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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 change 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.

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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? The full answer is surely complex, involving among other things turnover 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: elected officials should respond to public opinion by moving policy in its direction. Dynamic responsiveness of this kind 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]

Dynamic responsiveness has been documented primarily at the national level, especially in the United States but also in Canada and the United Kingdom. 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: *partisan selection* (the election of candidates of one partisan type rather than another) and *adaptation* (driven primarily by elected officials' anticipation of voter sanctions). While the dynamic responsiveness literature 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 citizens' influence over government policies.

These optimistic conclusions, however, have been subject to trenchant critiques. 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 selection. Indeed, notwithstanding the contrary arguments of Stimson, MacKuen, and Erikson (1995), the prevailing scholarly

1. We use the term *dynamic responsiveness* instead of *dynamic representation* (Stimson, MacKuen, and Erikson 1995) in order to distinguish responsiveness from alternative measures of representation, such as proximity or congruence (Achen 1978). Responsiveness is often considered the hallmark of democracy (Dahl 1971), though it is not by itself a sufficient condition. For other necessary conditions, see, e.g., Dahl (1989).

view is that partisan selection dominates adaptation as a mechanism of responsiveness in the United States—and in recent decades, increasingly so (Levitt 1996; 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 2012).

To some degree, these divergent conclusions stem from differences in research design. Most studies that emphasize ideological adaptation examine how policymaking responds to mass opinion in a single country over time (e.g., Stimson, MacKuen, and Erikson 1995; Soroka and Wlezien 2010; but see Kousser, Lewis, and Masket 2007). By contrast, work that stresses the dominance of partisan selection is overwhelmingly cross-sectional, typically examining roll-call voting in a single legislature.[2] Each approach has its advantages and limitations. Time-series studies have the advantage of being explicitly dynamic in orientation and also of focusing on government policies, which are arguably the ultimate metric of representation. But due to the inherent limitations of time-series analysis (small samples, model dependence, etc.), the results of within-country studies tend to be somewhat fragile. For their part, cross-sectional studies tend to have large sample sizes and often employ stronger identification strategies, such as regression-discontinuity (RD) designs. But they too are limited by their focus on within-legislature variation in roll-call voting or other forms of position-taking, which means that they cannot detect governments' *collective* responsiveness to popular preferences (Weissberg 1978).

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

2. 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.

not only the positions politicians take, but what governments actually *do*. A further advantage of state politics is that variation across states provides a natural point of comparison or benchmark for assessing the substantive magnitude of dynamic responsiveness.

Notwithstanding the methodological attractions, U.S. states present 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 and increasing nationalization of elections mean that state elections are powerfully affected by national tides, undermining the direct accountability relationship between state-level officials and their electorates (Rogers 2016; Hopkins 2016). 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 2012), 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 measurement 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, 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, 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 comparatively liberal, its policies tend to become more liberal relative to other states. 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 selection does indeed mediate dynamic responsiveness. But we also find that policy reacts directly to citizen liberalism, holding constant the party that controls the government. This suggests that adaptation is an important, and perhaps 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 that the cross-sectional relationship between opinion and policy has always been stronger outside the South, and we find some evidence of differential dynamic responsiveness between regions as well, though primarily in recent decades. We also consider various laws and institutions thought to influence representation—including suffrage restrictions, campaign contribution limits, direct democracy, and legislative professionalism—but find no reliable evidence that they moderate dynamic responsiveness.

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 2012). We 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 2012), 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 Framework

As a theoretical framework for our analysis, we sketch a dynamic model of representation, building on the work of Achen (1978) and others. In our framework, ideological variation is assumed to be one-dimensional within a given policy domain. We presume that governments respond on the margin to mass preferences, making policy more liberal when the public moves left and more conservative when it moves right. Such responsiveness does not imply, however, that policies are necessarily congruent with mass preferences. Rather, due to factors ranging from state governments' resource constraints to inequality of policy influence across citizens, policies may be systematically biased relative to what the average citizen desires. Nor is responsiveness necessarily proportionate; governments may respond by moving policy less than the public desires, or alternatively they may over-react to public opinion and oscillate between extreme policy positions.

Furthermore, in our model—and here we depart from cross-sectional models like Achen's—responsiveness need not be immediate. This acknowledges the numerous sources of status-quo bias in policymaking, including the prevalence of budgetary incrementalism, the veto power of pivotal legislators, limited space on the political agenda, and incumbents' insulation from mid-term removal. Together, these barriers conspire to make it difficult to overturn existing policies. Thus, even if elected officials are perfectly representative, they will often be

unable to bring all policies immediately in line with new configurations of mass preferences. Rather, a sudden one-time change in mass liberalism will be incorporated incrementally into policy liberalism, as in each year the state updates a portion of its policies. Eventually, if mass opinion remains stable, this model predicts that the state will reach a new policy equilibrium that reflects both the influence of the mass public and the persistent sources of policymaking bias in that state. In short, a dynamic model of representations implies that responsiveness should be incremental, with modest short-term effects potentially cumulating into large long-run differences.[3]

1.1 Mechanisms

In a representative democracy, there are two main mechanisms by which mass publics can influence policymaking, which we refer to as *selection* and *adaptation* (compare Miller and Stokes 1963; Stimson, MacKuen, and Erikson 1995; Fearon 1999). In the selection mechanism, citizens influence government policymaking by electing candidates whose ideological type best represents their views. In the contemporary American two-party system, this generally entails choosing between Democrats and Republicans—that is, *partisan selection*. For partisan selection to be an effective channel for responsiveness, a two-step process is required. First, mass liberalism must affect which party wins elections. Second, the partisan outcome of elections must affect policy liberalism. Partisan selection is thus the part of mass liberalism's effect on policy that is mediated by party control of government offices.

Adaptation, by contrast, is the portion of responsiveness not mediated by party control—that is, with party control held constant. Most theoretical work on adaptation has focused on individual incumbents' incentives to preempt electoral sanctions by responding preemptively to public sentiment (Downs 1957; Mayhew 1974; Kingdon 1989; Snyder and Ting 2003). In

3. It should be noted that our model of dynamic responsiveness differs from those of Stimson, MacKuen, and Erikson (1995) and Soroka and Wlezien (2010) in that we define mass liberalism as a measure of absolute preference. They, by contrast, conceptualize policy "mood" as a preference for policy *change*—that is, for more or less government than is currently being provided (see Stimson 1991). Their model thus implies that mood, being partly a function of current policy, should respond "thermostatically" to policy changes, whereas no such negative feedback loop is implied by our model.

principle, such individual-level adaptation can result in perfect responsiveness without the replacement of a single incumbent (and thus without any change in party control). As defined here, however, adaptation also encompasses within-party turnover: the replacement of moderate incumbents with more extreme members of the same party, or vice versa.

On the whole, the empirical literature on responsiveness emphasizes the dominance of selection over adaptation (Levitt 1996; Ansolabehere, Snyder, and Stewart 2001; Lee, Moretti, and Butler 2004; Poole 2007; but see Stimson, MacKuen, and Erikson 1995; Kousser, Lewis, and Masket 2007). There is certainly ample evidence for the second step in the selection mechanism, partisan effects on policy. At the state level, for example, electing Democrats rather than Republicans leads to much more liberal legislative representation and to modestly more liberal state policies (Shor and McCarty 2011; Caughey, Warshaw, and Xu, Forthcoming; Fowler and Hall, Forthcoming; Caughey, Tausanovitch, and Warshaw 2017). In the legislature, partisan effects on policy seem to be driven predominantly by shifts in majority control, the size of the majority having little independent effect on policy (Caughey, Warshaw, and Xu, Forthcoming). The evidence for the first step—mass liberalism's effect on elections—is less robust, especially in studies of dynamic responsiveness. Achen and Bartels, for example, stress the fragility and model-dependence of the evidence for partisan selection in national politics, leading them to conclude that mass policy preferences "are of relatively little importance in determining who wins" elections (Achen and Bartels 2016, 46). Though there is less empirical work on the subject, the dynamic relationship between mass liberalism and election outcomes is likely to be even weaker in the states, where electoral shifts are dominated by exogenous national conditions (Rogers 2016). In short, notwithstanding the evidence for party effects, it is unclear how much of state policy responsiveness is mediated through party control.

On the other hand, there is reason to believe that adaptation is a more important mechanism of state policy responsiveness than the existing literature suggests. Most existing studies focus on roll-call voting in a single legislature, which means that they cannot mea-

sure *collective* responsiveness to public opinion. Thus, if a state public moves to the right and all officials respond equally to this shift, a comparison of state legislators' roll-call votes will not detect any adaptation, only cross-sectional ideological differences between legislators.[4] The relatively few studies that examine opinion effects on policy rather than roll calls, whether in cross section (Erikson, Wright, and McIver 1993) or time series (Erikson, MacKuen, and Stimson 2002), tend to find greater evidence for responsiveness unmediated by party control. In sum, we expect adaptation to be a more important mechanism of state policy responsiveness than the more general literature on responsiveness suggests.

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 combine different policy domains (e.g., Erikson, Wright, and McIver 1993) or else focus almost exclusively on social policies (e.g., Lax and Phillips 2009, 2012). What evidence there is for responsiveness on economic issues tends to be somewhat weaker (Pacheco 2013).[5] This is not surprising, for there are several reasons to expect states to be less responsive on economic than social issues.

First, states tend to have less policymaking discretion on economic issues. Federal and state governments share responsibility over many policy areas, and 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. State taxing and spending choices are also constrained by economic competition with other jurisdictions. Thus, regardless of their citizens' preferences, states can increase taxes and regulations only so much before businesses and higher-income citizens vote with their feet by moving to other states (Oates 1972; Bailey and Rom 2004).

4. This is true unless the scaling bridges legislators' ideal points across time using comparable roll-call votes, which is rarely done (for an exception, see Bailey 2007).

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.

Economic and social issues differ at the mass level as well. Because social policies tend to be more symbolic than technical and to concern ends rather than means, they are more likely than economic policies to be "easy" issues for citizens. Citizens are thus likely to find it easier to "calculate relative positioning of parties and candidates" on social issues (Carmines and Stimson 1980, 82). Citizens' policy preferences on social issues are also likely to be more stable and coherent than their economic preferences, making it easier for politicians to discern signal from noise in public opinion.[6] 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 institutional and other contexts. Indeed, as Lax and Phillips (2012, 158) 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 by examining four 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*, mainly in Southern states (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

6. See Przeworski, Stokes, and Manin (1999, 8–9) on responsiveness as the relationship between signals (expressions of public preferences) and policies (authoritative government decisions).

tend to 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). 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 legislators' ideology (Barber 2016; La Raja and Schaffner 2015) and state policy (Besley and Case 2003; Werner and Coleman 2013), but no previous study has examined the effect of campaign finance rules on the responsiveness of state policies to public opinion.

Another set of institutions that possibly improve responsiveness are reforms designed to enhance what might be called *citizen governance*, such as direct democracy and term limits. Direct democracy might do so 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 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 help them learn about voter preferences (Matsusaka 2008). Despite sound theoretical reasons to expect that direct democracy might improve responsiveness, empirical studies of its effects have been ambiguous.[7]

Term limits might increase responsiveness by inducing greater turnover among legislators. This could lead to the election of legislators who better reflect constituents' (current) prefer-

7. Some studies find that direct democracy enhances responsiveness, at least in some policy areas (Arceneaux 2002; Gerber 1996; Matsusaka 2010), while other studies find that it has no effect on responsiveness (Monogan, Gray, and Lowery 2009; Lascher, Hagen, and Rochlin 1996; Lax and Phillips 2009, 2012).

ences. On the other hand, term limits could lead to shirking, particularly among legislators not planning to seek another office (Clark and Williams 2014). It could also 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 2012).

Finally, *legislative professionalism* may affect state governments' responsiveness to public opinion. Some states, such as California, have very professional legislatures that resemble the U.S. Congress, whereas others, such as Vermont, have part-time legislators that meet for only a few weeks a year (Squire 1992, 2007). Professional chambers can use their resources to assess changes in mass opinion. Also, there are greater incentives for lawmakers in professional chambers to be responsive to the public in order to retain office (Maestas 2000). As a result, we might expect states with more professionalized legislatures to be more responsive to public opinion. Two recent studies find that states with higher levels of legislative professionalism are more responsive to public opinion (Pacheco 2013; Lax and Phillips 2012), while another recent study finds no effect on responsiveness (Lax and Phillips 2009).

2 Modeling Strategy

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*.[8] Defined this way, responsiveness is a descriptive quantity: it simply captures the covaria-

8. 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 no difficulty in extending his conception of responsiveness to the latter. As noted by Achen and more recently by Matsusaka (2001), greater responsiveness does not necessarily imply that government outputs more proximate to or congruent with public preferences.

tion 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—its political culture, for example—confounds the relationship between mass liberalism and policy liberalism, or even the possibility that policy liberalism causes mass 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.[9] 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 dynamic relationship between mass liberalism and policy liberalism, accounting for policy liberalism's recent history.

Where we depart from these authors is in our use of TSCS data. A time-series–cross-sectional approach offers considerable advantages over a purely time-series one. It enables us to estimate a dynamic panel model that includes not only a lagged dependent variable (LDV), 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 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).[10] In substantive terms, the state FEs in particular can be interpreted as capturing the policymaking bias unique to each state.

9. 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.

10. Dynamic panel models suffer from finite-sample bias (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).

The inclusion of an LDV is also very important, however, for past policies are just the sort of time-varying state-specific confounders that FEs alone cannot account for.[11] Including an LDV also enables us to analyze how mass liberalism affects policy liberalism over both the short and the long term. In short, while our dynamic panel model 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.

Before describing the details of data and measures, we note a final important element of our empirical strategy, which is to account for the measurement error in our key variables. The main independent and dependent variables in this study—mass liberalism and policy liberalism in each issue domain—are latent quantities whose values must be inferred rather than directly observed. The measurement error in these latent quantities can bias point estimates and standard errors. Thus, in all of our regression analyses, we account for measurement error using a technique known as the "method of composition" (MOC) or "propagated uncertainty" (Tanner 1996, 52; Treier and Jackman 2008, 215–6; Kastellec et al. 2015, 791–2).[12] The main consequence of these adjustments is to attenuate the estimated effects of mass liberalism by about one-third relative to the unadjusted estimates.

3 Data and Measures

This section describes the data and measures we use in our analysis. For summary statistics on our key variables, see Supplementary Appendix C.

3.1 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 an-

11. State FEs explain only a small amount of additional variation once lagged policy liberalism is controlled for. An F test easily rejects the hypothesis that state FEs add no explanatory power, but a Lagrange multiplier test yields ambiguous conclusions.

12. See Supplementary Appendix D for more details.

nual 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. Moreover, there are often small samples available in any particular year, particularly for smaller states. These challenges make 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, to address sparse survey samples Pacheco smooths the state estimates with multilevel regression coupled with a five-year moving average, which improves the reliability of estimates in smaller states but 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-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, hierarchical group-level item-response model (Caughey and Warshaw 2015; see Supplementary Appendix for more details). While conceptually 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 mass liberalism measures differ from mood in two respects.[13] First, mood is a relative 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

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, the dynamic, hierarchical group-level IRT model accommodates cross-sectional and over-time variation within a common framework.

the policy *status quo* and are thus intended as measures of absolute, not relative, liberalism. This is important because the overwhelming majority of survey questions in our data either pertain to national policy or policy in the abstract, not state policies specifically. Our conception of mass liberalism as an absolute measure is thus primarily a practical concession to the available polling data.

A second difference is that we estimate mass liberalism separately for economic and social issues (compare Treier and Hillygus 2009; Stimson, Thiébaut, and Tiberj 2012).[14] We do so because mass policy preferences across domains have exhibited distinct temporal dynamics and were, until recently, only weakly correlated. This is 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 typically exhibit much more ideological structure than individuals. Thus while treating mass liberalism as 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

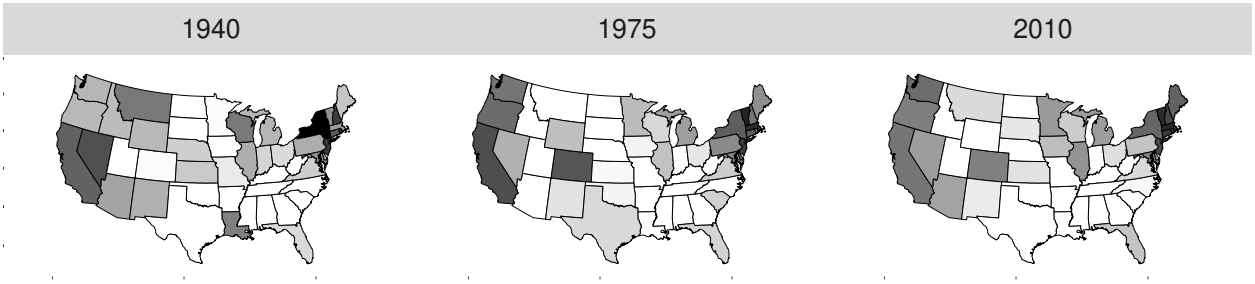
14. We also considered estimating liberalism on racial issues as well, but found that the relative paucity of survey questions in this domain made it difficult to estimate racial liberalism over a long time span.

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 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.

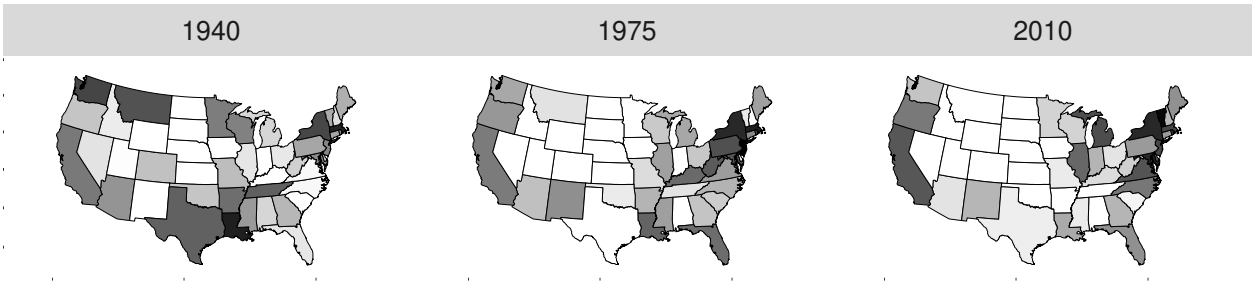
To construct our measure of mass liberalism, we first used a dynamic group-level IRT model to estimate annual average liberalism in groups defined by state, race, and urban residence.[15] Then, using data from the U.S. Census (Ruggles et al. 2010), we poststratified the group estimates to match the groups' 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 1 maps our estimates of mass social and economic 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 more conservative on the social dimension than the economic dimension. Moreover, 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, mass liberalism has shifted substantially over time in only a few states. States in New England have become more liberal, while states such as Idaho and Louisiana have become more conservative.

15. We estimate the IRT model using the R package `dgo` (Dunham, Caughey, and Warshaw 2016). Supplementary Appendix A provides more details on the model estimation procedure and Supplementary Appendix B demonstrates the validity of the estimates.



(a) Mass Social Liberalism



(b) Mass Economic Liberalism

Figure 1: Mass liberalism by state, 1940–2010. Darker shading indicates more liberal opinion. To accentuate the color contrasts, the estimates in this figure are standardized within year.

3.2 State Policies

We next require a measure of the liberalism of state policies. For consistency with our domain-specific measures of mass liberalism, 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.[16] The policy dataset underlying the policy liberalism scores is designed to

16. The model, which extends that of Quinn (2004), is dynamic in that policy liberalism is estimated

include all politically salient state policy outputs on which comparable data are available for at least five years.[17] 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).

3.3 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 governance (direct democracy and term limits) were obtained from Matsusaka (2008) and from the National Conference of State Legislatures. There are no existing measures of legislative professionalism that span our entire time period.[18] Thus, we construct a simple measure of latent legislative professionalism using the natural log of the number of days that each state legislature is in

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

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

18. This is largely due to the fact that data on staff and budgets are not readily available before the 1970s.

session during a two year period based on data from the Book of the States.[19] Data on the partisanship of state officials comes from Klarner (2013).

4 Responsiveness: Cross-Sectional and Dynamic

We now turn to the relationship between mass liberalism and the liberalism of government policies. We begin with a cross-sectional analysis typical of most studies of responsiveness. Figure 2 plots the state-level relationship between mass liberalism and policy 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 2 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.[20] 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 Southern states remain essentially uncorrelated with public opinion as well as substantially more conservative than in non-Southern states.

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 responsiveness on social

19. Data on legislative days were missing for 15% of state-term dyads. We linearly interpolated the (logged) missing values within states using the R package `Amelia` (Honaker, King, and Blackwell 2011). The cross-sectional correlation between our measure of professionalism and the more holistic measures from 1979, 1986, 1996, and 2003 in Squire (2007) is 0.7.

20. Mickey (2015) argues that the democratization of the former Confederacy was not complete until 1972. For the classic critique of the South's one-party system, see chapter 14 of Key (1949).

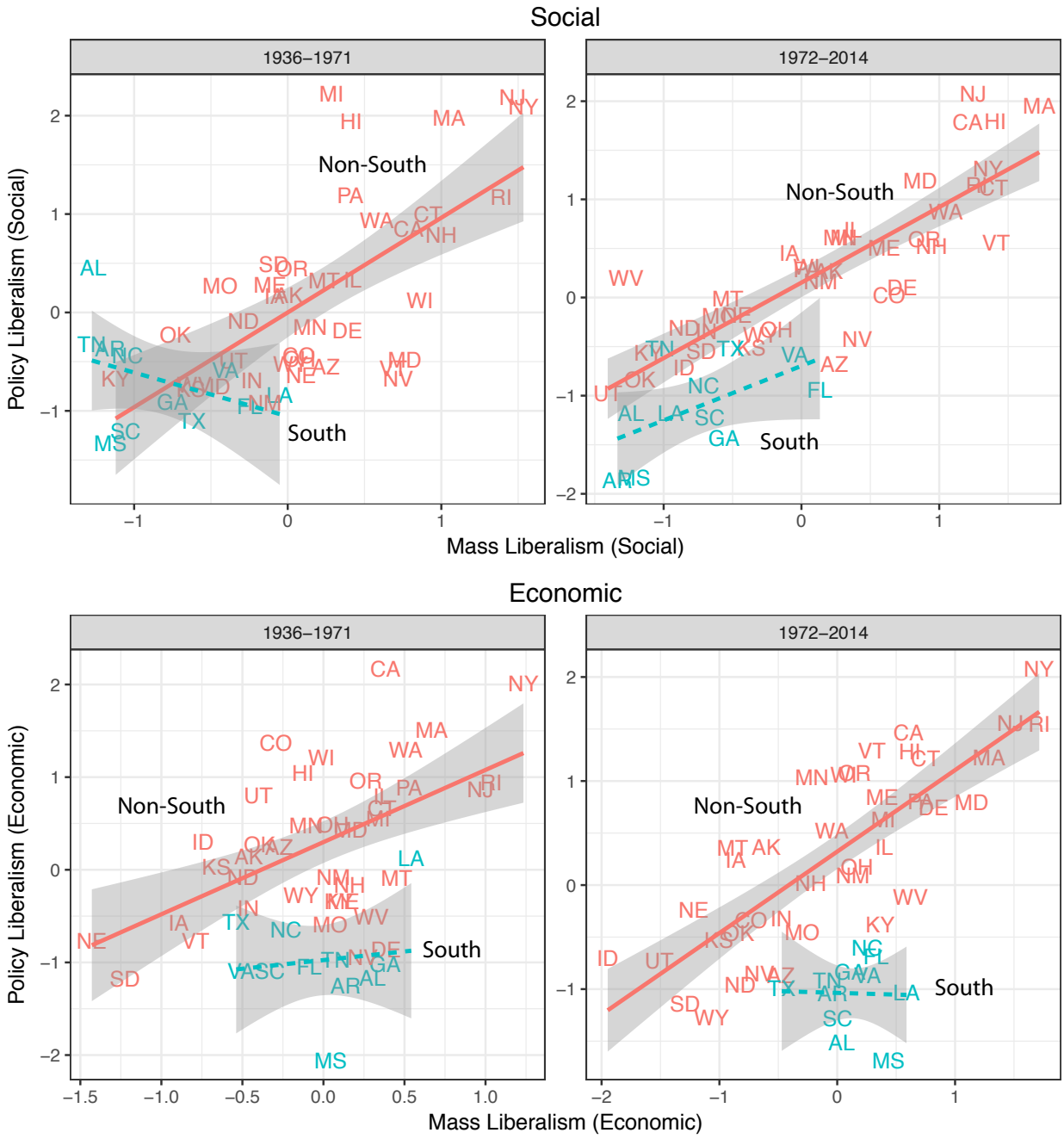


Figure 2: Cross-sectional relationship between mass and government policy liberalism, by era and issue domain.

and economic issues, respectively, averaged over the entire 1936–2014 period. (Note that these estimates correct for measurement error in the liberalism measures.) 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 Liberalism*$_{t-1}$ in column (1) indicates, outside the South there is nearly

a one-to-one cross-sectional relationship between mass and policy liberalism on social issues: a 1-SD difference on one is associated with a 0.8-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. 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 policy liberalism. Taken at face value as causal estimates, the coefficients from the two-way FE model 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. On economic issues, the opinion–policy relationship again disappears in the South, but on social issues it is if anything stronger 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. The responsiveness estimates in column (3) and (6), which control for lagged policy liberalism instead of state FEs, are an order of magnitude smaller in magnitude. As indicated by the lag coefficients, policy liberalism in both domains is powerfully predicted by its lagged values (though the lag coefficient is clearly less than 1, indicating mean-reversion). Adding state FEs back in, as in columns (4) and (8), shrinks the estimates only a little further. Nevertheless, all specifications supply evidence that non-

	DV: Domain-Specific Policy Liberalism (t)							
	——— Social ———				——— Economic ———			
	XS	FE	LDV	DP	XS	FE	LDV	DP
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mass Liberalism$_{t-1}$	**.816**	**.255**	**.042**	**.036**	**.669**	**.280**	**.022**	**.014**
	(.110)	(.076)	(.007)	(.009)	(.090)	(.071)	(.006)	(.008)
Mass Lib$_{t-1}$ × South	**−.468**	.162	−.023	−.019	**−.684**	**−.291**	−.017	−.010
	(.173)	(.143)	(.017)	(.025)	(.129)	(.089)	(.014)	(.016)
Policy Liberalism$_{t-1}$			**.971**	**.935**			**.979**	**.937**
			(.006)	(.014)			(.005)	(.012)
Year × South FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FEs	No	Yes	No	Yes	No	Yes	No	Yes
Observations	3,854	3,854	3,854	3,854	3,854	3,854	3,854	3,854
Adjusted R^2	.556	.797	.973	.973	.556	.797	.971	.972

Table 1: Cross-sectional and dynamic responsiveness, by issue domain and region. XS = pooled cross-sectional regression; FE = two-way fixed effects; LDV = lagged dependent variable; 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. Estimates are corrected for measurement error. Bold coefficients are statistically significant at the 10% level.

Southern states are responsive to their publics. Although the regional interactions in the dynamic models are statistically insignificant, we also cannot reject the hypothesis of no responsiveness in the South, especially on economic issues (we explore this further in our discussion of moderators below).

Consistent with our expectations regarding differences across policy domains, the substantive magnitude of dynamic responsiveness appears to be greater on social than economic issues. Averaging across regions, the dynamic panel model estimates a standardized opinion effect of 0.032 for social policy as compared to 0.012 for economic policy. That is, the policy effect of a 1-SD difference in mass opinion is almost 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]

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 mass liberalism over all future time periods (De Boef and Keele 2008, 191). On social issues, the estimated long-run multiplier of *Mass Liberalism*$_{t-1}$ is 0.57 (SE=0.17) in the non-South and 0.21 (0.30) in the South. On economic issues, the analogous estimates are 0.23 (0.12) for the non-South and 0.03 (0.11) for the South. That is, if the public of a non-Southern state suddenly became 1 unit more liberal on social issues, we would expect the state's social policy liberalism to eventually settle at a new equilibrium 0.57 units above its old equilibrium (assuming no national trends in social liberalism).[22] The effect would occur gradually, however. It would take more than 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 is

21. Supplementary Appendix F shows the robustness of these results to other model specifications.

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.

consistent with the hypothesis that the strong contemporaneous correlation between state policies and opinion is the product of the long-term, gradual accumulation of incremental policy responses to mass preferences.

5 Mechanisms: Partisan Turnover and Adaptation

As noted earlier, dynamic responsiveness to popular preferences can occur by two main mechanisms: partisan selection and adaptation. Partisan selection is a two-step process. First, voters' liberalism must affect their probability of electing candidates of one party over another. Second, the newly elected officials must implement different policies than their opponents would have. In short, if greater liberalism in the public causes the election of more Democrats, and electing more Democrats causes policies to become more liberal, then partisan selection mediates the effect of opinion on policy. Adaptation, by contrast, is that portion of dynamic responsiveness not mediated by the selection of candidates of one party or another, but rather is the result of officials in each party responding directly to shifts in public sentiment. In sum, evaluating the relative importance of partisan selection and adaptation entails estimating three causal effects: the effect of mass liberalism on party control of government, the effect of party control on policy liberalism, and the effect of mass liberalism on policy liberalism with party control held constant.

We begin our empirical analysis with the first effect, that of mass liberalism on party control. 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.[23] 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*

23. We focus on legislative control rather than seat share because in dynamic models, the Democratic share of all legislative seats is not a significant predictor of policy liberalism. Controlling for legislative seat share does not qualitatively affect our conclusions.

	DV: Democratic Control Index (t)			
	(1)	(2)	(3)	(4)
Mass Social Lib$_{t-1}$	**.030**		**.027**	.021
	(.016)		(.016)	(.015)
Mass Econ Lib$_{t-1}$		**.021**	.016	.013
		(.012)	(.013)	(.013)
Mass Dem PID$_{t-2}$				**.075**
				(.010)
Dem Control$_{t-1}$	**.662**	**.667**	**.661**	**.592**
	(.035)	(.036)	(.037)	(.032)
Year FEs	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes
Observations	1,755	1,755	1,755	1,497
Adjusted R^2	.718	.717	.718	.721

Table 2: Effect of mass policy preferences and partisanship on partisan turnover. 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.

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.[24] More relevant to our purposes here, the first and second rows of Table 2 show that *Mass Liberalism*$_{t-1}$ (that is, in the most recent election year) has a modest effect on changes in party control. A 1-SD difference in mass social liberalism increases *Democratic Control*$_t$ by 0.03 (column 1), and an analogous increase in economic liberalism does so by 0.02 (column 2). When mass social and economic liberalism are included in the same specification (column 3), mass economic liberalism is no longer distinguishable from 0,

24. This is consistent with the finding that a party that narrowly wins the governorship (Folke and Snyder 2012) or state legislature (Feigenbaum, Fouirnaies, and Hall, Forthcoming) tends to lose seats in the next election.

but the sum of the two coefficients (0.042, SE= 0.018) remains clearly positive.

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 (4) 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 0.08 for each SD change.[25] Accounting for mass PID modestly reduces the magnitude and significance of the mass liberalism coefficients, but the two liberalism coefficients still jointly predict *Democratic Control*$_t$ ($p = 0.05$).[26] Still, mass policy preferences are clearly a much less powerful predictor of election outcomes than mass partisanship.

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 selection 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 panel and RD designs have shown that Democratic control of the governorship or legislature modestly increases the liberalism of state policies (e.g., Brown 1995; 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

25. Note that *Mass Democratic* PID_{t-2} cannot affect *Democratic Control*$_{t-1}$ because the latter is determined by the election in year $t-3$.

26. This remains true if we control instead for *Mass Democratic* PID_{t-1}, which may be a consequence as well as a cause of mass liberalism in the same year ($p = 0.08$).

	DV: Domain-Specific Policy Liberalism (t)							
	Social				Economic			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dem Control$_t$	**.055**		**.051**		**.069**		**.069**	
	(.010)		(.011)		(.012)		(.012)	
Mass Lib$_{t-1}$		**.033**	**.030**			**.014**	**.011**	
		(.009)	(.008)			(.007)	(.007)	
Mass Lib$_{t-1}$ (No Elec$_{t-1}$)				**.030**				.009
				(.008)				(.008)
Mass Lib$_{t-1}$ (Elec$_{t-1}$)				**.034**				**.016**
				(.009)				(.009)
Policy Lib$_{t-1}$	**.944**	**.942**	**.936**	**.941**	**.922**	**.931**	**.918**	**.931**
	(.013)	(.014)	(.013)	(.014)	(.013)	(.012)	(.012)	(.011)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,632	3,854	3,632	3,854	3,632	3,854	3,632	3,854
Adjusted R^2	.973	.973	.974	.973	.971	.971	.972	.971

Table 3: partisan selection 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. Bold coefficients are significant at the 10% level.

policy liberalism in that year by 0.05–0.07 SDs.[27] Such complete switches in party control are rare, however. The standard deviation of Democratic Control is .39, which corresponds to a shift in one of the three government institutions that compose the index. By this standard, the effect of Democratic Control is roughly comparable to that of mass liberalism. The standardized effect of Democratic Control is 0.021 for social policy and 0.027 for economic, about the same size as the standardized effect of mass liberalism in each domain.

To assess the degree to which the effect of opinion on policy is mediated by party control (that is, through the mechanism of partisan selection), we rely on three complementary analyses. The first is to simply multiply the estimated effects of mass liberalism on Democratic control and Democratic control on policy liberalism. This method estimates the mediated effect to be 0.0016 (SE=0.0009) for social policy and 0.0014 (0.0009) for economic. These estimates are about 5–10% of the total effects of mass liberalism reported in columns (2) and (6) of Table 3. Very similar results are obtained if we use a different method: subtracting the controlled direct effect of *Mass Liberalism*$_{t-1}$ (column 3/7) from its estimated total effect (column 2/6).[28]

Finally, the same pattern appears if we 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 party control could conceivably change, with years not following an election, when it will generally be the same as in the previous year. Responsiveness in years where only adaptation is possible is captured by the coefficients labeled "Mass Lib$_{t-1}$ (No Elec$_{t-1}$)" in columns (4) and (8). Note that these coefficient estimates are nearly identical to their counterparts in columns (3) and (7), though in the case of economic policy we cannot reject zero responsiveness in years that do not follow elections. It is worth noting that dropping years after an election implicitly rules out not only change in party control, but (aside from mid-term departures) any incumbent turnover. The fact that this approach

27. These dynamic-panel estimates are similar in magnitude to the electoral RD estimates of the effects of Democratic governors and state legislatures reported in Caughey, Warshaw, and Xu (Forthcoming).

28. The results are qualitatively identical if we also control for Democratic seat share in the legislature.

yields estimates very close to those obtained by controlling explicitly for *Democratic Control* suggests that most of adaptation is explained not by within-party turnover or changes in the size of partisan majorities, but by the evolving positions of individual incumbents.

Given the imprecision of the mediation estimates and the strong assumptions required to interpret them causally, we should not focus too much on their exact magnitude. It is nevertheless striking how little support the mediation analyses provide for partisan selection as a mechanism of responsiveness. This is true not because party control has no policy effects—they are in fact quite large and robust—but rather because mass liberalism is only weakly related to shifts in party control. These results thus leave substantial scope for responsiveness in the absence of changes in party control. While we cannot determine exactly how much of the adaptation effect is due to within-party membership turnover, the evidence supports the hypothesis that the adaptation of reelection-motivated incumbents to shifts in public sentiment is an important, and perhaps the dominant, mechanism of responsiveness.

6 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 six such factors: time, region, suffrage restrictions, campaign contribution limits, reforms designed to enhance citizen participation in government, and legislative professionalism. Unlike time and region, the last four moderators are institutions that could potentially be manipulated to influence the quality of responsiveness. We emphasize, however, that the interaction effects 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. Moreover, an increase in responsiveness due to a particular institution does not necessarily imply that it makes policy more congruent with mass preferences (Matsusaka 2001).

	DV: Domain-Specific Policy Liberalism (t)					
	Social			Economic		
	(1)	(2)	(3)	(4)	(5)	(6)
Mass Liberalism$_{t-1}$	**.040**	**.039**	**.044**	**.020**	**.025**	**.024**
	(.009)	(.010)	(.013)	(.008)	(.008)	(.009)
Mass Lib$_{t-1}$× Pre-'72	**−.031**	−.021	−.021	**−.023**	**−.026**	−.024
	(.015)	(.016)	(.020)	(.012)	(.014)	(.015)
Mass Lib$_{t-1}$× South		−.017	−.004		**−.024**	**−.031**
		(.013)	(.022)		(.013)	(.015)
Mass Lib$_{t-1}$× Pre-'72 × South		.003	−.006		.021	.029
		(.030)	(.034)		(.018)	(.022)
Suffrage Restriction			.014			.002
			(.011)			(.015)
Suff Restrict × Mass Lib$_{t-1}$			.00002			.00004
			(.015)			(.012)
Contribution Limits			−.001			−.0004
			(.003)			(.003)
Contrib Limit × Mass Lib$_{t-1}$			.002			.004
			(.004)			(.003)
Citizen Governance			−.006			−.006
			(.016)			(.016)
Citizen Gov × Mass Lib$_{t-1}$			.010			.005
			(.010)			(.008)
Legislative Days (Logged)			.006			**−.010**
			(.009)			(.006)
Leg Days × Mass Lib$_{t-1}$			.003			−.006
			(.008)			(.006)
Policy Liberalism$_{t-1}$	**.936**	**.934**	**.918**	**.927**	**.925**	**.919**
	(.013)	(.014)	(.016)	(.012)	(.013)	(.014)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,854	3,854	3,552	3,854	3,854	3,552
Adjusted R^2	.973	.973	.970	.971	.971	.970

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. Bold coefficients are significant at the 10% level.

That being said, it is nonetheless interesting and important 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 seems to be yes.[29] We can see

29. This conclusion relies on the assumption that the mass and policy liberalism scales are comparable

this most clearly in columns (1) and (4) of Table 4, which interact $Mass\ Liberalism_{t-1}$ with an indicator for years before 1972. On both social and economic issues, dynamic responsiveness was significantly smaller before 1972. In fact, on economic issues the point estimate for the earlier period is close to 0.

Why might dynamic responsiveness have increased over time? One natural hypothesis is that it was driven by the democratization of the South, which was not fully democratic until the early 1970s (Mickey 2015). Surprisingly, we actually find little evidence for this conjecture. This can be seen in columns (2) and (5), which include a three-way interaction between mass liberalism, era, and region. The estimates in the second row, which now capture temporal differences in the non-South only, are of similar magnitude to those in columns (1) and (4). The coefficients in the third row indicate that responsiveness has been lower in the South even in the post-1972 period. Moreover, the triple interaction in the fourth row provides no evidence that the regional gap in dynamic responsiveness was once larger than it is now. In fact, column (5) seems to suggest that on economic issues Southern and non-Southern states were once about equally (un)responsive, whereas in recent years dynamic responsiveness has increased in the non-South but not in the South.[30]

One possible response to this puzzling finding is that undemocratic institutions such as poll taxes were not confined to Southern states, nor did all Southern states employ these devices over the entire pre-1972 period. It would be better, therefore, to examine the moderating effects of suffrage restrictions directly. By the same token, states have adopted numerous other reforms designed to limit the influence of money in politics and enhance

across years. We believe this assumption is more plausible for these measures than for other commonly used latent scales. What bridges NOMINATE scores between congresses, for example, is not repeated votes on the same bills, but rather assumptions about whether and how members of Congress change ideologically over time (Poole and Rosenthal 2007). By contrast, the bridging assumption in our analysis is that the discrimination parameters of survey questions and state policies repeated across years are constant over time. That is, the degree to which a question or policy distinguishes liberal and conservative states is assumed to be the same in every year. This is the same assumption that is implicitly invoked by studies that compare responsiveness on a single issue over time. Supplementary Appendix E provides further evidence that these results are not driven by differential measurement error across time.

30. However, when we subsample our opinion data to ensure equal sample sizes across time and re-run the models in Table 4, the results suggest that on economic issues responsiveness has increased roughly equally across regions (see Online Appendix E). We thus view these regional differences with some skepticism.

citizens' participation in policymaking. State legislatures have also generally become more professionalized over time, though at different rates, and this too may have influenced responsiveness. 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, and citizen governance (direct democracy and term limits)—and by the number of days a legislature spends in session (a proxy for professionalism). We present the analysis of these policies as indices (all centered at 0) in order to ameliorate the multiplicity problem of testing many interaction effects.

On the whole, we find little evidence that any of the institutions we consider moderate the effect of opinion on policy. Controlling for era and region, none of eight index interactions is statistically significant.[31] In this specification, the only significant association we find is for the main effect of logged legislative days, which appears to have a negative association with economic policy liberalism when that variable is at its mean. Essentially the same picture emerges if we analyze each institution individually (see Supplementary Appendix G).

In sum, our main findings are that the dynamic effect of opinion on policy is stronger in the present era than it was before 1972, and that in the modern era dynamic responsiveness seems to be stronger in non-Southern states, at least on economics. We find little evidence that any institution we considered 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, masking these reforms' positive effects. Thus, while our results suggest that previous studies may overstate the responsiveness-enhancing

31. Controlling for mass liberalism's interactions with era and region is important because the latter strongly predict the likelihood of adopting the reforms we consider and thus proxy for the numerous other factors that vary across time and geography that might confound the institutional interactions. However, if we drop these controls, we do find some suggestive evidence consistent with the hypotheses that suffrage restrictions inhibit responsiveness while campaign finance regulations and citizen governance reforms enhance it. The strongest evidence is for the campaign finance index, a 1-SD increase of which is estimated to increase responsiveness on economic issues by 0.0082 (SE=0.0047).

effects of these institutional reforms, this is clearly an area where more research is needed.

7 Discussion

What do our findings suggest about the character and functioning of American democracy? At the most basic level, they indicate that state policymaking responds to mass policy preferences, though more so now than in the past. 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 partisan selection is the dominant if not exclusive means by which voters affect government policies. Manifestions 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 or convergence in Congress (e.g., Lee, Moretti, and Butler 2004; Fowler and Hall 2016), and studies that emphasize the "leapfrog" nature of representation in the contemporary United States (e.g., Bafumi and Herron 2010; Lax and Phillips 2012). By contrast, our finding that adaptation is a major and perhaps the 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; Stimson, MacKuen, and Erikson 1995).

It should be emphasized that partisan selection 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

liberalism and partisan fortunes implies that citizens have little influence over government policies. Rather, mass liberalism and party control seem to exert fairly independent, and roughly equally important, effects on policy change. This pattern is consistent with Erikson et al.'s "statehouse democracy" model, in which the platforms of Democratic and Republican parties in a given state diverge from one another (resulting in partisan effects on policy) but are roughly centered on the state's median voter (resulting in adaptation). Contrary to some fears, however, neither party control nor mass liberalism seems to cause dramatic swings in policymaking. Even a full switch in party control, for example, changes policy liberalism in the short term 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. It is the cumulation of such incremental changes over many decades that arguably accounts for the strong cross-sectional relationship between opinion and policy.

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 2012). In particular, the fact that state policymaking is responsive on the margin does not preclude the existence of ideological bias in state policies. Indeed, the persistent gap in policy liberalism between Southern and non-Southern states with similar mass publics (see Figure 2) 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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Supplementary Information for "Policy Preferences and Policy Change: Dynamic Responsiveness in the American States, 1936–2014"

A Measurement Model for Citizen Policy Liberalism **A-2**

B Validation of Citizen Policy Liberalism Estimates **A-6**

C Summary Statistics for Key Independent Variables **A-14**

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H Survey Questions for Mass Liberalism Models **A-29**

A Measurement Model for Citizen Policy Liberalism

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.[32] We estimate citizen policy liberalism using the statistical 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 θ_i is governed by the question's threshold κ_{qt}, 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:

$$\pi_{iq} = \Phi\left(\frac{\theta_i - \kappa_{qt}}{\sigma_q}\right), \tag{1}$$

where the normal CDF Φ maps $(\theta_i - \kappa_{qt})/\sigma_q$ to the $(0, 1)$ interval.[33] 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_{qt}.

Accurate estimation of θ_i requires data from many respondents, each of whom answers

32. 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.

33. A common alternative way of writing the model in Equation (1) is $\Pr(y_{iq} = 1) = \Phi(\beta_q\theta_i - \alpha_q)$, where $\beta_q = 1/\sigma_q$ and $\alpha_q = \kappa_{qt} \times \beta_q$.

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 θ_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). Assuming that $\theta_{i[g]}$ is distributed normally with mean $\bar{\theta}_g$ and standard deviation σ_θ, we can re-write the individual-level IRT model at the group level as

$$p_{gj} = \Phi\left(\frac{\bar{\theta}_g - \kappa_j}{\sqrt{\sigma_j^2 + \sigma_\theta^2}}\right), \tag{2}$$

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

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

where n_{gq} 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.[34] 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).

34. 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."

Even with our large-scale dataset of 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 ξ_t be an intercept common to all groups and $\boldsymbol{x}_{g\bullet}$ a vector of hierarchical predictors (*Race*, *Urban*, and *State*), we model the group means in each year as

$$\bar{\theta}_{gt} \sim N(\delta_t \bar{\theta}_{g,t-1} + \xi_t + \mathbf{x}'_{g\cdot}\gamma_t,\ \sigma^2_{\bar{\theta}t}), \tag{4}$$

That is, the prior expected value for $\bar{\theta}_{gt}$ is a weighted combination of its lagged value and predictions based on demographically similar groups, with the variance of the prior determined by $\sigma^2_{\bar{\theta}}$. 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. 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.[35] 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 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

35. 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.

state year.

Overall, the responses of approximately 1.5 million distinct individuals are represented in the data.[36] We use the dynamic group-level IRT model described above to estimate average liberalism in groups defined by state, race, and urban residence.[37] 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 (Ruggles et al. 2010). Finally, we standardized the citizen liberalism estimates in order to make them easier to interpret.

36. The model of social policy liberalism includes 801,372 respondents from 485 individual polls that were asked 72 distinct policy questions. The model of economic policy liberalism includes 1,199,876 respondents from 779 polls that were asked 285 distinct policy questions.

37. 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).

B Validation of Citizen Policy Liberalism Estimates

In this section, we provide evidence for the validity of our measures of citizen policy liberalism with two kinds of validation: convergent and construct (Adcock and Collier 2001). The purpose of convergent validation is to show that a new measure is empirically associated with alternative measures of the same concept. We do this by comparing our liberalism estimates with alternative estimates of the mass public's ideology or mood. We then turn to construct validation, the goal of which is demonstrate the empirical association between a new measure of a given concept and existing measures of different concepts widely believed to be causally related to the concept of interest. We do this by evaluating the relationship between our estimates and presidential vote share, state government policy liberalism, and the median ideology of state legislators.

In addition to showing the validity of our estimates of mass citizen liberalism, we also show that our estimates have stronger convergent and concept validity than the main alternative, dynamic measures of the mass public's ideology at the state level that are available over a long period of time. First, we compare our measures to the citizen ideology estimates of Berry et al. (1998), which are available from 1960-2013 and are based on a weighted average of the scaled roll call votes of elected officials.[38] Next, we compare our measures to the state-level mood estimates of Enns and Koch (2013), which are available from 1956-2008 and are based on a combination of Stimson's mood algorithm and multi-level regression with post-stratification (MRP).[39] Finally, we compare our estimates to the symbolic ideology estimates from Pacheco (2014). These estimates are available from 1978-2010 and are based on the application of an MRP model to a moving window of symbolic ideology data from CBS/NY Times polls during this period.

38. There is a vigorous debate in the literature about the appropriateness of using these measures to estimate mass liberalism. Critics worry that they lack face validity and display overly volatile longitudinally (Erikson, Wright, and McIver 2007; Enns and Koch 2015).

39. There is also a debate about the validity of these measures between Berry et al. (2015) and Enns and Koch (2015).

Convergent Validation

The purpose of convergent validation is to show that a new measure is empirically associated with alternative measures of the same concept (Adcock and Collier 2001). Here, we show that our measures of the mass public's liberalism are very highly correlated with high-quality measures of ideology that are available for recent decades. In addition, we show that they have a stronger relationship with these measures of ideology than the alternative dynamic ideology measures that are available over a long period of time.

Table A1 shows the percentage of the variation in a number of high-quality cross-sectional measures of ideology explained by the available dynamic measures of mass liberalism.[40] First, we examine each measure of mass liberalism's relationship with state-level estimates of symbolic ideology derived from exit polls.[41] Exit polls have large samples in each state, which enables us to relatively precisely capture cross-state variation.[42] The main disadvantage of using exit polls is that their population frame is slightly different from our estimates: they focus on voters rather than general population. In addition, state exit polls are only available going back to 1984. Our measure of mass liberalism explains 75% of the variation in the ideological composition of state electorates in exit polls. Moreover, it has a stronger correlation with exit poll-based measures of ideology than the alternative measures of mass liberalism. The Enns and Koch (2013) estimates display a particularly weak relationship with exit polls.

Next, we examine each dynamic ideology measure's relationship with measures of symbolic ideology derived from CBS/NY Times polls between 1976-2003 (Erikson, Wright, and McIver 2006). These polls have been widely used in past work as measures of state publics' ideologies. The main disadvantage of these measures is that they sometimes have small sam-

40. Each model also includes year fixed effects to account for idiosyncratic over time variation.

41. While symbolic ideology is not exactly the same concept as operational (or policy) ideology, they are very similar concepts.

42. To further reduce sampling error in the exit polls, we only include state/years with more than 200 exit poll respondents in our analysis.

Table A1: Global Convergent Validation: The table shows the percentage of the variation in a number of high-quality cross-sectional measures of ideology explained by the available dynamic measures of mass liberalism.

	Symbolic Ideology Measures		Cross-Sectional Ideology Measures			
	Exit Polls ('84-'10)	CBS News ('76-'03)	EWM (1982)	Tausanovitch & Warshaw (2008)	Carsey & Harden (2006)	Lax & Phillips (2010)
Our measures of Mass Liberalism	0.74	0.61	0.70	0.90	0.78	0.87
Fording et al (state ideology)	0.67	0.53	0.60	0.71	0.58	0.79
Enns and Koch (state mood)	0.34	0.20	0.01	0.31	0.43	0.39
Pacheco (state ideology)	0.62	0.63	0.48	0.73	0.65	0.72

ple sizes in individual states.[43] Once again, our measures explain much more of the variation in the CBS/NY Times based measures of ideology than Berry et al. (1998) and Enns and Koch (2013). The measures from Pacheco (2014) have a slightly stronger relationship with the CBS polls, but this isn't surprising since the CBS polls constitute the raw data for her MRP-based measures.

Finally, the right panel of A1 examines the relationship between each measure of mass liberalism and several static, cross-sectional measures of ideology based on survey data from the past decade. Each of these measures are widely used in applied work on representation and elections. For each analysis, we use the year from the dynamic estimates that corresponds most closely to the cross-sectional estimates.

We begin by investigating the dynamic measures' relationship with the cross-sectional estimates of state-level ideology in the 1970s and 1980s from Erikson, Wright, and McIver (1993).[44] Then, we examine the relationship with ideology measures from the period between 2000-2010 from Tausanovitch and Warshaw (2013).[45] We also examine each measures' relationship with the estimates of state-level mood from 2008 from Carsey and Harden (2010).[46]

43. To reduce sampling error in these polls, we only include state/years with more than 200 respondents in our analysis.

44. These are based on pooling the symbolic ideology questions on CBS News/NY Times polls during this period.

45. These are based on the combination of an Item-response theory (IRT) model to measure ideology at the individual level and an MRP model to smooth opinion across states. Their model draws upon over 250,000 survey respondents from the 2000 and 2004 Annenberg National Election Surveys and the 2006-2010 Cooperative Congressional Election Studies.

46. These are based on the combination of a factor analysis model to estimate individual ideology using data from the 2006 Cooperative Congressional Election Study.

Finally, we compare each measure with the average of the issue-specific preferences in Lax and Phillips (2012). In each case, our measures explain the bulk of the variation in these static measures of ideology (70% or greater). Moreover, they have a much stronger association with these static measures of ideology than the alternative, dynamic measures of ideology.

Another convergent validation approach is to compare the ideology measures with raw survey data on specific domains. In Table A2, we compare the proportion of the variation in survey data on salient social and economic issues in four discrete time periods that is explained by each measure of ideology. For the social policy domain, we examine the relationship between each measure and state-level opinion on a ban on alcohol around 1950. For later time periods, we examine survey questions on abortion. For the economic domain, we examine the relationship between each measure and the average response in each state to surveys on universal healthcare.

Once again, our measure performs particularly well. In all four time periods, and on both the economic and social domains, it explains the bulk of the variation in the raw survey data. In contrast, the alternative measures sometimes perform reasonably well in the most recent time period, but generally perform poorly in earlier time periods (and are completely unavailable in the earliest time period).

Table A2: Domain-Specific Convergent Validation with Individual Survey Questions (Cross-Sectional): The table shows the percentage of the variation in a number of individual survey items during a variety of time periods that are explained by the available dynamic measures of mass liberalism.

	Social Domain				Economic Domain			
Issue:	Ban Alcohol	Abortion			Universal Healthcare			
Time Range:	1947-52	1975-95	2000	2009-11	1949-51	1975-95	2000	2009-11
Source:	Gallup	Various	NAES	CCES	Gallup	Various	NAES	CCES
Our measure of Mass Social Liberalism	0.88	0.71	0.69	0.89				
Our measure of Mass Economic Liberalism					0.66	0.56	0.69	0.73
Fording et al (state ideology)		0.36	0.40	0.65		0.36	0.32	0.67
Enns and Koch (state mood)		0.28	0.17	0.45		0.44	0.45	0.24
Pacheco (state ideology)		0.61	0.42	0.72		0.38	0.38	0.49

Construct Validation

Next, we examine the construct validity of our measures of mass liberalism. The purpose of construct validation is to demonstrate that a measure conforms to well-established hypotheses relating the concept being measured to other concepts (Adcock and Collier 2001, 542–3).

The left two columns of Table A3 examine the percentage of the variation between each dynamic measure of mass ideology 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. The table shows that our measures explain much of the state-level variation in presidential vote share. Figures A1 and A2 show the year-by-year relationship between our estimates of mass policy liberalism and presidential vote share between 1936 and 2012 in non-southern states. It indicates that the relationship is strong across the period, but the relationship between ideology and presidential vote 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). Finally, Table A3 shows that our measure has a stronger relationship with presidential vote than the other available measures of mass liberalism.

The right two columns of Table A3 show the relationship between our measure (as well as the other available ideology measures) with two other concepts that should be associated with mass liberalism: state policy (Caughey and Warshaw 2016) and the median ideal point in the state house house (Shor and McCarty 2011). In both cases, our measure of mass liberalism explains much of the variation in these theoretically related measures. Moreover, it has a higher correlation with them than the other available dynamic measures of mass

ideology.

Table A3: Concept Validation: The table shows the percentage of the variation in a number of concepts that are theoretically related to ideology that are explained by the available dynamic measures of mass liberalism.

	Presidential Vote (1960-2008) All States	Presidential Vote (1960-2008) Non-South	State Policy Liberalism (1960-2008) Caughey & Warshaw	Median Ideal Point in State House (1994-2010) Shor & McCarty
Our measures of Mass Liberalism (1956-2010)	0.67	0.75	0.60	0.50
Fording et al (1956-2010)	0.61	0.74	0.55	0.4
Enns and Koch (1956-2010)	0.50	0.57	0.11	0.26
Our measures of Mass Liberalism (1978-2010)	0.73	0.77	0.66	0.50
Pacheco (1978-2010)	0.57	0.59	0.56	0.4

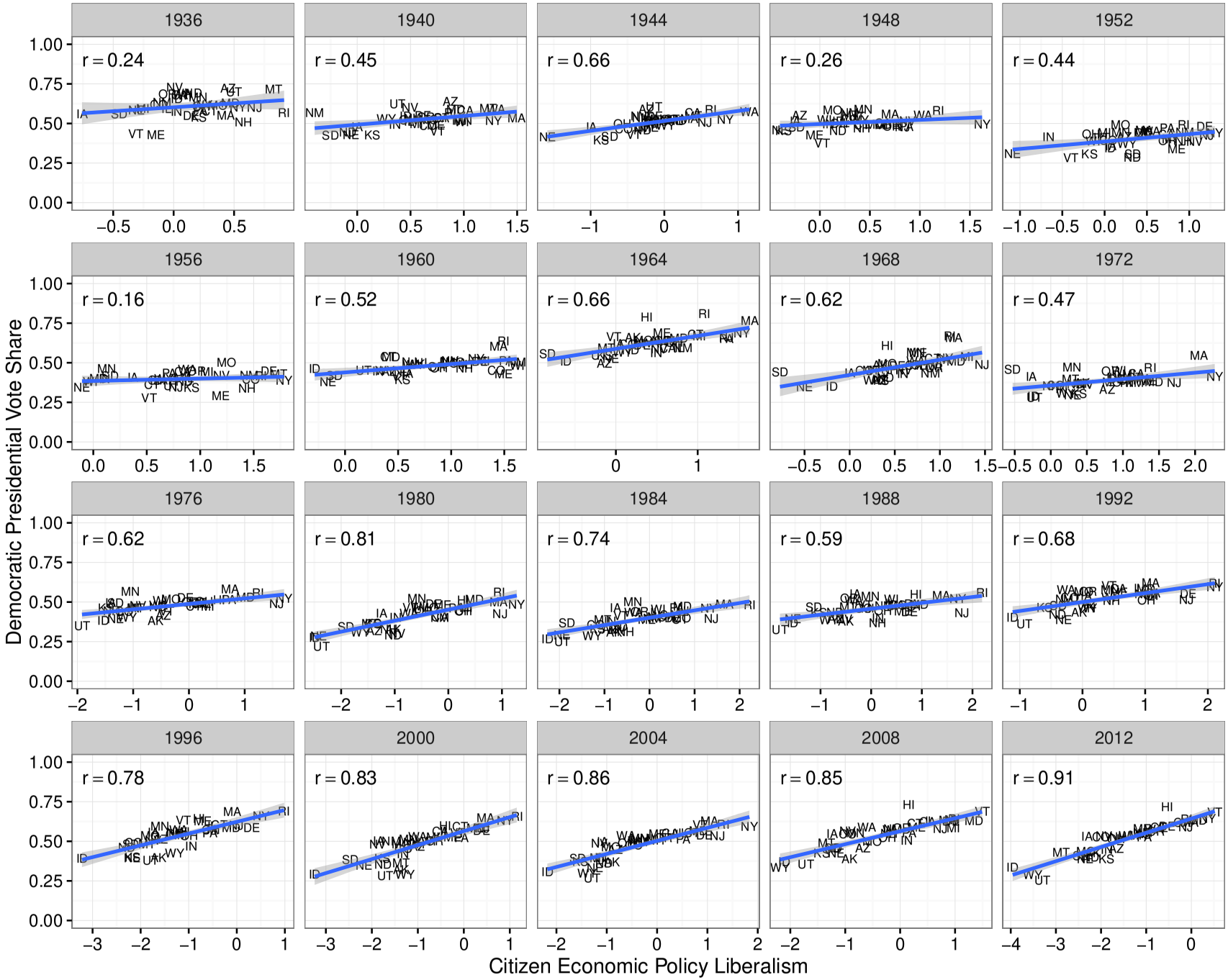

Figure A1: Relationship between economic policy liberalism and Democratic presidential vote share in the non-south, 1936–2012.

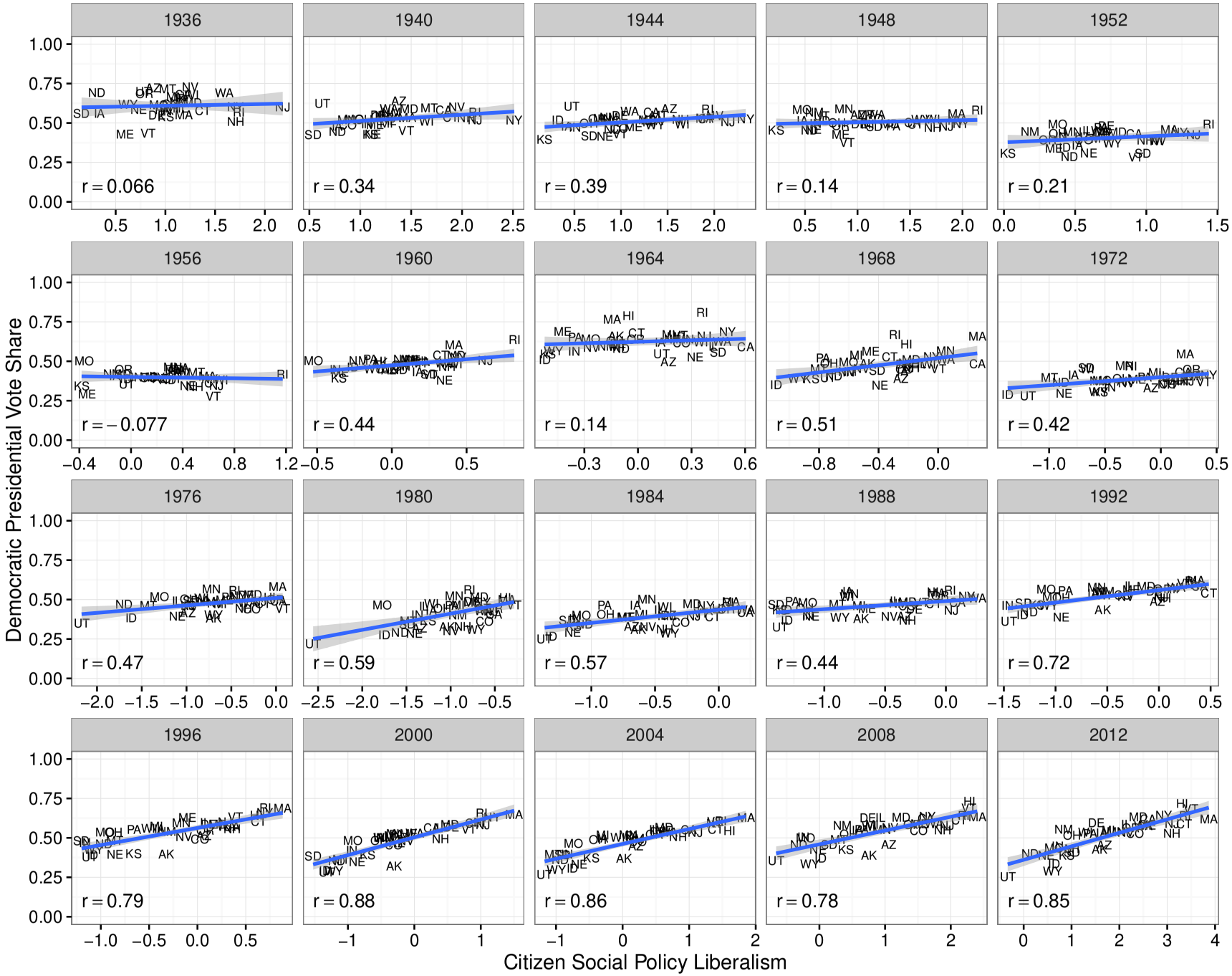

Figure A2: Relationship between social policy liberalism and Democratic presidential vote share in the non-south, 1936–2012.

C Summary Statistics for Key Independent Variables

Table A4 shows summary statistics for our key independent variables.

Table A4: 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

D Methodology for Addressing Measurement Error

The key independent and dependent variables in this study—mass liberalism and policy liberalism in each issue domain—are latent quantities whose values must be inferred rather than directly observed. As such, they are subject to measurement error. Even if it is independent and mean-zero, measurement error can bias point estimates and standard errors. However, since we can estimate the values' measurement error from their Monte Carlo sampling distributions, we can account for measurement error using a technique known as the "method of composition" (MOC) or "propagated uncertainty" (Tanner 1996, 52; Treier and Jackman 2008, 215–6; Kastellec et al. 2015, 791–2).

The idea behind MOC is that we wish to estimate the marginal distribution (i.e., the posterior or, given flat priors, likelihood) of a parameter vector $\boldsymbol{\beta}$, intergrating over the measurement error in variables $\boldsymbol{X}$:

$$p(\boldsymbol{\beta}|\boldsymbol{W}) = \int_X p(\boldsymbol{\beta}|\boldsymbol{W}, \boldsymbol{X}) p(\boldsymbol{X}|\boldsymbol{Z}) d\boldsymbol{X}, \tag{5}$$

where $\boldsymbol{W}$ is a matrix of variables measured without error and $\boldsymbol{X}$ is a matrix of variables estimated with error conditional on data $\boldsymbol{Z}$ (and a measurement model). That is, we wish to intergrate the joint density of $\boldsymbol{\beta}$ and $\boldsymbol{X}$ over the distribution of $\boldsymbol{X}$. As Treier and Jackman (2008, 215) explain, this can be done via the following iterative procedure: At each iteration s,

1. Sample $\boldsymbol{X}^{(s)}$ from the distribution $p(\boldsymbol{X}|\boldsymbol{Z})$.
2. Sample $\tilde{\boldsymbol{\beta}}^{(s)}$ from $p(\boldsymbol{\beta}|\boldsymbol{W}, \boldsymbol{X}^{(s)})$ in two steps:
   (a) Conditional on $\boldsymbol{W}$ and $\boldsymbol{X}^{(s)}$, estimate the parameter vector $\hat{\boldsymbol{\beta}}^{(s)}$ and its variance-covariance matrix $\hat{\boldsymbol{V}}^{(s)}$.
   (b) Draw one sample $\tilde{\boldsymbol{\beta}}^{(s)}$ from a multivariate normal distribution with mean vector $\hat{\boldsymbol{\beta}}^{(s)}$ and variance-covariance matrix $\hat{\boldsymbol{V}}^{(s)}$.

```r
MOC <- function (data, its, model, vcov) {
  tildeB <- as.data.frame(matrix(nrow=length(its), ncol=length(coef(model))))
  names(tildeB) <- names(coef(model))
  ## In each iteration...
  for (s in seq_along(its)) {
    it <- its[s]
    ## (1) Sample from p(X)
    data_s <- subset(data, It == it)
    ## (2) Sample from p(B|X_s):
    ##     (a) Estimate B_s and Cov(B_s) conditional on X_s.
    mod_s <- update(model, data=data_s)
    hatB_s <- coef(mod_s)
    hatV_s <- vcov(mod_s)
    ##     (b) Sample \tilde{B_s} from MV(\hat{B_s}, \hat{Cov(B_s)}).
    tildeB[s, ] <- mvrnorm(n = 1, mu = hatB_s, Sigma = hatV_s)
  }
  return(tildeB) ## samples from p(B), intergrating over p(X)
}
```

Listing 1: R Function for the Method of Composition

Each draw $\tilde{\boldsymbol{\beta}}^{(s)}$ is a sample from (the normal approximation of) the marginal distribution $p(\boldsymbol{\beta}|\boldsymbol{W})$, with the iterative algorithm performing the intergration over $p(\boldsymbol{X}|\boldsymbol{Z})$.[47] The R function we used to implement the MOC algorithm is reproduced in Listing 1.

Using this approach, we accounted for measurement error in four variables: *Mass Social Liberalism*, *Mass Economic Liberalism*, *Social Policy Liberalism*, and *Economic Policy Liberalism*. Independently for each measure, we drew 500 samples from the joint posterior distribution of state-year values. We combined each four-variable sample with a copy of the other variables in our dataset, which were presumed to be measured without error. Then, following the algorithm above, we re-estimated each model on the 500 versions of the dataset and drew 500 samples of the model parameters. We then used these samples to calculate point estimates and standard errors, on which we based our inferences. Generally, the MOC point estimates for the effect of mass liberalism on policy liberalism were about two-thirds as large as the non-MOC estimates, but the standard errors were about the same size.

47. Treier and Jackman (2008, 216) note that this procedure relies on two conditional independence assumptions. The first is that the data used to estimate the latent variables $\boldsymbol{X}$ do not supply information about the model parameters $\boldsymbol{\beta}$ except through $\boldsymbol{X}$. That is, $p(\boldsymbol{\beta}|\boldsymbol{W},\boldsymbol{X}) = p(\boldsymbol{\beta}|\boldsymbol{W},\boldsymbol{X},\boldsymbol{Z})$. The second assumption is that inferences about each latent variable $\boldsymbol{x}_j$ are not informed by the other latent variables $\boldsymbol{X}_{-j}$ or by the manifest variables $\boldsymbol{W}$. That is, $p(\boldsymbol{x}_j|\boldsymbol{Z}) = p(\boldsymbol{x}_j|\boldsymbol{Z},\boldsymbol{X}_{-j},\boldsymbol{W})$. Together, these assumptions separate the estimation of the measurement model from the estimation of the structural model.

E Robustness to Differential Sample Sizes Across Time

A possible concern about our finding of increased responsiveness over time is that it may be the spurious consequence of differential measurement error across time. Specifically, one might suspect that the seemingly weaker responsiveness in earlier eras is driven by the fact that the frequency of polling has increased over time, and thus public opinion in recent decades is simply better measured. This concern is partially addressed by our use of MOC to account for measurement error. Indeed, as long as we are correctly characterizing the uncertainty in our measures, the MOC procedure should fully account for any spurious effects of measurement error. One thing that MOC cannot account for, however, is that the "weight" our measurement model give to different years depends on the sample size in those years. If survey respondents are more numerous later in the period, then these years will have greater influence on the likelihood for parameters that are pooled over time, such as the IRT discrimination parameters.

To address this possibility, we re-estimated our model on a subsample of our full dataset. Specifically, we divided the dataset into four equal time periods and calculated the average sample size across years in each period. For both domains, the 1954–72 period had the smallest sample sizes. Then, we randomly dropped survey respondents such that the average yearly sample size in each period matched that in the 1954–72 period. Thus, in the subsampled data the sample there is no general trend towards larger samples over time. We then estimated mass liberalism using the same measurement model as for the main analysis and estimated the same regression models on the results. The results of these analyses, reported in Tables A5–A8, are extremely similar to those for the full dataset. In particular, the point estimates for the pre/post-1972 difference in the responsiveness of non-Southern states are essentially identical to those for the full data, though the difference is not quite statistically significant for economics (Table A8). The most salient difference from the main analyses is that regional differences in responsiveness on economics disappear in the subsampled analyses: both regions appear to be responsive post-1972 but neither were pre-1972.

	DV: Domain-Specific Policy Liberalism (t)							
	——— Social ———				——— Economic ———			
	XS	FE	LDV	DP	XS	FE	LDV	DP
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mass Liberalism$_{t-1}$	**.693**	**.199**	**.032**	**.026**	**.714**	**.328**	**.023**	**.016**
	(.110)	(.077)	(.006)	(.008)	(.109)	(.084)	(.007)	(.009)
Mass Lib$_{t-1}$ × South	**−.390**	.201	−.015	−.006	**−.819**	**−.341**	−.024	−.010
	(.184)	(.140)	(.014)	(.023)	(.164)	(.107)	(.018)	(.022)
Policy Liberalism$_{t-1}$			**.975**	**.938**			**.980**	**.936**
			(.006)	(.014)			(.005)	(.012)
Year × South FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FEs	No	Yes	No	Yes	No	Yes	No	Yes
Observations	3,854	3,854	3,854	3,854	3,854	3,854	3,854	3,854
Adjusted R^2	.531	.795	.973	.973	.531	.804	.971	.972

Table A5: Replication of Table 1 with Subsampled Data

	DV: Democratic Control Index (t)			
	(1)	(2)	(3)	(4)
Mass Social Lib$_{t-1}$	**.033**		**.028**	.021
	(.018)		(.016)	(.016)
Mass Econ Lib$_{t-1}$		**.030**	**.025**	**.026**
		(.015)	(.015)	(.015)
Mass Dem PID$_{t-2}$				**.075**
				(.010)
Dem Control$_{t-1}$	**.661**	**.662**	**.658**	**.592**
	(.037)	(.036)	(.036)	(.033)
Year FEs	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes
Observations	1,755	1,755	1,755	1,497
Adjusted R^2	.719	.719	.720	.723

Table A6: Replication of Table 2 with Subsampled Data

	DV: Domain-Specific Policy Liberalism (t)							
	——— Social ———				——— Economic ———			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dem Control$_t$	**.055**		**.053**		**.069**		**.068**	
	(.010)		(.011)		(.012)		(.012)	
Mass Lib$_{t-1}$		**.024**	**.021**			**.017**	**.016**	
		(.008)	(.008)			(.009)	(.008)	
Mass Lib$_{t-1}$ (No Elec$_{t-1}$)				**.023**				**.018**
				(.008)				(.009)
Mass Lib$_{t-1}$ (Elec$_{t-1}$)				**.025**				.016
				(.009)				(.010)
Policy Lib$_{t-1}$	**.944**	**.944**	**.938**	**.944**	**.922**	**.929**	**.917**	**.930**
	(.013)	(.014)	(.013)	(.014)	(.013)	(.012)	(.011)	(.012)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,632	3,854	3,632	3,854	3,632	3,854	3,632	3,854
Adjusted R^2	.973	.973	.974	.973	.971	.971	.972	.971

Table A7: Replication of Table 3 with Subsampled Data

	DV: Domain-Specific Policy Liberalism (t)					
	Social			Economic		
	(1)	(2)	(3)	(4)	(5)	(6)
Mass Liberalism$_{t-1}$	**.029**	**.030**	**.029**	**.023**	**.024**	**.024**
	(.008)	(.008)	(.011)	(.009)	(.009)	(.011)
Mass Lib$_{t-1}$× Pre-1972	**−.029**	−.017	−.012	−.022	−.021	−.020
	(.012)	(.014)	(.016)	(.014)	(.016)	(.016)
Mass Lib$_{t-1}$× South		−.017	.003		−.002	−.005
		(.014)	(.020)		(.009)	(.012)
Mass Lib$_{t-1}$× Pre-1972 × South		−.014	−.026		−.006	−.005
		(.029)	(.031)		(.026)	(.029)
Suffrage Restriction			.006			.002
			(.012)			(.014)
Suff Restrict × Mass Lib$_{t-1}$			−.004			−.0001
			(.015)			(.015)
Contribution Limits			.0001			−.001
			(.003)			(.004)
Contrib Limit × Mass Lib$_{t-1}$			.003			.0004
			(.004)			(.003)
Citizen Governance			−.012			−.001
			(.016)			(.016)
Citizen Gov × Mass Lib$_{t-1}$			.006			−.008
			(.011)			(.008)
Legislative Days (Logged)			.006			**−.011**
			(.009)			(.005)
Leg Days × Mass Lib$_{t-1}$			.001			.002
			(.009)			(.007)
Policy Liberalism$_{t-1}$	**.939**	**.937**	**.923**	**.927**	**.926**	**.921**
	(.013)	(.014)	(.017)	(.013)	(.013)	(.014)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,854	3,854	3,552	3,854	3,854	3,552
Adjusted R^2	.973	.973	.969	.971	.971	.970

Table A8: Replication of Table 4 with Subsampled Data

F Alternative Dynamic Specifications

F.1 Autoregressive Distributed Lag Model

The autoregressive distributed lag, or ADL$(p, q; n)$, model is a general dynamic model with p lags of the dependent variable and q lags of each of n exogenous regressors. The primary specification we use in the main text of our paper is

$$y_{st} = \alpha_s + \gamma_t + \rho_1 y_{s,t-1} + \beta_1 x_{s,t-1} + \epsilon_{st}, \tag{6}$$

which is a modified ADL$(1, 1; 1)$ model with state (α_s) and year (γ_t) intercepts but without a term for contemporaneous mass liberalism (x_{st}). The last modification is what De Boef and Keele (2008, 187) call a "dead start" ADL model. The exclusion of x_{st} from the specification is designed to ensure that mass liberalism is measured before the value of policy liberalism in year t is realized. We set p to 1 because this is the average optimal lag length of the 50 state-specific time series of policy liberalism, as selected by the Akaike Information Criterion. We set q to 0 because it relieves us of stipulating how long it takes for mass liberalism to affect policy liberalism. β_1 thus captures the effects of $x_{s,t-1}$ and (insofar as they are correleted with $x_{s,t-1}$) of higher-order lags of x, net of the portion of their effect that is mediated through $y_{s,t-1}$.

A still more general specification, however, would include higher-order lags of both the independent and dependent variables. We select $p = q = 5$ because this is the first lag length at which neither policy liberalism nor mass liberalism independently predicts y_{st} in either issue domain (conditional on lags 1–4). This results in a modified ADL$(5, 5; 1)$ model, again with x_{st} omitted:

$$y_{st} = \alpha_s + \gamma_t + \sum_{p=1}^{5} \rho_p y_{s,t-p} + \sum_{q=1}^{5} \beta_q x_{s,t-q} + \epsilon_{st}. \tag{7}$$

The results of estimating this model are reported in Table A9. The salient results are:

	DV: Policy Liberalism (t)	
	Social	Economic
	(1)	(2)
Mass Liberalism ($t-1$)	0.018	0.003
	(0.013)	(0.009)
Mass Liberalism ($t-2$)	0.004	0.002
	(0.015)	(0.009)
Mass Liberalism ($t-3$)	0.016	−0.0002
	(0.016)	(0.010)
Mass Liberalism ($t-4$)	0.002	0.011
	(0.015)	(0.009)
Mass Liberalism ($t-5$)	0.003	0.005
	(0.014)	(0.009)
Policy Liberalism ($t-1$)	0.878***	0.804***
	(0.046)	(0.029)
Policy Liberalism ($t-2$)	0.050	0.073**
	(0.039)	(0.031)
Policy Liberalism ($t-3$)	0.012	0.020
	(0.027)	(0.020)
Policy Liberalism ($t-4$)	−0.0003	0.065***
	(0.023)	(0.022)
Policy Liberalism ($t-5$)	0.002	−0.015
	(0.020)	(0.017)
Year FEs	Yes	Yes
State FEs	Yes	Yes
Sum Mass Lib, Lags 1–4	0.04***	0.016
	(0.014)	(0.012)
Sum Mass Lib, Lags 1–5	0.043***	0.02**
	(0.01)	(0.01)
Sum Policy Lib, Lags 1–5	0.941***	0.947***
	(0.01)	(0.009)
Observations	3,654	3,654
R^2	0.973	0.974
Adjusted R^2	0.972	0.973
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table A9: Results for Autoregressive Distributed Lag Model

- For both social and economic issues, no individual lag of mass liberalism is distinguishable from 0 when all five are included in the same model.

- For both social and economic issues, the sum of the five mass liberalism lags is clearly positive, and very similar in magnitude to the estimated effect of mass liberalism $t-1$ when only it is included in the model (see Table 3, columns 2 and 6).

- For social but not economic issues, the sum of the first- through fourth-order lags of mass liberalism is the same magnitude as the sum of 1–5. This is captures the effect of *change* in liberalism since $t-5$ (i.e., over a full four-year election cycle).

- The sum of the lags of policy liberalism closely matches the estimated coefficient for policy liberalism $t-1$ when only it is included in the model. Like the first-order lag alone, the sum of the lags is clearly less than 1, indicating a (slight) tendency of policy liberalism to revert its state-specific long-term mean.

F.2 First-Differenced Policy Liberalism

As a further robustness check, we replicate our main results with first-differenced policy liberalism (i.e., $\Delta y_{st} = y_{st} - y_{s,t-1}$) as the dependent variable. The results are summarized in Table A10. Columns (1) and (5) report the results an error-correction model (ECM), which is simply an algebraic re-arrangement of an ADL model (De Boef and Keele 2008, 189–90). Under this re-arrangement, the coefficient on $y_{s,t-1}$ should be negative when a time series is stationary, which is clearly the case for both social and economic policy liberalism. As they should be, the point estimates are essentially identical to the dead start ADL specification we employ in the main text (the slight discrepancies from Table 3 stem from simulation error in the method of composition procedure).

Columns (2) and (6) remove lagged policy liberalism from the specification. This is equivalent to constraining the lag coefficient for $y_{s,t-1}$ to equal 1. With state fixed effects in the model as well, this restriction implies that there is a long-term linear trend in policy liberalism within each state. It is worth noting that this restriction is clearly implausible in light of the estimates in the second row of columns (1) and (5). When we make this

restriction, mass liberalism still predicts change in policy liberalism in the social domain, but not on economics. When we remove state fixed effects from the specification, as reported in columns (3) and (7), the economic result reappears.

Finally, columns (4) and (8) predict change in y between $t-1$ and t with change in x one year previous (i.e., $t-2$ to $t-1$), controlling for y_{t-1} and x_{t-2}. This specification is equivalent to what De Boef and Keele (2008) call the "general" ECM model, except that the independent variables are shifted back one year in time relative to the DVs. For social issues, there is clear evidence that recent changes in mass liberalism predict changes in policy liberalism, though x_{t-2} continues to have predictive power as well. This is consistent with the results for the sum of lag coefficients for the ADL model reported in Table A9. For economic issues, change in x between $t-2$ and $t-1$ is not statistically significant, though similar in magnitude to the estimated effect of $x_{s,t-1}$ in the corresponding dynamic panel specification (see column 6 of Table 3). The coefficient for x_{t-2}, however, is highly predictive. This suggests either that x_{t-2} is proxying for other time-varying state-specific confounders or that the policy effects of mass economic liberalism take more than one year to be fully felt (a possibility also consistent with Table A9, in which the fourth-order lag of mass economic policy liberalism has the largest estimated coefficient).

	DV: First-Differenced Policy Liberalism (t)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Policy Lib ($t-1$)	$-.058^{***}$			$-.026^{***}$	$-.070^{***}$			$-.014^{***}$
	(.013)			(.006)	(.012)			(.003)
Mass Lib ($t-1$)	$.033^{***}$	$.015^{**}$	$.019^{***}$		$.013^{*}$	$-.002$	$.008^{**}$	
	(.008)	(.007)	(.004)		(.007)	(.006)	(.004)	
Δ Mass Lib ($t-1$)				$.029^{**}$				.007
				(.012)				(.008)
Mass Lib ($t-2$)				$.044^{***}$				$.018^{***}$
				(.007)				(.005)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	No	No	Yes	Yes	No	No
Observations	3,854	3,854	3,854	3,804	3,854	3,854	3,854	3,804
R^2	.064	.038	.033	.047	.105	.072	.066	.067
Adjusted R^2	.031	.005	.013	.027	.074	.041	.047	.047
Note:								*p<0.1; **p<0.05; ***p<0.01

Table A10: Results for First Differences Models

G Effect of Individual Institutions on Responsiveness

Table A11 reports the results of a re-analysis of columns (3) and (6) of Table 4, replacing the institution indices with their component variables. The institution variables in Table A11 are standardized to be mean-zero, unit-variance across state years. Across the 16 interactions between mass liberalism and individual institutions, the only significant coefficient is for a ban on campaign contributions from unions, which is estimated to increase responsiveness on economic policies by .012 (.006). The estimation of 1 significant effect out of 16 tests is not very surprising. Moreover, there is only modest evidence that institutional interactions matter collectively. Table A11 shows this by reporting the estimated sum of the institution interactions (multiplying the poll tax and literacy test estimates by -1 to reflect the expectation that they should have undermined responsiveness). The estimated sums are positive, but not statistically distinguishable from 0.

The major difference between this table and Table 4 is that including all institutions individually largely wipes out the temporal differences in policy responsiveness reported in the main text. This suggests that at least part of the differences across eras may be attributable to institutional reforms. It should be noted, however, that the variables in this specification suffer from a relatively high degree of multicollinearity due to the correlations among institutional variables and between them and geographic and time effects. This inflates the variance of the estimates and makes it difficult to draw firm conclusions about how responsiveness varies across different conditions.

	DV: Domain-Specific Policy Liberalism (t)	
	Social	Economic
	(1)	(2)
Mass Liberalism$_{t-1}$	**.035**	**.020**
	(.014)	(.009)
Mass Lib$_{t-1}$× Pre-1972	−.006	−.013
	(.025)	(.014)
Mass Lib$_{t-1}$× South	−.003	−**.031**
	(.022)	(.014)
Mass Lib$_{t-1}$× Pre-1972 × South	−.002	.034
	(.034)	(.025)
Union Contrib Ban	−**.011**	−**.011**
	(.007)	(.006)
Individual Contrib Limit	.003	.006
	(.007)	(.007)
Corporate Contrib Limit	.010	.006
	(.007)	(.009)
Poll Tax	.002	.004
	(.005)	(.007)
Literacy Test	.005	−.002
	(.007)	(.007)
Direct Democracy	.011	.002
	(.021)	(.014)
Term Limits	−.006	−.002
	(.005)	(.005)
Legislative Days (Logged)	.005	−**.007**
	(.006)	(.004)
Union Contrib Ban × Mass Lib$_{t-1}$	.003	**.012**
	(.007)	(.006)
Individual Contrib Limit × Mass Lib$_{t-1}$	.009	.002
	(.007)	(.005)
Corporate Contrib Limit × Mass Lib$_{t-1}$	−.004	−.005
	(.007)	(.005)
Poll Tax × Mass Lib$_{t-1}$	−.006	−.003
	(.006)	(.005)
Literacy Test × Mass Lib$_{t-1}$	.003	.001
	(.007)	(.005)
Direct Democracy × Mass Lib$_{t-1}$	.005	−.0001
	(.007)	(.005)
Term Limits × Mass Lib$_{t-1}$	.004	.003
	(.006)	(.004)
Leg Days × Mass Lib$_{t-1}$	.002	−.003
	(.005)	(.004)
Year FEs & State FEs & LDV	Yes	Yes
Sum of Signed Institution Interactions	.021	.011
	(.015)	(.013)
Observations	3,552	3,552
R^2	.971	.971
Adjusted R^2	.970	.970

Table A11: Institutional Moderators of Mass Liberalism

H Survey Questions for Mass Liberalism Models

Table A12: Social Domain - Survey Question Text

Year	Month	Item	Organization
1936	December	Ban alcohol	Gallup
1936	December	Death penalty	Gallup
1937	December	Ban alcohol	Gallup
1937	January	Women on juries	Gallup
1938	October	Ban alcohol	Gallup
1938	October	Birth control for married people	Gallup
1938	April	Handgun registration	Gallup
1939	December	Ban alcohol	Gallup
1939	June	Ban alcohol	Gallup
1939	December	Birth control for married people	Gallup
1940	October	Ban alcohol	Gallup
1940	August	Ban communist party	Gallup
1940	January	Birth control for married people	Gallup
1941	June	Allow liquor in army camps	Gallup
1941	November	Ban alcohol	Gallup
1941	May	Ban communist party	Gallup
1942	February	Ban alcohol	Gallup
1942	January	Ban alcohol	Gallup
1942	November	Ban alcohol	Gallup
1942	September	Ban alcohol	Gallup
1942	June	Ban communist party	Gallup
1942	January	Equal pay for women	Gallup
1943	August	Ban alcohol	Gallup
1943	December	Birth control for married people	Gallup
1943	March	Sex education	Gallup
1944	January	Ban alcohol	Gallup
1944	November	Ban alcohol	Gallup
1944	October	Ban alcohol	Gallup
1944	September	Ban alcohol	Gallup
1945	November	Ban alcohol	Gallup
1945	October	Ban alcohol	Gallup
1945	September	Equal pay for women	Gallup
1946	December	Ban alcohol	Gallup
1946	July	Ban alcohol	Gallup
1946	October	Ban alcohol	Gallup
1946	June	Ban communist party	Gallup
1946	March	Ban communist party	Gallup
1946	NA	Ban communists in civil service	Gallup
1947	June	Assisted Suicide	Gallup
1947	November	Ban alcohol	Gallup
1947	October	Ban alcohol	Gallup
1947	March	Ban communist party	Gallup
1947	October	Ban communist party	Gallup
1947	March	Ban communists in civil service	Gallup
1947	March	Birth control for married people	Gallup
1948	August	Ban alcohol	Gallup
1948	October	Ban alcohol	Gallup
1948	September	Ban alcohol	Gallup
1948	May	Communists must register	Gallup
1949	December	Ban alcohol	Gallup
1949	March	Ban communist party	Gallup
1949	November	Ban communist party	Gallup
1949	February	Ban communists in civil service	Gallup
1949	March	Communists must register	Gallup
1949	November	Communists must register	Gallup
1949	April	Test tube babies	Gallup
1950	January	Assisted Suicide	Gallup
1950	June	Ban alcohol	Gallup
1950	July	Ban communist party	Gallup

Survey Text A12 Continued from previous page

Year	Month	Item	Organization
1950	July	Communists must register	Gallup
1951	January	Ban communist party	Gallup
1951	January	Sex education	Gallup
1952	December	Ban alcohol	Gallup
1953	October	Allow liquor at officers' clubs	Gallup
1953	April	Communists must register	Gallup
1953	November	Death penalty	Gallup
1953	April	God in pledge to the flag	Gallup
1953	April	Test tube babies	Gallup
1954	December	Ban alcohol	Gallup
1954	October	Corporal punishment in schools	Gallup
1955	August	Ban alcohol	Gallup
1956	August	Ban alcohol	Gallup
1956	January	Ban alcohol	Gallup
1956	April	Death penalty	Gallup
1956	August	Limits on liquor consumption	Gallup
1957	March	Allow women to drink in public places	Gallup
1957	March	Ban alcohol	Gallup
1957	September	Death penalty	Gallup
1958	August	Ban alcohol	Gallup
1958	December	Corporal punishment in schools	Gallup
1959	December	Allow birth control for everyone	Gallup
1959	December	Ban alcohol	Gallup
1959	July	Permits for handguns	Gallup
1960	April	Ban alcohol	Gallup
1960	March	Death penalty	Gallup
1961	March	Allow birth control for everyone	Gallup
1961	May	Ban alcohol	Gallup
1961	May	Ban alcohol	Gallup
1962	August	Allow abortion for people with no money	Gallup
1962	August	Allow abortion if deformed baby	Gallup
1962	August	Allow abortion if healthy mother	Gallup
1962	August	Allow birth control for everyone	Gallup
1963	May	Allow birth control for everyone	Gallup
1963	December	Permits for handguns	Gallup
1964	October	Allow atheist teachers	NORC
1964	November	Allow birth control for everyone	Gallup
1964	October	Ban school prayer	NORC
1964	October	Corporal punishment in schools	NORC
1964	October	Death penalty	NORC
1964	October	Death penalty	NORC
1964	October	School prayer	ANES
1964	September	School prayer amendment	Gallup
1964	October	Teachers with beards	NORC
1964	October	Teachers with beards	NORC
1965	July	Allow 18 year olds to vote	Gallup
1965	December	Allow abortion for people with no money	Gallup
1965	December	Allow abortion if deformed baby	Gallup
1965	December	Allow abortion if healthy mother	Gallup
1965	November	Communists must register (v2)	Gallup
1965	January	Death penalty	Gallup
1965	January	Permits for handguns	Gallup
1965	September	Permits for handguns	Gallup
1965	April	Sex education	Gallup
1966	January	Ban alcohol	Gallup
1966	December	Birth control	Gallup
1966	May	Death penalty	Gallup
1966	December	Free birth control	Gallup
1966	August	Permits for handguns	Gallup
1966	October	School prayer	ANES
1967	March	Allow 18 year olds to vote	Gallup
1967	August	Allow birth control for everyone	Gallup
1967	June	Death penalty	Gallup
1967	August	Permits for handguns	Gallup
1968	July	Allow 18 year olds to vote	Gallup

Survey Text A12 Continued from previous page

Year	Month	Item	Organization
1968	November	Police authority	Comparative State Elections Project
1968	October	School prayer	ANES
1969	October	Allow 18 year olds to vote	Gallup
1969	September	Allow abortion for people with no money	Gallup
1969	September	Allow abortion if deformed baby	Gallup
1969	September	Allow abortion if healthy mother	Gallup
1969	November	Allow abortion in first trimester	Gallup
1969	October	Birth control	Gallup
1969	January	Death penalty	Gallup
1969	October	Free birth control	Gallup
1969	October	Legalize marijuana	Gallup
1970	March	Allow 18 year olds to vote	Gallup
1970	October	Allow 18 year olds to vote	Gallup
1970	September	Allow 18 year olds to vote	Gallup
1970	April	Corporal punishment in schools	Gallup
1970	October	Legalize marijuana	Gallup
1970	April	Sex education	Gallup
1971	November	Death penalty	Gallup
1971	October	Permits for handguns	Gallup
1972	October	Abortion legal (v3)	ANES
1972	December	Allow abortion in first trimester	Gallup
1972	March	Death penalty	Gallup
1972	November	Death penalty	Gallup
1972	March	Legalize marijuana	Gallup
1972	May	Permits for handguns	Gallup
1973	May	Birth control	Gallup
1973	various	Death penalty	GSS
1973	various	Legal abortion after rape	GSS
1973	January	Legalize marijuana	Gallup
1973	various	Permit required to own gun	GSS
1974	March	Allow abortion in first trimester	Gallup
1974	October	Allow abortion in first trimester	Gallup
1974	May	Constitutional amendment to allow aid for parochial schools	Gallup
1974	April	Death penalty	Roper
1974	October	Death penalty	Gallup
1974	various	Death penalty	GSS
1974	October	Equal rights amendment	Gallup
1974	various	Legal abortion after rape	GSS
1974	October	Legalize marijuana	Gallup
1974	various	Permit required to own gun	GSS
1974	October	Permits for handguns	Gallup
1974	May	School prayer amendment	Gallup
1975	April	Abortion legal	Gallup
1975	various	Death penalty	GSS
1975	December	Equal rights amendment	Roper
1975	July	Equal rights amendment	Roper
1975	March	Equal rights amendment	Gallup
1975	various	Legal abortion after rape	GSS
1975	December	Legalize marijuana	Roper
1975	various	Permit required to own gun	GSS
1976	October	Abortion legal (v3)	ANES
1976	October	Constitutional amendment to ban abortion (v2)	ANES
1976	April	Death penalty	Gallup
1976	various	Death penalty	GSS
1976	March	Equal rights amendment	Gallup
1976	various	Legal abortion after rape	GSS
1976	various	Permit required to own gun	GSS
1977	December	Abortion legal	Gallup
1977	various	Death penalty	GSS
1977	December	Equal rights amendment	Roper
1977	March	Equal rights amendment	Time
1977	November	Equal rights amendment	Time
1977	various	Legal abortion after rape	GSS

Survey Text A12 Continued from previous page

Year	Month	Item	Organization
1977	various	Legal abortion for any reason	GSS
1977	April	Legalize marijuana	Gallup
1977	December	Legalize marijuana	Roper
1977	various	Permit required to own gun	GSS
1977	December	Sex education	Gallup
1978	October	Abortion legal (v3)	ANES
1978	March	Death penalty	Gallup
1978	various	Death penalty	GSS
1978	July	Equal rights amendment	Roper
1978	various	Legal abortion after rape	GSS
1978	various	Legal abortion for any reason	GSS
1978	January	Sex education	CBS
1979	February	Abortion legal	Gallup
1979	May	Ban alcohol	Gallup
1979	March	Equal rights amendment	Roper
1979	July	Legalize marijuana	CBS
1979	May	Legalize marijuana	Gallup
1980	October	Abortion legal (v3)	ANES
1980	September	Allow gays to teach in school	Gallup
1980	November	Constitutional amendment to allow aid for parochial schools	Gallup
1980	June	Constitutional amendment to ban abortion	CBS
1980	various	Death penalty	GSS
1980	August	Equal rights amendment	Time
1980	February	Equal rights amendment	CBS
1980	July	Equal rights amendment	Gallup
1980	July	Equal rights amendment	Gallup
1980	June	Equal rights amendment	CBS
1980	March	Equal rights amendment	Time
1980	March	Equal rights amendment	CBS
1980	November	Equal rights amendment	LATimes
1980	October	Equal rights amendment	CBS
1980	October	Equal rights amendment	CBS
1980	September	Equal rights amendment	Gallup
1980	September	Equal rights amendment	CBS
1980	September	Equal rights amendment	CBS
1980	various	Legal abortion after rape	GSS
1980	various	Legal abortion for any reason	GSS
1980	June	Legalize marijuana	Gallup
1980	various	Permit required to own gun	GSS
1980	October	School prayer	ANES
1980	March	School prayer amendment	Gallup
1981	May	Abortion legal	Gallup
1981	January	Ban alcohol	Gallup
1981	April	Ban handguns	CBS
1981	February	Death penalty	Gallup
1981	April	Equal rights amendment	LATimes
1981	April	Equal rights amendment	CBS
1981	December	Equal rights amendment	Gallup
1981	July	Equal rights amendment	Gallup
1981	May	Equal rights amendment	Time
1981	September	Equal rights amendment	Time
1981	May	Sex education	Gallup
1982	October	Abortion legal (v3)	ANES
1982	September	Ban handguns	Gallup
1982	September	Constitutional amendment to ban abortion	CBS
1982	September	Death penalty	Gallup
1982	various	Death penalty	GSS
1982	June	Equal rights amendment	Gallup
1982	October	Equal rights amendment	ANES
1982	September	Equal rights amendment	Gallup
1982	various	Legal abortion after rape	GSS
1982	various	Legal abortion for any reason	GSS
1982	various	Permit required to own gun	GSS
1982	September	School prayer amendment	Gallup
1983	December	Death penalty	Time

Survey Text A12 Continued from previous page

Year	Month	Item	Organization
1983	September	Death penalty	Time
1983	various	Death penalty	GSS
1983	December	Equal rights amendment	Time
1983	September	Equal rights amendment	Time
1983	various	Legal abortion after rape	GSS
1983	various	Legal abortion for any reason	GSS
1983	July	School prayer amendment	Gallup
1984	October	Abortion legal (v3)	ANES
1984	July	Ban alcohol	Gallup
1984	October	Constitutional amendment to ban abortion	CBS
1984	September	Constitutional amendment to ban abortion	CBS
1984	October	Constitutional amendment to ban abortion (v2)	ANES
1984	September	Death penalty	Time
1984	various	Death penalty	GSS
1984	October	Equal rights amendment	Gallup
1984	September	Equal rights amendment	Time
1984	October	Government should help women	ANES
1984	various	Legal abortion after rape	GSS
1984	various	Legal abortion for any reason	GSS
1984	various	Permit required to own gun	GSS
1984	October	School prayer	ANES
1984	May	School prayer amendment	Gallup
1984	September	School prayer amendment	CBS
1985	January	Abortion legal	Gallup
1985	August	Constitutional amendment to allow aid for parochial schools	Gallup
1985	February	Death penalty	LATimes
1985	January	Death penalty	Gallup
1985	various	Death penalty	GSS
1985	various	Legal abortion after rape	GSS
1985	various	Legal abortion for any reason	GSS
1985	June	Legalize marijuana	Gallup
1985	May	Legalize marijuana	ABC
1985	various	Permit required to own gun	GSS
1986	October	Abortion legal (v3)	ANES
1986	January	Death penalty	Gallup
1986	July	Death penalty	LATimes
1986	various	Death penalty	GSS
1986	April	Legalize marijuana	ABC
1986	August	Legalize marijuana	ABC
1986	March	Legalize marijuana	ABC
1986	May	Legalize marijuana	ABC
1986	October	School prayer (v2)	ANES
1987	July	Ban alcohol	Gallup
1987	May	Constitutional amendment to ban abortion	CBS
1987	various	Death penalty	GSS
1987	April	Equal rights amendment	ABC
1987	August	Equal rights amendment	LATimes
1987	May	Equal rights amendment	CBS
1987	various	Legal abortion after rape	GSS
1987	various	Legal abortion for any reason	GSS
1987	November	Legalize marijuana	Gallup
1987	various	Permit required to own gun	GSS
1987	April	School prayer amendment	ABC
1987	April	School prayer amendment	Gallup
1987	May	School prayer amendment	Gallup
1987	May	School prayer amendment	CBS
1987	September	School prayer amendment	CBS
1988	September	Abortion legal	Gallup
1988	October	Abortion legal (v3)	ANES
1988	October	Abortion legal (v4)	ANES
1988	October	Ban handguns	CBS
1988	October	Death penalty	CBS
1988	September	Death penalty	Gallup
1988	various	Death penalty	GSS
1988	various	Gay marriage	GSS

Survey Text A12 Continued from previous page

Year	Month	Item	Organization
1988	October	Laws to protect gays	ANES
1988	various	Legal abortion after rape	GSS
1988	various	Legal abortion for any reason	GSS
1988	September	Legalize marijuana	ABC
1988	various	Permit required to own gun	GSS
1988	October	School prayer (v2)	ANES
1988	August	School prayer amendment	CBS
1989	April	Abortion legal	Gallup
1989	July	Abortion legal	Gallup
1989	March	Abortion notification	LATimes
1989	March	Assault weapon ban	CBS
1989	June	Ban flag burning	Gallup
1989	March	Ban handguns	CBS
1989	January	Death penalty	CBS
1989	March	Death penalty	LATimes
1989	October	Death penalty	Gallup
1989	various	Death penalty	GSS
1989	various	Legal abortion after rape	GSS
1989	various	Legal abortion for any reason	GSS
1989	various	Permit required to own gun	GSS
1990	October	Abortion legal (v3)	ANES
1990	October	Abortion legal (v4)	ANES
1990	July	Abortion notification	ABC
1990	May	Abortion notification	Time
1990	October	Ban flag burning	ANES
1990	October	Death penalty	ANES
1990	various	Death penalty	GSS
1990	various	Legal abortion after rape	GSS
1990	various	Legal abortion for any reason	GSS
1990	various	Permit required to own gun	GSS
1990	October	School prayer (v2)	ANES
1991	June	Abortion legal	Gallup
1991	various	Death penalty	GSS
1991	various	Legal abortion after rape	GSS
1991	various	Legal abortion for any reason	GSS
1991	various	Permit required to own gun	GSS
1992	January	Abortion legal	Gallup
1992	July	Abortion legal	Gallup
1992	June	Abortion legal	Gallup
1992	October	Abortion legal (v3)	ANES
1992	October	Abortion legal (v4)	ANES
1992	September	Allow gays to adopt children	Time
1992	January	Assault weapon ban	CBS
1992	January	Ban handguns	CBS
1992	October	Death penalty	ANES
1992	October	Laws to protect gays	ANES
1992	October	School prayer (v2)	ANES
1993	March	Abortion legal	Gallup
1993	December	Assault weapon ban	Gallup
1993	December	Assault weapon ban	LATimes
1993	March	Assault weapon ban	Gallup
1993	November	Assault weapon ban	ABC
1993	December	Ban handguns	CBS
1993	various	Death penalty	GSS
1993	various	Legal abortion after rape	GSS
1993	various	Legal abortion for any reason	GSS
1993	various	Permit required to own gun	GSS
1994	March	Abortion legal	Gallup
1994	September	Abortion legal	Gallup
1994	October	Abortion legal (v3)	ANES
1994	June	Allow gays to adopt children	Time
1994	June	Allow gays to teach in school	Time
1994	June	Allow gays to teach in school	Time
1994	October	Assault weapon ban	ANES
1994	June	Ban alcohol	Gallup

Survey Text A12 Continued from previous page

Year	Month	Item	Organization
1994	January	Ban handguns	CBS
1994	September	Death penalty	Gallup
1994	various	Death penalty	GSS
1994	various	Legal abortion after rape	GSS
1994	various	Legal abortion for any reason	GSS
1994	various	Permit required to own gun	GSS
1994	October	School prayer (v2)	ANES
1994	July	School prayer amendment	CBS
1994	November	School prayer amendment	CBS
1994	November	School prayer amendment	Gallup
1995	February	Abortion legal	Gallup
1995	September	Abortion legal	Gallup
1995	April	Assault weapon ban	CBS
1995	April	Assault weapon ban	Gallup
1995	February	Assault weapon ban	CBS
1995	April	Ban handguns	CBS
1995	January	Death penalty	ABC
1995	May	Death penalty	Gallup
1995	September	Legalize marijuana	Gallup
1995	January	School prayer amendment	ABC
1996	July	Abortion legal	Gallup
1996	November	Abortion legal	Gallup
1996	October	Abortion legal (v3)	ANES
1996	June	Allow gay marriage	Pew
1996	April	Assault weapon ban	Gallup
1996	August	Assault weapon ban	ABC
1996	January	Assault weapon ban	CBS
1996	June	Ban alcohol	Gallup
1996	February	Ban handguns	CBS
1996	October	Ban on partial birth abortion	ANES
1996	October	Constitutional amendment to ban abortion (v2)	ANES
1996	August	Death penalty	ABC
1996	June	Death penalty	Pew
1996	various	Death penalty	GSS
1996	October	Laws to protect gays	ANES
1996	various	Legal abortion after rape	GSS
1996	various	Legal abortion for any reason	GSS
1996	various	Permit required to own gun	GSS
1996	October	School prayer (v2)	ANES
1996	August	School prayer amendment	ABC
1996	November	School prayer amendment	ABC
1997	August	Abortion legal	Gallup
1997	November	Abortion legal	Gallup
1997	March	Ban on partial birth abortion	Gallup
1997	November	Ban on partial birth abortion	Gallup
1997	June	Death penalty	Time
1997	June	Death penalty	CBS
1998	January	Abortion legal	Gallup
1998	October	Abortion legal (v3)	ANES
1998	October	Allow gays to adopt children	Time
1998	October	Ban on partial birth abortion	ANES
1998	various	Death penalty	GSS
1998	various	Legal abortion after rape	GSS
1998	various	Legal abortion for any reason	GSS
1998	various	Permit required to own gun	GSS
1998	October	School prayer (v2)	ANES
1998	September	Trigger locks on guns	Pew
1999	May	Abortion legal	Gallup
1999	various	Abortion legal (v5)	NAES
1999	April	Assault weapon ban	CBS
1999	September	Assault weapon ban	ABC
1999	April	Ban handguns	CBS
1999	August	Ban handguns	CBS
1999	September	Ban handguns	ABC
1999	May	Ban on partial birth abortion	Gallup

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Year	Month	Item	Organization
1999	February	Death penalty	Gallup
1999	various	Gun control	NAES
1999	various	Law protecting gay from discrimination (v2)	NAES
1999	various	School vouchers	NAES
1999	August	Trigger locks on guns	CBS
1999	July	Trigger locks on guns	CBS
1999	May	Trigger locks on guns	ABC
1999	September	Trigger locks on guns	ABC
2000	April	Abortion legal	Gallup
2000	January	Abortion legal	Gallup
2000	October	Abortion legal (v3)	ANES
2000	various	Abortion legal (v5)	NAES
2000	March	Assault weapon ban	CBS
2000	May	Assault weapon ban	ABC
2000	May	Assault weapon ban	CBS
2000	October	Assault weapon ban	Gallup
2000	March	Ban handguns	CBS
2000	May	Ban handguns	ABC
2000	April	Ban on partial birth abortion	Gallup
2000	October	Ban on partial birth abortion	Gallup
2000	February	Death penalty	Gallup
2000	June	Death penalty	Gallup
2000	various	Death penalty	GSS
2000	various	Gays in military	NAES
2000	various	Gun control	NAES
2000	various	Law protecting gay from discrimination (v2)	NAES
2000	various	Laws to protect gays	NAES
2000	various	Legal abortion after rape	GSS
2000	various	Legal abortion for any reason	GSS
2000	various	Permit required to own gun	GSS
2000	May	School prayer amendment	WashPost
2000	September	School prayer amendment	Gallup
2000	various	School prayer amendment	NAES
2000	various	School vouchers	NAES
2000	July	Trigger locks on guns	CBS
2000	May	Trigger locks on guns	CBS
2000	May	Trigger locks on guns	ABC
2001	August	Abortion legal	Gallup
2001	March	Abortion legal	Gallup
2001	April	Death penalty	CBS
2001	August	Death penalty	CBS
2001	February	Death penalty	Gallup
2001	June	Death penalty	CBS
2001	August	Legalize marijuana	Gallup
2001	January	School vouchers	CBS
2001	March	School vouchers	CBS
2002	February	Abortion legal	Gallup
2002	March	Abortion legal	Gallup
2002	March	Death penalty	Pew
2002	various	Death penalty	GSS
2002	various	Legal abortion after rape	GSS
2002	various	Legal abortion for any reason	GSS
2002	various	Permit required to own gun	GSS
2003	January	Abortion legal	Gallup
2003	October	Abortion legal	Gallup
2003	various	Abortion legal (v5)	NAES
2003	Jan	Abortion notification	Gallup
2003	various	Allow gay marriage	NAES
2003	various	Assault weapon ban	NAES
2003	various	Ban on partial birth abortion	NAES
2003	various	Civil unions	NAES
2003	various	Constitutional amendment to ban abortion (v2)	NAES
2003	January	Death penalty	ABC
2003	June	Death penalty	Pew
2003	various	Gay marriage amendment	NAES

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Year	Month	Item	Organization
2003	various	Gun control	NAES
2003	various	School vouchers	NAES
2004	October	Abortion legal (v3)	ANES
2004	various	Abortion legal (v5)	NAES
2004	August	Allow gay marriage	Pew
2004	February	Allow gay marriage	Pew
2004	July	Allow gay marriage	Pew
2004	March	Allow gay marriage	Pew
2004	various	Assault weapon ban	NAES
2004	various	Ban on partial birth abortion	NAES
2004	August	Civil unions	Pew
2004	July	Civil unions	Pew
2004	March	Civil unions	Pew
2004	various	Death penalty	GSS
2004	various	Gay marriage	GSS
2004	August	Gay marriage amendment	Pew
2004	February	Gay marriage amendment	Gallup
2004	July	Gay marriage amendment	Gallup
2004	March	Gay marriage amendment	Gallup
2004	March	Gay marriage amendment	Pew
2004	various	Gun control	NAES
2004	October	Laws to protect gays	ANES
2004	various	Legal abortion after rape	GSS
2004	various	Legal abortion for any reason	GSS
2004	various	Permit required to own gun	GSS
2004	various	School vouchers	NAES
2005	June	Abortion legal	Gallup
2005	March	Abortion legal	Gallup
2005	November	Abortion legal	Gallup
2005	July	Abortion notification	CBS
2005	July	Abortion notification	Pew
2005	November	Abortion notification	Gallup
2005	July	Ban flag burning	Pew
2005	June	Ban flag burning	Gallup
2005	November	Constitutional amendment to ban abortion (v2)	Gallup
2005	August	Death penalty	Pew
2005	July	Death penalty	Pew
2005	June	Death penalty	ABC
2005	April	Gay marriage amendment	ABC
2005	April	Gay marriage amendment	Gallup
2005	July	Gay marriage amendment	Pew
2005	March	Gay marriage amendment	Gallup
2005	August	Immigration reform	ABC
2005	December	Immigration reform	ABC
2005	January	Immigration reform	ABC
2005	January	Immigration reform	ABC
2005	August	Stem cell research	Gallup
2005	July	Stem cell research	CBS
2005	June	Stem cell research	ABC
2005	May	Stem cell research	Gallup
2005	May	Stem cell research	CBS
2006	June	Abortion legal	Gallup
2006	June	Allow gay marriage	Pew
2006	March	Allow gay marriage	Pew
2006	November	Allow gay marriage	Pew
2006	June	Ban flag burning	Gallup
2006	June	Death penalty	ABC
2006	May	Death penalty	Gallup
2006	various	Death penalty	GSS
2006	various	Gay marriage	GSS
2006	July	Gay marriage amendment	Pew
2006	July	Gay marriage amendment	Pew
2006	June	Gay marriage amendment	Pew
2006	June	Gay marriage amendment	Pew
2006	April	Immigration reform	ABC

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Year	Month	Item	Organization
2006	April	Immigration reform	Pew
2006	April	Immigration reform	CBS
2006	June	Immigration reform	Pew
2006	May	Immigration reform	CBS
2006	October	Immigration reform	Pew
2006	April	Immigration reform (Senate bill)	ABC
2006	April	Immigration reform (Senate bill)	CBS
2006	April	Immigration reform (Senate bill)	Pew
2006	April	Immigration reform (Senate bill)	Gallup
2006	May	Immigration reform (Senate bill)	Gallup
2006	October	Immigration reform (Senate bill)	Pew
2006	various	Legal abortion after rape	GSS
2006	various	Legal abortion for any reason	GSS
2006	various	Permit required to own gun	GSS
2006	July	Stem cell research	Gallup
2007	August	Abortion legal (v2)	Pew
2007	November	Abortion legal (v2)	Pew
2007	October	Abortion legal (v2)	Pew
2007	August	Allow gay marriage	Pew
2007	January	Allow gay marriage	Pew
2007	November	Allow gay marriage	Pew
2007	September	Allow gays to adopt children	Gallup
2007	April	Assault weapon ban	ABC
2007	December	Death penalty	ABC
2007	April	Immigration reform (Senate bill)	ABC
2007	December	Immigration reform (Senate bill)	Pew
2007	January	Immigration reform (Senate bill)	Pew
2007	June	Immigration reform (Senate bill)	CBS
2007	June	Immigration reform (Senate bill)	Pew
2007	March	Immigration reform (Senate bill)	Gallup
2007	May	Immigration reform (Senate bill)	ABC
2007	May	Immigration reform (Senate bill)	CBS
2007	October	Immigration reform (Senate bill)	ABC
2007	April	Stem cell research	Gallup
2007	April	Stem cell research	ABC
2007	January	Stem cell research	ABC
2008	August	Abortion legal (v2)	Pew
2008	October	Abortion legal (v2)	Pew
2008	October	Abortion legal (v3)	ANES
2008	August	Allow gay marriage	Pew
2008	June	Allow gay marriage	Pew
2008	May	Allow gay marriage	Pew
2008	August	Civil unions	Pew
2008	May	Civil unions	Pew
2008	various	Death penalty	GSS
2008	various	Gay marriage	GSS
2008	October	Laws to protect gays	ANES
2008	various	Legal abortion after rape	GSS
2008	various	Legal abortion for any reason	GSS
2008	various	Permit required to own gun	GSS
2009	July	Abortion legal	Gallup
2009	April	Abortion legal (v2)	Pew
2009	August	Abortion legal (v2)	Pew
2009	April	Allow gay marriage	Pew
2009	April	Assault weapon ban	CBS
2009	April	Civil unions	Pew
2009	March	Gays in military	Pew
2009	January	Legalize marijuana	CBS
2009	July	Legalize marijuana	CBS
2010	January	Allow gay marriage	Pew
2010	May	Death penalty	CBS
2010	various	Death penalty	GSS
2010	various	Gay marriage	GSS
2010	August	Gays in military	CBS
2010	August	Gays in military	CBS

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Year	Month	Item	Organization
2010	December	Gays in military	CBS
2010	December	Gays in military	Pew
2010	December	Gays in military	ABC
2010	February	Gays in military	ABC
2010	February	Gays in military	Pew
2010	November	Gays in military	CBS
2010	October	Gays in military	CBS
2010	October	Gays in military	ANES
2010	May	Law protecting gay from discrimination	CBS
2010	May	Law protecting gay from discrimination	CBS
2010	various	Legal abortion after rape	GSS
2010	various	Legal abortion for any reason	GSS
2010	April	Legalize marijuana	CBS
2010	January	Legalize marijuana	ABC
2010	March	Legalize marijuana	Pew
2010	October	Legalize marijuana	ABC
2010	various	Permit required to own gun	GSS
2011	July	Abortion legal	Gallup
2011	June	Abortion legal	Gallup
2011	March	Abortion legal (v2)	Pew
2011	November	Abortion legal (v2)	Pew
2011	October	Abortion legal (v2)	Pew
2011	January	Assault weapon ban	CNN
2011	January	Assault weapon ban	CBS
2011	November	Death penalty	Pew
2011	October	Death penalty	Pew
2011	April	Legalize marijuana	CNN
2012	April	Abortion legal (v2)	Pew
2012	August	Abortion legal (v2)	Pew
2012	October	Abortion legal (v2)	Pew
2012	October	Abortion legal (v3)	ANES
2012	April	Allow gay marriage	Pew
2012	August	Allow gay marriage	Pew
2012	August	Assault weapon ban	CNN
2012	December	Assault weapon ban	CNN
2012	December	Assault weapon ban	Gallup
2012	December	Death penalty	Gallup
2012	October	Laws to protect gays	ANES
2013	July	Abortion legal (v2)	Pew
2013	March	Allow gay marriage	Pew
2013	May	Allow gay marriage	Pew
2013	April	Assault weapon ban	Gallup
2013	April	Assault weapon ban	ABC
2013	April	Assault weapon ban	CNN
2013	April	Assault weapon ban	ABC
2013	January	Assault weapon ban	CNN
2013	January	Assault weapon ban	ABC
2013	January	Assault weapon ban	Pew
2013	March	Assault weapon ban	ABC
2013	May	Assault weapon ban	Pew
2013	February	Death penalty	CBS
2013	May	Law protecting gay from discrimination	PRRI
2013	December	Legalize marijuana	ReasonRupe
2014	March	Abortion legal (v2)	Pew
2014	September	Abortion legal (v2)	Pew
2014	June	Allow gay marriage	ABC
2014	March	Allow gay marriage	Pew
2014	March	Allow gay marriage	ABC
2014	September	Allow gay marriage	Pew
2014	April	Death penalty	Pew
2014	June	Death penalty	ABC
2014	May	Death penalty	CBS
2014	March	Legalize marijuana	Pew

Table A13: Economic Domain - Survey Question Text

Year	Month	Item	Organization
1936	December	Allow regulations	Gallup
1936	December	Governmnent should own banks	Gallup
1936	December	Governmnent should own railroad industry	Gallup
1936	December	Limit private wealth	Gallup
1936	December	WPA wages	Gallup
1936	November	Social security (v1)	Gallup
1937	April	Abolish WPA	Gallup
1937	April	Favor single large union	Gallup
1937	April	Limit hours people can work	Gallup
1937	April	Remove sit-ins by force	Gallup
1937	August	Governmnent should own banks	Gallup
1937	August	Govt aid for striking workers	Gallup
1937	August	Limit hours people can work	Gallup
1937	August	Minimum wage (v2)	Gallup
1937	August	Regulations	Gallup
1937	August	Regulations	Gallup
1937	August	Unemployment benefits for striking workers	Gallup
1937	December	Government should help poor	Gallup
1937	December	Governmnent should own railroad industry	Gallup
1937	December	Remove sit-ins by force	Gallup
1937	January	Favor single large union	Gallup
1937	January	Regulate business profits during wartime	Gallup
1937	January	Social security (v1)	Gallup
1937	January	Takeover businesses during wartime	Gallup
1937	July	Governmnent should own banks	Gallup
1937	July	Unions - closed shops	Gallup
1937	June	Favor single large union	Gallup
1937	June	Govt provide health care to poor	Gallup
1937	June	Labor unions - general support	Gallup
1937	June	Limit private wealth	Gallup
1937	June	Tax on chain stores	Gallup
1937	June	Tax on chain stores	Gallup
1937	March	Allow regulations	Gallup
1937	March	Favor single large union	Gallup
1937	March	Remove sit-ins by force	Gallup
1937	March	Remove sit-ins by force	Gallup
1937	March	Remove sit-ins by force	Gallup
1937	March	Sit down strikes illegal	Gallup
1937	March	Sit down strikes illegal	Gallup
1937	May	Limit hours people can work	Gallup
1937	May	Minimum wage (v2)	Gallup
1937	May	Tax on chain stores	Gallup
1937	November	Committee sets minimum wage	Gallup
1937	November	Minimum wage (v1)	Gallup
1938	April	Increase govt. spending (1938)	Gallup
1938	April	Minimum wage (v1)	Gallup
1938	August	Old age pensions	Gallup
1938	December	Approve hours	Gallup
1938	December	Should be secretary of welfare	Gallup
1938	February	Veterans pension (v1)	Gallup
1938	January	Governmnent should own electricity industry	Gallup
1938	January	Increase govt. spending (1938)	Gallup
1938	January	Tax on chain stores	Gallup
1938	January	Union reports	Gallup
1938	July	Approve hours	Gallup
1938	July	Government should help poor	Gallup
1938	July	Governmnent should own railroad industry	Gallup
1938	July	Increase govt. spending (1938)	Gallup
1938	July	Social security (v1)	Gallup
1938	June	Increase govt. spending (1938)	Gallup
1938	March	Increase govt. spending (1938)	Gallup
1938	March	Taxes vs spending	Gallup
1938	May	Committee sets minimum wage	Gallup
1938	May	Govt provide health care to poor	Gallup

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Year	Month	Item	Organization
1938	May	Increase govt. spending (1938)	Gallup
1938	May	Regulated for big businesses	Gallup
1938	NA	Social security (v2)	Gallup
1938	NA	Unemployment benefits for striking workers	Gallup
1938	October	Labor unions - general support	Gallup
1939	August	Labor unions - general support	Gallup
1939	December	Rights to refuse to hire unions	Gallup
1939	December	Wagner Labor Act	Gallup
1939	December	WPA union	Gallup
1939	February	Regulated for big businesses	Gallup
1939	February	Sit down strikes illegal	Gallup
1939	February	Wagner Labor Act	Gallup
1939	January	Government spending on relief (1939)	Gallup
1939	January	Increase old age pensions	Gallup
1939	January	Increase old age pensions	Gallup
1939	January	Old age pensions	Gallup
1939	January	Social security (v2)	Gallup
1939	July	Ban Strikes	Gallup
1939	June	Increase govt. spending (1939)	Gallup
1939	May	Abolish WPA	Gallup
1939	May	Labor unions - general support	Gallup
1939	May	Unions - closed shops	Gallup
1939	May	Unions - union hops	Gallup
1939	May	Workers rights to join unions	Gallup
1939	November	Govt aid for striking workers	Gallup
1939	November	Labor unions - general support	Gallup
1939	November	Old age pensions	Gallup
1939	November	Unemployment benefits for striking workers	Gallup
1939	November	Workers rights to join unions	Gallup
1939	October	Takeover businesses during wartime	Gallup
1940	August	Governmnent should own electricity industry	Gallup
1940	February	Reduce farm spending 30 percent	Gallup
1940	February	Reduce public works spending 20 percent	Gallup
1940	February	WPA union	Gallup
1940	January	Government spending on relief (1940)	Gallup
1940	January	Reduce farm spending 30 percent	Gallup
1940	January	Reduce public works spending 20 percent	Gallup
1940	January	Wagner Labor Act	Gallup
1940	May	Labor unions - general support	Gallup
1940	May	Labor unions - general support	Gallup
1940	May	Regulate businesses (1940)	Gallup
1940	May	Veterans pension (v1)	Gallup
1940	October	Regulate businesses (1940)	Gallup
1940	October	Regulate businesses (1940)	Gallup
1940	October	Wagner Labor Act	Gallup
1941	August	Forbid all strikes (vital industries)	Gallup
1941	December	Forbid all strikes (vital industries)	Gallup
1941	July	Old age pensions	Gallup
1941	June	Labor unions - general support	Gallup
1941	June	Regulate businesses (1941)	Gallup
1941	March	Rights to refuse to hire unions	Gallup
1941	May	Forbid all strikes (vital industries)	Gallup
1941	November	Forbid all strikes (vital industries)	Gallup
1941	November	Unions - closed shops	Gallup
1941	October	Forbid all strikes (vital industries)	Gallup
1941	October	Government workers can strikes	Gallup
1941	October	Labor unions - general support	Gallup
1941	October	Unions - closed shops	Gallup
1941	October	Unions - union hops	Gallup
1941	September	Unions - elect director	Gallup
1942	April	Abolish CCC	Gallup
1942	April	Over time	Gallup
1942	December	Forbid all strikes (non-vital industries)	Gallup
1942	December	Forbid all strikes (vital industries)	Gallup
1942	December	Limit executives income	Gallup

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Year	Month	Item	Organization
1942	December	Limit income	Gallup
1942	December	Limit movie stars income	Gallup
1942	December	Union reports	Gallup
1942	March	Forbid all strikes (vital industries)	Gallup
1942	March	Union reports	Gallup
1942	May	Limit income	Gallup
1942	September	Forbid all strikes (vital industries)	Gallup
1943	February	Unemployment	Gallup
1943	June	Govt should prevent labor unions from forcing employers to hire more workers than are needed	Gallup
1943	June	Govt should prevent labor unions from forcing employers to hire more workers than are needed	Gallup
1943	May	Forbid all strikes (vital industries)	Gallup
1943	May	Labor unions - general support	Gallup
1943	NA	Govt provide health care to poor	Gallup
1943	NA	Social security (v7)	Gallup
1943	NA	Subsidize education expenses for poor	Gallup
1943	November	Forbid all strikes (vital industries)	Gallup
1943	November	Union reports	Gallup
1944	August	Unemployment insurance	Gallup
1944	December	Social security (v3)	Gallup
1944	December	Social security (v4)	Gallup
1944	December	Social security (v5)	Gallup
1944	December	Social security (v6)	Gallup
1944	January	Forbid all strikes (vital industries)	Gallup
1944	January	Govt should prevent labor unions from forcing employers to hire more workers than are needed	Gallup
1944	May	Forbid all strikes (vital industries)	Gallup
1944	NA	Social security (v7)	Gallup
1945	August	Government-run health care (v1)	Gallup
1945	August	Minimum wage (65 cents)	Gallup
1945	August	Should be secretary of welfare	Gallup
1945	December	Govt should prevent labor unions from forcing employers to hire more workers than are needed	Gallup
1945	July	Forbid all strikes	Gallup
1945	July	Right to work	Gallup
1945	July	Unions - closed shops	Gallup
1945	July	Unions - union hops	Gallup
1945	June	Fund cancer research	Gallup
1945	June	Governmnent should own electricity industry	Gallup
1945	June	Governmnent should own railroad industry	Gallup
1945	June	Taxes to pay for cancer research	Gallup
1945	June	Unemployment	Gallup
1945	March	Forbid all strikes (vital industries)	Gallup
1945	May	Military plans vs private business	Gallup
1945	November	Truman health care bill (knowledgeable)	Gallup
1945	October	Atomic energy	Gallup
1945	October	Ceiling on rents	Gallup
1945	October	Ration meat	Gallup
1945	October	Unemployment benefits	Gallup
1945	September	Committee sets minimum wage	Gallup
1945	September	Governmnent should own banks	Gallup
1945	September	Governmnent should own coal industry	Gallup
1945	September	Governmnent should own electricity industry	Gallup
1945	September	Governmnent should own railroad industry	Gallup
1945	September	Minimum wage (60 cents)	Gallup
1945	September	Minimum wage (65 cents)	Gallup
1945	September	Unemployment benefits	Gallup
1946	April	Ceiling on rents	Gallup
1946	April	Government-run health care (v2)	Gallup
1946	April	Government-run health care (v3)	Gallup
1946	April	Military plans vs private business	Gallup
1946	August	Ration meat	Gallup
1946	February	Ban Strikes	Gallup
1946	February	Labor unions - general support	Gallup

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Year	Month	Item	Organization
1946	February	Military plans vs private business	Gallup
1946	January	Max tax rate of 50 percent	Gallup
1946	January	Price controls	Gallup
1946	May	Ban Strikes	Gallup
1946	May	Fund cancer research	Gallup
1946	May	Unemployment benefits for striking workers	Gallup
1946	November	Right to work	Gallup
1946	November	Tax decrease (1946)	Gallup
1946	November	Union reports	Gallup
1946	November	Unions - closed shops	Gallup
1946	November	Unions - union hops	Gallup
1946	October	Ceiling on rents	Gallup
1946	October	Minimum wage (65 cents)	Gallup
1946	October	Ration meat	Gallup
1946	October	Tax decrease (1946)	Gallup
1946	September	Ceiling on rents	Gallup
1946	September	Ration meat	Gallup
1946	September	Teachers allowed to form unions	Gallup
1947	April	Tax decrease (1947)	Gallup
1947	August	Taft Hartley bill (v2)	Gallup
1947	February	Atomic energy	Gallup
1947	February	Tax decrease (1947)	Gallup
1947	January	Ban Strikes	Gallup
1947	January	Governmnent should own banks	Gallup
1947	January	Governmnent should own coal industry	Gallup
1947	January	Governmnent should own electricity industry	Gallup
1947	January	Governmnent should own railroad industry	Gallup
1947	January	Jurisdictional strikes	Gallup
1947	July	Forbid all strikes	Gallup
1947	July	Labor unions - general support	Gallup
1947	July	Taft Hartley bill (v1)	Gallup
1947	July	Taft Hartley bill (v2)	Gallup
1947	June	Governmnent should own electricity industry	Gallup
1947	March	Ban Strikes	Gallup
1947	March	Government workers can strikes	Gallup
1947	March	Governmnent should own electricity industry	Gallup
1947	May	Ban Strikes	Gallup
1947	May	Governmnent should own banks	Gallup
1947	May	Governmnent should own coal industry	Gallup
1947	May	Governmnent should own electricity industry	Gallup
1947	May	Governmnent should own railroad industry	Gallup
1947	May	Max tax rate of 50 percent	Gallup
1947	May	Minimum wage (65 cents)	Gallup
1947	May	Should be secretary of welfare	Gallup
1947	May	Tax decrease (1947)	Gallup
1947	November	Max tax rate of 50 percent	Gallup
1947	November	Taft Hartley bill (v4)	Gallup
1948	April	Ration some products	Gallup
1948	December	Federal aid for schools	Gallup
1948	December	Governmnent should own banks	Gallup
1948	December	Governmnent should own coal industry	Gallup
1948	December	Governmnent should own electricity industry	Gallup
1948	December	Governmnent should own railroad industry	Gallup
1948	December	Labor unions - general support	Gallup
1948	December	Social security (v3)	Gallup
1948	December	Social security (v4)	Gallup
1948	December	Social security (v5)	Gallup
1948	December	Social security (v6)	Gallup
1948	February	Ceiling on rents	Gallup
1948	January	Laws regulating unions are too strict	Gallup
1948	January	Minimum wage (75 cents)	Gallup
1948	January	Ration some products	Gallup
1948	January	Taft Hartley bill (v2)	Gallup
1948	January	Taft Hartley bill (v4)	Gallup
1948	July	Minimum wage (75 cents)	Gallup

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Year	Month	Item	Organization
1948	July	Ration some products	Gallup
1948	July	Taft Hartley bill (v2)	Gallup
1948	July	Taft Hartley bill (v3)	Gallup
1948	July	Taft Hartley bill (v4)	Gallup
1948	May	Federal aid for schools	Gallup
1948	May	Fund heart disease research	Gallup
1948	May	Governmnent should own banks	Gallup
1948	May	Governmnent should own coal industry	Gallup
1948	May	Governmnent should own electricity industry	Gallup
1948	May	Governmnent should own railroad industry	Gallup
1948	May	Laws regulating unions are too strict	Gallup
1948	May	Taft Hartley bill (v2)	Gallup
1948	May	Taft Hartley bill (v4)	Gallup
1948	November	Ceiling on rents	Gallup
1948	November	Laws regulating unions are too strict	Gallup
1948	November	Minimum wage (75 cents)	Gallup
1948	November	Ration some products	Gallup
1948	November	Slum clearance	Gallup
1948	November	Taft Hartley bill (v1)	Gallup
1948	November	Taft Hartley bill (v2)	Gallup
1948	November	Taft Hartley bill (v2)	Gallup
1948	November	Taft Hartley bill (v4)	Gallup
1948	November	Taft Hartley bill (v4)	Gallup
1949	April	Taft Hartley bill (v1)	Gallup
1949	April	Taft Hartley bill (v4)	Gallup
1949	February	Govt should prevent labor unions from forcing employers to hire more workers than are needed	Gallup
1949	February	Jurisdictional strikes	Gallup
1949	February	Right to work	Gallup
1949	February	Unions - closed shops	Gallup
1949	February	Unions - right to strike	Gallup
1949	February	Unions - union hops	Gallup
1949	January	Taft Hartley bill (v1)	Gallup
1949	January	Taft Hartley bill (v2)	Gallup
1949	January	Taft Hartley bill (v4)	Gallup
1949	January	Truman health care bill	Gallup
1949	January	Union reports	Gallup
1949	January	Unions - closed shops	Gallup
1949	July	Should be secretary of welfare	Gallup
1949	July	Unemployment benefits for striking workers	Gallup
1949	June	Taft Hartley bill (v4)	Gallup
1949	March	Truman health care bill (knowledgeable)	Gallup
1949	March	Veterans pension (v2)	Gallup
1949	May	Federal aid for schools	Gallup
1949	May	Minimum wage (60 cents)	Gallup
1949	May	Minimum wage (65 cents)	Gallup
1949	May	Taxes	Gallup
1949	May	Truman health care bill	Gallup
1949	November	Laws regulating unions are too strict	Gallup
1949	November	Taft Hartley bill (v2)	Gallup
1949	November	Taft Hartley bill (v3)	Gallup
1949	November	Taft Hartley bill (v4)	Gallup
1949	November	Truman health care bill (knowledgeable)	Gallup
1949	October	Raise taxes	Gallup
1949	October	Taft Hartley bill (v3)	Gallup
1949	October	Truman health care bill	Gallup
1949	September	Unions are monopoly	Gallup
1950	February	Unions are monopoly	Gallup
1950	January	Taxes	Gallup
1950	July	Forbid all strikes (vital industries)	Gallup
1950	June	Taft Hartley bill (v3)	Gallup
1950	March	Spending on public works	Gallup
1950	March	Spending on social welfare	Gallup
1950	March	Taft Hartley bill (v3)	Gallup
1950	November	Truman health care bill	Gallup

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Year	Month	Item	Organization
1950	October	Taft Hartley bill (v3)	Gallup
1950	October	Truman health care bill	Gallup
1951	August	Maximum tax rate of 25 percent	Gallup
1951	August	Police allowed to form unions	Gallup
1951	December	Labor unions - general support	Gallup
1951	June	Price controls	Gallup
1951	June	Price freeze (early 1950s)	Gallup
1951	March	Reconstruction Finance Corporate	Gallup
1952	December	Price controls	Gallup
1952	December	Price freeze (early 1950s)	Gallup
1952	July	Taft Hartley bill (v3)	Gallup
1952	May	Laws regulating unions are too strict	Gallup
1952	May	Maximum tax rate of 25 percent	Gallup
1952	May	Taft Hartley bill (v2)	Gallup
1952	May	Taft Hartley bill (v4)	Gallup
1952	NA	Government should do more	Gallup
1952	November	Should be secretary of welfare	Gallup
1952	October	Taft Hartley bill (v2)	Gallup
1952	October	Taft Hartley bill (v4)	Gallup
1952	October	Truman health care bill	Gallup
1952	October	Truman health care bill	Gallup
1952	September	Truman health care bill	Gallup
1952	September	Truman health care bill	Gallup
1953	April	Tax to pay for highways	Gallup
1953	December	Ban stikes by communications workers	Gallup
1953	February	Maximum tax rate of 25 percent	Gallup
1953	November	35 hour work week	Gallup
1953	November	Max tax rate on 50k in income	Gallup
1953	October	Laws regulating unions are too strict	Gallup
1953	September	Governmnent should own coal industry	Gallup
1953	September	Governmnent should own electricity industry	Gallup
1953	September	Minimum wage (1 dollar)	Gallup
1954	December	Minimum wage (1 dollar and 25 cents)	Gallup
1954	February	Union stikes - secret vote	Gallup
1954	June	Fund heart disease research	Gallup
1954	March	Fund cancer research	Gallup
1954	March	Tax to pay for highways	Gallup
1954	May	Public works	Gallup
1955	August	Build big dams	Gallup
1955	December	Federal aid for schools	Gallup
1955	February	Federal aid for schools	NORC
1955	February	Government pay for health care	NORC
1955	February	Governmnent should own banks	NORC
1955	February	Governmnent should own electricity industry	NORC
1955	February	Governmnent should own railroad industry	NORC
1955	February	Maximum salary	NORC
1955	February	Regulation of business	NORC
1955	February	Right to work	NORC
1955	January	35 hour work week	Gallup
1955	January	Maximum tax rate of 35 percent	Gallup
1955	January	Public works	Gallup
1955	July	Build big dams	Gallup
1955	July	Tax to pay for school	Gallup
1955	September	Bonus for babies	Gallup
1956	April	Build new highways	Gallup
1956	February	Max tax rate on 50k in income	Gallup
1956	June	Regulate monopolies	Gallup
1956	March	Fine for littering	Gallup
1956	May	Higher taxes on big companies	Gallup
1956	November	Drivers should be required to get physical examination	Gallup
1956	November	Summer vacation for schools	Gallup
1956	October	Federal aid for schools	ANES
1956	October	Guaranteed jobs (likert)	ANES
1956	October	Privatize electricity companies	ANES
1956	October	Subsidize health care	ANES

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Year	Month	Item	Organization
1956	October	Universal healthcare (ANES- v1)	ANES
1957	April	Max tax rate on 50k in income	Gallup
1957	April	Unions accounting should be public	Gallup
1957	January	Federal aid for schools	Gallup
1957	January	Minimum wage (1 dollar and 25 cents)	Gallup
1957	July	Federal aid for schools	Gallup
1957	July	Four day work week	Gallup
1957	July	Right to work	Gallup
1957	June	Max tax rate of 25-35 percent	Gallup
1958	April	Tax cuts versus spending	Gallup
1958	January	Free college tuition	Gallup
1958	May	Tax cuts versus spending	Gallup
1958	October	Federal aid for schools	ANES
1958	October	Guaranteed jobs (likert)	ANES
1958	October	Price freeze (late 1950s)	ANES
1958	October	Privatize electricity companies	ANES
1958	October	Unions accounting should be public	Gallup
1959	January	Police unions	Gallup
1959	January	Teacher unions	Gallup
1959	January	Laws regulating unions are too strict	Gallup
1959	January	Same pay for 35 hour work week as 40 hour week	Gallup
1959	May	Price freeze (late 1950s)	Gallup
1959	November	Special court for union disputes	Gallup
1959	October	Youth CCC	Gallup
1960	April	Govt should fix automobile prices	Gallup
1960	April	Govt should fix drug prices	Gallup
1960	January	Federal aid for schools	Gallup
1960	October	Guaranteed jobs (likert)	ANES
1960	October	Privatize electricity companies	ANES
1960	October	Subsidize health care	ANES
1960	October	Universal healthcare (ANES- v1)	ANES
1961	December	Limit corporate profits	NORC
1961	December	Maximum salary	NORC
1961	December	Regulation of business	NORC
1961	December	Right to work	NORC
1961	December	Unions should be legal	NORC
1961	February	Federal aid for schools	Gallup
1961	February	Minimum wage (1961)	Gallup
1961	May	Medicare should be passed	Gallup
1961	May	Laws regulating unions are too strict	Gallup
1961	May	Favor allowing unions to require more workers than are needed for a job	Gallup
1961	November	Welfare	Gallup
1961	October	Mandatory automobile insurance	Gallup
1961	October	Welfare	Gallup
1961	September	Favor allowing unions to require more workers than are needed for a job	Gallup
1962	February	35 hour work week	Gallup
1962	July	Laws regulating unions are too strict	Gallup
1962	July	Income tax cut versus reduce deficitn	Gallup
1962	July	35 hour work week	Gallup
1962	June	Medicare should be passed	Gallup
1962	March	Medicare should be passed	Gallup
1962	March	Favor allowing unions to require more workers than are needed for a job	Gallup
1962	October	Federal aid for schools	ANES
1962	October	Price freeze (late 1950s)	ANES
1962	October	Universal healthcare (ANES- v1)	ANES
1963	April	Income tax cut versus reduce deficitn	Gallup
1963	January	Ban stikes by communications workers	Gallup
1963	January	Ban stikes by transportation workers	Gallup
1963	January	Income tax	Gallup
1963	July	Favor allowing unions to require more workers than are needed for a job	Gallup
1963	May	Lottery	Gallup

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Year	Month	Item	Organization
1964	April	Lottery	Gallup
1964	November	Welfare - require 60 day residency	Gallup
1964	October	Guaranteed jobs (non-likert)	ANES
1964	October	Universal healthcare (ANES- v1)	ANES
1964	October	Federal aid for schools (v2)	Gallup
1964	October	Medicare should be passed	Gallup
1964	October	Medicare should be passed	NORC
1964	October	Medicare should be passed	NORC
1964	September	Federal aid for schools (v2)	Gallup
1964	September	Medicare should be passed	Gallup
1965	August	Minimum wage (1 dollar and 50 cents)	Gallup
1965	February	Federal aid for schools (v3)	Gallup
1965	May	Build power lines underground	Gallup
1965	May	Taxes for new power lines	Gallup
1965	May	Require people to join unions if in unionized business	Gallup
1965	October	Allow policy strikes	Gallup
1965	October	Allow teacher strikes	Gallup
1965	October	Police allowed to form unions	Gallup
1965	October	Teachers allowed to form unions	Gallup
1965	October	Require people to join unions if in unionized business	Gallup
1965	September	Minimum annual income	Gallup
1965	September	Require people to join unions if in unionized business	Gallup
1966	August	Ban stikes by communications workers	Gallup
1966	August	Ban stikes by transportation workers	Gallup
1966	December	Return 3 percent of federal revenue to states	Gallup
1966	January	Minimum wage (1 dollar and 50 cents) (v2)	Gallup
1966	January	Laws regulating unions are too strict	Gallup
1966	January	Same pay for 35 hour work week as 40 hour week	Gallup
1966	January	Require people to join unions if in unionized business	Gallup
1967	April	Strikes - Govt. mediate after 21 days	Gallup
1967	April	Return 3 percent of federal revenue to states	Gallup
1967	June	Return 3 percent of federal revenue to states	Gallup
1967	March	Require people to join unions if in unionized business	Gallup
1967	November	Build power lines underground	Gallup
1968	December	Allow teacher strikes	Gallup
1968	December	Equalize welfare payments (v1)	Gallup
1968	December	Guaranteed jobs (likert)	Gallup
1968	December	Police allowed to form unions	Gallup
1968	December	Strikes - Govt. mediate after 21 days	Gallup
1968	December	Teachers allowed to form unions	Gallup
1968	December	Return 3 percent of federal revenue to states	Gallup
1968	February	Allow teacher strikes	Gallup
1968	February	Teachers allowed to form unions	Gallup
1968	January	Strikes - Govt. mediate after 21 days	Gallup
1968	March	Guaranteed jobs (likert)	Gallup
1968	November	Help unions	Comparative State Elections Project
1968	November	Increase social security benefits	Comparative State Elections Project
1968	November	Stop poverty programs	Comparative State Elections Project
1968	October	Guaranteed jobs (non-likert)	ANES
1968	October	Universal healthcare (ANES- v1)	ANES
1969	April	Food stamps	Gallup
1969	January	Equalize welfare payments (v2)	Gallup
1969	January	Welfare costs	Gallup
1969	January	Favor allowing unions to require more workers than are needed for a job	Gallup
1969	June	Child care for the poor	Gallup
1970	April	Guidance counselor in schools	Gallup
1970	April	State taxes for education	Gallup
1970	October	Inflation	ANES

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Year	Month	Item	Organization
1970	October	Polluion	ANES
1970	October	Universal healthcare (ANES- v2)	ANES
1971	August	Mandatory drivers education	Gallup
1971	January	National health insurance	Gallup
1971	January	Return 3 percent of federal revenue to states	Gallup
1971	November	College spending	Gallup
1971	November	Price controls (1971)	Gallup
1971	October	Build power lines underground	Gallup
1972	August	Price controls (1972)	Gallup
1972	December	Spending vs taxes	Gallup
1972	January	Price controls (1972)	Gallup
1972	March	Price controls (1972)	Gallup
1972	March	Strikes - Govt. mediate after 21 days	Gallup
1972	October	Guaranteed jobs (multi-point scale)	ANES
1972	October	Pollution	ANES
1972	October	Universal healthcare (ANES- v2)	ANES
1973	April	Price controls (1973)	Gallup
1973	February	Government run railroads	Gallup
1973	February	Favor allowing unions to require more workers than are needed for a job	Gallup
1973	July	Utility bills vs pollution	Gallup
1973	June	Require seat belts	Gallup
1973	May	State taxes for education (v2)	Gallup
1974	August	Price controls (mid-late 1970s)	Gallup
1974	December	Price controls (mid-late 1970s)	Roper
1974	May	Reduce school spending differences	Gallup
1974	October	Guaranteed jobs (multi-point scale)	ANES
1974	October	Five percent surtax on the rich	Gallup
1974	October	Gas tax of 20 cents	Gallup
1974	October	Price controls (mid-late 1970s)	Gallup
1974	October	Freeze pollution control	Roper
1974	October	Spending cuts	Roper
1975	April	National health insurance	Roper
1975	October	Allow policy strikes	Roper
1975	October	Allow teacher strikes	Roper
1975	October	National health insurance	Roper
1975	September	Allow policy strikes	Gallup
1975	various	Govt help poor	GSS
1975	various	Govt help pay medical bills	GSS
1976	April	Government size	CBS
1976	December	Price controls (mid-late 1970s)	Gallup
1976	June	Government size	CBS
1976	March	Guaranteed jobs (likert)	CBS
1976	March	Balance budget amendment	Gallup
1976	May	Government size	CBS
1976	May	Guaranteed jobs (likert)	CBS
1976	November	Government size	CBS
1976	October	Guaranteed jobs (multi-point scale)	ANES
1976	October	Universal healthcare (ANES- v2)	ANES
1976	October	Government size	CBS
1976	October	Guaranteed jobs (likert)	CBS
1976	October	National health insurance	Roper
1976	September	Government size	CBS
1976	September	National health insurance	CBS
1977	August	Ban cigarette advertisements	Gallup
1977	July	Guaranteed income of 2200 dollars	CBS
1977	March	Guaranteed jobs (likert)	Time
1977	October	Require seat belts	Gallup
1977	September	National health insurance	Roper
1978	August	Allow policy strikes	Gallup
1978	August	Allow teacher strikes	Gallup
1978	August	Allow policy strikes	Roper
1978	August	Allow teacher strikes	Roper
1978	December	Price controls (mid-late 1970s)	Gallup
1978	January	Guaranteed jobs (likert)	CBS

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Year	Month	Item	Organization
1978	January	Allow policy strikes	Gallup
1978	January	Allow teacher strikes	Gallup
1978	June	Balance budget amendment	Gallup
1978	October	Guaranteed jobs (multi-point scale)	ANES
1978	October	Universal healthcare (ANES- v2)	ANES
1978	September	National health insurance	Roper
1978	various	Govt reduce income differences between rich and poor	GSS
1979	August	Price controls (mid-late 1970s)	Gallup
1979	December	Balance budget amendment	Gallup
1979	February	National health insurance	Roper
1979	January	Government spending for the poor	CBS
1979	January	Doctors at low cost	Gallup
1979	May	Allow policy strikes	Gallup
1979	May	Allow teacher strikes	Gallup
1979	May	Price controls (mid-late 1970s)	Gallup
1979	November	Gas tax	Gallup
1980	April	Price controls (mid-late 1970s)	Roper
1980	August	Unemployment	CBS
1980	February	National health insurance	CBS
1980	March	Government size	CBS
1980	March	National health insurance	CBS
1980	March	Nuclear power	CBS
1980	March	Balance budget amendment	Gallup
1980	March	Price controls (mid-late 1970s)	Time
1980	May	Allow teacher strikes	Gallup
1980	October	Guaranteed jobs (multi-point scale)	ANES
1980	September	Income tax cut	CBS
1980	September	Nuclear power	Gallup
1980	September	Price controls (mid-late 1970s)	Gallup
1980	various	Govt reduce income differences between rich and poor	GSS
1981	April	National health insurance	CBS
1981	April	Tax cuts	CBS
1981	April	Balance budget amendment	Gallup
1981	August	Allow policy strikes	Gallup
1981	January	Income tax cut	CBS
1981	January	Price controls (mid-late 1970s)	Gallup
1981	June	Ban cigarette advertisements	Gallup
1981	June	Price controls (mid-late 1970s)	Gallup
1981	March	Tax cuts	LATimes
1981	October	Environment	CBS
1981	September	Balance budget amendment	Gallup
1982	August	Balance budget amendment	Gallup
1982	July	Require seat belts	Gallup
1982	May	Eliminate tax cuts	CBS
1982	May	Reduced spending	CBS
1982	May	Balance budget amendment	Gallup
1982	October	Guaranteed jobs (multi-point scale)	ANES
1982	September	Environment	CBS
1983	April	Nuclear power	ABC
1983	April	Environment	CBS
1983	January	Eliminate tax cuts	CBS
1983	January	Reduced spending	CBS
1983	January	Cut entitlements	Gallup
1983	June	Eliminate tax cuts	CBS
1983	June	Balance budget amendment	Gallup
1983	June	Cut entitlements	Gallup
1983	June	Nuclear power	Roper
1983	September	National health insurance	Roper
1983	various	Govt reduce income differences between rich and poor	GSS
1983	various	Govt help poor	GSS
1983	various	Govt help pay medical bills	GSS
1984	December	Cut entitlements	Gallup
1984	February	Balance budget amendment	Gallup
1984	July	Government size	ABC
1984	July	Government size	ABC

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Year	Month	Item	Organization
1984	October	Guaranteed jobs (multi-point scale)	ANES
1984	October	More spending	ANES
1984	October	Universal healthcare (ANES- v2)	ANES
1984	various	Govt reduce income differences between rich and poor	GSS
1984	various	Govt help poor	GSS
1984	various	Govt help pay medical bills	GSS
1985	December	No welfare for ablebodied adults	ABC
1985	June	Cut entitlements	Gallup
1985	June	Lower top tax bracket	Gallup
1985	November	Taxes vs spending (1985)	CBS
1985	September	Ban cigarette advertisements	ABC
1985	various	Govt responsibility to provide health care for sick	GSS
1985	various	Govt responsibility to provide jobs for all	GSS
1986	January	Environment	CBS
1986	October	Guaranteed jobs (multi-point scale)	ANES
1986	September	Taxes vs spending (1986)	Time
1986	various	Govt reduce income differences between rich and poor	GSS
1986	various	Govt help poor	GSS
1986	various	Govt help pay medical bills	GSS
1987	January	Guaranteed jobs (non-likert)	ABC
1987	March	Ban cigarette advertisements	Gallup
1987	May	Balance budget amendment	CBS
1987	various	Govt reduce income differences between rich and poor	GSS
1987	various	Govt help poor	GSS
1987	various	Govt help pay medical bills	GSS
1987	Various	Govt provide minimum standard of living	Pew
1988	August	Balance budget amendment	CBS
1988	August	Layoffs	CBS
1988	July	Government size	ABC
1988	July	Environment	CBS
1988	July	Ban cigarette advertisements	Gallup
1988	May	Layoffs	ABC
1988	May	Government size	CBS
1988	May	Govt provide minimum standard of living	Gallup
1988	November	Spending on health care	ANES Senate
1988	November	Spending on schools	ANES Senate
1988	October	Guaranteed jobs (multi-point scale)	ANES
1988	October	Universal healthcare (ANES- v2)	ANES
1988	October	Government size	CBS
1988	various	Govt reduce income differences between rich and poor	GSS
1988	various	Govt help poor	GSS
1988	various	Govt help pay medical bills	GSS
1989	April	Minimum wage (1989)	ABC
1989	April	Environment	CBS
1989	April	Clean Air Act Amendments	Gallup
1989	August	Capital gains tax cut	ABC
1989	February	Capital gains tax cut	ABC
1989	January	Government size	CBS
1989	June	Clean Air Act Amendments	ABC
1989	June	Minimum wage (1989)	ABC
1989	May	Minimum wage (1989)	Gallup
1989	October	Capital gains tax cut	Gallup
1989	various	Govt reduce income differences between rich and poor	GSS
1989	various	Govt help poor	GSS
1989	various	Govt help pay medical bills	GSS
1989	various	Govt responsibility to provide jobs for all	GSS
1989	various	Govt provide minimum standard of living	Pew
1990	April	Clean Air Act Amendments	NBC
1990	April	Nuclear power	NBC
1990	February	Capital gains tax cut	ABC
1990	July	Ban cigarette advertisements	Gallup
1990	March	Nuclear power	ABC
1990	March	Clean Air Act Amendments	WashPost
1990	November	Spending on health care	ANES Senate
1990	November	Spending on homeless	ANES Senate

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Year	Month	Item	Organization
1990	November	Spending on schools	ANES Senate
1990	October	Guaranteed jobs (multi-point scale)	ANES
1990	October	National health insurance	CBS
1990	various	Govt reduce income differences between rich and poor	GSS
1990	various	Govt help poor	GSS
1990	various	Govt help pay medical bills	GSS
1990	various	Govt responsibility to provide health care for sick	GSS
1990	various	Govt responsibility to provide jobs for all	GSS
1991	April	Nuclear power	Time
1991	August	National health insurance	CBS
1991	December	National health insurance	Time
1991	July	Universal healthcare (v2)	Gallup
1991	June	National health insurance	CBS
1991	October	Family leave	ABC
1991	October	Unemployment (1992)	ABC
1991	October	Unemployment (1992)	ABC
1991	October	Government size	CBS
1991	October	Ban cigarette advertisements	Gallup
1991	September	Unemployment (1992)	LATimes
1991	various	Govt reduce income differences between rich and poor	GSS
1991	various	Govt help poor	GSS
1991	various	Govt help pay medical bills	GSS
1991	various	Govt responsibility to provide jobs for all	GSS
1991	various	Govt provide minimum standard of living	Pew
1992	January	National health insurance	CBS
1992	July	Government size	ABC
1992	July	National health insurance	CBS
1992	June	Balance budget amendment	ABC
1992	June	Guaranteed jobs (non-likert)	CBS
1992	June	Universal healthcare (v2)	CBS
1992	March	Nuclear power	Time
1992	May	Environment	CBS
1992	May	Family leave	Gallup
1992	November	Spending on health care	ANES Senate
1992	November	Spending on homeless	ANES Senate
1992	November	Spending on schools	ANES Senate
1992	November	Family leave	Gallup
1992	October	Guaranteed jobs (multi-point scale)	ANES
1992	October	Universal healthcare (ANES- v2)	ANES
1992	October	Family leave	CBS
1992	various	Govt provide minimum standard of living	Pew
1993	January	Family leave	ABC
1993	January	Family leave	CBS
1993	January	National health insurance	CBS
1993	June	Government size	LATimes
1993	September	Universal healthcare (v2)	CBS
1993	September	Universal healthcare (v2)	Gallup
1993	various	Govt reduce income differences between rich and poor	GSS
1993	various	Govt help poor	GSS
1993	various	Govt help pay medical bills	GSS
1994	December	Welfare - 5 year max	Gallup
1994	February	Balance budget amendment	ABC
1994	January	Balance budget amendment	Gallup
1994	July	Universal healthcare (v2)	CBS
1994	March	Ban cigarette advertisements	Gallup
1994	November	Balance budget amendment	Gallup
1994	October	Guaranteed jobs (multi-point scale)	ANES
1994	October	Universal healthcare (ANES- v2)	ANES
1994	various	Govt reduce income differences between rich and poor	GSS
1994	various	Govt help poor	GSS
1994	various	Govt help pay medical bills	GSS
1995	December	Universal healthcare for poor	CBS
1995	December	National health insurance	NYTimes
1995	December	Scale back Medicaid	NYTimes
1995	December	Scale back welfare	NYTimes

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Year	Month	Item	Organization
1995	February	Minimum wage (1995)	Gallup
1995	January	Balance budget amendment	ABC
1995	January	Minimum wage (1995)	ABC
1995	January	Government size	LATimes
1995	January	Minimum wage (1995)	LATimes
1995	January	National health insurance	NA
1995	October	Government size	LATimes
1995	September	Government size	LATimes
1996	April	Minimum wage (1995)	AP
1996	April	Minimum wage (1995)	CBS
1996	April	Balance budget amendment	Gallup
1996	April	Government size	LATimes
1996	April	Minimum wage (1995)	LATimes
1996	August	Balance budget amendment	ABC
1996	August	Government size	ABC
1996	August	Welfare - 5 year max	ABC
1996	August	Welfare reform	ABC
1996	February	Government size	CBS
1996	February	Guaranteed jobs (likert)	CBS
1996	February	Universal healthcare (v2)	CBS
1996	June	Welfare - 5 year max	AP
1996	June	Environment	CBS
1996	May	Minimum wage (1995)	Gallup
1996	May	Minimum wage (1995)	Time
1996	October	Environmental protection	ANES
1996	October	Guaranteed jobs (multi-point scale)	ANES
1996	October	Universal healthcare (ANES- v2)	ANES
1996	October	Welfare - 5 year max	CBS
1996	various	Govt reduce income differences between rich and poor	GSS
1996	various	Govt help poor	GSS
1996	various	Govt help pay medical bills	GSS
1996	various	Govt responsibility to provide health care for sick	GSS
1996	various	Govt responsibility to provide jobs for all	GSS
1997	February	Balance budget amendment	CBS
1997	February	Balance budget amendment	Time
1997	January	Welfare - 5 year max	AP
1997	January	Welfare reform	CBS
1997	January	Balance budget amendment	Gallup
1997	June	Ban cigarette advertisements	ABC
1997	June	Medicare reform	Gallup
1997	June	Medicare reform	Pew
1997	March	Balance budget amendment	ABC
1997	November	Environment	CBS
1997	September	Ban cigarette advertisements	Gallup
1997	September	Medicare reform	LATimes
1997	various	Govt provide minimum standard of living	Pew
1998	December	Privatize social security (v2)	Gallup
1998	December	National health insurance (v2)	Kaiser
1998	July	Privatize social security (v2)	Gallup
1998	October	Environmental protection	ANES
1998	October	Guaranteed jobs (multi-point scale)	ANES
1998	various	Govt reduce income differences between rich and poor	GSS
1998	various	Govt help poor	GSS
1998	various	Govt help pay medical bills	GSS
1998	various	Govt responsibility to provide jobs for all	GSS
1999	April	Minimum wage (2000)	Gallup
1999	December	Health care for children	Kaiser
1999	June	Government size	WashPost
1999	October	National health insurance (v2)	Kaiser
1999	October	Minimum wage (2000)	Pew
1999	September	Government size	CBS
1999	September	Guaranteed jobs (non-likert)	CBS
1999	various	Flat tax	NAES
1999	various	Reduce inequality	NAES
1999	various	Taxes on rich	NAES

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Year	Month	Item	Organization
1999	various	Universal healthcare	NAES
1999	various	Govt provide minimum standard of living	Pew
2000	December	Bush tax cuts (2001)	CBS
2000	December	Health care for children	Kaiser
2000	December	National health insurance (v2)	Kaiser
2000	January	Privatize social security (v2)	Gallup
2000	January	Universal healthcare (v2)	Gallup
2000	July	Government size	ABC
2000	July	Universal healthcare (v2)	CBS
2000	July	Health care for children	Kaiser
2000	July	National health insurance (v2)	Kaiser
2000	June	Privatize social security (v2)	Gallup
2000	March	Government size	ABC
2000	May	Privatize social security (v3)	ABC
2000	October	Government size	ABC
2000	October	Privatize social security (v3)	ABC
2000	October	Bush tax cuts (2001)	ANES
2000	October	Environmental protection	ANES
2000	October	Guaranteed jobs (multi-point scale)	ANES
2000	October	Universal healthcare (ANES- v2)	ANES
2000	October	Minimum wage (2000)	Gallup
2000	October	Government size	WashPost
2000	September	Government size	ABC
2000	September	Universal healthcare (v2)	Gallup
2000	September	Government size	LATimes
2000	various	Govt reduce income differences between rich and poor	GSS
2000	various	Govt help poor	GSS
2000	various	Govt help pay medical bills	GSS
2000	various	Estate tax	NAES
2000	various	Flat tax	NAES
2000	various	Health care for children	NAES
2000	various	Privatize social security	NAES
2000	various	Reduce inequality	NAES
2000	various	Taxes on rich	NAES
2000	various	Universal healthcare	NAES
2001	April	Drilling in Arctic	ABC
2001	April	Nuclear power	ABC
2001	April	Privatize social security (v3)	ABC
2001	April	Bush tax cuts (2001)	CBS
2001	April	Bush tax cuts (2001)	CBS
2001	April	Drilling in Arctic	CBS
2001	April	Bush tax cuts (2001)	Pew
2001	August	Bush tax cuts (2001)	CBS
2001	August	Drilling in Arctic	CBS
2001	February	Bush tax cuts (2001)	CBS
2001	February	Drilling in Arctic	CBS
2001	February	Bush tax cuts (2001)	Pew
2001	January	Drilling in Arctic	ABC
2001	January	Government size	CBS
2001	June	Nuclear power	ABC
2001	June	Environment	CBS
2001	March	Bush tax cuts (2001)	ABC
2001	March	Privatize social security (v3)	ABC
2001	March	Bush tax cuts (2001)	CBS
2001	March	Drilling in Arctic	CBS
2001	March	Estate tax	CBS
2001	March	Estate tax	CBS
2001	May	Drilling in Arctic	Gallup
2001	May	Privatize social security (v2)	Gallup
2001	November	Drilling in Arctic	Gallup
2001	November	Privatize social security (v2)	Gallup
2001	November	Government size	LATimes
2001	October	Government size	CBS
2002	August	Government size	WashPost
2002	December	Privatize social security (v2)	Gallup

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Year	Month	Item	Organization
2002	January	Drilling in Arctic	ABC
2002	January	Government size	ABC
2002	January	Environment	CBS
2002	January	Government size	CBS
2002	January	Drilling in Arctic	Gallup
2002	January	Privatize social security (v2)	Gallup
2002	July	Government size	ABC
2002	July	Privatize social security (v3)	ABC
2002	November	Environment	CBS
2002	November	Estate tax	Gallup
2002	November	Privatize social security (v2)	Gallup
2002	October	Bush tax cuts (2001)	ANES
2002	October	Estate tax	ANES
2002	October	Guaranteed jobs (non-likert)	ANES
2002	various	Govt reduce income differences between rich and poor	GSS
2002	various	Govt help poor	GSS
2002	various	Govt help pay medical bills	GSS
2002	various	Govt provide minimum standard of living	Pew
2003	April	Bush tax (2003)	Gallup
2003	August	Bush tax (2003)	Pew
2003	July	Government size	CBS
2003	May	Bush tax (2003)	Gallup
2003	May	Bush tax (2003)	Pew
2003	November	Government size	CBS
2003	October	Privatize social security (v2)	Gallup
2003	various	Privatize social security	NAES
2003	various	Reduce inequality	NAES
2003	various	Universal healthcare	NAES
2003	various	Govt provide minimum standard of living	Pew
2004	December	Privatize social security (v3)	ABC
2004	December	Minimum wage	Pew
2004	June	Government size	ABC
2004	October	Guaranteed jobs (multi-point scale)	ANES
2004	October	Universal healthcare (ANES- v2)	ANES
2004	various	Govt reduce income differences between rich and poor	GSS
2004	various	Govt help poor	GSS
2004	various	Govt help pay medical bills	GSS
2004	various	Estate tax	NAES
2004	various	Health care for children	NAES
2004	various	Privatize social security	NAES
2004	various	Reduce inequality	NAES
2004	various	Universal healthcare	NAES
2005	April	Privatize social security (v3)	ABC
2005	April	Privatize social security (v2)	Gallup
2005	January	Privatize social security (v3)	ABC
2005	January	Government size	LATimes
2005	July	Spending on the poor	Pew
2005	July	Universal healthcare (v2)	Pew
2005	June	Drilling in Arctic	ABC
2005	June	Nuclear power	ABC
2005	June	Privatize social security (v3)	ABC
2005	March	Privatize social security (v3)	ABC
2005	March	Privatize social security (v2)	Gallup
2005	March	Drilling in Arctic	Pew
2005	March	Drilling in Arctic	Pew
2005	September	Drilling in Arctic	Pew
2006	February	Federal spending on renewable energy (2006)	Pew
2006	January	Universal healthcare (v2)	CBS
2006	January	Drilling in Arctic	Pew
2006	July	Minimum wage	CBS
2006	June	Privatize social security (v2)	Gallup
2006	March	Minimum wage	Pew
2006	May	Drilling in Arctic	CBS
2006	May	Drilling in Arctic	Pew
2006	October	Environment	CBS

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Year	Month	Item	Organization
2006	October	Health care for children	CBS
2006	September	Expand Medicaid for children	ABC
2006	September	Health care for children	ABC
2006	various	Govt reduce income differences between rich and poor	GSS
2006	various	Govt help poor	GSS
2006	various	Govt help pay medical bills	GSS
2006	various	Govt responsibility to provide health care for sick	GSS
2006	various	Govt responsibility to provide jobs for all	GSS
2007	April	Drilling in Arctic	ABC
2007	April	Drilling in Arctic	CBS
2007	April	Environment	CBS
2007	February	Expand Medicaid for children	CBS
2007	February	Health care for children	CBS
2007	January	Minimum wage	ABC
2007	January	Minimum wage	Pew
2007	November	Government size	Pew
2007	October	Government size	ABC
2007	September	Expand Medicaid for children	ABC
2007	September	Health care for children	ABC
2007	various	Govt provide minimum standard of living	Pew
2008	April	Raise taxes on wealthy	CBS
2008	December	Auto bailout	ABC
2008	December	Expand Medicaid for children	ABC
2008	December	Health care for children	ABC
2008	December	Auto bailout	CBS
2008	December	Auto bailout	Gallup
2008	December	Auto bailout	Gallup
2008	December	Auto bailout	Pew
2008	February	Drilling in Arctic	Pew
2008	February	Federal spending on renewable energy (2008)	Pew
2008	July	Clean energy bill	ABC
2008	July	Nuclear power	ABC
2008	June	Federal spending on renewable energy (2008)	ABC
2008	June	Government size	ABC
2008	June	Drilling in Arctic	Pew
2008	March	Government size	CBS
2008	November	Auto bailout	Gallup
2008	October	Guaranteed jobs (multi-point scale)	ANES
2008	October	Government size	Pew
2008	September	Drilling in Arctic	Pew
2008	September	Federal spending on renewable energy (2008)	Pew
2008	various	Universal healthcare (ANES- v2)	ANES
2008	various	Govt reduce income differences between rich and poor	GSS
2008	various	Govt help poor	GSS
2008	various	Govt help pay medical bills	GSS
2009	April	Limit GHG emissions (v1)	ABC
2009	April	Government size	CBS
2009	April	Federal spending on renewable energy (2009)	Pew
2009	August	Nuclear power	ABC
2009	December	Affordable Care Act (v1)	ABC
2009	December	Clean energy bill	ABC
2009	December	Clean energy bill	ABC
2009	December	Limit GHG emissions (v1)	ABC
2009	February	Recovery Act	ABC
2009	February	Recovery Act	CBS
2009	February	Recovery Act	Pew
2009	July	Universal healthcare (v2)	CBS
2009	July	Clean energy bill	Pew
2009	July	Clean energy bill	Pew
2009	June	Clean energy bill	ABC
2009	June	Government size	ABC
2009	June	Limit GHG emissions (v1)	ABC
2009	June	Universal healthcare (v2)	CBS
2009	June	Recovery Act	Pew
2009	March	Universal healthcare (v2)	CBS

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Year	Month	Item	Organization
2009	March	Government size	Pew
2009	March	Recovery Act	Pew
2009	May	Expand Medicaid for children	CBS
2009	May	Health care for children	CBS
2009	May	Nuclear power	Pew
2009	November	Affordable Care Act (v1)	ABC
2009	November	Clean energy bill	ABC
2009	November	Affordable Care Act (v2)	CBS
2009	October	Affordable Care Act (v1)	ANES
2009	October	Clean energy bill	ANES
2009	October	Expand Medicaid for children	ANES
2009	October	Federal spending on renewable energy (2009)	ANES
2009	October	Health care for children	ANES
2009	October	Clean energy bill	Pew
2009	October	Recovery Act	Pew
2009	September	Universal healthcare (v2)	CBS
2009	September	Government size	Pew
2009	various	Govt provide minimum standard of living	Pew
2010	April	Financial reform bill	ABC
2010	April	Government size	CBS
2010	April	Government size	Pew
2010	August	Expand oil drilling	Pew
2010	August	Government size	Pew
2010	August	Limit GHG emissions (v2)	Pew
2010	February	Financial reform bill	CBS
2010	February	Government size	CBS
2010	February	Raise taxes on wealthy	CBS
2010	February	Clean energy bill	Pew
2010	February	Federal spending on renewable energy (2010)	Pew
2010	February	Government size	Pew
2010	January	Affordable Care Act (v1)	ABC
2010	January	Government size	ABC
2010	July	Affordable Care Act (v2)	CBS
2010	July	Financial reform bill	CBS
2010	June	Financial reform bill	ABC
2010	June	Federal spending on renewable energy (2010)	Pew
2010	June	Limit GHG emissions (v2)	Pew
2010	March	Affordable Care Act (v2)	CBS
2010	May	Financial reform bill	CBS
2010	May	Federal spending on renewable energy (2010)	Pew
2010	October	Government size	ABC
2010	October	Government size	CBS
2010	October	Raise taxes on wealthy	CBS
2010	October	Federal spending on renewable energy (2010)	Pew
2010	September	Government size	CBS
2010	September	Raise taxes on wealthy	CBS
2010	various	Govt reduce income differences between rich and poor	GSS
2010	various	Govt help poor	GSS
2010	various	Govt help pay medical bills	GSS
2011	April	Government size	CBS
2011	April	Raise taxes on wealthy	CBS
2011	April	Raise taxes on wealthy	Gallup
2011	February	Light bulb law	Gallup
2011	January	Affordable Care Act (v1)	ABC
2011	January	Expand oil drilling	Gallup
2011	June	Affordable Care Act (v2)	CNN
2011	March	Affordable Care Act (v2)	CNN
2011	March	Federal spending on renewable energy (2011)	Pew
2011	October	Raise taxes on wealthy	CNN
2011	October	Government size	Pew
2011	October	Minimum wage (10 dollars)	PRRI
2011	September	Government size	ABC
2011	September	Government size	Pew
2012	April	Affordable Care Act (v1)	ABC
2012	August	Government size	ABC

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Year	Month	Item	Organization
2012	August	Government size	CBS
2012	August	Raise taxes on wealthy	Kaiser
2012	December	Raise taxes on wealthy	CBS
2012	January	Government size	Pew
2012	July	Affordable Care Act (v1)	ABC
2012	March	Affordable Care Act (v1)	ABC
2012	March	Affordable Care Act (v2)	CNN
2012	March	Federal spending on renewable energy (2012)	Pew
2012	November	Affordable Care Act (v2)	CNN
2012	October	Guaranteed jobs (multi-point scale)	ANES
2012	October	Universal healthcare (ANES- v2)	ANES
2012	September	Government size	Pew
2012	various	Govt provide minimum standard of living	pew
2013	April	Keystone pipeline	Gallup
2013	December	Affordable Care Act (v1)	ABC
2013	December	Affordable Care Act (v2)	CNN
2013	December	Minimum wage (10 dollars)	ReasonRupe
2013	July	Affordable Care Act (v1)	ABC
2013	May	Affordable Care Act (v2)	CNN
2013	November	Affordable Care Act (v1)	ABC
2013	November	Affordable Care Act (v2)	CNN
2013	October	Affordable Care Act (v1)	ABC
2013	October	Affordable Care Act (v2)	CNN
2013	September	Affordable Care Act (v1)	ABC
2013	September	Affordable Care Act (v2)	CNN
2013	September	Affordable Care Act (v2)	Pew
2013	September	Federal spending on renewable energy (2013)	Pew
2013	September	Government size	Pew
2013	September	Keystone pipeline	Pew
2013	September	Limit GHG emissions (v2)	Pew
2014	April	Affordable Care Act (v1)	ABC
2014	February	Minimum wage (10 dollars)	CBS
2014	January	Affordable Care Act (v1)	ABC
2014	January	Minimum wage (10 dollars)	CBS
2014	January	Minimum wage (10 dollars)	Gallup
2014	July	Affordable Care Act (v2)	CNN
2014	June	Limit GHG emissions (v1)	ABC
2014	March	Keystone pipeline	ABC
2014	March	Affordable Care Act (v2)	CNN
2014	March	Affordable Care Act (v2)	Pew
2014	March	Keystone pipeline	Pew
2014	March	Universal healthcare (v2)	Pew
2014	May	Keystone pipeline	CBS
2014	November	Keystone pipeline	Pew
2014	November	Limit GHG emissions (v2)	Pew
2014	September	Affordable Care Act (v1)	ABC
2014	September	Affordable Care Act (v2)	Pew
2014	September	Government size	Pew

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