.024)
Policy Liberalism $t-4$					0.042
					(0.019)
State & Year FEs	X	X	X	X	X
Observations	3,678	3,632	3,586	3,540	3,493
States	49	49	49	49	49
R-squared	0.871	0.987	0.988	0.988	0.988

Note: Standard errors produced by block bootstraps (clustered at the state level) of 1,000 times are in the parentheses. Nebraska is not included in the sample. Coefficients statistically significant at the 5% level are in bold font type.

A.10 Disentangling Seat Share and Majority Status

The dynamic panel models reported in the main text 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 A8, 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 stable 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.

Table A8: Disentangling Share and Control

Outcome variable	Policy Liberalism t			
	(1)	(2)	(3)	(4)
Dem. Governor	**0.011**	**0.011**	**0.011**	**0.011**
	(0.004)	(0.004)	(0.004)	(0.004)
Dem. House Majority	**0.025**	**0.029**	**0.026**	**0.019**
	(0.007)	(0.006)	(0.006)	(0.036)
Dem. Senate Majority	**0.019**	0.016	0.017	0.051
	(0.006)	(0.008)	(0.008)	(0.030)
Dem. House Seat Share	0.025		0.018	0.013
	(0.026)		(0.033)	(0.046)
Dem. House Seat Share				0.012
$\times$ Dem. House Majority				(0.070)
Dem. Senate Seat Share		0.023	0.010	0.054
		(0.026)	(0.033)	(0.038)
Dem. Senate Seat Share				-0.070
$\times$ Dem. Senate Majority				(0.056)
State & Year FEs	X	X	X	X
Policy Liberalism $t-1$	X	X	X	X
Policy Liberalism $t-2$	X	X	X	X
Observations	3,586	3,586	3,586	3,586
States	49	49	49	49
R-squared	0.988	0.988	0.988	0.988

Note: Standard errors produced by block bootstraps (clustered at the state level) of 1,000 times are in the parentheses. Nebraska is not included in the sample. Coefficients statistically significant at the 5% level are in bold font type.

A.11 Measuring Ideological Divergence in the Mass Public

In order to measure the ideological difference between Democrats and Republicans in the mass public, we need a measure of the mass public's policy preferences in every year. We define ideology as the underlying latent policy preferences that structure people's responses to individual survey questions. This definition follows a large body of work on the preferences of members of Congress (Poole and Rosenthal 2007) as well as other recent studies of the American public (Treier and Hillygus 2009; Jessee 2009; Tausanovitch and Warshaw 2013).

A.11.1 Statistical Model

Until recently, the lack of a valid, time-varying measure of citizen policy liberalism has been one of the main barriers to the study of polarization in the mass public. To overcome this challenge, we apply a modified version of the dynamic hierarchical group-level IRT model developed by Caughey and Warshaw (2015), which estimates the average policy liberalism of defined subpopulations (e.g., Democrats, Republicans, and Independents in each state).[38] This approach builds upon three important approaches to modeling public opinion: item-response theory, multilevel regression and poststratification, and dynamic measurement models. Crucially, the model does not require multiple questions per respondent, allowing the use of the vast number of historical surveys that do not meet this standard.

Our model allows us to combine multiple survey questions into scaled measures of ideology, while addressing the problems of sparse survey data discussed above. It begins by adopting the general framework of item-response theory (IRT). In an IRT model, respondents' question responses are jointly determined by their score on some unobserved trait—in

38. Our approach bears a close relation to the literature on "public policy mood" (Stimson 1991). Works in this tradition use Stimson's Dyad Ratios algorithm to estimate changes in public preferences for government activity (i.e., left-liberalism). Recently, Enns and Koch (2013) have combined the Dyad Ratios algorithm with MRP to generate state-level estimates of policy mood. As McGann (2014) observes, though, the Dyad Ratios algorithm has several unappealing features, most notably its ideological asymmetry and its lack of a grounding in a coherent individual-level model. As an alternative, he proposes a group-level IRT model for national mood that is similar to the approach we take. However, our dynamic group-level IRT model, accommodates cross-sectional and over-time variation within a common framework.

our case, their economic policy liberalism—and by the characteristics of the particular question. The relationship between responses to question q and the unobserved trait θ_i is governed by the question's threshold κ_q, which captures the base level of support for the question, and its dispersion σ_q, which represents question-specific measurement error. Under this model, respondent i's probability of selecting the liberal response to question q is

$$\pi_{iq} = \Phi\left(\frac{\theta_i - \kappa_q}{\sigma_q}\right), \tag{8}$$

where the normal CDF Φ maps $(\theta_i - \kappa_q)/\sigma_q$ to the $(0, 1)$ interval.[39] The model assumes that greater liberalism (i.e., higher values of θ_i) increases respondents' probability of answering liberally. The strength of this relationship is inversely proportional to σ_q, and the threshold for a liberal response is governed by κ_q. Estimating the relationship of each question to the latent trait in this way allows the model to overcome the first challenge outlined above, considerably reducing the model's sensitivity to which questions are asked when.

The fact that each respondent answers no more than a few questions (sometimes only one) prevents us from using an IRT model to estimate the liberalism of individual respondents. Our ultimate interest, however, is not individuals but rather groups defined by the cross-classification of party ID and state. We therefore instead estimate a group-level IRT model, building on the work of Mislevy (1983), McGann (2014) and particularly Caughey and Warshaw (2015). The focus of this model is estimating the average liberalism $\bar{\theta}_g$ in each state-party g, for which there are many observations in a given survey. Under the assumption that θ_i is normally distributed within groups, the probability that a randomly sampled member of group g correctly answers item q is

$$\pi_{gq} = \Phi\left(\frac{\bar{\theta}_g - \kappa_q}{\sqrt{\sigma_q^2 + \sigma_\theta^2}}\right), \tag{9}$$

39. A common alternative way of writing the model in Equation (8) is $\Pr(y_{iq} = 1) = \Phi(\beta_q \theta_i - \alpha_q)$, where $\beta_q = 1/\sigma_q$ and $\alpha_q = \kappa_q \times \beta_q$. This exposition assumes dichotomous response choices; we discuss ordinal choices below.

where σ_θ is the standard deviation of θ_i within groups. We connect Equation (9) to the data through the sampling model

$$s_{gq} \sim \text{Binomial}(n_{gq},\ \pi_{gq}), \tag{10}$$

where n_{gq} is group g's total number of non-missing responses to question q and s_{gq} is the number of those responses that are liberal.[40] The estimates of $\bar{\theta}_g$ may be of interest in themselves, or they can be poststratified into estimates of, for example, average liberalism in each state (cf. Park, Gelman, and Bafumi 2004).

Even with an annual minimum of 2,000 respondents, many group cells are likely to be small or empty in a given year. To address this sparseness, we use a dynamic linear model to smooth the estimated group means across both time and states. The specific model we use, which differs somewhat from that in Caughey and Warshaw (2015), is

$$\bar{\theta}_{gt} \sim N(\delta_t \bar{\theta}_{g,t-1} + \xi_t + \mathbf{x}'_{g\cdot}\gamma_t,\ \sigma^2_{\bar{\theta}t}), \tag{11}$$

where $\bar{\theta}_{g,t-1}$ is g's mean in the previous year, ξ_t is a year-specific intercept, and $\mathbf{x}_{g\cdot}$ is a vector of attributes of g (e.g., its state or party). Each group-year mean is thus modeled as a function of the group's mean in the previous year, year-specific changes common to all groups, and changes in relative liberalism of groups with similar characteristics (i.e., the same party or state). The posterior estimates of $\bar{\theta}_{gt}$ are a thus compromise between this prior and the likelihood implied by Equations (9) and (10), with the relative weight placed on the likelihood determined by the prior standard deviation $\sigma_{\bar{\theta}t}$, which is estimated from the data and allowed to evolve across years. When a lot of survey data are available for a given year, the likelihood will dominate. If no survey data are available at all, the prior acts

40. Following Ghitza and Gelman (2013) and Caughey and Warshaw (2015, 202–3), we adjust the raw values of s_{gq} and n_{gq} to account for survey weights and for respondents who answer multiple questions. The latter is particularly important in this application because of the way that we deal with ordinal questions, which is to break each such question into a set of dichotomous questions, each of which indicates whether the response is above a given response level. For example, a question with three ordinal response choices, (1) "disagree", (2) "neutral", and (3) "agree," would be converted into two dichotomous variables respectively indicating whether the response is above "disagree" and above "neutral."

as a predictive model that imputes $\bar{\theta}_{gt}$.

Our dynamic group-level IRT model estimates opinion in groups defined by states and party (i.e., Democrats, Independents and Republicans). In order to mitigate sampling error for small states, we model the state effects as a function of state *Proportion Evangelical/Mormon*, *Percent Hispanic* and *Percent Urban.* The inclusion of state attributes in the model partially pools information across similar geographical units, improving the efficiency of state estimates (e.g., Park, Gelman, and Bafumi 2004).

To generate annual estimates of average opinion in each state, we pre-weighted our survey data to match raked targets for gender and education level in each state public, based on data from the U.S. Census (Ruggles et al. 2010). Our model produces estimates of the ideology of Democrats, Republicans, and Independents in each state. We aggregated these estimates up to the national level based on post-stratification weights generated by a model of the smoothed proportions of Democrats, Republicans, and Independents in each state/year.

A major advantage of simulation-based estimation is that it facilitates proper accounting for uncertainty in functions of the estimated parameters. For example, the estimated mean opinion in a given state is a weighted average of mean opinion in each demographic group, which is itself an estimate subject to uncertainty. The uncertainty in the group estimates can be appropriately propagated to the state estimates via the distribution of state estimates across simulation iterations. Posterior beliefs about average opinion in the state can then be summarized via the means, standard deviations, and so on of the posterior distribution. We adopt this approach in presenting the results of the model in the application that follows.

In order to assemble our dataset, we attempted to compile every economic policy question on face-to-face and phone surveys of the American public over the past 70 years.[41] Our data includes canonical academic surveys, such as the American National Election Study and the General Social Survey. But it also includes hundreds of polls from commercial polling

41. Our preliminary analysis indicates that online surveys, such as the Cooperative Congressional Election Studies (CCES), show more polarization and sorting than phone surveys. Thus, we omit online surveys in order to ensure the inter-temporal comparability of our results.

organizations such as Gallup, CBS News/NYTimes, ABC News/Washington Post, Time Magazine, Pew, and many others. In the end, our public opinion data consists of survey responses to over a hundred domestic policy questions spread across nearly 600 public-opinion surveys fielded between 1946 and 2014. The questions cover traditional economic issues such as taxes, social welfare, and labor regulation. For conceptual clarity and comparability with policy mood, this application includes only questions for which the "liberal" answer involved greater government spending or activity.[42]

In order to ensure the comparability of our estimates over time, we use question series with consistent question wording and response categories as bridge items. While no individual survey item is asked consistently between 1946 and 2012, there are many survey questions that are asked consistently for shorter periods of time. These items glue our estimates from one time period together with our estimates for other time periods. We also do not use any "relative" questions about whether the government should do more in our model since these questions are not comparable longitudinally due to changes in the policy status quo.[43]

The responses of over 800,000 different Americans are represented in the data. On average, we have 9,000 respondents and 7 policy questions in any individual year of our data. Moreover, we have at least 3 policy questions and 2,000 surveys responses in every single year in our data.

42. For example, questions about restricting access to abortion were not included. Stimson (1999, 89–91) notes that the temporal dynamics of abortion attitudes are distinct from other issues, at least before 1990.

43. For instance, we do not include the GSS questions about whether the government should spend more or less on individual programmatic areas. In future drafts of this paper, we may include these spending items in the model separately in each year. In other words, we would not use them to bridge the model together over time, but would use them to increase the cross-sectional precision of our estimates.

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