Policy Preferences and Policy Change:
Dynamic Responsiveness in the
American States, 1936–2014

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Abstract
In a democracy, government policies should not just be correlated with citizens’ preferences, but also respond dynamically to them. Using eight decades of data, we examine the magnitude, mechanisms, and moderators of dynamic responsiveness in the American states. We show that on both economic and (especially) social issues, the liberalism of state publics predicts future changes in state policy liberalism. Dynamic responsiveness is gradual, however; large policy shifts are the result of the cumulation of incremental responsiveness over many years. Partisan control of government mediates only a fraction of responsiveness, suggesting that, contrary to conventional wisdom, responsiveness occurs mainly through the adaptation of incumbent officials. Dynamic responsiveness has increased over time but does not seem to be influenced by institutions such as direct democracy or campaign finance regulations. We conclude that our findings, though in some respects normatively ambiguous, on the whole paint a reassuring portrait of statehouse democracy.
What drives policy change? What prompts government officials to raise taxes or cut them, to increase or decrease spending, to create new programs and regulations or repeal old ones? The full answer is surely complex, involving among other things changes in government personnel, the emergence of new policy problems, and the availability of potential solutions (e.g., Kingdon 1995). But in a democracy, policy change should also be driven by citizens’ policy preferences. When the public gets more liberal or more conservative, we should expect the government to follow suit. If it does not, the polity’s democratic bona fides must be called into question.¹

Dynamic responsiveness has been documented primarily at the national level, especially in the United States but also in Canada and the United Kingdom.² National policymaking has been shown to respond both to policy-specific changes in mass opinion (Page and Shapiro 1983) and to the public’s overall “policy mood”—its global preference for more or less government activity (Stimson, MacKuen, and Erikson 1995; Soroka and Wlezien 2010). Moreover, responsiveness to public mood has been found to operate through two main channels: turnover (the selection of challengers of one ideological stripe to replace incumbents of another) and adaptation (driven mainly by election-minded incumbents’ anticipation of voter sanctions). Thus, while the dynamic responsiveness literature also leaves plenty of room for policy determinants other than public opinion, the seemingly robust relationship between mass preferences and policy change offers reassuring evidence of ordinary citizens’ influence over government policies.

These optimistic conclusions, however, have been subject to trenchant critiques. One line of criticism, stemming from Brody and Page’s (1972) seminal distinction

¹. While responsiveness is often considered the hallmark of democracy (Dahl 1971), it is not by itself a sufficient condition; for other necessary conditions, see, e.g., Dahl (1989).

². We use the term dynamic responsiveness instead of the better-known dynamic representation (Stimson, MacKuen, and Erikson 1995) in order to distinguish responsiveness from alternative measures of representation, such as proximity or congruence (see Achen 1978).
between policy voting, persuasion, and projection, emphasizes the possibility that citizens, rather than driving policy change, may simply adopt the views of their favored politicians (Lenz 2012). But even granting the causal priority of public opinion, scholars have questioned the substantive interpretation of findings of dynamic responsiveness. Achen and Bartels (2016, 45–6), for example, argue that the impact of adaptation pales relative to the effect of partisan control of government offices. They thus conclude that “citizens affect public policy—insofar as they affect it at all—almost entirely by voting out one partisan team and replacing it with another,” that is, through partisan turnover. Indeed, notwithstanding the contrary arguments of Stimson, MacKuen, and Erikson (1995), the prevailing scholarly view is that turnover dominates adaptation as a mechanism of responsiveness in the United States—and in recent decades, increasingly so (Ansolabehere, Snyder, and Stewart 2001; Lee, Moretti, and Butler 2004; Poole 2007; Fowler and Hall, Forthcoming). This has in turn raised normative concerns about “leapfrog representation” by partisan extremists, whose actions may be responsive to, but are rarely congruent with, the preferences of the relatively moderate public (Bafumi and Herron 2010; see also Poole and Rosenthal 1984; Lax and Phillips 2011).

To some degree, these divergent conclusions stem from issues of research design. National-level studies like Stimson, MacKuen, and Erikson (1995) and Soroka and Wlezien (2010) are inherently fragile due to the limitations of purely time-series analysis, such as their inability to rule out unobserved time-specific confounders that affect both series (e.g., the arrival of a new policy “solution” onto the national agenda). For their part, studies that emphasize the dominance of turnover are overwhelmingly based on cross-sectional analyses, typically of roll-call voting in a single legislature.³ These latter studies thus suffer from two limitations. Being cross-sectional, they are

³. For instance, even though the data used by Ansolabehere, Snyder, and Stewart (2001) cover many decades, their analysis essentially consists of a sequence of cross-sectional regressions.
more vulnerable to reverse-causation and other forms of confounding, and in any case are ill-suited to studying dynamic responsiveness. More subtly, because they compare roll-call patterns within a single legislature, they cannot detect collective responsiveness to changes in public opinion, such as the extent to which an entire legislature responds to uniform shifts in the public’s preferences (Weissberg 1978). A final limitation of these studies is that they focus on campaign platforms, roll call votes, or other forms of position taking, which are merely precursors to what is arguably the ultimate metric of representation: public policies.

The U.S. states offer potentially fertile ground for overcoming these limitations. By examining fifty states over many years, we can employ combined time-series–cross-sectional (TSCS) analyses that avoid many of the pitfalls of either approach on its own. Moreover, by using state policies as the outcome of interest, we can explore how public opinion influences not only the positions politicians take, but what governments actually do. A further advantage of state politics is that the states themselves provide a natural point of comparison or benchmark for assessing the substantive magnitude of dynamic responsiveness. If, for example, a modest liberal shift in state opinion leads Mississippi to adopt policies that look like those of Massachusetts, then state governments are probably over-responsive to their citizens‘ preferences.

Notwithstanding its methodological attractions, state politics presents something of a hard case for dynamic responsiveness. Due to “fiscal federalism” and other constraints on state governments, structural and economic conditions may dominate public opinion as determinants of state policies (Oates 1972; see also Dye 1966). Moreover, the lower salience of state politics means that state elections are powerfully affected by national tides, undermining the direct accountability relationship between state-level officials and their electorates (Rogers 2016). In recent decades, as state politics has becoming increasingly nationalized, voters’ willingness and capacity to recognize and reward responsive officials has further diminished (Hopkins
Thus, despite the “awesome” cross-sectional association between the liberalism of state policies and publics (Erikson, Wright, and McIver 1993; see also Gray et al. 2004; Lax and Phillips 2011), public opinion may be only one relatively minor causal factor among the many that explain change in state policies (see Ringquist and Garand 1999). Finally, studying dynamic responsiveness in the states presents formidable practical challenges, for doing so requires yearly summaries of policy outputs and public preferences in each state over many decades.

Fortunately, recent methodological advances have made such an analysis possible. Using newly developed models for estimating the ideological orientation of state publics (Caughey and Warshaw 2015) and policies (Caughey and Warshaw 2016), we construct dynamic measures of mass and government policy liberalism in each year between 1936 and 2014. The mass liberalism scores, estimated separately for economic and social issue domains, are based on a dataset of approximately 1.5 million individuals’ responses to over 300 domestic policy questions. From the same dataset we also derive analogous time series of party identification (PID) in each state-year. The government policy liberalism scores, also estimated separately for economic and social policies, are based on an annual dataset of nearly 150 continuous and categorical state policies. Combining these measures with data on party control of state offices (Klarner 2013), we use a series of dynamic panel models to examine state-level dynamic responsiveness as well as its mediators and moderators.

Our analyses reveal that on both economic and (especially) social issues, the policy liberalism of state publics is a robust predictor of future changes in the liberalism of state policies. In other words, when a state’s citizens are relatively liberal, the state tends to adopt more liberal policies. Dynamic responsiveness is gradual, however; large policy shifts are the result of the cumulation of incremental responsiveness over many years. Mass liberalism also predicts the election of more Democratic officials, though less strongly than does the state-level balance of mass PID. Democratic
control of state government in turn leads to more liberal policies, suggesting that partisan turnover does indeed mediate dynamic responsiveness. But we also find that policy reacts directly to citizen liberalism, holding constant the party that controls the government, suggesting that adaptation too is an important, and the probably dominant, mechanism of dynamic responsiveness.

In addition to examining the mediators of the opinion–policy relationship, we also investigate what factors moderate this relationship. Our most robust finding is that dynamic responsiveness has increased over time, on both social and economic issues. We find no evidence of differential dynamic responsiveness between Southern and non-Southern states, even in the pre–civil rights era, though the cross-sectional relationship between opinion and policy is consistently stronger in the non-South. Nor do we find reliable evidence that any institution, such as campaign donation limits, direct democracy, or term limits, affects dynamic responsiveness. In sum, we find little evidence that dynamic responsiveness varies across institutional conditions.

We close our paper with a discussion of the normative implications of our findings. This is a difficult issue, for dynamic responsiveness is but one indicator of the quality of representation, and under some circumstances an increase in responsiveness may even degrade other indicators, such as proximity or congruence (Achen 1978; Matsusaka 2001; Bafumi and Herron 2010; Lax and Phillips 2011). We tentatively conclude, however, that our findings are on the whole normatively positive. In addition to being powerfully related to citizen policy liberalism at any point in time, state policy liberalism is also responsive on the margin to shifts in public preferences. Given the many reasons for doubting the existence of policy voting and responsiveness (Achen and Bartels 2016)—reasons that are if anything more compelling at the state than the national level—the mere existence of state-level dynamic responsiveness is reassuring. On the other hand, contrary to many cross-sectional studies (e.g., Lax and Phillips 2011), we find little indication that policy liberalism is over-responsive.
to citizen preferences. Rather, within-state differences in citizen preferences lead to changes in policy liberalism that are small relative to the differences across states.

1 Theoretical Background

One of the most fundamental assumptions of democratic theory is that the views of citizens should influence government policy decisions. In particular, policy change should be driven by citizens’ policy preferences: when the public is relatively liberal, we should expect the government to follow suit. The slope of this relationship—the dynamic association between mass preferences and public policies—is what we refer to as dynamic responsiveness. Dynamic responsiveness can be thought of as a minimal standard for democratic representation. If policy change has no empirical relationship with mass preferences, then it is unlikely that citizens exercise the kind of control over government that lies at the core of democratic theory.

1.1 Mechanisms

How should we expect dynamic responsiveness to operate? One mechanism is through the replacement of incumbent officials of one ideological stripe with challengers of another, which we refer to as turnover (see the stylized causal model of dynamic responsiveness in Figure 1). For this mechanism to operate, an increase in the liberalism of citizens’ policy preferences must lead to the selection of officials who implement more liberal policies than their predecessors. Aside from one-party states where electoral competition occurs within rather than between parties (e.g., in the South before the 1960s; see Key 1949), ideological turnover is mainly accomplished through the replacement of Republicans with Democrats or vice versa. In short, if greater citizen liberalism causes the election of more Democrats, and these Democrats enact more liberal policies once in office, then partisan turnover will at least partially mediate
Figure 1: Causal diagram of dynamic responsiveness, showing the two mechanisms of turnover and adaptation.

dynamic responsiveness. The existing evidence for responsive turnover, especially as regards mass liberalism’s effect on the selection of Democrats into office, is quite mixed. Achen and Bartels, for example, claim that Erikson, MacKuen, and Stimson’s (2002) apparent evidence for ideological selection “required delicate controls” for candidate positions and mass partisanship and falls apart in alternative specifications. They therefore conclude that mass policy preferences “are of relatively little importance in determining who wins” elections (Achen and Bartels 2016, 46).

As Stimson, MacKuen, and Erikson (1995) observe, however, partisan turnover is not the only possible mechanism for dynamic responsiveness. In addition, the mere threat of electoral sanctions can induce reelection-minded incumbent officials to adapt preemptively to shifts in public sentiment. These authors present evidence that both adaptation and turnover drive national policy change, a pattern also uncovered by some cross-sectional studies of state policy responsiveness (Erikson, Wright, and McIver 1993). Most of the empirical literature on responsiveness, however, which focuses overwhelming on legislative position-taking, finds that selection dominates adaptation (Ansolabehere, Snyder, and Stewart 2001; Lee, Moretti, and Butler 2004; 4. See also Arnold (1990). Snyder and Ting (2003) show formally that retrospective electoral sanctions can induce incumbents to moderate toward the median voter, even if voters lack any information about challengers aside from their party label. [7]
Poole 2007; Fowler and Hall, Forthcoming; but see Kousser, Lewis, and Masket 2007).

1.2 Variation Across Issue Domains

Nearly all studies that have found strong evidence of state-level policy responsiveness either employ general measures of liberalism–conservatism that conflate different policy domains (e.g., Erikson, Wright, and McIver 1993) or else focus almost exclusively on social policies (e.g., Lax and Phillips 2009, 2011). What evidence there is for responsiveness on economic issues tends to be somewhat weaker (Pacheco 2013). There are several reasons we might expect such differences between economic and social policies. States tend to have less policymaking discretion on economic issues. A large share of state government monies come from the federal government (Pew Charitable Trusts 2016), which is largely unresponsive to shifts in state-level public opinion. Federal and state governments also share responsibility over many policy areas (Peterson 1995). State taxing and spending choices are also highly constrained by economic competition from other jurisdictions—irrespective of the public’s preferences, states can increase taxes and regulations only so much before businesses and higher-income citizens vote with their feet by moving to states with lower costs (Oates 1972; Bailey and Rom 2004; cf. Peterson 1981).

Economic and social issues differ at the mass level as well. Because social policies tend to be more symbolic than technical and are more likely to concern questions of ends as opposed to means, they are more likely than economic policies to be “easy” issues for citizens. As a consequence, easy issues facilitate citizens’ ability to “calculate relative positioning of parties and candidates,” as Carmines and Stimson (1980, 82) emphasize. In addition, citizens’ policy preferences on social issues are also

5. In her study of state welfare and education spending, Pacheco (2013, 319) notes that “conclusions regarding dynamic policy representation [i.e., responsiveness] vary depending on model specification” and are not robust to the inclusion of year fixed effects.
likely to be more stable and coherent than their economic preferences, making it easier for politicians to discern signal from noise in public opinion. In short, because social policies are both more amenable to state control and easier for citizens to understand, we should expect state-level responsiveness to be stronger on social than economic issues.

### 1.3 Institutional Moderators

In addition to varying across issue domains, dynamic responsiveness may also vary across different institutional and other contexts. Indeed, as Lax and Phillips (2011) note, “many of the largest debates in the state politics literature involve which, if any, institutional features of state government enhance or undercut the relationship between policy and opinion.” We explore this possibility with a focus on three sets of institutions that might moderate state policy responsiveness.

The past eight decades have witnessed large changes in the institutional structure of American democracy, none more important than the 1960s-era dismantlement of *suffrage restrictions* in the formerly one-party South (Key 1949; Mickey 2015). These restrictions both changed the demographic and ideological composition of the electorate and reduced voter turnout overall (J. M. Kousser 1974; Springer 2014). As a result, one might hope and expect that the elimination of undemocratic institutions in the South led to greater responsiveness to citizens’ policy preferences in those states. On the other hand, there is recent evidence to suggest that the one-party South was not obviously less responsive to the eligible electorate than the two-party North (Caughey 2016). Since the preferences of different social groups tend move in parallel with one another (Page and Shapiro 1992), this means that dynamic responsiveness to one group often implies responsiveness to the public as a whole (Stimson 2009).

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6. Cf. Przeworski, Stokes, and Manin (1999, 8–9), on responsiveness as the relationship between signals (expressions of public preferences) and policies (authoritative government decisions).
To the extent that this is true, then the elimination of suffrage-restricting institutions may not have had a substantial effect on dynamic responsiveness in the South.

There are also reasons to believe that campaign contribution limitations may influence policy responsiveness by affecting politicians’ incentives to focus on the preferences of the median voter. Indeed, contributions from corporations and wealthy individuals could incentivize elected officials to focus more on their opinions than the opinion of the median voter (Bartels 2008; Gilens 2012). We therefore expect limits on campaign contributions to increase the responsiveness of policy to public opinion. Several previous studies have examined the direct effect of campaign finance limits on state policy, but no previous study has examined the effect of campaign finance rules on the responsiveness of state policies to public opinion.

A last set of institutions that could improve responsiveness are progressive reforms designed to enhance what might be called citizen government, such as direct democracy and term limits. Direct democracy could enhance responsiveness by giving citizens the ability to circumvent elected officials and enact their preferred policy through the ballot box (Matsusaka 2008). In addition, the threat of the initiative may

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7. There are also reasons to expect intercept shifts due to campaign contribution limits. Labor unions are clearly to the left of the median voter. So we might expect bans on contributions from labor unions to shift policy to the right (La Raja and Schaffner 2014). The positions of corporations and wealthy individuals vis-à-vis the median voter are more ambiguous. So we do not have a firm expectation about the average effect of limits on contributions from these actors on policy liberalism.

8. Besley and Case (2003) find that bans on contributions from corporations lead to more liberal outcomes on two of the five policies they examine, more conservative outcomes on one policy, and have no effect at all on two other two policies. Overall, Besley and Case’s analysis provides suggestive evidence that corporate campaign contribution limits lead to more liberal policies. But their analysis focuses on a small number of policy outputs, and uses a relatively simple difference-in-differences framework that assumes parallel trends. Two more recent studies focus on the impact of campaign finance limits on other policy outputs using more complex dynamic panel models. Werner and Coleman (2013) finds that bans on contributions from unions and corporations have no effect on minimum wage laws, while La Raja and Schaffner (2014) find that bans on contributions from corporations and unions have no effect on corporate tax revenues.
lead elected officials to change their behavior in order to preempt future ballot measures (Gerber 1996). Finally, even if elected officials do not actively seek to preempt future initiatives, the results of initiatives may give them more accurate information about voter preferences (Matsusaka 2005). Despite sound theoretical reasons to expect that direct democracy might improve responsiveness, empirical studies of its effects have been ambiguous.⁹

Term limits could also increase responsiveness by inducing greater turnover among legislators. This could lead to the election of legislators who better reflect constituents’ preferences. However, term limits could lead to less experienced legislators, which might reduce their capacity to assess and respond to public opinion. Term limits may also reduce incentives to respond to public opinion by limiting the value of a seat in the legislature (T. Kousser 2005). There have been few empirical studies of the effect of term limits on representation, but one recent study finds that cross-sectional responsiveness is stronger in states with term limits (Lax and Phillips 2011).

2 Research Design

Achen (1978) argues that citizens’ influence over the government can be measured by the expected difference in government outputs associated with a given difference in the preferences of the average citizen—that is, the regression slope, which he labels responsiveness.¹⁰ Defined this way, responsiveness is a descriptive quantity: it simply

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⁹. Some studies find that direct democracy enhances responsiveness, at least in some policy areas (Arceneaux 2002; Gerber 1996; Matsusaka 2010), while others find that direct democracy has no effect on responsiveness in state governments (Monogan, Gray, and Lowery 2009; Lascher, Hagen, and Rochlin 1996; Lax and Phillips 2009, 2011).

¹⁰. More precisely, Achen (1978) defines responsiveness as both the intercept and slope of the regression, where the intercept indexes the “bias” of the electoral system (492). Since the intercept in our application has no natural meaning, we focus only on the regression slope, as do most studies of responsiveness. Achen also focuses on the opinions of elected representatives rather than on policy outputs, but there is
captures the covariation between citizens’ preferences and governmental outputs. Due to data limitations, most previous studies have focused on this cross-sectional link between the mass public’s policy preferences and government policy. But a major problem with cross-sectional analyses of representation is that it is very difficult to rule out the possibility that some third, unmeasured characteristic of states—level of economic development, for example—confounds the relationship between citizen liberalism and policy liberalism, or even that policy liberalism causes citizen liberalism.

The normative significance of responsiveness, however, largely hinges on whether the relationship is causal—that is, on whether government outputs would have differed had citizens’ preferences been different. Estimating responsiveness in a causal sense requires isolating exogenous variation in citizens’ preferences, a tall order indeed. Nevertheless, such causal inferences can be made more credible by exploiting temporal variation in citizens’ preferences. As Stimson, MacKuen, and Erikson (1995, 543) note, representation is a process that is “inherently structured in time.” We therefore follow Stimson, MacKuen, and Erikson (1995) and Soroka and Wlezien (2010) in examining the relationship between liberalism in the mass public and future changes in policy liberalism. This is what in a time-series context is known as Granger causality.

Where we depart from these authors is in our use of time-series–cross-sectional (TSCS) data. A TSCS approach offers considerable advantages over a purely time-series one. It enables us to estimate a dynamic panel model that includes not only lagged dependent variables (LDVs), as a typical time-series model would, but also state and year fixed effects (FEs). The state and year FEs enable us to rule out two

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11. This is not to deny that responsiveness in a descriptive sense is also interesting and important. At the very least, the empirical covariation between preferences and policy provides a normative benchmark for the representativeness of a political system.
threats to causal inference that time-series data alone cannot: time-invariant state-specific confounders and year-specific shocks that affect all states equally (Angrist and Pischke 2009). As we show below, however, it is important that we control for LDVs as well as FEs, for lagged policy liberalism is just the sort of time-varying state-specific confounder that FEs alone cannot account for. In sum, while our TSCS data and modeling strategy cannot rule out all confounders of the opinion–policy relationship, it provides a firmer basis for causal inference than either time-series or cross-sectional analysis alone.

2.1 Mass Policy Preferences

Estimating the relationship between mass preferences and state policies requires measures of each construct for each state in each year. A major difficulty with obtaining such annual measures is that although thousands of Americans have been surveyed on their policy preferences in each year since 1936, the specific survey questions asked have been sparsely and unevenly distributed across time. This lack of consistent question series makes it practically impossible to examine policy-specific responsiveness at the state level over any long time span. The most ambitious existing effort is Pacheco’s (2013) analysis of the responsiveness of state education and welfare spending to public preferences for more spending, issues where state-level polling has been particularly dense in the period she covers (1977–2000). Even so, the opinion data are sufficiently sparse that Pacheco smooths the state estimates with multilevel regression coupled with a five-year moving average, which dampens yearly fluctuations in state opinion (see also Pacheco 2011). Aside from Pacheco (2013), all other studies have dealt with the problem of sparse survey data by using proxies for mass policy preferences derived from ideological self-identification, presidential vote, or the roll-

12. Dynamic panel models are known to be 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).
call records of the state congressional delegation (e.g., Erikson, Wright, and McIver
1993; Levitt 1996; Berry et al. 1998).

We take an alternative approach: inferring the latent policy liberalism of state
publics by aggregating responses to many distinct policy questions across many polls.
We do so using a dynamic group-level item-response model (Caughey and Warshaw
2015; Dunham, Caughey, and Warshaw 2016; see Supplementary Appendix for more
details). While similar to the estimates of “public policy mood” estimated by Stimson
(1991) at the national level and by Enns and Koch (2013) in the states, our estimates
of mass policy liberalism differ from mood in two respects. First, mood is a rela-
tive measure; it captures whether the public wants more or less government, relative
to what is being currently provided. By contrast, our mass liberalism estimates are
based only on policy questions that do not explicitly or implicitly reference the policy
status quo and are thus intended as measures of absolute, not relative, liberalism.
Second, and more important, we estimate mass liberalism separately for economic
and social issues (cf. Treier and Hillygus 2009). We do so because mass policy
preferences across domains were, until recently, quite weakly correlated and exhibited
distinct temporal dynamics. This was true not only at the level of individuals, whose
lack of issue constraint is well known, but also at the level of geographic or partisan
groups, who due to the “miracle of aggregation” typically exhibit much more ideo-
logical structure than individuals (Converse 2000). While treating mass liberalism as

13. These works use Stimson’s Dyad Ratios algorithm to estimate policy mood.
McGann (2014) observes that 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. Whereas McGann
(2014) captures only longitudinal variation, however, Caughey and Warshaw’s (2015)
dynamic group-level IRT model accommodates cross-sectional and over-time variation
within a common framework.

14. We also considered estimating liberalism on racial issues as well, but found that
the relative paucity of survey questions in this domain made it difficult to estimate
rational liberalism over a long time span.
unidimensional is often a reasonable approximation in contemporary American politics (see, e.g., Jessee 2009; Tausanovitch and Warshaw 2013), the long time span of our study make it much less tenable.

To estimate mass liberalism in each domain, we rely on a dataset of survey responses to over 300 domestic policy questions spread across nearly 1,000 public-opinion surveys fielded between 1936 and 2014. Overall, the responses of nearly 1.5 million distinct individuals are represented in the data. This dataset includes nearly all policy questions asked on U.S. national surveys in more than one year and the vast majority of questions asked for only a single year, particularly early in the time period when policy questions were sparse. It includes canonical academic surveys, such as the American National Election Study and the General Social Survey, as well as hundreds of polls from commercial polling organizations such as Gallup, CBS News/NYTimes, ABC News/Washington Post, and many others. Out of the 3,846 state-years in our dataset, 95% contain at least some opinion data on social issues, and 98% contain at least some data on economic opinion.

As noted above, we estimate economic and social liberalism separately. The economic questions cover issues such as taxes, social welfare, and labor regulation. The social questions include ones about alcohol, abortion, gay rights, women’s rights, school prayer, and other cultural (but not racial) issues. In order to ensure the comparability of our estimates over time, we use question series with consistent question wording, substantive meaning, and response categories as bridge items. While no individual survey item is asked consistently between 1936 and 2014, 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

15. We generally do not include “relative” questions about whether the government should do more since these questions are not comparable longitudinally due to changes in the policy status quo. In the few cases where we include relative questions, we code them as separate questions in each year.
periods. Since almost all these surveys also include a question about party identification, we use the same dataset to estimate the proportions of Democrats, Republicans, and Independents in each state year.

To construct our measure of mass liberalism, we first used a dynamic group-level IRT model to estimate annual average liberalism in groups defined by state, race, and urban residence.¹⁶ Then, using data from the U.S. Census (Ruggles et al. 2010), we poststratified the group estimates to match the groups’ proportions in the state population to produce estimates of average liberalism in each state-year. Finally, to aid interpretability of the estimates we standardized them to have a mean of 0 and a variance of 1 across state-years.

Figure 2 maps our estimates of mass economic and social liberalism in 1940, 1975 and 2010. The cross-sectional patterns are generally quite sensible—New York, California, and Massachusetts are always among the most liberal states. However, it is worth noting that the southern states are typically much more conservative on the social dimension than the economic dimension. Moreover, Figure 2 confirms that the states have remained generally stable in their relative liberalism, consistent with Erikson, Wright, and McIver’s (2006) finding that state publics have been stable in terms of ideological identification. According to our estimates, only a few states’ policy liberalism has shifted substantially over time. States such as Idaho and Louisiana have become somewhat more conservative over time, while states in New England

¹⁶. We also raked the survey data to match interpolated targets for gender and education level in each state public, based on microdata from the U.S. Census (Ruggles et al. 2010). In order to mitigate sampling error for small states, we model the state effects in the first time period as a function of state Proportion Evangelical/Mormon. The inclusion of state attributes in the model partially pools information across similar geographical units in the first time period, improving the efficiency of state estimates (e.g., Park, Gelman, and Bafumi 2004). We drop Proportion Evangelical/Mormon after the first period because we found that the state intercept in the previous period tends to be much more predictive than state attributes. We estimate the dynamic, hierarchical group-level IRT model using the dgo package in R (Dunham, Caughey, and Warshaw 2016). The full details of the model, and an extensive validation of the estimates of latent opinion, are in Supplementary Appendix A.
2.2 State Policies

Next, we require a measure of the liberalism of state policies. For consistency with our domain-specific measures of the mass public’s policy preferences, we separate state policy liberalism by domain as well, using the measures of economic and social policy liberalism estimated by Caughey and Warshaw (2016). It is worth noting, however, that throughout the period we examine, there has consistently been a much higher correlation between the liberalism of states’ economic and social policies than between the economic and social liberalism of state mass publics.

These measures of domain-specific policy liberalism are based on a total of nearly
150 individual state policies. The 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. The policy dataset underlying the policy liberalism scores is designed to include all politically salient state policy outputs on which comparable data are available for at least five years. The economic dimension covers a wide range of policy areas, including social welfare (e.g., AFDC/TANF benefit levels), taxation (e.g., income tax rates), labor (e.g., right-to-work), and the environment (e.g., state endangered species acts). The social dimension includes 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), religion (e.g., public schools can post the Ten Commandments), criminal justice (e.g., death penalty), and drugs (e.g., marijuana decriminalization).

2.3 Institutions

Our data on potential institutional moderators of dynamic responsiveness are drawn from various sources. We obtained data on suffrage restrictions (poll taxes and literacy tests) from Springer (2014). We drew our data on campaign finance regulations (limits on the contributions of individuals, corporations, and unions) from a wide range of sources. These include state statutes, academic analyses (Stratmann and Aparicio-Castillo 2006; La Raja and Schaffner 2014), various editions of The Book of the States and the FEC’s Analysis of Federal and State Campaign Finance Law,
and other reference works (e.g., Ford 1955; Alexander and Denny 1966). Data on reforms intended to enhance citizen government (direct democracy and term limits) were obtained from Matsusaka (2005) and from the National Conference of State Legislatures.

### 3 Responsiveness—Cross-Sectional and Dynamic

We now turn to the relationship between mass policy liberalism and the liberalism of government policies (see Supplementary Appendix B for summary statistics for our key independent variables). We begin with a cross-sectional analysis typical of most studies of responsiveness. Figure 3 plots the state-level relationship between mass liberalism and government liberalism separately by policy domain (social and economic), time period (before and since 1972), and region (South and non-South). Within time period, each state’s mass and government liberalism have been averaged across years, so these relationships can be interpreted roughly as the average cross-sectional responsiveness in each domain, period, and region.

Figure 3 reveals several noteworthy patterns. First, in the period before 1972, when disenfranchisement and lack of partisan competition were still very much live issues in Southern states, mass and government policy liberalism were essentially uncorrelated within that region. By contrast, in the more democratic non-South, government policy liberalism has always had a robust relationship with mass liberalism. The relationship in the non-South has strengthened somewhat over time, with the correlation increasing from 0.49 to 0.74 on social issues and from 0.41 to 0.72 on economic issues. The cross-sectional correlation on social issues has increased in the South as well (to 0.44 in the post-1972 period), but the economic policies of South-

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19. Mickey (2015) argues that the democratization of the former Confederacy was not complete until 1972. For the classic critique politics in the one-party system, see chapter 14 of Key (1949).
ern states remain essentially uncorrelated with public opinion as well as substantially more conservative than in non-Southern states.

Figure 3: Cross-sectional relationship between mass and government policy liberalism, by era and issue domain.

These regional differences in cross-sectional responsiveness can also be seen in columns (1) and (4) of Table 1, which report estimates of cross-sectional responsive-
ness on social and economic issues, respectively, averaged over the entire 1936–2014 period. All the variables in this table are scaled to have a standard deviation (SD) of 1 across state-years. As the main effect of $Mass_{Social,t-1}$ indicates, outside the South there is nearly a one-to-one cross-sectional relationship between mass and government liberalism on social issues: a 1-SD difference on one is associated with a 1-SD difference in the other. On economic issues, the opinion–policy relationship in the non-South is only slightly less strong. But as the interactions with $South$ show, cross-sectional responsiveness on social issues is about half as strong in the South as in the non-South, and on economic issues is wholly absent.

Quite a different conclusion emerges, however, if we examine responsiveness from a dynamic rather than cross-sectional perspective. A first cut at such an over-time perspective is provided by columns (2) and (5) of Table 1, which report the results of specifications that include fixed effects (FEs) for state as well as year. These specifications capture the opinion–policy relationship within states net of national trends, thus eliminating the influence of time-invariant state-specific confounders (e.g., the enduring legacy of the South’s system of racial hierarchy). The estimates indicate that, in both regions, state-years in which mass liberalism was higher than average for that state also tended to have higher-than-average government liberalism. Taken at face value as causal estimates, the two-way FE coefficients are strikingly large. They imply that in the non-South a 1-SD change in mass liberalism has an immediate effect of 0.44 SDs on social policy liberalism and 0.35 SDs on economic policy liberalism. They also suggest that on social issues, the within-state relationship between mass and government liberalism is actually stronger in the South than in the non-South.

These inferences, however, hinge on the standard assumptions of two-way FE models, notably that there are no state-specific time-varying confounders. One very obvious such confounder is past state policies, which influence future policies in the direct sense of being path dependent and difficult to change. And indeed, as the
Table 1: Cross-sectional and dynamic responsiveness, by issue domain and region. XS = pooled cross-sectional regression; FE = two-way fixed effects; DP = dynamic panel. In the cross-sectional specification, year intercepts are allowed to vary by region. Standard errors are clustered by state and are robust to autocorrelation. Variables are scaled to have a standard deviation of 1.
dynamic panel (DP) estimates in columns (3) and (6) show, current policy liberalism in both domains is powerfully predicted by its lagged values (though the sum of the lag coefficients is clearly less than 1, indicating stationarity). Including these lagged dependent variables (LDVs) also dramatically reduces the estimated effect of mass liberalism relative to the two-way FE specification, suggesting the existence of within-state trends not accounted for by state FEs. Moreover, once states’ recent policy history is accounted for, regional differences fade to statistical insignificance.20

The broader point remains, however: the liberalism of state policies responds dynamically to the liberalism of state publics. Specifically, mass liberalism in year $t - 1$ predicts change in government liberalism at year $t$, what in a purely time-series setting is called “Granger causality.” The predictive power of mass liberalism suggests that state governments respond on the margin to the preferences of their citizens. This appears to be true even in the South, at least on social issues, which may be why the cross-sectional relationship between social opinion and policy has slowly come into alignment in that region (see Figure 3).

Consistent with our expectations regarding differences across policy domains, the substantive magnitude of dynamic responsiveness appears to be greater on social than economic issues. Pooling across regions, a dynamic panel model estimates a standardized opinion effect of 0.052 (SE=0.008) for social policy as compared to 0.016 (SE=0.007) for economic policy. That is, the policy effect of a 1-SD difference in mass opinion is three times larger on social than economic issues. Even on social issues, however, the immediate effect of mass liberalism is still an order of magnitude smaller than what the two-way FE estimates naively imply.21

20. The results from a dynamic model without fixed effects are extremely similar, except that the regional difference in dynamic responsiveness on economic policy is marginally significant. Also, in the social issues model the coefficient on Mass Liberalism shrinks substantially in magnitude if year FEs are removed from the dynamic panel specification, suggesting that trends common to all states confound the within-state relationship between social liberalism and policy.

21. There is some evidence that the effect of Mass Liberalism$_{t-1}$ is not fully realized
Due to policy liberalism’s strong persistence over time, however, the long-term effects of mass liberalism are much larger than the immediate effect. One way to see this is to calculate the long-run multiplier of Mass Liberalism\(_{t-1}\), which can be interpreted as the total effect of a permanent one-unit increase in Policy Liberalism over all future time periods (De Boef and Keele 2008, 191). On social policies, the estimated long-run multiplier of Mass Liberalism\(_{t-1}\) is approximately 0.9; on economic policies, it is around 0.3. That is, if Mass Liberalism shifted permanently upward by 1 unit, we would expect Policy Liberalism to eventually settle at a new equilibrium about 0.9 or 0.3 units above the old equilibrium, depending on the domain.\(^{22}\) The effect would occur gradually, however. It would take over a decade, for example, for 50% of the long-run effect to be realized, and half a century for 95% to be realized. Note that compared to the immediate dynamic effects of Mass Liberalism, the long-run effects are much closer in magnitude to the cross-sectional relationships reported in Table 1. This suggests that the strong contemporaneous correlation between state policies and opinion may be largely the product of the long-term, gradual accumulation of incremental policy responses to mass preferences.

4 Mechanisms: Turnover and Adaptation

As noted earlier, dynamic responsiveness to popular preferences can occur by two main mechanisms: turnover and adaptation. Turnover is a two-step process. First, voters must select candidates of one ideological stripe to replace incumbents of another. In the United States, this typically means replacing Democrats with Republicans or vice versa. Second, the newly elected officials must implement different

\(^{22}\) This equilibrium is the point at which the effect of Mass Liberalism is exactly counterbalanced by the mean-reverting impact of the lagged dependent variables.
policies than their electoral opponents would have. If greater liberalism in the public causes the election of more Democrats, and electing more Democrats causes policies to become more liberal, then partisan turnover mediates the effect of opinion on policy. Adaptation, by contrast, is that portion of dynamic responsiveness not mediated by turnover in government officials, but rather is the result of sitting incumbents responding directly to shifts in public sentiment. In sum, evaluating the relative importance of partisan turnover and adaptation entails inference about three causal effects: the effect of mass liberalism on the partisan composition of government, the effect of this partisan composition on policy liberalism, and the effect of mass liberalism on policy liberalism with partisan composition held fixed.

We begin our empirical analysis with the first effect, that of mass liberalism on the partisan composition of government. To measure the latter concept, we create indicators for whether the Democratic Party controls the governorship, the lower house of the state legislature, and the upper house. We combine these indicators into a single summative index of Democratic Control, normalized to range from 0 to 1. Except in rare circumstances, Democratic Control can change only in years following state elections, which in all but four states occur in even years. We therefore subset to years that follow a state election, estimating the effect on Democratic Control of mass liberalism in the preceding election year.

Table 2 summarizes the results of this analysis, which employs a dynamic panel specification similar to Table 1. As indicated by the coefficients for Democratic Control$_{t-1}$ in the bottom row, the partisan composition of the legislature is moderately autocorrelated, but not nearly as much so as policy, suggesting a fairly strong tendency towards alternation in party control over time.\textsuperscript{23} More relevant to our purposes here, the first and second rows of Table 2 show that Mass Liberalism$_{t-1}$ (that is, in

\textsuperscript{23} This would be consistent with the finding that a party that narrowly wins control of the governorship (Folke and Snyder 2012) or legislature (Feigenbaum, Fourinaies, and Hall 2015) of a state tends to lose votes in the following election.
Table 2: Effect of mass policy preferences and partisanship on partisan selection. The data have been subsetted to years following state elections, which in most states are odd years. Standard errors are clustered by state and are robust to autocorrelation. The Democratic Control Index ranges from 0 to 1. Other variables are scaled to have a standard deviation of 1 across state-years.

The most recent election year) clearly Granger-causes Democratic Control$_t$. A 1-SD difference in mass social liberalism increases Democratic Control$_t$ by 0.05 (column 1), and an analogous increase in economic liberalism does so by 0.03 (column 2). Moreover, when mass social and economic liberalism are included in the same specification, both continue to independently predict shifts in Democratic Control$_t$ (column 3).

One potential concern with these results is that the apparent effect of mass liberalism may be confounded by Democratic party identification. That is, the proportion of Democratic identifiers in the public may affect both mass liberalism and voters’ willingness to elect Democrats. Column (2) assesses this possibility by controlling for Mass Democratic PID$_{t-2}$, the year before mass liberalism is measured. Mass Democratic PID$_{t-2}$ clearly has a powerful effect on Democratic Control$_t$, increasing the proportion of government controlled by Democrats by 0.07 for each SD change.\footnote{Note that Mass Democratic PID$_{t-2}$ cannot affect Democratic Control$_{t-1}$ be-

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| DV: Democratic Control Index ($t$) | (1)   | (2)   | (3)   | (4)   |
|-----------------------------------|-------|-------|-------|-------|
| Mass Social Lib$_{t-1}$           | 0.046*** | 0.037** | 0.028* |       |
| Mass Econ Lib$_{t-1}$             |       | 0.033** | 0.024* | 0.019 |
| Mass Dem$_{t-2}$                  |       |       | 0.019  | 0.019 |
| Dem Control$_{t-1}$               | 0.660*** | 0.660*** | 0.654*** | 0.590*** |
| Year FEs                         | Yes   | Yes   | Yes   | Yes   |
| State FEs                        | Yes   | Yes   | Yes   | Yes   |

Note: *p<0.1; **p<0.05; ***p<0.01
Accounting for mass PID modestly reduces the magnitude and statistical significance of the mass liberalism coefficients, but according to an $F$ test the two liberalism coefficients together still jointly predict $Democratic Control_t$ ($p = 0.02$).\(^{25}\)

The preceding analysis thus shows that mass liberalism has a modest predictive effect on Democratic control, even accounting for the partisan leanings of the mass public. In order for partisan turnover to be a mechanism of dynamic responsiveness, however, the partisan composition of the government must also affect the liberalism of state policies. As many classic studies of state politics emphasize, the cross-sectional relationship between Democratic control and policy liberalism is actually close to 0, or even negative (e.g., Erikson, Wright, and McIver 1993). But more recent analyses employing regression-discontinuity and dynamic-panel designs have estimated that Democratic control of the governorship or state legislature causes modest but clearly positive changes in state policy liberalism (Caughey, Warshaw, and Xu, Forthcoming). We replicate this latter finding in columns (1) and (5) of Table 3, which show the effect of $Democratic Control_t$ on $Policy Liberalism_t$ in the economic and social domains, respectively. (For this analysis we use the full sample of years.) In both domains, going from full Republican to full Democratic control of the elected branches increases domain-specific policy liberalism in that year by 0.07–0.08 SDs.\(^{26}\)

The coefficients for $Democratic Control_t$ are of the same order of magnitude as the corresponding coefficients for domain-specific mass liberalism reported in columns (2) and (6). These estimates imply that a total switch in party control—which, it should be noted, rarely occurs in a single election—has the same policy impact as an increase in mass liberalism of 1 SD in the social domain and 4 SDs in the economic domain.

\(^{25}\) The the two coefficients remain jointly significant if $Mass Democratic PID_{t-1}$, which may be a consequence as well as a cause of mass liberalism in the same year, is included in the specification instead ($p = 0.04$).

\(^{26}\) These effect sizes are comparable to the regression-discontinuity estimates reported in Caughey, Warshaw, and Xu (Forthcoming). Also, note that the effects of $Democratic Control_t$ on $Policy Liberalism_{t+1}$ are about one-and-a-half-times larger.
Table 3: Turnover and adaptation as mechanisms of dynamic responsiveness. Standard errors are clustered by state and are robust to autocorrelation. The Democratic Control Index ranges from 0 to 1. Other variables are scaled to have a standard deviation of 1 across state-years. The interactive specifications in columns (4) and (8) also allow the LDV coefficients to vary between election and non-election years.

|                  | Social             | Economic          |
|------------------|--------------------|-------------------|
|                  | (1)               | (2)               | (3)               | (4)               | (5)               | (6)               | (7)               | (8)               |
| Dem Control<sub>t</sub> | 0.083*** (0.015)   | 0.051*** (0.009)   | 0.070*** (0.011)   | 0.068*** (0.012)   |
| Mass Lib<sub>t-1</sub> | 0.052*** (0.008)   | 0.047*** (0.008)   | 0.046*** (0.009)   | 0.016** (0.007)    | 0.014* (0.008)    | 0.012* (0.007)    |
| Mass Lib<sub>t-1</sub> × Election<sub>t-1</sub> | 0.014 (0.009)     |                   |                   |                   | 0.008             |
| Year FEs         | Yes               | Yes               | Yes               | Yes               | Yes               | Yes               | Yes               | Yes               |
| State FEs        | Yes               | Yes               | Yes               | Yes               | Yes               | Yes               | Yes               | Yes               |
| 3rd-order LDVs   | Yes               | Yes               | Yes               | Yes               | Yes               | Yes               | Yes               | Yes               |
To assess the degree to which the effect of opinion on policy is mediated by party control (that is, through the mechanism of partisan turnover), we rely on two complementary analyses. First, in the spirit of traditional mediation analysis, we estimate the effect of $\text{Mass Liberalism}_{t-1}$ on $\text{Policy Liberalism}_t$ conditional on $\text{Democratic Control}_t$.\footnote{The results are qualitatively identical if we also control for the Democratic share of each house of the legislature.} We then subtract the estimated direct effect of $\text{Mass Liberalism}_{t-1}$ (column 3/7) from its estimated total effect (column 2/6) to estimate the mediated effect.\footnote{This method is valid under the assumptions of sequential ignorability and no interaction (as well as correct model specification). For details, see Imai, Keele, and Yamamoto (2010).} By this calculation, the estimated mediated effect for social policy is 0.005, or about 10% of the total effect, and the estimate for economic policy is 0.002, or 13% of the total. Similar estimates are obtained if we simply multiply each domain-specific effect of mass liberalism on party control from Table 2 by the effect of party control on policy liberalism from Table 3.

A second way to compare these quantities is to hold Democratic control fixed by design rather than through statistical control. We do this by comparing dynamic responsiveness in years that follow an election, when $\text{Democratic Control}$ could conceivably change, with years not following an election, when $\text{Democratic Control}$ should be the same as in the previous year. Responsiveness in years where only adaptation is possible is captured by the $\text{Mass Liberalism}_{t-1}$ coefficients in columns (4) and (8). Note that these coefficient estimates are nearly identical to their counterparts in columns (3) and (7), and like them are clearly positive. Moreover, the coefficient $\text{Mass Liberalism}_{t-1} \times \text{Election}_{t-1}$, which indicates how much more opinion affects policy when partisan turnover is available as a mechanism, are smaller than the adaptation-only coefficients and not statistically distinguishable from either 0 or the estimated mediated effects reported above.

Given the imprecision of the mediation estimates and the rather strong assump-
tions required to interpret them causally, we should not focus too much on their exact magnitude. At a more general level, however, the results of the foregoing analysis can be interpreted as consistent with adaptation being an important, and perhaps the dominant, mechanism by which mass opinion influences policy change. In other words, it suggests that policymaking is not merely a function of the pre-existing policy commitments of the candidates selected into office, but also of incumbent officials’ responsiveness to shifts in public sentiment.

5 Heterogeneity: Time, Region, and Institutions

In addition to operating through multiple mechanisms, dynamic responsiveness may also be stronger under certain conditions than others. In other words, there may be factors that moderate the effect of opinion on policy. Here we examine five such factors: time, region, suffrage restrictions, campaign contribution limits, and reforms designed to enhance citizen participation in government. Unlike time and region, the last three moderators are institutions that could potentially be manipulated to influence the quality of responsiveness. We emphasize, however, that the interaction effects estimated in the analysis below are purely observational, and nothing about the research design ensures that the effects are not confounded by other attributes of the states where these institutions were adopted.

This being said, it is nonetheless interesting to assess whether and how dynamic responsiveness differs across contexts. The first context we examine is historical era. Has dynamic responsiveness increased over time? The answer is clearly yes, but only in recent decades.\(^{29}\) As indicated by the interactions between \(Mass\ Lib_{t-1}\)
|                        | Social          | Economic        |
|------------------------|-----------------|-----------------|
| Mass Lib<sub>t−1</sub> (1994–2014) | 0.058*** (0.010) | 0.038*** (0.008) | 0.032*** (0.013) |
| Pre-'72 South<sub>t</sub> | 0.039* (0.021)  | 0.021 (0.014)   |
| Suffrage Restriction<sub>t</sub> |                  | 0.019* (0.006)  | 0.007 (0.006)    |
| Contribution Limits<sub>t</sub> | -0.001 (0.003) | -0.002 (0.003)  |
| Citizen Government<sub>t</sub> | -0.004 (0.014) | -0.004 (0.013)  |
| Mass Lib<sub>t−1</sub> × 1972–93<sub>t</sub> | -0.026*** (0.008) | -0.027*** (0.008) | -0.027*** (0.008) |
| Mass Lib<sub>t−1</sub> × 1954–71<sub>t</sub> | -0.021 (0.027) | -0.034** (0.014) | -0.030** (0.015) |
| Mass Lib<sub>t−1</sub> × 1936–53<sub>t</sub> | -0.029** (0.015) | -0.037* (0.020) | -0.036* (0.019) |
| Mass Lib<sub>t−1</sub> × Pre-'72 South<sub>t</sub> | 0.012 (0.014) | -0.008 (0.014)  |
| Mass Lib<sub>t−1</sub> × Suff Restrict<sub>t</sub> | 0.006 (0.009) | -0.009 (0.007)  |
| Mass Lib<sub>t−1</sub> × Contrib Limit<sub>t</sub> | -0.0001 (0.003) | 0.003 (0.002)   |
| Mass Lib<sub>t−1</sub> × Citizen Gov’t<sub>t</sub> | 0.009* (0.005) | 0.001 (0.006)   |

Year FEs: Yes, State FEs: Yes, 3rd-order LDVs: Yes

Note: *p<0.1; **p<0.05; ***p<0.01

Table 4: Moderators of Dynamic Responsiveness: Standard errors are clustered by state and are robust to autocorrelation. Continuous variables are scaled to have a standard deviation of 1 across state-years.
and dummies for the periods 1972–93, 1954–71, and 1936–53 (Table 4), opinion’s dynamic effect on policy has been much larger in the last two decades than it was in the preceding six. Indeed, the estimates in column (3) suggest that on economic issues dynamic responsiveness was almost wholly absent before 1972. Note that the apparent lack of dynamic responsiveness on economic issues does not appear to be a consequence of the undemocratic nature of Southern politics at that time, for the temporal differences remain when mass liberalism is interacted with a dummy variable for the pre-1972 South (which is itself not distinguishable from zero).

Why might responsiveness have increased over time? One possibility is the abolition of various undemocratic institutions, such as poll taxes and literacy tests (which, it should be noted, were not limited to Southern states). A second possible culprit is the implementation of reforms designed to limit the influence of money in politics and enhance citizens’ participation in policymaking. To assess these possibilities, we examine whether the effect of Mass Liberalism_{t−1} is moderated by three indices of related policies: suffrage restrictions (poll tax and literacy test), campaign contribution limits (individual limit, corporate limit, and union contribution ban), and citizen government (direct democracy and term limits). We present the analysis of these policies as indices in order to ameliorate the multiplicity problem of testing many interaction effects, but the same overall picture emerges if we analyze them individually.

On the whole, there is little evidence that these institutions moderate the effect of opinion on policy. The only one of the six index interactions that is statistically significant at the 10% level is Citizen Government, which appears to be associated with stronger dynamic responsiveness on social issues. In an alternative specifica-

30. With six independent tests of true null hypotheses, there is a 47% probability...
tion with individual coefficients for every institution, however, the two component institutions of the citizen government index are very far from statistically significant, either individually or jointly (compare Pacheco 2013). On the other hand, this alternative specification also estimates a significantly positive interaction between a union contribution ban and $\text{Mass Liberalism}_{t-1}$ in the economic domain. This contradicts Table 4, which finds no discernable effect of contribution limits in general.

In sum, aside from a generally increasing trend in responsiveness over time, there is no consistent evidence that one institution or set of institutions moderates dynamic responsiveness. Given that the interaction effects are essentially correlational estimates, however, we should not draw firm conclusions either way about the causal effect of these institutions. It is possible, for example, that reforms such as contribution limits are implemented precisely to counteract a particularly unresponsive state government, thus masking the positive effect of such reforms. Thus, while our results suggest that previous studies may have overstated the responsiveness-enhancing effects of these institutional reforms, this is clearly an area where more research is needed.

6 Discussion

What do our findings suggest about the character and functioning of American democracy? At the most basic level, they indicate that state policymaking clearly responds to mass policy preferences. Given the particularly high barriers to responsiveness in state politics—limited state control over some policies, the competitive constraints of federalism, citizens' inattentiveness to state politics—this fact alone should provide a counter to more pessimistic accounts of American democracy. Our results also call into question an emerging scholarly sense, approaching a consensus, that of rejecting at least one hypothesis at the 10% level ($1 - 0.9^6 = 0.469$).
partisan selection is the dominant if not exclusive means by which voters affect government policies. Manifestations of this quasi-consensus can be seen in theoretical work that stresses candidates’ inability to commit to moderate policies (e.g., Alesina 1988; Besley and Coate 1997), causal analyses that find little evidence of adaptation in Congress (e.g., Lee, Moretti, and Butler 2004; Fowler and Hall, Forthcoming), and studies that emphasize the “leapfrog” nature of representation in the contemporary United States (e.g., Bafumi and Herron 2010; Lax and Phillips 2011). By contrast, our finding that adaptation is a major if not dominant mechanism of responsiveness is consistent with classic studies that emphasize politicians’ attentiveness to public sentiment and their capacity and incentives to adapt to shifts in mass opinion (e.g., Mayhew 1974; Arnold 1990; Erikson, Wright, and McIver 1993; Stimson, MacKuen, and Erikson 1995).

It should be emphasized that partisan turnover is a comparatively minor mechanism of responsiveness not because party control has no policy effects, but rather because mass policy preferences explain relatively little of the variation in party fortunes. In other words, both public opinion and party control affect state policies, but variation in one is not strongly related to the other. This suggests an important qualification to the dim view, expressed by Achen and Bartels (2016) and others, that the apparently weak relationship between mass policy preferences and partisan fortunes implies that citizens have little influence over government policies. Rather, mass liberalism and party control seem to exert fairly independent and, according to Table 3, roughly equally important effects on policy change. At the same time, neither mass liberalism nor party control cause dramatic swings in policymaking, as some have feared. Even a full switch in party control, for example, changes policy liberalism by less than a tenth of a standard deviation. In general, large shifts in policy liberalism occur only through the compounding of many small responses to party control and mass preferences. Indeed, it is the cumulation of incremental changes
over many decades that arguably accounts for the nearly one-to-one cross-sectional relationship between opinion and policy that we see today.

In these respects, then, our findings provide some reassurance regarding the health of American democracy. In other respects, however, our analyses are indeterminate or even pessimistic. First, since our measures of mass and state policy liberalism are not on the same scale, we cannot directly evaluate whether state policies are congruent with mass preferences at any given moment (cf. Achen 1978; Matsusaka 2001; Lax and Phillips 2011). In particular, the fact that state policymaking is responsive on the margin does not preclude the existence of ideological bias in state policies. Indeed, notwithstanding the lack of evidence for regional differences in dynamic responsiveness, the persistent gap in policy liberalism between Southern and non-Southern states with similar mass publics (see Figure 3) implies that the policies of one set of states are systematically biased in a liberal or conservative direction. Relatedly, our results do not rule out the possibility of differential responsiveness across subsets of the population, such as racial minorities or the poor (e.g., Gilens 2012). Finally, our analysis of institutional moderators, though hardly the final word on the subject, suggests little reason for faith in institutional reforms, at least among those that have been widely implemented at the state level, as a means of improving dynamic responsiveness.

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A: Details on Model of Citizen Policy Liberalism

In the main paper, we examine dynamic responsiveness in the American states using new estimates of state-level citizen policy liberalism, 1936–2014. Here, we focus on describing and validating these estimates.

**Measurement Model**

The lack of a valid, time-varying measure of citizen policy liberalism has been one of the main barriers to the study of responsiveness in the American states. As a result, studies of state politics have overwhelmingly relied on proxies for public opinion such as ideological self-identification (Erikson, Wright, and McIver 1993), presidential vote (Shor and McCarty 2011), or the roll-call records of the state congressional delegation (Berry et al. 1998).

To overcome this challenge, we estimate the latent policy liberalism of state publics in every year between 1936 and 2014 by aggregating responses to many distinct policy questions across many polls.\(^{31}\) We estimate citizen policy liberalism using the sta...
tical framework of item-response theory (e.g., Tausanovitch and Warshaw 2013). In the two-parameter IRT model, the relationship between responses to question \( q \) and the unobserved trait \( \theta_i \) is governed by the question’s threshold \( \kappa_{qt} \), which captures the base level of support for the question, and its dispersion \( \sigma_q \), which represents question-specific measurement error. Under this model, respondent \( i \)'s probability of selecting the liberal response to question \( q \) is:

\[
\pi_{iq} = \Phi \left( \frac{\theta_i - \kappa_{qt}}{\sigma_q} \right),
\]

(1)

where the normal CDF \( \Phi \) maps \((\theta_i - \kappa_{qt})/\sigma_q\) to the (0, 1) interval.\(^{32}\) The model assumes that greater liberalism (i.e., higher values of \( \theta_i \)) increases respondents’ probability of answering liberally. The strength of this relationship is inversely proportional to \( \sigma_q \), and the threshold for a liberal response is governed by \( \kappa_{qt} \).

Accurate estimation of \( \theta_i \) requires data from many respondents, each of whom answers many items (Lewis 2001, 277). Prior to the 2000s, however, few surveys included more than a handful of policy questions, and those questions that were included were rarely asked in consistent form across many years. 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 the average citizen liberalism in each group (e.g., state-year). Fortunately, as Bailey (2001), Lewis (2001), and others have noted, it is possible to make inferences about the average level of \( \theta_i \) in each group even when individual-level estimation is not feasible.

Following Caughey and Warshaw (2015), we do this by treating individual citizens as having been randomly sampled from a given subpopulation \( g \) defined by demographic and geographic characteristics (e.g., rural, white Alabamans). Assum-

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32. A common alternative way of writing the model in Equation (1) is \( \text{Pr}(y_{iq} = 1) = \Phi(\beta_q \theta_i - \alpha_q) \), where \( \beta_q = 1/\sigma_q \) and \( \alpha_q = \kappa_{qt} \times \beta_q \).
ing that $\theta_{i[g]}$ is distributed normally with mean $\bar{\theta}_g$ and standard deviation $\sigma_\theta$, we can re-write the individual IRT model at the group level as

$$p_{gj} = \Phi \left( \frac{\bar{\theta}_g - \kappa_j}{\sqrt{\sigma^2_j + \sigma^2_\theta}} \right), \quad (2)$$

where $p_{gj}$ is the probability that a randomly sampled citizen from group $g$ will give a liberal answer to item $j$ (Mislevy 1983). We then model group $g$’s total number of liberal answers to item $j$ as

$$s_{gj} \sim \text{Binomial}(n_{gj}, p_{gj}), \quad (3)$$

where $n_{gj}$ is group $g$’s total number of non-missing responses to question $j$ and $s_{gj}$ is the number of those responses that are liberal.\textsuperscript{33} 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 our large-scale dataset of approximately 1.5 million survey 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. Letting $\xi_t$ be an intercept common to all groups and $x_g$, a vector of hierarchical predictors ($\text{Race}$, $\text{Urban}$, and $\text{State}$), we model the group

\textsuperscript{33} 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.”

A-3
means in each year as

\[
\tilde{\theta}_{gt} \sim N(\delta_t \tilde{\theta}_{g,t-1} + \xi_t + x'_g \gamma_t, \sigma^2_{\tilde{\theta}_t}),
\]

(4)

That is, the prior expected value for \( \tilde{\theta}_{gt} \) is a weighted combination of its lagged value and predictions based on demographically similar groups, with the variance of the prior determined by \( \sigma^2_{\tilde{\theta}_t} \). If there are no survey responses from group \( g \) in year \( t \), (4) acts as an imputation model for the missing data. Mean citizen liberalism in each state can be estimated by poststratifying the group estimates to match groups’ proportions of the population (Park, Gelman, and Bafumi 2004).

To estimate mass liberalism on the economic and social domains, we rely on a dataset of survey responses to over 300 domestic policy questions spread across nearly 1,000 public-opinion surveys fielded between 1936 and 2014. Overall, the responses of approximately 1.5 million distinct individuals are represented in the data.\(^{34}\) We use the dynamic group-level IRT model described above to estimate average liberalism in groups defined by state, race, and urban residence.\(^{35}\) To generate annual estimates of average opinion in each state, we poststratified the group estimates to match the groups’ proportions in the state population, based on data from the U.S. Census.

\(^{34}\) The model of social policy liberalism includes 722,620 respondents from 423 individual polls that were asked 53 distinct policy questions. The model of economic policy liberalism includes 1,177,652 respondents from 783 polls that were asked 310 distinct policy questions.

\(^{35}\) We also raked the survey data to match interpolated targets for gender and education level in each state public, based on microdata from the U.S. Census (Ruggles et al. 2010). In order to mitigate sampling error for small states, we model the state effects in the first time period as a function of state Proportion Evangelical/Mormon. The inclusion of state attributes in the model partially pools information across similar geographical units in the first time period, improving the efficiency of state estimates (e.g., Park, Gelman, and Bafumi 2004). We drop Proportion Evangelical/Mormon after the first period because we found that the state intercept in the previous period tends to be much more predictive than state attributes. We estimate the dynamic, hierarchical group-level IRT model using the \texttt{dgo} package in \texttt{R} (Dunham, Caughey, and Warshaw 2016). The full details of the model, and an extensive validation of the estimates of latent opinion, are in Supplementary Appendix A.
(Ruggles et al. 2010). Finally, we standardized the citizen liberalism estimates in order to make them easier to interpret.

**Construct Validation**

To assess our estimates’ validity as a measure of policy liberalism, we examine their correlation with measures of theoretically related constructs (a procedure Adcock and Collier 2001 refer to as “construct validation”). Specifically, we examine the cross-sectional correlation between our measure of policy liberalism and Democrats’ presidential vote share. While presidential election results are not a perfect measure of citizens’ policy preferences (Levendusky, Pope, and Jackman 2008; Kernell 2009), a variety of previous scholars have used presidential election returns to estimate state and district preferences (Ansolabehere, Snyder, and Stewart 2001; Canes-Wrone, Brady, and Cogan 2002). Thus, to the extent that policy attitudes predict presidential partisanship, a high correlation with Democratic presidential vote share would suggest that our estimates are accurate measures of states’ policy preferences. Figures A1 and A2 show that there is indeed a strong cross-sectional relationship between our estimates of citizen policy liberalism and presidential vote share between 1936 and 2012 in non-southern states.\(^3\) Moreover, the relationship increases in strength over time, mirroring the growing alignment of policy preferences with partisanship and presidential voting at the individual level (Fiorina and Abrams 2008, 577–82).

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\(^3\) We find a similarly strong relationship between our estimates of state policy liberalism and estimates of state ideology from exit polls over the past few decades.
Figure A1: Relationship between economic policy liberalism and Democratic presidential vote share in the non-south, 1936–2012.
Figure A2: Relationship between social policy liberalism and Democratic presidential vote share in the non-south, 1936–2012.
B: Summary statistics for key independent variables

Table A1 shows summary statistics for our key independent variables.

Table A1: Summary Statistics of the Main Variables.

| Variable                        | N   | Mean | St. Dev. | Min  | Max  |
|---------------------------------|-----|------|----------|------|------|
| Social Policy Liberalism        | 3,904 | 0.00 | 1.00     | -2.90 | 3.12 |
| Economic Policy Liberalism      | 3,904 | 0.00 | 1.00     | -2.36 | 3.32 |
| Mass Social Liberalism          | 3,950 | 0.00 | 1.00     | -2.56 | 5.01 |
| Mass Economic Liberalism        | 3,950 | 0.00 | 1.00     | -4.21 | 2.76 |
| Democratic PID                  | 3,450 | 0.00 | 1.00     | -2.08 | 4.23 |
| Democratic Control              | 3,678 | 0.58 | 0.39     | 0.00  | 1.00 |
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